# PHILOSOPHY OF SCIENCE AND HISTORY OF SCIENCE

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*A Productive Engagement*

Eric Palmer

ISBN #: Softcover 0-7388-2497-6



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*This work is dedicated to the science studies program at the University of California, San Diego, which is its father, if it has any. Thanks go especially to those most influential in the forming of my ideas over the past several years, and the final argument's construction: Philip Kitcher, my advisor, Robert Westman, whom I gratefully acknowledge as my 'anti-advisor', and Gerald Doppelt, who has provided the final point of view necessary to stabilize a triumvirate. Thanks also to Paul Churchland, Michael Dietrich, Martin Rudwick, and Stephen Stich for their contributions, particularly in the early conception of the project. And more thanks to Carolyn Butler Palmer, for much more aid, on many more fronts.*

# ABSTRACT OF THE DISSERTATION

## Philosophy of Science and History of Science: A Productive Engagement

by
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Philosophy of science and history of science both have a significant relation to science itself; but what is their relation to each other? That question has been a focal point of philosophical and historical work throughout the second half of this century. An analysis and review of the progress made in dealing with this question, and especially that made in philosophy, is the focus of this thesis.

Chapter one concerns logical positivist and empiricist approaches to philosophy of science, and the significance of the criticisms levelled at them by analytic epistemologists such as Willard Quine and 'historicist' philosophers of science, especially Thomas Kuhn. Chapter two details the attempts by Kuhn and Lakatos to integrate these historicist criticism with historically oriented philosophy of science, in their separate attempts at providing rational explanations of historical developments. Kuhn's latest work seeks to mend fences with philosophy, but his efforts remain too closely

tied to the epistemological approaches strongly criticized in his earlier work. Lakatos' treatment of history is much more subtle than most have understood it to be, but the conception of scientific rationality that arises out of it is transformed into an abstract cultural product, more reminiscent of Hegel's geist than of individual human rationality.

Chapters three and four discuss the recommendations of Lakatos and Laudan to historians with regard to historiography, and the actual historiographies and philosophy of history of practicing historians and historians of science. The philosophers' contributions indicate little concern for the historians' own methods, materials, and purposes; and the historians' writings present methodologies for history of science that are independent of the normative demarcations of philosophy of science, pace Lakatos and Laudan.

Chapter five develops a philosophical position that fosters a more productive engagement between philosophy and history of science, a 'methodological historicism' that embraces the possibility of an important role for social and political factors in a philosophical study of scientific development. The epistemological relativism that might accompany such a historicist position need not be the radical epistemological anarchism of Feyerabend, though it will allow for a significant underdetermination of scientific development by reason nonetheless.

# INTRODUCTION

## I. Topic

Clearly philosophy of science and history of science both have a significant relation to science itself; but what is their relation to each other? That question has been a focal point of philosophical and historical work throughout the second half of this century, particularly noticeable in the writings of Thomas Kuhn, Imre Lakatos, Paul Feyerabend and Larry Laudan. An analysis and review of the progress made in dealing with this question, and especially that made by these authors, is the focus of this thesis.

Many authors have written concerning both sides of the relation—the impact that philosophy should have on history, and history on philosophy—and they have also reflected on their views regarding the character of their own disciplines, as those views are relevant to determining the relation. Three areas of discussion, then, present themselves, for members of both disciplines. My field of study, acquaintance with materials, and abilities will lead me to focus particularly on the field of philosophy, and the adequacy of the philosophers' development of the interdisciplinarity debate, but I will also take up the historians' end as well, and the use and abuse of philosophy in their discipline. So, for example, Lakatos holds that the best philosophy of science is that which accords with the most of the best developments of past science, and he holds that history of science can only be written under the interpretive aegis of a philosophy of science. Of course, a worry immediately looms: if history is so parasitic on philosophy of science, it is unclear just how one can know what the best developments of past science are, or know anything at all about the past of science and its fit with philosophy. Lakatos attempts some answers to the problem, but none of them strike me as nearly so methodologically sophisticated, nor so intellectually satisfying, as the rest of his analysis. Other authors present their analyses of the relations, which, I will suggest, manifest different strengths and weaknesses. I will present an attempt of my own to lay out the terms for a productive engagement from the philosopher's side in a plea for relativism at the end of the final chapter.

The general moral that I draw from my investigation, and show through the development of the thesis, will be that in such interchange between the two disciplines, the members of one discipline tend to learn a great deal from the other, gaining useful information which serves to profit them in the development of some aspects of their own self-conception of their field. When attempting to draw conclusions regarding the relevance of their field to the concerns of the other, however, they have a far inferior record, for they tend not to teach the other profitably in the exchange, try as they might, because their regard for their own aims and methods tends to distort those embraced by the other discipline—in that area, interdisciplinarity verges on colonization. I conclude that a *cautious interdisciplinarity* is called for, to foster useful discourse between philosophy of science and history of science. Each discipline might gain from the other by reducing its own naïveté, but should not attempt to build the other in its own image. Philosophy of science and history of science have related, but different goals: the one tending to a study of the character of knowledge, the other to a broader frame for the explanation of human action and past development.



## II. Outline of Chapters

### 1. Philosophy of science and the historical path: Does philosophy of science need history of science?

***Positivists and empiricist against history:*** We begin with a position that denies the importance of history of science for philosophy of science, found in the writings of logical positivists and logical empiricists. The exclusion of history has a carefully set-out basis in the theorists' conception of philosophy: philosophy of science concerns itself with science *per se*, an ideal that actual practice only approximates more or less. This relation is manifest in the positivist distinction between contexts of justification and discovery, and the idealizations of history often used by philosophers. Philosophy's *normative* relation to science further distances it from history, for case-studies and historical developments have only the status of *examples* of scientific development, whereas philosophy takes the role of an external critique, drawing its own justification from logic and epistemology.

The fall of logical empiricism is by now well examined[1], and an instructive starting point for our study. The approach suffered damaging *internal* epistemological attacks of clear and striking relevance, especially from the writing of Quine, Kuhn, and Feyerabend; more debatable is the precise importance of assertions by Kuhn and Feyerabend that the history of science can be brought to bear in epistemological criticism as well. I suggest that these 'historicist' critics exploited two areas of weakness in logical empiricism: they indicated that the development of scientific aims, method and knowledge might have a historical moment—that the assessment of theories might be a historically developing process—and they pointed up a need to reconceive the relation between the normative discipline of philosophy and its subject, as a consequence of the first point. I argue that that relation should be understood along the lines of the philosophical concepts of explication and perspicuous representation, and tied more closely to the actual historical material, and a study of the practitioners and their judgment, in the context of discovery.

All of the philosophical approaches to follow, my own and those of Kuhn, Lakatos, and Laudan, can be considered attempts at solving the problems presented to philosophy by historicist criticism, and by *The Structure of Scientific Revolutions* in particular.

### 2. Kuhn and Lakatos on the rational explanation of science: First attempts at historical philosophy of science

Thomas Kuhn and Imre Lakatos present the first substantial attempts to incorporate the lessons of historicist criticism into historically oriented philosophy of science, in their separate attempts at providing rational explanations of historical developments. Kuhn is truly a liminal figure for both philosophers and historians, and his attempts to integrate the concerns of both disciplines have led him through a complex and intriguing development. In his earliest work, Kuhn writes as an orthodox intellectual historian, taking his methodology from James B. Conant, among others. New philosophical concerns surface in *The Structure of Scientific Revolutions,* challenging both traditional epistemology and Kuhn's own historiography. Many of the conclusions in *Structure* are not, however, satisfactory to philosophers, nor to the philosopher in Kuhn: his later writings in both history and philosophy show attempts to regain ground for rational explanation that had been lost to psychological and social explanation. The attempts do not appear to succeed: Kuhn's regained rational framework actually departs very little from that of *Structure,* and where he attempts to reconcile traditional philosophy and history, Kuhn comes out with too little appreciation of the radicalness of the historicist criticisms of epistemology suggested by *Structure*. Kuhn appears to have marshalled history to the benefit of philosophy, but he himself remains too



closely tied to tradition in later philosophical development of his views.

Lakatos argues, as I mentioned above, that the justification for a position in philosophy of science is dependent on the consilience between past development and the rational reconstruction of history that the philosophy engenders. The past is very important to philosophy of science, then, and the philosophical account that Lakatos promotes, his methodology of scientific research programmes, also takes some of the historicist criticisms of *Structure* very seriously. Lakatos argues that, within a single scientific research programme, the standards of Popperian philosophy and Kuhnian normal science govern rational change; history of science is, however, also the history of a string of different, competing research programmes, ones perhaps incommensurable in their development, and so Lakatos includes further criteria for the rational elimination of research programmes. Lakatos' amendments of philosophical analysis and rational explanation are remarkably subtle, but ultimately unsatisfying, for the project falls short of several desirable philosophical goals. In particular, because the identification of research programmes and progressive development is often possible only in retrospect, his philosophy is not an analysis of the rationality of individuals' actions: it concerns an abstract cultural product, "scientific rationality", beyond the reach of scientific practitioners, and reminiscent of Hegel's *spirit*. His rational reconstructions have a similarly complex relation to both the past itself and to history as historians write it.

### 3. Does history of science need philosophy of science?: Lakatos and Laudan on history

Here I discuss a variety of explicit recommendations given by philosophers of science to historians on the subject of writing history of science, and argue that their comments indicate an inadequate conception of the concerns, resources, and methods of historians.

Though Lakatos' conception of the importance of past development to philosophy of science is original and commendable, his view regarding the importance of philosophy of science to history of science is less enlightening. Lakatos claims that history of science requires a prior philosophy of science in its historiography because 'science' is a category selected in a normative manner, and Laudan holds a similar view. I argue that the demarcation of 'science' may proceed in a variety of different ways for historians, many of which are unrelated to philosophical conceptions. These other approaches, I suggest, might also prove a useful study to philosophers, suggesting directions in which philosophy might expand to allow for an adequate accounting of scientific knowledge.

If historians were to adopt the philosophers' recommendations, I argue, their methods and concerns would likely suffer. Many, perhaps most historians, are interested in studying the production of knowledge: a historical study based in a philosophical account of progressive development would not serve that purpose, as Kuhn realizes. A factual and anthropological account of the production of knowledge could be considered a realistic historian's approach, and one which would contrast fairly clearly with Lakatos' claim that the goal of the historian is to produce a rational explanation of the growth of knowledge.

### 4. Historians and historiographies: Does history of science need philosophy of science? (Part 2)

The philosophers' concern for history of science might be grounded in doubts that the latter discipline maintains a coherent methodology. Here I proceed to examine several historians' conceptions of their own discipline, and those of philosophers of history, and attempt to elucidate general historiographies and historiographical concerns.

The philosophers' ideal of rational explanation of history can be seen to reflect the ideal of explanation expressed in Hempel's covering-law model for historical explanation. Though a non-rational sort of historical explanation which has its resemblances to



that ideal is widely practiced in history, another compelling and quite unrelated endeavor is pursued by many historians of science. I argue that the applicability of the covering-law model to historical explanation is limited, by constructing a complementary model explicating the other approach to history, and by showing the distinct status of these two historical endeavors by indicating differences in their methods, goals, and standards. What I call *prosopographic-nomological* historical explanation—an approach practiced by Merton, Price, and "second generation" Annales historians—can be seen to proceed along lines very much like those for which Hempel argues: it is heavily involved in constructing laws and utilizing them in explanation. *Sympathetic* historical explanation is an approach less easily articulated, but fundamentally different in method and pragmatics from law-explanation in that it is grounded primarily in terms of relations of meaning and understanding rather than empirical or causal relations, and has no pretensions of arguing from or of producing law-like generalities.

Philosophers of science, especially Laudan, find prosopographic history quite amenable to some purposes within philosophy of science; sympathetic history, however, requires quite a different treatment to become useful to philosophical theorizing, and appears not to be addressed by any of our philosophical authors, with the exception of Kuhn. It is a straightforward enough method of gaining knowledge about the past, and one held under healthy critical scrutiny among historians today.

### 5. A productive engagement: a proposal for science studies

I do not pretend that I have a position for philosophy of science that answers all of the reasonable goals of the different positions that I have surveyed in the philosophy of science: different approaches do have different advantages. I do, however, suggest one important direction for further development in philosophy of science: the study of the historical development of science, as opposed to a rarefied conception of scientific rationality; to be achieved partly through the study of the writings of professional historians of science. All of the philosophical authors considered above limit the advantages that a study of history may provide to philosophy, because they remain too entrenched in traditional patterns of rational and foundational argument, and very closely tied to a narrow focus upon epistemological inquiry. A position that takes historicist criticism and history's challenges more seriously is a 'methodological historicism' that accepts the possibility of an important role for social and political factors in a philosophical study of scientific development. I present a philosophical methodology to ground this historicism, and through a brief examination of a historical case, I attempt to show the importance of non-rational historical features to the progress of scientific thought. Through more traditional philosophical argument, I also attempt to show that the epistemological relativism which might accompany my historicist position need not suggest the radical epistemological anarchism of Feyerabend, though it nonetheless allows for the possibility of a significant underdetermination of scientific development by reason.

# CHAPTER 1: PHILOSOPHY OF SCIENCE AND THE HISTORICAL PATH

## Introduction

This thesis concerns the relation between history of science and philosophy of science; most especially, the appropriate and inappropriate demands which each may put upon the other. In order to examine the relation between the two disciplines, however, we must first establish that there *may be* a significant relation, for some philosophers of science deny that they need be related at all, and assert instead, through some fairly plausible argument, that philosophy can carry on without a regard for history, and, presumably, *vice versa.*[2] This chapter will engage one such challenge, and argue that philosophy of science should concern itself with history of science; that philosophy of science is improved if an effort is made to relate it to history, and if its relation to science is reconceived as well; and improved in ways that non-historical approaches to the study of science cannot duplicate. The advantages of philosophy of science for history of science, the converse, will be considered in chapters three and four.

In this chapter we will consider one philosophical approach to the study of science that presents a reasonable argument for excluding history from philosophy of science—a "pole position" in opposition to historically oriented philosophy of science—representative of many studies in both logical positivism and its heir, logical empiricism. The methodology of those approaches suggests that philosophy of science is responsible for uncovering the logical and epistemological structure of scientific argument and growth; consequently, as a "formal" discipline, philosophy in its normal development is understood to need no reference to episodes in actual history for its justification: no more than the discipline of mathematics need refer to instances of counting or adding.

I will argue, then, that these logically oriented approaches present an analysis of the relation to which historically oriented philosophers must respond; and of course, Feyerabend and Kuhn, the leaders of the historical movement with which we are concerned, saw their work as a direct response to those approaches. My goal will not be to present an accounting of the damage that their criticism does to the logical approaches, which have received enough such attention in their lifetimes; it will instead be to develop a good-sized list of significant *challenges* from the pens of these two authors, and from Quine as well, that appear to cause worries, and so at the very least require a response from newer logically oriented philosophies, and all philosophers of science henceforth. These "historicist" criticisms will be of importance throughout the thesis: various attempts to develop philosophies of science largely in response to these challenges will be the subjects of Chapters 2 and 5.

The positivists and empiricists, however, appear to have a principled response to criticism based in historical analysis; so, as well as the criticism's nature, its validity as philosophical criticism, and particularly as criticism of the logical approaches—the *spirit* of the criticism—will be considered in detail. Proponents of a philosophical approach tend to re-make philosophy of science in their own image, defining its problems in accordance with the strengths of their theories. Reichenbach, we will see, argues that the very *concerns* of historical philosophy of science are not properly philosophical, but his argument only seems compelling under the assumptions of logical positivism, and not ones which are broadly



philosophical. A sensitivity to the various advantages of different approaches to philosophy of science, I suggest, is in order. I attempt to argue that positivist and empiricist approaches, though they provide interesting logical analyses of scientific theories, fail in the role of explicating scientific development, in light of the historicist criticisms: the logically oriented approach's focus on external epistemological *justification* of scientific knowledge also detracts from another reasonable philosophical goal, analyzing the development and actual practice of science.

## Part I. Logical Positivism and Empiricism: challenges to historical philosophy of science

Logical positivism and logical empiricism—which I will often treat as one extended "logicist" programme in the philosophy of science[3]—hold an important place in our study, for they present implicit challenges to the "historicist" turn in philosophy of science, which developed as a reaction to their orthodoxy. To begin my argument for the importance of historical approaches, then, I will make an effort in this section to indicate the basis of the logicist methodology for studying science; and particularly, the basis of a tenet I believe to grow out of that framework, that philosophy of science and history of science are distinct and well-separated exercises. Logicism exhibits a reasonable, principled basis for maintaining their separation: the logicist self-conception of philosophy as an epistemological critique of science, with its own justification based *outside* of science and in logic, presents a fair argument against the importance of history, or of any actual scientific practice, to philosophy. The position will be articulated below, and historicist criticisms and an analysis of their relevance will be considered in subsequent sections.

Logicism might be considered to be a modest proposal for the division of labor in the construction knowledge: in its scheme, scientists are to develop theories and make experiment, and philosophers are to analyze and assess the formal mettle of the theories, and relate experiments as tests of theories. Logicist studies focus primarily on two areas: the study of theory structure, and the study of confirmation of those theories; and both of these areas are studied with one goal uppermost: that of constructing an account of the *logical structure* or *foundation* of the area. The two areas and one goal are intended to add up to an account of the structure of knowledge; and of course, logicists particularly concern themselves with discerning the formal structure of empirical knowledge, and science in particular. The only non-empirical disciplines that pass for knowledge are the formal sciences, mathematics and logic, and the foundations of those are to be studied in logic itself, in the philosopher's domain[4]. Presumably, one studies the logical basis of knowledge because of logic's particular lucidity or security with respect to the rest of our knowledge; it also appears to be a component, along with language, in the construction or testing of all theoretical knowledge. Thus, it is the philosopher's responsibility, among others, to explain the meanings of the following words, and determine when in practice they would be applied correctly: "empirical", "theory", "confirmation", "test", and "knowledge".[5] An alternative formulation of the proposal, then, is that scientists hold up the creative and empirical end, and philosophers hold up the formal and linguistic end of the knowledge game.

I will develop here neither a detailed account of, nor a concerted attack on, the logicist analyses of theory structure and confirmation: there are many fine studies on those topics available[6], and our central concern is the logicist account of the relation between philosophy, science, and history. The goal of formalizing the presentation of theories and regulating the use of language is not merely a descriptive enterprise for these epistemologists: the logicists' goal is to provide distinctions for critical use in the process of knowledge construction, a goal that gives the study a particular *normative* relation to its subject-field, science. The familiar career of the positivists' verifiability criterion, which was also portentously called the criterion of *meaningfulness* or *cognitive significance*, indicates the importance that can accrue to the study of words. An articulation of the relation between philosophy and science envisioned by logicism, perhaps the *locus classicus,* lies in Reichenbach's *Experience and Prediction*, wherein the author introduces the distinction between the contexts of justification and discovery expressly for the purpose of distinguishing the subject of philosophy of science—scientific theories "ranged in a consistent system"—from the study of the actual practice of science, and consequently from the history of science. To serve this purpose, how the knowledge is arrived at is *ignored*—the history of the production of knowledge is neglected—and the character or logical structure of the product itself is at the focus of attention:



> The internal structure of knowledge is the system of connections as it is followed in thinking. From such a definition we might be tempted to infer that epistemology is the giving of a description of thinking processes; but that would be entirely erroneous. There is a great difference between the system of logical interconnections of thought and the actual way in which thinking processes are performed. . . . Epistemology does not regard the processes of thinking in their actual occurrence; this task is entirely left to psychology. What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system . . . Epistemology thus considers a logical substitute rather than real processes. For this logical substitute the term *rational reconstruction* has been introduced . . .[7]

Philosophy, then, for Reichenbach, deals not with the practice of science, but with rational reconstructions of theories. To promote this end, Reichenbach and Carnap developed systems of mathematical axiomatization for the representation of rationally reconstructed and ideal scientific theories.[8] According to these systematizations, the axioms represent the scientific laws of the theory that has been reconstructed, and the axioms themselves are built of terms from three disjoint vocabulary sets, the observational, theoretical, and logical vocabularies. All theoretical vocabulary is defined (in later work "partially defined", a detail we need not consider here) with regards to correspondences with observational and logical expressions, and logic is considered true either *a priori* or by convention. Thus, the internal structure of knowledge is laid bare, and rationally reconstructed theories are built upon clearly displayed and separable logical and empirical foundations.

A statement regarding the relation of philosophy to other studies of science provided by Nagel indicates a similar view of philosophy's separation from those fields; and the focus on *language* in the passage, rather than scientific practice, underscores Reichenbach's view in this later logicist author:

> The conclusions of science are the fruits of an institutionalized system of inquiry which plays an increasingly important role in the lives of men. Accordingly, the organization of that social institution, the circumstances and stages of its development and influence, and the consequences of its expansion have been repeatedly explored by sociologists, economists, historians, and moralists. However, if the nature of the scientific enterprise and its place in contemporary society are to be properly understood, the types and the articulation of scientific statements, as well as the logic by which scientific conclusions are established, also require careful analysis. This is a task—a major, if not exclusive task—that the philosophy of science undertakes to execute. Three broad areas for such an analysis are in fact suggested . . . the logical patterns exhibited by explanations in the sciences; the construction of scientific concepts; and the validation of scientific conclusions.[9]

Nagel and Reichenbach give very clear accounts of the relation of philosophy of science to science as practiced: philosophy is intended to provide a normative and *external*[10] critique of scientific development. Philosophy gains its critical basis largely outside of science, from logic and the meanings of words—particularly the words "knowledge", and "empirical". It critically assesses the adequacy of putative instances of scientific development to the standards implicit in those words; it considers the rationally reconstructed product of science, and not the actual process of its construction. By implication, philosophy can present methodological recommendations to scientists regarding appropriate practice, especially regarding the relation of experiment to confirmation, but philosophy focuses on the 'context of justification', a position from which one reviews development to examine its adequacy, rather than the context of discovery.[11]

What of the relation between philosophy and history of science? There should be some relation—one of representation of historical fact by its rational reconstruction—but little is said by logicists on this score.[12] History probes the realm of discovery, and discovery belongs to the realm of psychology; philosophical analysis of a theory, arising out of the context of justification, concerns the epistemological warrantability of a knowledge claim without regard for its origins.[13] It is no wonder that Reichenbach and Nagel, then, maintain that history is not philosophical: for scientific practice is not philosophical, it is psychological. The extent to which rational reconstruction should be adequate to historical fact, then, *appears* to be limited to *the appropriate characterization of theories*. But even this conclusion appears to need some weakening. McMullin provides an illuminating example of the character of the resulting relation between rational reconstructions and science's history:

> If [the logician] aims to formalize Newtonian mechanics, he can scarcely do this without some reference to the documents. Yet this reference may serve only as a starting point; he may settle for some convenient textbook account of Newtonian mechanics and focus on the logical issues

> involved in it, without pausing to ask whether the system he is analyzing is really that of Newton. . . . Thus his analysis of "Newtonian" mechanics is likely to identify this mechanics with a broad class of systems, independent of any particular historical text. Yet he may after all rightly claim that his analysis illuminates (at least to *some* degree) Newton's own work, its conceptual implications and its weaknesses.[14]

Consequently, to the extent that history might be appealed to by the logicist, it is only used for the purpose of *illustration* rather than *justification* of the logical story: *perhaps* there is a place for history, as the source of situations requiring philosophical elucidation, but the justification of a logicist structural analysis is "external" to the standards of science or the course of history, for it rests in the lucidity of philosophy, language, and logic.[15]

Logicist philosophy of science, then, has a tenuous and predominantly prescriptive relation to actual science, and even less of a relation to history of science. The view of these relations does have a principled basis, in a self-conception of philosophy as an external and fundamentally epistemological *critique* of scientific development; and one that consequently ties to science through rational reconstructions of theories in an ideal context of justification. A glance at the writings of logicist philosophers indicates just how far from a study of the surface of scientific practice such analysis could wander: many of Carnap's writings, in particular, are dominated by attempts to construct ideal languages, separate theories into their theoretical, conventional, and empirical components, and to determine precise definitions of words such as "confirmation" and "testing". In "Testability and Meaning", for example, there is barely even a mention of specific scientific events and advances, because the epistemological goal Carnap sees is instead to explicate scientific advance *grosso modo*. This *extreme* rarefaction of the contact between philosophy and science as practiced, and the focus on formalization of concepts, I must stress, is not an unmotivated aberration: it is, I expect, the product of a particular view



of the state of science (or, more accurately, physics) in Carnap's times, and a belief regarding science's likely future development, and the appropriate parallel development for philosophy. Carnap presents such a belief towards the end of one of his presentations concerning formal axiomatization:

> The development of physics in recent centuries, and especially in the past few decades, has more and more led to that method in the construction, testing, and application of physical theories which we call *formalization*, i.e., the construction of a calculus supplemented by an interpretation. It was the progress of knowledge and the particular structure of the subject matter that suggested and made practically possible this increasing formalization. In consequence it became more and more possible to forego an "intuitive understanding" of the abstract terms and axioms and theorems formulated with their help. The possibility, and even necessity of abandoning the search for an understanding of that kind was not realized for a long time.[16]

## Part II. Epistemological and Historical Criticisms

Logicism was a clear philosophy of science, with principled positions regarding the appropriate relations between philosophy and science, and philosophy and history. The logicist approach had a long and distinguished career, spanning over half of a century; by the 1960's, however, it was failing, and in spite of its earlier successes as a strong and unified analytical tool, many pronounced it "dead" by the 1970's.[17] The successor theories which still fill its place—recent confirmation theory, bayesianism, and the semantic analysis of theory structure—have not been as successfully unified as were the elements of the older approach.[18]

Logical empiricism's fall can be reasonably attributed to two forms of attack to which it succumbed: internal epistemological criticism, and external charges that it failed in its explicatory task. I have placed the internal-external dividing line in this discussion in such a position that I think that logicists themselves would certainly agree that those classified as 'internal' do pose genuine problems to the approach; I will only mention a few of the important criticisms that led to logicism's fall, and particularly those relevant to the wider concerns of this thesis (a fuller accounting can be found in Suppe (1973). The internal criticism arose as a result of various developments in the field of epistemology: criticisms such as Quine's on the tenability of the analytic-synthetic distinction, and some interpretations of Kuhn's and Feyerabend's incommensurability arguments, were very effective in undermining the epistemological foundations of the approach. Those criticisms deemed 'external', on the other hand, on one interpretation might be considered to miss the point of logicism, but on another, and still philosophical one, might be considered to indicate the inherent shortcomings of the framework of logicism, specifically its inapplicability to the study of science. Criticism particularly focused on 'historicist' theses, that the aims and methods of scientists have changed greatly over time, and that those changes may be significantly affected by psychological and social features—'historical', rather than 'intellectual' considerations. On the success of some of these criticisms, the reputation of historically oriented philosophy as a viable program was largely built. The criticisms will play a large role in much of the following development of this thesis, as many of the authors that will be considered explicitly respond to them with new philosophical positions.

### Part II, §1 Internal epistemological criticisms

#### §1.1 Quine's attack on logical foundations

As early as 1936, Quine dealt a telling blow directly to the logical positivist approach to philosophy of science in "Truth by convention". All of the elements for a similar attack upon mainstream



epistemology were present in that article; however, Quine's most famous article of fifteen years later, "Two dogmas of empiricism", is the one generally acknowledged as a watershed in the criticism of both logical positivism and contemporary empiricist epistemology. I will take it that the arguments of these two articles are quite familiar, and need no extended exposition. In both pieces, Quine seriously undermines arguments for dividing statements into separate exclusive classes as analytic or synthetic, definitional or empirical; and these divisions correspond to one at the foundation of the logicists' account of theory structure, that the terms used to express a scientific theory may be meaningfully divided into three classes, the theoretical, the logical, and the observational. Quine's criticism, then, calls the logicist account of theory structure into question; it also undermines the idea that logic's certainty lies in its non-empirical status.

Quine's argument is difficult to characterize, and many authors agree that its importance to epistemology is even more difficult to analyze[19]. Though Quine does not attempt to deny the *possibility* that these distinctions hold, he does argue that attempts to draw the distinctions have been largely unsatisfactory, because the distinctions are usually explained exclusively by reference to a small circle of similar terms, notably 'substitution *salva veritate*', and 'synonymy'. These few terms, he suggests, are all usually defined by referring to others within the same set, and the interdefinition among them is so solidly closed by this means that the circle of terms itself has no empirically discernible or extensional meaning. Quine argues that, for example, substitution of terms *salva veritate* is itself insufficient to distinguish apparently analytic truths such as "All bachelors are unmarried men" from supposedly synthetic ones, such as "All creatures with hearts are creatures with kidneys". 'Creature with a heart' may be substituted for 'creature with kidneys' in English sentences just about as reliably as 'bachelor' may be substituted for 'unmarried man': the former pair have *extensional* credentials for synonymy as strong as the latter pair.[20] And an extensional distinction, one not solely grounded in the supposed meanings of words, is what the analytic-synthetic distinction is often claimed to provide. So Quine draws the conclusion:

> It is obvious that truth in general depends on both language and extralinguistic fact. The statement 'Brutus killed Caesar' would be false if the world had been different in certain ways, but it would also be false if the word 'killed' happened rather to have the sense of 'begat'. Thus one is tempted to suppose in general that the truth of a statement is somehow analyzable into a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements. But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith.[21]

Quine's conclusion, of course, calls the certainty of logical truth—or, at least, traditional accounts of the basis of its certainty—into question, because logically valid statements (tautologies) are usually understood to be true because they are either analytic, or true by definition. To replace the analytic—synthetic divide within knowledge, in the later article Quine erects an alternative holistic empiricism, in which all knowledge is empirical and theoretical, logic included. Even logic has an element of content to it, and is not immune to revision, though cognitive economy has ruled in favor of maintaining logic in much the same form at *almost* every turn.[22]

Quine's argument calls many central tenets of logicism into question. Logic is a remarkable tool for philosophical investigation, but its clarity, Quine suggests, has not arisen because knowledge of logic is different *in kind* from other sorts of knowledge.



Whether logic may be cleanly separated from an empirical component in an analysis of theories, allowing for an epistemological foundation for theoretical terms of science and an empirical aspect unmuddied-muddied by theory, is called into question. The separation that allowed philosophy the status of an entirely *external* critique of science, then, is also called into question by Quine; and at the same stroke, some doubt is cast on the idea that philosophy of science, or even epistemology proper, may best be conceived as an external critique of science. Quine, for example, opts for a radical re-orientation, away from logic and language, and towards psychology and human physiology. Quine trumpets the virtues of a "naturalized" epistemology:

> Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input—certain patterns of irradiation in assorted frequencies, for instance—and in the fullness of time the subject delivers as output a description of the three-dimensional external world. The relation between the meager input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence.[23]

**Part II, §1.2 Logical incommensurability**

Another important criticism of logicism that I see as internal is the thesis of incommensurability presented by Kuhn and Feyerabend; but it appears to amounts to different things for different interpreters, and so will be treated in several places. In this section, I will attempt to examine its importance as an internal critique of logicism, particularly as it is presented by Feyerabend, and in some aspects of Kuhn's work; I will consider *other* possible interpretations and significances, particularly others put forward by Kuhn, in the external criticism section, and in the following chapter.

The thesis of incommensurability arises out of arguments suggesting that scientific theories with different ontologies cannot be compared without misrepresenting one or both of them; they are "incommensurable" because there is no common standard against which to measure them. In "Explanation, reduction, and empiricism", Feyerabend presents the argument on three fronts: in epistemological and logical challenges to the coherence of Nagel's and Hempel's accounts of theory reduction and explanation, in historical arguments against their adequacy as explications of actual science, and in methodological arguments regarding their undesirability for application to science. for now, we will only consider Feyerabend's logical challenge.

Feyerabend's choice of opponents situates his discussion at the heart of logical empiricism: Nagel's argument is that newer physical theories assimilate the theoretical development of older ones, making the older theories special cases of the newer and broader ones; as with Newton's assimilation of Galileo's law of falling bodies.[24] Hempel's argument regarding the character of scientific explanation is similar and related, insofar as explanations involve deductive entailment from combinations of observation statements and scientific laws.[25] Feyerabend's primary epistemological argument is his analysis of the incompatibility of theories that present different conceptual relations among their terms: If the meanings of the terms differ from theory to theory, then they are *incommensurable,* and are not logically comparable, and consequently, standard theory reduction and explanation are inapplicable.

As a thesis regarding problems with reduction, the argument is most easily presented by example. Feyerabend argues that Galileo's law of falling bodies is not reducible to Newton's law of attraction because Galileo's force-analogue remains constant throughout the course of the body's fall, whereas the force of attraction varies, according to Newton's law, with respect to the distance between the bodies, which, of course, changes over the course of a fall. Over a short distance of fall, the two laws will be empirically indistinguishable—one might say that Galileo's law is empirically equivalent to Newton's, within certain bounds; but Nagel's account of theory reduction requires a *logical* compatibility, including an invariance of meaning among terms, which is not present in this case, or in most plausible candidates for theory reduction.[26] A similar problem inheres to theoretical explanation: if a scientific law is used to explain an event which has been characterized according to a different theory, such as one of the folk-theories of a common-sense understanding of the world, a parallel logical compatibility which Hempel supposes to arise in explanation[27] is also not present. Once again, for example: if one wishes to know why heavy objects fall, the terms "heavy" and "fall" must be converted to "mass" and "attract" in Newton's system, incorporating changes in meaning (such as the inclusion of the earth's mass in the relation of attraction) that break logical compatibility with the original question.

What to make of this argument? Feyerabend is certainly *not* suggesting that reduction and explanation are nonsensical concepts: clearly, once the incompatibility of the theories is acknowledged, one sees that the *point* of reducing Galileo's law to Newton's is to show that the *empirical basis* for Galileo's law is accommodated by Newton's, which might otherwise deviate from Galileo's law insofar as it is superior.[28] The point of Feyerabend's criticism, then, is to suggest a weakness in Nagel's approach: if accounts of nature are to be compared—and *some* sort of comparison is made by scientists in practice, in scientific advance—then a different tack is necessary to justify that comparison, one that overleaps the problem of logical incompatibility. Explanation is to be treated similarly: Feyerabend points out that explanations such as the one alluded to above often begin by tacitly *correcting* the formulation of a query, or even the questioner's observation.[29] Feyerabend does not find reduction and explanation to be pointless[30], he shows instead that the prevalent accounts are insufficient; and so, he and many other authors have proceeded with attempts to repair the weaknesses of these analyses, with theories of reference and pragmatic accounts of the nature of explanation.[31] Nagel himself might be seen to present a response in other sections of his discussion, concerning "non-homogeneous" reductions.[32]

As I mentioned above, there are many interpretations of the thesis of incommensurability. The one that I have just sketched I take to be the least provoking interpretation offered; it is also, however, the least contestable one, and there its merits lie. I will call it *logical* incommensurability, because, depending on one's response to it, it might be taken as little more than a logical point, and perhaps one about Nagel's and Hempel's theory of language rather than scientific theories and their comparison. Logical incommensurability might, then, be no more than a weakness in Nagel's theory of language, which would need repair, probably through a more sophisticated theory of reference, for reduction to go through.[33]

### Part II, §1.3 Data loss and explanatory loss incommensurability

Kuhn[34], like Feyerabend, presents the thesis of logical incommensurability in *The Structure of Scientific Revolutions,*[35] but he appears to have many stronger claims in mind, one of which, regarding incommensurability in scientific practice, also might be considered to be a criticism internal to logicist concerns. Those theses I will call *data loss* and *explanatory loss* incommensurability.[36] The thesis of data loss claims that different theories will account for significantly different sets of data, thus presenting problems for an accounting of their relative merits. Data loss concerns the incommensurability of theories indirectly. As we have seen, the logical incompatibility of theories that utilize different ontologies might suggest that philosophy of language needs repair, but it also implies that different theories make incompatible predictions: Newton's laws contradicts Galileo's law, as is particularly evident



for objects very far from the earth's surface. In some cases, such as this one, the difference can easily be chalked up to theoretical progress; for one can even do an (approximately) *crucial* experiment to decide the relative adequacy of the theories, and one would (I expect) find Galileo's law the inferior one in terms of accounting for the data, in situations where the contradictions are empirically discernible. The same argument, I think, goes for explanations: incommensurable changes in explanatory structure also arise as a result of changes in ontology.

Some empirical differences among theories can be attributed to progressive development; Kuhn, however, argues that in other cases the differences are straightforward non-overlapping divergences. If two theories present partially overlapping, and partially non-overlapping data-sets, then assessing the relative virtues of the theories is not so simple; even if crucial experiments between the two can be staged for areas where predictions do overlap, the non-overlapping remainder must be dealt with. Kuhn argues, though only through examples difficult to tame into philosophical criticisms, that in real scientific cases the non-overlapping aspects of competing theories can be substantial. Doppelt (1978) provides one example of data loss between Galilean and Aristotelian theories[37]; below is an example of Kuhn's regarding explanatory loss:

> Before Newton was born, the "new science" of the century had at last succeeded in rejecting Aristotelian and scholastic explanations expressed in terms of the essences of material bodies. To say that a stone fell because its "nature" drove it toward the center of the universe had been made to look a mere tautological word-play, some-thing it had not previously been. Henceforth the entire flux of sensory appearances, including color, taste, and even weight, was to be explained in terms of the size, shape, position, and motion of the elementary corpuscles of base matter. The attribution of other qualities to the elementary atoms was a resort to the occult and therefore out of bounds for science.

> . . . Yet, though much of Newton's work was directed to problems and embodied standards derived from the mechanico-corpuscular world view, the effect of the paradigm that resulted from his work was a further and partially destructive change in the problems and standards legitimate for science. Gravity, interpreted as an innate attraction between every pair of particles of matter, was an occult quality in the same sense as the scholastics' "tendency to fall" had been.[38]

Kuhn's point is that the new physicists eventually replaced one occult (unexplained) property—a stone's nature and its love of its place—with another—gravitational attraction between separated particles. Radically different sorts of explanation resulted, and judging the merits of adopting one framework rather than the other in terms of the explanatory virtues of each appears difficult. A similar problem of data loss is manifest in the transition: what counts as a *datum* for explanation changes as well.[39]

Data loss and explanatory loss appear to be problems for which logical empiricism is quite ill-equipped, a feature of scientific debate that would require a substantial revision to be accounted for. The troubles they create appear to have been dismissed in the past by allowing that these changes were the result of progressive theoretical development; but such a claim merely papers over the problem of non-overlapping domains unless some serious work on accounting for the loss is produced.[40] We will see in the following section that Kuhn argues that scientists resolve the dilemmas of experimental loss and data loss with a psychological account, based in their commitment to scientific paradigms: conceptual differences in successive paradigms yield the net result that the loss is ignored, and perhaps not understood as loss by the practitioners. On Kuhn's account, it remains an unsolved problem for philosophical accounting.



### Part II, §2 External historicist criticisms

Kuhn's last point above may be seen to present an uneasy relation with logicism: on the one hand, it suggests that logicism lacks in that it does not explicate a phenomenon that is apparently manifest in science; on the other hand, logicism's normative, explicatory, and external relation to science allows it some freedom from following every turn of science's historical development. The relation between the philosophy and its subject may be a complex one: the historical case chosen could be a freak incident, or not actually representative of good science; and since a refutation of such a historical thesis would require one to delve into history, the philosopher may also be excused from developing a serious reply. Such criticism from historical example, then, has the disadvantage that it does not target specific epistemological theses, nor does it suggest local-level revisions that remedy the problems exposed; so it might be dismissed as vague; or perhaps an interesting problem, but one that cannot be dealt with according to philosophical theory. These are reasonable—even if *ad hoc*—responses to isolated criticisms: external criticism of logicism, or such borderline internal criticism as Kuhn presents, must do better to be obviously worthy of attention.

In this section, I will attempt to bolster the case for history by providing a variety of historical criticisms together, and by indicating how they have been placed in one critical framework and explained as features of the structure of scientific paradigms by Kuhn. The framework is known as *"historicism",* and it might be described as follows: historicists suggest that philosophically salient features of science, especially the aims (goals), methods, and standards of science, are historically developing characteristics; much like theories, which are considered by historicists and non-historicists alike as developing. Such an analysis only provides a vague gesture at a clear demarcation, however, for certainly non-historicists would allow that there are *some* changes at these levels: standards of testing, for example, appear to covary with available material technology. Similarly, many historicists, and certainly Kuhn[41], maintain that all or most scientists through history have maintained some very weak universal intellectual constraints. What constitutes a historicist position, then, and what constitutes a non-historicist position, is largely a matter of degree. To clarify the explanation: historicists claim that historical development is of enough significance to scientific development that it deserves mention as a central feature in an explication of science; that is, without the analysis of historical development of aims, methods, and standards, science could not be characterized with any appropriate degree of fidelity in philosophy of science.

Kuhn argues for a historicist framework from historical cases, as I will indicate below, and he also presents a brilliant explanation of why science is a historically developing practice, rather than one that exhibits a constant, wholly rational development: science's aims, methods, and standards develop historically because the path of development of science, and of those features of science, is governed by changing *paradigms*. Kuhn's thesis of the "priority of paradigms"[42], both as guides to research and as impassable blocks to rational consensus, will take a central role in this chapter and following chapters.

### Part II, §2.1 The historicality of science

The cases that Feyerabend and Kuhn discuss above have provided some examples of historicality in science, but they were classed as internal criticisms of logicist epistemology because they nonetheless allowed for a reasonably stable conception of aims, methods, and standards in science. The problems of logical incommensurability, data loss, and explanatory loss might be taken to indicate weaknesses in the logicists' theories of language, and perhaps some more serious problems for characterizing scientific progress in the face of losses, apparent or genuine; but latter-day logicists might attempt to fight back against these criticisms with a position exhibiting a good deal of extension and alteration of the position, yet



with some fundamental similarities to earlier frameworks retained. Philip Kitcher, in *The Advancement of Science*, and Wesley Salmon, in *Scientific Explanation and the Causal Structure of the World,* might be considered to work this vein. There are other arguments, however, that call the logicist framework more directly into question, because they suggest the historicality of scientific aims, methods and standards.

***Problem-field changes and other shifts in aims:*** The case of explanatory loss incommensurability above presents a problem for the logical empiricist approach that shades towards a sharper criticism of the logicist framework with regard to the constancy of aims in science. In Kuhn's example (which I, like Kuhn, must present in a regrettably allusive and schematic manner), we see one occult property substituted for another, and a change in explanatory structure that must be accounted for; and Kitcher, for one, attempts to account for such developments in terms of practical progress in the manipulation of nature, and explanatory and cognitive progress in science.[43] To adjudicate this case, Kitcher writes:

> NATURAL MOTION [Kitcher's formal representation of Aristotle's concept] is abandoned in Newtonian mechanics. But is it correct? No. We do not regard it as formulating the correct dependencies because it wrongly suggests that differences in tendencies to rise and fall result from differences in the composition of bodies out of particular substances.[44]

Kitcher's response is eminently reasonable, and suggests that explanatory loss, to be convincingly established as a roadblock to logical empiricism, rather than a stumbling block, requires a sharper argument. But though he might be taken to make an argument for accounting for some explanatory losses in this passage[45], such episodes of change *also* exhibit both a re-arrangement of explanatory dependencies *and* an explicit change in the area of concern—the *problem-field*—of a field of science. In passages such as the one above, Kitcher attempts to account for changes in explanatory dependencies in terms of improvements in our grasp of the natural dependencies in the world, but explaining problem-field *shifts* appears to be a much more difficult task. A significant field shift appears to occur in this example, as Kuhn makes evident in a later paper:

> When the term "motion" occurs in Aristotelian physics, it refers to change in general, not just to the change of position of a physical body. Change of position, the exclusive subject of mechanics for Galileo and Newton, is one of a number of subcategories of motion for Aristotle. Others include growth (the transformation of an acorn into an oak), alterations of intensity (the heating of an iron bar), and a number of more general qualitative changes (the transition from sickness to health). As a result, though Aristotle recognizes that the various subcategories are not alike in *all* respects, the basic characteristics relevant to the recognition and analysis of motion must apply to change of all sorts.[46]

The implication, then, is that a reorientation of aims for a field also may found its development: later European physical thinking gained its superiority partially as a result of a shift in aims.

*Problem-field shifts are not brought up here to indicate a shortcoming of scientific development;* they are intended to indicate that aims may and do in fact change, and that an accounting of scientific development ought consider such changes. If the claim that a problem-field shift has occurred is taken as a criticism of scientific development, as an argument that Newton's solution was regressive, then certainly there is a good answer to the charge: *on balance,* late 17th century physicists, or perhaps early 18th century physicists, found Newton's theory scientifically superior to Cartesian theory, and Aristotelian theory. Presumably, this was so particularly because of the advantages accruing to a theory of universal gravitation, despite its occult properties; and presumably a similar assessment of the virtues of Newtonian theory is to be found among the



present day analysts referred to in Kitcher's quote. The development was not, then, regressive at all; one might call this an appeal to *global gains* to explain genuine local losses. But the point of the problem-field shift argument, I think, is to show that the case does nonetheless manifest a change in aims that needs explanation; and the century-long move from Aristotelian approaches to Newtonian ones *might well* have been regressive from in Aristotle's mind, were Aristotle to hear of it, because Aristotle had different aims for his theory, aims not satisfied in Newton's approach.

One more example might be appropriate: consider the following quote from Newton,which indicates the relevance of religion to the acceptability of his system:

When I wrote my treatise [Principia] about our Systeme I had an eye upon such Principles as might work wth considering men for the beleife of a Deity . . . [47]

Newton appears to have felt that religious issues provided support for his system, that they lay within the relevant evidential domain; and similar references to the relevance of religious beliefs in science, and especially the written word of the Bible, are easy to come by.[48] But these concerns have, over time, been ruled out as irrelevant in science, and such a shift in the relevant evidential domain of a theory introduces a variety of new problems into theory appraisal.

Kitcher does not consider problem-field shifts in his discussion of the case of natural motion, because Kuhn only concerns himself with explanatory loss in his (1983), the article that Kitcher refers to. What would Kitcher's response be, then? I expect that he would bring up globality again, as I have done in the above paragraph: *more global* features of scientific practice, especially global values, govern this apparent change in scientific aims. Kitcher often appeals to permanent goals in science that guide development, particularly, the goal of attaining significant truth. [49] Kitcher would, I expect, argue that such a change was not a basic change in aims, but a strategic alteration of means towards that most general goal, arising as a result of a re-assessment of the *attainability* of a project such as Aristotle's.[50]

Such an argument, an appeal to global values to establish the stability of an apparent shift in aims, may have its merits; a detailed historical argument about the transition is called for to seal it. Kitcher does have a plausible case for his position: what appears to be a saltation in the extreme long-term might be a much tamer process of negotiations at the micro-level, as Laudan in particular is at pains to stress, and changes in theory due to gains in knowledge about the world do seem appropriate enough in science! The appropriateness of this response will be considered in Part III of the chapter; I will suggest that such a reply is promising enough on its own (logicist) terms, but may misrepresent other reasonable projects available to philosophers of science. I will also indicate shortly that Kuhn presents an argument that competes with Kitcher's: he has argued that on occasions of crucial importance to scientific development, such reorientation is not the result of a rational process of adjudication, but is rather the result of differences in scientific paradigms, a product of the psychology of science. The debate will not, of course, be resolved by presenting such an argument, but I think the plausibility of Kuhn's position can be established.

***Changes in standards and methods:*** Kuhn suggests that, along with the changes in aims that are most clearly manifest in problem-field changes, science also exhibits historical changes in its methods and standards of practice. In one example, Kuhn indicates how standards of argument may vary with theoretical commitments:

> At the end of the eighteenth century it was widely known that *some* compounds ordinarily contained fixed proportions by weight of their constituents. . . . But no chemist made use of these regularities except in recipes, and no one until almost the end of the century thought of generalizing them. Given the obvious counter-instances, like glass or like salt in water, no generalization was possible without an abandonment of affinity theory and a reconceptualization of the chemist's domain. That consequence became explicit at the very end of the century in a famous debate between the French chemists Proust and Berthollet. The first claimed that all chemical reactions occurred in fixed proportion, the latter that they did not. Each collected impressive experimental evidence for his view. Where Berthollet saw a compound that could vary in proportion, Proust saw only a physical mixture. To that issue neither experiment nor a change of definitional convention could be relevant.[51]



As with arguments concerning problem-field changes, such a claim *by itself*, I think, should not be interpreted as an argument that chemists of the time could not reasonably judge the relative merits of the two positions: it merely represents an argument that each theory is, by reason of its structure, partly sheltered from criticism presented by the other.

This case of a change in standards is tied to a change in theory: Kuhn refers to these changes as "holistic", for he maintains that the shifts will often occur on all three levels at one time. The holism arises from an interdependency among theoretical commitment, aims, methods, and standards such as the interdependency involved in the above example; and changes in aim and theory might be seen to be the usual causes of changes at the other levels. In extending his case-study of chemistry, Kuhn indicates a parallel shift of aims and methods:

> It was to determine [the sizes and weights of atoms] that Dalton finally turned to chemistry, supposing from the start that, in the restricted range of reactions that he took to be chemical, atoms could only combine one-to-one or in some other simple whole-number ratio. That natural assumption did enable him to determine the sizes and weights of elementary particles, but it also made the law of constant

> proportion a tautology. For Dalton, any reaction in which the ingredients did not enter in fixed proportion was *ipso facto* not a purely chemical process. A law that experiment could not have established before Dalton's work became, once that work was accepted, a constitutive principle that no single set of chemical measurements could have upset.[52]

***Conclusion regarding criticisms of logicism:*** Logicism has insulated itself from historicist criticism by effectively removing itself from certain possible relations to history of science and scientific practice. The isolation, I have suggested, is made explicit through a two-fold separation: philosophy is removed from science so far as it purports to be a normative, external, epistemological critique of science, and so far as it is a study of an idealization of science, itself situated in the context of justification. The isolation is justified to the extent that these features of the logicist approach serve it well in the pursuit of certain philosophical goals: particularly, the goals of analyzing the epistemological character of, and providing an epistemological justification for, science's product.

The relevance of the above criticisms to logicist philosophy of science should, I expect, be clear: shifts in problem-field, methods, and standards of science would appear to be very important features of scientific development, and features to be accounted for in an epistemological study intended to shed light on the growth of scientific knowledge. Logicism was developed primarily with the goal in mind of representing scientific theories and the nature of epistemologically unproblematic confirmation; the historical cases that Kuhn examines, however, suggest that such accounts of theories and confirmation are simply not sufficient as accounts of the character of scientific development or scientific knowledge: much more needs to be said towards explaining scientific growth. The structure of a theory appears to have *less* relevance to its acceptance *in practice*, in some regards, than logicists have suggested; and confirmation *in practice*, despite the epistemologists' efforts, appears to be far from the epistemologically unproblematic process envisioned. Though logicist analyses may hold appropriately as idealizations for a great deal of historical development in science, Kuhn's cases suggest that they do not always hold, and do not hold in cases that, it would appear, are some of the most profoundly important in the shaping of scientific knowledge. A study of scientific knowledge that would have nothing to say concerning these events would be at best limited, and at worst idle.



### Part II, §2.2 "The priority of paradigms"

***Paradigms:*** Kuhn's criticisms are, perhaps, a mixed bag of persuasive and less persuasive worries about the limitations of the logicist approach and other approaches in the philosophy of science, based in a few salient historical incidents. Each would appear to require either a careful refutation, or a significant modification to philosophical theory; but their importance is greatly enhanced when they are seen as arising out of one analysis of the mechanism behind the troublesome historical features that lie at the heart of the criticism. Kuhn presents an analysis of science which, we might say, serves to replace the older dream of a logic of scientific discovery—one which purports to *explain* the problematic historical developments grounding historicist criticism.

The feature that promises the advantages of unifying and explaining historical shifts in aims, methods, and goals is the scientific paradigm. What precisely a paradigm amounts to has been up for a good deal of discussion since *The Structure of Scientific Revolutions*[53]: the details concerning how the vaguely sketched concept might be tamed are less important at this point in my discussion than is an explanation of how the paradigm serves to answer the historical puzzles that Kuhn has pointed out; and so, such details will be treated in chapters to come. Here is Kuhn's introduction of the concept, immediately preceding his introduction of the term:

> Aristotle's *Physica,* Ptolemy's *Almagest,* Newton's *Principia* and *Opticks,* Franklin's *Electricity,* Lavoisier's *Chemistry,* and Lyell's *Geology*—these and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. They were able to do so because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to solve.[54]

A scientific paradigm, then, to a first approximation, is a momentous discovery, a "concrete scientific achievement" that catches the attention of practitioners, and also provides a new direction for scientific research, and a "shared locus of scientific agreement".[55]

Kuhn stresses the presence of a *dual* aspect to the paradigm: the pair of a recognized concrete achievement and an unfinished project indicate both a *start position* and a *direction of development* for future activity. The paradigm provides the foundation and starting point of a tradition of activity, a research programme that is drawn out of its promise. Kuhn calls this a "normal science" research tradition; the details concerning how a paradigm inspires the development of a research tradition I will here present only insofar as they affect our points of concern here (for more on the 'poetic' qualities of paradigms, see Kuhn, Ch. 3). The paradigm itself, of course, has grown out of some sort of prior tradition as well, but it is also in some respects extraordinary, and set off from the tradition:

> Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute. . . . The success of a paradigm . . . is at the start largely a promise of success discoverable in selected still incomplete examples. Normal science consists in the actualization of that promise . . .[56]



A new paradigm, then, is adopted both because of its own successes and its perceived promise for future development. It provides a plan of action for further research; but it also provides much more.

***Cognitive influences:*** By its illumination of a path for future work in normal science, the paradigm provides the key that Kuhn uses to unlock the historical puzzles so troublesome to logicism. Paradigms guide development, but in so doing, he argues, they *also* augment the supply of methods and standards utilized to determine further theoretical development and experimental work. Different paradigms augment the shared methods and standards in different ways, and the historical conflicts mentioned in the preceding sub-section, Kuhn claims, are to be accounted for in terms of cognitive influences exerted by different paradigms.

My analysis above, and Kuhn's examples, should indicate the path of his argument. Kuhn maintains (like Kitcher) that there are rational principles governing debate and development in science, "commitments without which, no man is a scientist"[57]; those rules, however, are not themselves sufficient to determine a consensus on theory choice in many situations in which the products of two paradigms, of two relatively independent research traditions, are compared. If shared commitments tell practitioners to improve precision and scope, how are they to make a judgment between Aristotelian mechanics and Newtonian mechanics? The former surely wins on the measure of scope, enfolding a much broader conception of motion; the latter appears to be more accurate in its predictions. Which one wins on the scale of empirical adequacy, of saving the phenomena? Kuhn suggests that such a question really cannot be answered: the opposing virtues of scope and accuracy must be weighed. Should the Aristotelian position, which was intended as a qualitative analysis, be held to the standards of a

Newtonian quantitative analysis? Better—should the Newtonians be held to the standards of Aristotelian analysis, and be asked to give an explanation of gravitation?

Certainly Newton and the Newtonians had their views on these matters, and found their solutions superior to the Aristotelian approach, by then far out of development and out of date. Kuhn mentions that a more direct challenge to the Newtonian account of gravitation was provided by Cartesian views, which gave a mechanical analysis to terrestrial and celestial mechanics that the Newtonian view lacked. Kuhn's point in comparing two very dissimilar views, however, I take to be that making assessments of the *relative value* of the knowledge claims presented under such divergent paradigms appears much more difficult, *and perhaps a less reasonable enterprise*, than the much simpler task of assessing the *respective virtues* of the theories. The decision as to value, he suggests, is underdetermined by shared reasons, and determined by the conjunction of those shared reasons and others provided by the paradigms; for different paradigms provide the reasons behind the historical disagreements in aims, methods, and standards:

> Successive paradigms tell us different things about the population of the universe and about that population's behavior. . . . But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared "unscientific." . . . And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.[58]



**Part III. Kuhn's philosophical argument**

The argument that I have constructed above is something of a rational reconstruction of several aspects of Kuhn's argument in *The Structure of Scientific Revolutions.* I have attempted to separate out specific features: specific criticisms of logical empiricism, historical theses concerning the process of historical development in science, and conclusions regarding the causes of development in scientific paradigms. What remains to be considered is how precisely these arguments might be considered to affect philosophy of science.

***Where is the argument?:*** Kuhn clearly intends to link changes in paradigms to changes in aims, methods, and standards, and to incommensurability; but the outline that I have sketched above of his analysis of the importance and priority of paradigms may appear to constitute more of a statement than an argument. Where is the argument for this grand scheme, which impinges on history, philosophy, sociology, and psychology of science? And particularly, for our concerns in this chapter, what is the philosophical content of the argument? Portions of Kuhn's argument, I believe, are difficult to characterize in traditional philosophical terms because they are primarily rooted in an approach from history, and one which questions traditional philosophical approaches. I will also suggest in the following chapter that Kuhn is not in the end attempting to produce a straightforwardly philosophical position in *The Structure of Scientific Revolutions:* he instead proposes an approach that is an intriguing polyglot of intellectual history, philosophy, and psychology.

Significant internal philosophical criticisms of the logicist approach are, I suggest, provided by Kuhn in the various incommensurability theses that he promotes. Logical incommensurability suggests the need for an improved account of theory comparison, perhaps in an improved philosophy of language. Data loss and explanatory loss would appear to require more substantive responses; these might be responded to by lesser adjustments than the adoption of the paradigm model of scientific practice, however, as Kitcher suggests. The historicist arguments regarding shifts in aims, methods, and standards, on the other hand, appear to require more serious treatment; and we will return to the Kitcherian response I mentioned above shortly.

Kuhn hasn't much of a standard philosophical argument to establish the epistemological significance of these shifts; and he doesn't have one, I expect, partly because the shifts cut sharply against traditional conceptions of philosophy of science and epistemology. By arguing that the aims of the scientific enterprise shift, Kuhn is attacking conceptions of the nature of science's product as a unified and single object; he is attacking *universal* conceptions of scientific knowledge of all kinds, and not merely theses regarding cumulativity. In arguing for the *impotence* of shared aims in science, Kuhn effectively argues for an *incommensurability of knowledge claims* across paradigm shifts that undermines logicist, and broader foundationalist, epistemological approaches.

Many other contemporary attacks on foundationalist analyses of knowledge support Kuhn's efforts, not the weakest of which is Quine's, discussed above. Quine, we have seen, recommends a shift towards a naturalized approach, to a study of epistemology as a specific branch in psychological theory. In arguing for differences in methods and standards, however, Kuhn also undermines this sort of *reliabilist*[59] epistemology. Kuhn's resolution of the problems posed by his historicist criticisms, then, is based in neither of these approaches, and if it is to be considered epistemology, it must be an epistemology radically reconceived. What, precisely, Kuhn attempts to replace traditional epistemological analysis of science with will be considered in the following chapter. It is clear at this point, however, that Kuhn's views on aims suggest that it must be a form of *epistemological relativism*, the view that there



exist no universal rational standards by which knowledge claims can be judged.

Kuhn might have presented more clearly-framed philosophical criticism of his philosophical opponents, and later apologists for Kuhn, such as Doppelt[60], do trim his arguments towards those directions; but Kuhn is concerned with many audiences, and particularly historians as well as philosophers. I have attempted to separate Kuhn's arguments into more straightforward criticisms of philosophy, but I have not attempted to augment or repair them here, and Kuhn has, I think, provided fair enough criticism. His argument that aims, methods, and standards do shift is historically stated, but the philosophical implication, particularly for logicism, is clear, and I don't think the onus is on him to strengthen or re-develop his argument to establish its plausibility as philosophical critique, despite its being external to and critical of traditional approaches in the philosophy of science.

Kuhn's conception of the scientific paradigm, as a guide to development and a necessary source of scientific values, on the other hand, provides an attack on traditional philosophical approaches that is radically at odds with them, and harder to classify as philosophical criticism. For Kuhn does not develop it as criticism, but rather as *an explanatory framework developed in opposition to traditional epistemology.* The approach to the analysis of scientific knowledge involving paradigms competes with traditional epistemology because it suggest that *extra-rational* criteria must be taken into consideration in order to understand the character of scientific development and the establishment and nature of scientific knowledge: the choice of paradigms cannot be based solely on "good reasons"[61] because of a paucity of universally shared aims, methods, and standards; these paradigms, however, provide some of the cognitive tools—some of the aims, methods, and standards—necessary for producing and assessing science's product, scientific knowledge. As an alternative frame, Kuhn's stands to be assessed less as a criticism of other approaches, and more upon its own merits as explanation. And I do think that Kuhn's analysis of the significance of paradigms provides the basis for an explanation of the putative phenomena of shifting aims, methods, and standards in science. Much of the argument in the balance of this thesis, and particularly Chapter 5, will work towards showing the advantages of the approach.

***Shifting aims, methods, and standards:*** The historical shifts in aims, methods, and standards that Kuhn finds in scientific activity are on his analysis due to the cognitive influences of paradigms upon practitioners. I have suggested that Kitcher would reply to Kuhn's view by arguing for the presence of more global aims, more general rules guiding these changes. Has Kuhn a reply?

Kuhn's argument for the priority of paradigms over rules begins, like others, from the discipline of history, but this time in a generalization regarding historians. Kuhn begins with the claim that historians have had difficulty finding universal rules of practice that govern scientific communities, under any conditions of practice, within research traditions as well as in cases of conflict. He then suggests that no such rules need exist: "Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research. Normal science can be determined in part by the direct inspection of paradigms, a process that is often aided by but does not depend upon the formulation of rules and assumptions."[62] This appeal to history might itself constitute a fair enough empirical argument, though Kuhn does not go to great efforts to explicitly defend it. Kitcher's argument, however, could be clarified at this point: rules may guide development, in a reasonably clear sense, though they might not be formulated explicitly by practitioners. Knowing what scientists do, and saying what they do, are two different things; the former is the scientist's job, the latter the philosopher's and historian's.

Kuhn does, however, have more arguments, though they are roughly stated. His view is that the scientist's way of knowing a discipline may be built out of a 'hands-on' experience in the lab, indoctrination, experience, and familiarity with a paradigm. Here is a sample:



> Scientists work from models acquired through education and through subsequent exposure to the literature often without quite knowing or needing to know what characteristics have given these models the status of community paradigms. And because they do so, they need no full set of rules. The coherence displayed by the research tradition in which they participate may not imply even the existence of an underlying body of rules and assumptions that additional historical or philosophical investigation might uncover.[63]

Scientific practice might, then, proceed as a *practice acquired through acculturation*, and acquired, perhaps, *independently* of formulated, and even of formulatable rules. In addition to detailing a variety of acculturation practices that he sees as operative in scientific education and practice, Kuhn alludes to Wittgenstein's philosophical defense of non-rule-bound practices, and specifically his analysis of concepts.[64]

Kuhn has not provided a direct attack on the argument for global aims; but he has, I think, established to a reasonable degree of plausibility that practice in each research tradition and in the comparison of them, may be underdetermined by shared rules, and might, in important respects, not be rule-bound. Perhaps a response can be made to this position with an appeal to universal values; one such as I expect would be suggested by Kitcher, to be carried out on the 'turf' of history. It is clearly not to Kuhn's credit that the historical work that he presents in his book is largely drawn in sketchy gestures, and is not profoundly comprehensive, nor terribly convincing. On the other hand, Kuhn should not be portrayed as deserving all of the burden for producing sonorous historical backing for his views, for he also provides *some* well thought-out philosophical foundations, in the various plausible interpretations of incommensurability and the arguments concerning shifting aims, and persuasive argument of other sorts in his references to practices of acculturation and in his unifying conception of the explanatory power of scientific paradigms.

**Part IV. Conclusion**

Kuhn's argument suggests that a logicist analysis, or any rational analysis of the components of scientific practice, will not satisfactorily explain the development of scientific practice, nor the nature of scientific knowledge. His development of the concept of the scientific paradigm, I believe, embodies an attempt to develop a view that will suffice to explain development. Paradigms do the job of explaining how commitment to a scientific theory is formed for the scientist, and also explain why one cannot expect that a rational explanation of scientific commitment, even in a context of justification, is forthcoming: for the paradigms provide extra rules beyond universally shared rules of practice that are necessary to establish commitment to one theory rather than a present alternative; and while paradigms are held for good reasons, good reasons will not determine the choice of one paradigm over another present alternative.

Kuhn has just a little to say on what, besides good reasons, guides the choice of paradigms, and I will lay out his examination of the basis of paradigm commitment in the coming chapter. But it appears to me that he is quite tight-lipped with regards to explaining this very global commitment that guides so many moves for a scientist. And this direction I take to be a most interesting one for development in philosophy of science: if reasons don't suffice to guide paradigm choice (or theory choice, if we wish to accept Kuhn's historicist criticisms, but hedge on the explanation provided by paradigms), then we are naturally led to wonder what else aids it. I will suggest in the final chapter that extra-rational factors of the broadest sort—socio-economic, geographical, political, and historical—may be profitably called upon to fill the void; and perhaps they are all that is available to fill that void. The importance of history to philosophy on my scheme, then, should be obvious.

Kuhn wrote an entire book to argue that theory commitment



is guided by paradigm commitment; so why is he relatively quiet on the matter of explaining the sources of paradigm commitments? Kuhn's views make his philosophy of science, as well as his criticisms, "historicist" in the common terminology. Generally, a *historicist* interpretation of science I will take to be one that maintains that science, in one or more aspects, has been in practice—and consequently should be considered to be in explication—a historically developing process, one changing in an ordered manner.[65] As I have indicated before, Kuhn's historicist approach, particularly his belief in a historicism of aims, leads him also to epistemological relativism. But Kuhn found there to be great dangers lying in that direction. Richard Burian points out the danger in a review of developments since logicism, in mentioning one relativist position, "strong historicism": the position that "There are no universally valid methodological and epistemological standards by means of which both science in general and the special sciences may be evaluated."[66] Feyerabend's *Against Method* is, of course, the obvious representative for this position, which is explicitly defended in the book both as a historical and as a philosophical thesis: Feyerabend argues that "anything goes" appropriately characterizes the greatest of scientific accomplishments, such as Galileo's astronomical work, and also leads to the most fruitful proliferation of debate within science. Feyerabend's position is not quite historicist on the above definition: his own term, *"epistemological anarchism",* is more appropriate, for whereas a historicist such as Kuhn finds patterns or development in history (pre-normal, revolutionary, and normal science), and might find scientific practice largely though not completely constrained by reasoned activity, Feyerabend argues for no such positions. Epistemological anarchism is a degenerate form of historicism, in the mathematical sense of 'degenerate': one parameter, *order*, is at zero value. The possibility that historicist philosophy's relativism might degenerate into an epistemological anarchism, I think, may have held Kuhn back from freely discussing the extra-rational, social sources of theory commitment in science, and have pushed him to an active retreat from the radical aspects of his views in later works. Avoiding epistemological anarchism in the analysis of a scientific practice underdetermined by good reasons, then, is the subject of efforts by Kuhn and Lakatos, to be considered in the following chapter.

# CHAPTER 2: KUHN AND LAKATOS ON THE RATIONAL EXPLANATION OF SCIENCE

## Introduction

We have seen that Kuhn, in *The Structure of Scientific Revolutions,* presented some sharp criticisms to traditional philosophy of science. Some of these were cast in the previous chapter as internal epistemological criticisms of logical positivism and empiricism, and some were presented as the "historicist" criticisms that appear to call for a new framework for philosophy of science, particularly one that reconceives the relation of history of science to philosophy of science, and the normative relation between philosophy and actual science. We now turn to the solution to those criticisms and the reconception of those relations offered by Kuhn and Lakatos.

### Part I. Kuhn's Progress

Kuhn is a particularly interesting author to study with regards to these issues, for he engages in discussion in both history and philosophy of science, but is a liminal character for both disciplines. This is likely to surprise most philosophers, who, in my experience, tend to classify him as a historian first and foremost. When this opinion is brought to the attention of historians of science, however, they often disagree, for many (perhaps most) have all along assumed the opposite![67] If we look at Kuhn's *opus* in chronological order, as I intend to in this chapter, we find that Kuhn alters his views and affiliations greatly over the years, and I find it a useful approximation to divide his development into three stages. Most of Kuhn's earliest writing is produced from the standpoint of an orthodox intellectual historian, a methodology drawn, perhaps, from Kuhn's mentor, James B. Conant. *The Structure of Scientific Revolutions,* by contrast (according to Kuhn himself[68]), presents a critique of this form of historiography as well as a critique of the logical empiricist approach to philosophy of science. What Kuhn replaces it with is an explanatory form that is a mix of intellectual history, philosophy, sociology, and psychology. Finally, in writings after *Structure*, Kuhn attempts to tame the beast he has created, clarifying and developing the aspects of his views that lay as close to the lines of traditional history of science and philosophy of science as his insights allow.

Kuhn's progress is particularly interesting because Kuhn develops views along the lines of both history and philosophy of science that interact and grow in tandem: they may be considered to be two aspects of one account of scientific development. In his construction of his new frameworks, however, Kuhn does not provide sufficient replies to all of the criticisms of philosophical and historical approaches that *The Structure of Scientific Revolutions* suggests, according to the development of them in the previous chapter.

**Part I, § 1 Kuhn I: The intellectual historian**

We see Kuhn's central historiographical approach in his first book, *The Copernican Revolution,* in passages such as the following account of Kepler's determination of the elliptical orbit of Mars:

> A long series of unsuccessful trials forced Kepler to conclude that no system based upon compounded circles would solve the problem. Some other geometric figure must, he thought, contain the key. He tried various sorts of ovals, but none eliminated the discrepancies between his tentative theory and observation. Then, by chance, he noticed that the discrepancies themselves varied in a familiar mathematical fashion, and investigating this regularity he discovered that theory and observation could be reconciled if the planets moved in elliptical orbits with variable speeds governed by a simple law which he also specified. These are the results that Kepler announced in *On the Motion of Mars,* first published at Prague in 1609. A mathematical technique simpler than any employed since Apollonius and Hipparchus yielded predictions far more accurate than any that had ever been made before. The problem of the planets had at last been solved, and it was solved in a Copernican universe.[69]



This short quote presents a fair epitome of Kuhn's early effort. An individual, Kepler, is presented with a problem situation, determines an approach to the problem, and comes up with a solution. Note the rapidity with which the historical ground is covered; note the questions *not* addressed. First, the problem situation which Kepler grapples with appears to be a natural direction for research in the science: it is, one might say, taken as a naturally occurring problem, one *internal* to the field: why Kepler chose it for a focus receives little (or no) attention. Then, only the barest outline of an account of how the solution was arrived at is offered: Kuhn appears to assume that his reader will grant that the details of the process would be clear enough, and evident upon investigation. Finally, once the solution to the problem has been found the individual is lost, and the "mathematical technique . . . yield[s] predictions": a shift to passive voice announces that the problem "has been solved". The solution is no longer Kepler's, it stands on its own: his is only the credit for finding the solution. Presumably, if a story is to be told for acceptance by the community, as one was for the discovery, it is that all other rational individuals see the significance of the problem, note the solution, and accept the development into the fold of the science; since on occasions when they do not, irrationality has banned the solution. For such a case, compare Kuhn's explanation of some of the opposition to Galileo's telescopic evidence for Copernicanism:

> A few of Galileo's more fanatical opponents refused even to look through the new instrument, asserting that if God had meant man to use such a contrivance in acquiring knowledge, He would have endowed men with telescopic eyes. Others looked willingly or even eagerly, acknowledged the new phenomena, but claimed that the new objects were not in the sky at all; they were apparitions caused by the telescope itself. Most of Galileo's opponents behaved more rationally.[70]

My purpose in pointing to these passages is not to make light of Kuhn's abilities or of the quality of his analysis of the assimilation of Copernicanism: A few chosen quotes will not show, for example, the very wide variety of factors that he feels have contributed to the adoption of Copernicanism, nor in what way they contributed. Kuhn's book is a notably important and useful work of history, still considered worthy of attention by historians and philosophers. It is not at all subtle, however, in *some* aspects of its historiography. What Kuhn can achieve under the constraints of his historiography is marvelous; what walls the approach throws up that he *cannot* scale is my focus.[71]

Kuhn's book, especially in passages like those noted above, is representative of a strong historiographical tradition which I will call *the history of ideas* [72] in the history of science; a tradition carried forward particularly from the work of George Sarton, Alexandre Koyré, and James Bryant Conant by Kuhn and others. The central theses of this historiographical orientation, which I should point out and which are evident in the quotes, are:



(1) *Internalism:* That scientific knowledge grows from its own natural problem structure, and autonomously of social influences, which cannot contribute substantially, and may serve to retard knowledge's growth.

(2) *Universal rationality:* New knowledge becomes established within the existing institution through the free and uncoerced assent of all rational individual scientists, some of whom make discoveries, and the rest of whom recognize, for similar reasons, the value of a discovery when it is presented to them. The tacit mutual assent of all of the enlightened, and most of the practicing scientists, on the basis of sufficient reasons, is the prime motor of theoretical change and science's advance. Along with the scientists, the historian (and audience) recognize the value of the discovery for similar reasons: thus the rationality is universal *over time* as well.

Showing that these principles approximate the central methodological tenets of many or most approaches dubbed history of ideas in the history of science would, I am afraid, present a long side-track in this chapter: the matter will be taken up again in the fourth chapter. That they are central to history of ideas in general is suggested by several recent reviews of the approach[73]; and for the case of the history of science, consider the following quote from Conant, which, I think, is indicative of these assumptions for his case:

> Experience shows that a man who has been a successful investigator in any field of experimental science approaches a problem in pure or applied science, even in an area in which he is quite ignorant, with a special point of view. One may designate this point of view 'understanding science'; it is independent of a knowledge of the scientific facts or techniques in the new area. Even a highly educated and intelligent citizen without research experience will almost always fail to grasp the essentials in a discussion that takes place among scientists concerned with a projected in-

> quiry. This will be so not because of the layman's lack of scientific knowledge or his failure to comprehend the technical jargon of the scientist; it will be to a large degree because of his fundamental ignorance of what science can or cannot accomplish, and his subsequent bewilderment in the course of a discussion outlining a plan for future investigation. He has no 'feel' for what we may call 'the tactics and strategy of science'.[74]

I believe that Conant's work, and this quote especially, point to internalism and the universal rationality assumption. In context, the passage is part of an argument to show that historians of science can teach the public "the tactics and strategy of science".

Two clarificatory remarks: First, note that the claim about a substantive universal rationality is significantly more of a commitment than would be a claim about universal agreement among scientists. Conant's quote suggests it, and some of Kuhn's writing does too, but it is difficult to be certain of the stronger thesis because neither author tends to tell the details of the story of how group consensus is forged—it simply *arises*—and this fact, I think, tacitly implies the universal rationality assumption.[75] Second, I do not intend to suggest that intellectual historians such as Conant and Kuhn use these tenets exclusively in their history writing: intellectual historians in their work will, on occasion, shift to other positions, but these tenets appear to prop up the greatest bulk of their argument.[76]

Kuhn is a particularly complex author in this regard: as well as adopting the paradigm of intellectual history, in other passages, and particularly the first and last chapters of his book, he adopts a more complex position, superadding a discussion of "conceptual schemes" and "conceptual economy" in the development of theories and thought[77], and statements regarding the cumulative development of knowledge in the "permanent" achievements of each conceptual advance[78]. In these claims, Kuhn shows that he is very heavily influenced by Duhem's philosophy of science[79]: but they



do not appear to penetrate much of the detailed history of development that Kuhn sketches. Conceptual schemes and permanent achievement play the roles of psychology and philosophy of science respectively, and though in certain respects they provide explanatory augmentation to the internalism and universal rationality tenets of intellectual history[80], they are not integrated into the micro-level historical explanations of Kepler's and Galileo's developments. *The Copernican Revolution* is, for the most part, a fine example of the history of ideas; the story of how a collection of arguments and discoveries, developed within competing conceptual schemes, ultimately leads the scientific community to general agreement regarding the new shape of the universe. Though there exists dissent for a time, and though rational individuals, such as Bellarmine and Tycho, do raise significant temporary doubts about the revolution, according to Kuhn, the preponderance of argument *eventually* rests with a Copernican world-view. And 'preponderance' appears to be the operative term for rational decision on this view: When the greater weight of rationally compelling argument falls to Copernicanism, subscription is rational; when the preponderance is not obvious, subscription is optional. Kuhn argues, for example, that denial only became irrational or externally motivated after the findings of Kepler and Galileo were added to Copernicus' argument; after those arguments were assimilated, the case for Ptolemaic astronomy was closed:

> The continuing opposition to the results of telescopic observation is symptomatic of the deeper-seated and longer-lasting opposition to Copernicanism during the seventeenth century. Both derived from the same source: a subconscious reluctance to assent in the destruction of a cosmology that for centuries had been the basis of everyday practical and spiritual life. The conceptual reorientation that, after Kepler and Galileo, meant economy to scientists frequently meant a loss of conceptual coherence to men like Donne and Milton whose primary concerns were in other fields, and some men

> whose first interests were religious, moral, or aesthetic continued to oppose Copernicanism bitterly for a very long time.[81]

Though individuals and their idiosyncrasies may be needed to produce creative thought—Kuhn maintains that this is the case with Kepler, whose neoplatonism guided his innovation—apparently a universally accessible rationality operates within the context of *acceptance by the scientific community at large*, one related in terms of an increase in conceptual economy. To the extent that knowledge progresses, no story need be told regarding how individual scientists were convinced of the plausibility of a development—why they would all agree is taken as needing no further detailed explanation, beyond a passing reference to conceptual economy. Because all free, rationally guided individuals *would* agree regarding the significance of many historical developments, discussion of individuals championing positions is actually optional, and at times is dropped altogether in many texts in the history of ideas, and in Kuhn's work, as is the case for Kepler, quoted at the start of this section. The history is a history of *ideas*, and ideas *proceeding through an internally defined, rational development,* in an important sense autonomous with respect to individuals as well as the social environment, resting in a universal abstract mind.

### Part I, § 2 Kuhn II: Psychohistorian and technological determinist

That Kuhn is operating under such a historiography becomes even clearer if we consider his second book, *The Structure of Scientific Revolutions,* for that work manifests a perceived breakdown in the explanation of science from the history of ideas. *The Copernican Revolution* appears to have been written under the assumption that the growth of science operates through the near-universal consent of the scientific community, and through the presence of rationally compelling argument for change at every juncture. In his later work, however, Kuhn suggests that neither of these components is present in some instances of scientific change. Kuhn argues the point on two fronts: he provides philosophical puzzles that serve to criticize a broad variety of intellectual accounts of scientific change, including intellectual history and logical empiricism, and he also supplements, and, to a great degree, replaces intellectual explanation of change with a great deal of psychological and sociological explanation, re-emphasizing the limits of intellectual explanation.

In the first chapter, I discussed *The Structure of Scientific Revolutions* specifically in terms of the general problems it presents to philosophy of science. I cast those problems mostly as criticisms of specific epistemological positions often taken up by philosophers, and particularly by logical positivists and empiricists; and for the most part they *need* not be broadly construed as criticisms of rational or intellectualist approaches to explaining science (which I regard as approaches from philosophy and history respectively, similar in motivation, and somewhat less so in methodology). Kuhn's analysis of scientific development as being largely governed by paradigms, for example, may provide for a *revised* internalist and intellectual analysis of development: if paradigms are admitted as intellectual resources in science, then the character of a paradigm will guide a good deal of genuine progress of science, providing rules and heuristics for development.[82] I do not wish to suggest, then, that there is no room left in *The Structure of Scientific Revolutions* for rational explanation.

The guiding power of a paradigm, however, is limited to episodes in what Kuhn has called 'normal science'. Revolutionary science calls upon practitioners to make a decision *among* paradigms with only partially overlapping rules and heuristics, so clearly a rational decision would require reasons apart from those provided by the individual contending paradigms: a scientific rationality standing above all paradigms. Whereas Kuhn *does* hold that there are good reasons for holding to a paradigm,[83] he suggests that at this juncture, rational explanation might prove quite insufficient to adequately *explain* paradigm choice. There may be good reasons for holding to a paradigm, but at times of revolution, these are not sufficient to ground a decision for choosing one paradigm over another; and explaining that decision, which largely shapes the future development of science, is Kuhn's goal. Ironically, Kuhn even feels that he finds sufficient *philosophical* reasons for judging that the switch cannot be entirely rational: "Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience."[84] Note, then, that it is not for a *lack* of rationality in historical shifts from paradigm to paradigm that Kuhn suggests that rational explanation will not suffice: rational analysis falls short because Kuhn wishes to do more than distill the rational element of science; he wishes to explain development as well.[85]

In aid of rational explanation, then, Kuhn introduces sociological and psychological forces affecting scientific development. He considers social factors such as the ages and institutional entrenchment of individuals, suggesting that older and more established scientists might be less inclined towards a new paradigm than younger participants. A paradigm shift, he conjectures, might require a generation to be completed if fealty is determined along these lines.[86] In his extended comparison of scientific revolution and political revolution, the latter of which "must finally resort to techniques of mass-persuasion, including force", Kuhn also hints at, but never quite admits, the position that Lakatos would later call "irrationalism"[87]. But Kuhn, perhaps because he comes more directly from the tradition of intellectual history, focuses more of his attention towards accounting for change in terms of individual psychology.

The phenomena to be explained are the characteristics of debate during revolutionary episodes, and debate's result, that most practitioners adopt a new paradigm for research in cases of successful revolution. Kuhn writes of the prelude to extra-normal science, of individuals' growing awareness of factual anomalies, which arise through research along the lines of the natural unfolding of the



prevalent paradigm, but which are also out of harmony with it. In attempting to account for these discoveries, scientists put forward new sorts of explanations, that conflict with the paradigm, and lead to crisis and the development of another paradigm. Kuhn gives some detailed discussion regarding such late developments in a paradigm's 'lifetime', but little analysis of the procedure of paradigm construction[88]; his interest appears to lie mostly with the process according to which scientists adopt the new paradigm, or 'convert' to the new paradigm, as he calls it. And conversion it is for Kuhn, beyond the bounds of reason, as he leans heavily on very specific psychological theories in his explanations:

> How, then, are scientists brought to make this transposition? Part of the answer is that they are very often not. Copernicanism made few converts for almost a century after Copernicus' death.
>
> 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.
>
> Neither good reasons nor translation constitute conversion, and it is that process we must explicate in order to understand an essential sort of scientific change.[89]

Why scientists convert, and why the conversion is not a rational process, are two of Kuhn's main foci, to which he devotes chapters 9 and 10 of his book. The answer to the first question appears to be technological determinism (as in the marxian sense, i.e., the intellectual and material products of the concepts' application provide the conditions that eventually leave the concept inadequate). A paradigm, as was noted above, provides the researcher a guide to development—almost an exclusive guide, and in its articulation, its inadequacies are exposed:

The man who is striving to solve a problem defined by existing knowledge and technique is not, however, just looking around. He knows what he wants to achieve, and he designs his instruments and directs his thoughts accordingly. Unanticipated novelty, the new discovery, can emerge only to the extent that his anticipations about nature and his instruments prove wrong. . . . There is no other effective way in which discoveries might be generated.[90]

These two quotes are particularly interesting when placed side by side, because they suggest a Hegelian (or Marxist) character to Kuhn's philosophy of history: i.e., the fruit of a scientific paradigm, the discovery, carries with it the seeds of the paradigm's own destruction. This technological determinism is limited, however: the paradigm's internal development may be the source of its eventual downfall, but Kuhn sees no clear path to explaining the *subsequent* paradigm's character as a result of the character of the previous paradigm; thus, Kuhn's position differs from the intellectual internalism of his previous position, and of logicism.

To answer the question regarding why conversion to a new paradigm is not rationally determined, Kuhn introduces a new apparatus: psychological explanation, and particularly *gestalt* psychology[91] , a branch that stresses the presence of a constructive, Kantian element in human perception. The basic gestaltist tenet that Kuhn appeals to is that individuals bring a prior theory, or mind-set, to perception of (and action in[92] ) the world; gestalt perception experiments, such as the famous 'duck-rabbit' and figure-background puzzles, are put forth in service of attempts to explain the character of those theories. Once again, for Kuhn, paradigms are at the core of his analysis, as they take the role of providing the crucial element that guides the individual scientist to the perception of a specific gestalt: the paradigm provides the feature that allows a particular interpretation of the world, *and* an interpretation of appropriate scientific methodology, to coalesce. Differences among paradigms also provide the differences that lead to different perceptions of gestalt; thus Kuhn's claim that advocates of different paradigms "live in different worlds".



If different scientists live in different worlds, we know something about their minds, perhaps: how, on Kuhn's view, do different paradigms affect the character and content of science and make for a non-rational basis for paradigm choice? Technological determinism points to one answer, but Kuhn appears to be suggesting more: that the paradigm also greatly affects the direction of development and content of a field. The following example suggests the paradigm's pervasive influence

> Can it conceivably be an accident, for example, that Western astronomers first saw change in the previously immutable heavens during the half-century after Copernicus' new paradigm was first proposed? The chinese, whose cosmological beliefs did not preclude celestial change, had recorded the appearance of many new stars in the heavens at a much earlier date. Also, even without the aid of a telescope, the chinese had systematically recorded the appearance of sunspot centuries before these were seen by Galileo and his contemporaries. . . . The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world.[93]

Kuhn, I think, is suggesting technological determinism for the development of science in this passage: he is arguing that European efforts lagged behind chinese developments because no-one was led to look for change in the heavens. He may also be suggesting that observed change might have been discounted as mistaken partially *because* of the European paradigm's influence, presenting a weak theory-ladenness of perception thesis (or better, theory ladenness of acceptance).

Consensus and good reasons, then, are demands too strong for the historical record to bear, and Kuhn finds himself forced to look elsewhere for explanatory apparatus adequately suited to the history. Kuhn's bold adoption of gestalt psychology suggests that he has abandoned hope that the historiographic principles of the history of ideas can serve to explain scientific development, because they do not allow for the development that he sees arising out of some of the most significant episodes in the history of science. This move from explanation within the canon of history of ideas to psychological explanation of science led Lakatos to criticize Kuhn as treating science on a par with "religious change"[94].

### Part I, §3 Kuhn III: New epistemologist and modified intellectual historian

The standard approach along the lines of the history of ideas clearly did not survive *The Structure of Scientific Revolutions,* because reasons could not govern conversion experiences. But especially as a result of charges from Lakatos and others that his position advocates "mob psychology", irrationalism, and sociology of science, Kuhn has recently attempted to extend and re-shape his view[95], moving back towards the lines of traditional philosophy of science and intellectual history. How Kuhn has moved back towards orthodoxy I will lay out very briefly here. Particularly, he has introduced an allowance for rational variation in opinion among scientists that might explain dissent at times of revolution, he has sharpened his views regarding his key concept, the paradigm, and he has attempted to replace psychological explanation, and especially gestalt psychology, with philosophical explanation of the characteristics of paradigms.

I have attempted to argue that Kuhn's thesis that scientists immersed in different paradigms live in different worlds might receive a very powerful defense in the theory of Gestalt psychology. A closer analysis regarding the integrity of Gestalt theory, which, unfortunately, I am not knowledgeable enough to provide, would go far in making Kuhn's case; but if it is at least plausible that perception of wholes or practical world-views *might* be mediated via theory, and if a scientific paradigm does have the pervasive influence on practice that Kuhn suggest it has, then different paradigms might well have the power to impose different gestalts for scientists. But Kuhn's references to Gestalt psychology tail off quickly after *The Structure of Scientific Revolutions,* and I expect they do because he attempts to re-examine the features of science that he has explained with this psychological theory in order to show that they are explainable in philosophical terms. That is, Kuhn attempts to explain the features of revolutionary science—especially discontinuity of aims and method, data-loss, partial communication, and irresolvable disagreement—in several philosophical arguments.



To make revolution a philosophically respectable concept, first of all, Kuhn focuses more heavily than ever on explicating the putative feature of revolutionary science that is obviously clothed in philosophical garb: incommensurability. The incommensurability thesis in its simplest interpretation, I have argued, presents a challenge to Nagel's reduction thesis, and more specifically, to his account of the relation of language to phenomena. In that form, it merely suggests that philosophy of language needs tuning. Kuhn exploits it for more than that, however, suggesting that ontology, as it is represented in language, greatly governs *thought* as well. Kuhn suggests the importance of this sort of *conceptual* incommensurability through an analysis of the transition from Aristotelian to Newtonian dynamics, in which several terms lose all meaning ("place", "natural potential"), several are radically reconceived ("motion", "void"), and aims change (physics drops the analysis of organic growth). In such cases of revolutionary change, Kuhn argues, the changes in concept and vocabulary are radical enough that meanings must change *holistically:*

> What characterizes revolution is, thus, change in several of the taxonomic categories prerequisite to scientific descriptions and generalizations. That change, furthermore, is an adjustment not only of criteria relevant to categorization,

> but also of the way in which given objects and situations are distributed among preexisting categories. Since such redistribution always involves more than one category and since those categories are interdefined, this sort of alteration is necessarily holistic. That holism, furthermore, is rooted in the nature of language, for the criteria relevant to categorization are *ipso facto* the criteria that attach the names of those categories to the world.[96]

I will leave this position of Kuhn's here, for, as I will suggest in the next section, it would require a much more detailed formulation than Kuhn has provided to make its theoretical content clear enough for philosophical criticism.[97] Kuhn makes some similar arguments in *The Structure of Scientific Revolutions,* regarding conceptual incommensurability[98] , but here the argument appears to be directed more towards *replacing* the allusions to Gestalt psychology and "different worlds".[99]

The second way in which Kuhn has moved his position back towards philosophy and history of ideas is in a commendable analysis of the notion of the paradigm, at the prodding of Margaret Masterman, who hunted down twenty-one often-conflicting conceptions in *The Structure of Scientific Revolutions*.[100] Masterman's article presents a fine case for the importance of Kuhn's innovation, and "Second thoughts on paradigms" presents Kuhn's own analysis. Kuhn notes three key features, and re-christens the paradigm the "disciplinary matrix" of a specific (perhaps sociologically distinguished) disciplinary community. The matrix includes symbolic generalizations, exemplars, and models.[101] The first are, roughly speaking, the laws of the field accepted by the members, the second are paradigmatic concrete problem solutions, and the third, models, provide the basis of ontological and analogical thinking. Kuhn's further articulation of these features through the article and in some of his other writings[102] serves to provide clearer concepts that fit much more easily into a fortified conception of intellectual history, one such as I suggested at the beginning of the previous sub-section. Kuhn himself appears to consider this clarification to be ground reclaimed for the historian, presumably the intellectual historian.[103]



Finally, in "Objectivity, Value Judgment, and Theory Choice", Kuhn attempts to define more clearly the limits of rational explanation of scientific change, and as a result, he might well have improved the case for rational explanation. The territory reclaimed is gained by weakening the universal rationality thesis supposed in intellectual history, allowing for the possibility of *rational variation* in the opinions of scientists. In *The Structure of Scientific Revolutions,* the history of ideas faced the problem that scientists often remained at loggerheads for long periods, disagreeing regarding the significance of revolutionary developments. The simple, consensual model of a univocal scientific community noting and rationally adopting development at every turn, then, was in need of replacement. Consequently, Kuhn explicitly reduces the scope of the thesis of universal rationality:

> If subjective factors are required to account for the decisions that initially divide the profession, they may still be present later when the profession agrees. Though I shall not here argue the point, consideration of the occasions on which a scientific community divides suggests that they actually do so.[104]

In this quote, Kuhn holds that variation among scientists accounts for divisions in the community, so Kuhn has dropped the simple model of universal rationality. He replaces it with rational variation, allowing that there are subjective differences—matters of taste—which are differences in the weight that different scientists would give to various criteria used in deciding the virtues of theories; criteria such as the measures of consistency, scope and accuracy of the new theoretical development in comparison to the older one.

> I am suggesting, of course, that the criteria of choice with which I began function not as rules, which determine choice, but as values, which influence it. Two men deeply committed to the same values may nevertheless, in particular situations, make different choices as, in fact, they do.[105]

The variation in opinion that he finds present in history arises due to a variation in the *values* which different scientists hold. The variation should not be considered an irrationality of scientific practice, but rather, one of the "facts of scientific life".[106]

I think that Kuhn has here satisfactorily solved the problem which accrues to intellectual history in the universal rationality assumption[107] . It appears appropriate to allow that there should be some rational cognitive variation in a group of individuals that are of different ages, are trained by many different teachers, and have different life experiences and scientific experiences. His emendation of historiography for intellectual history fits 'history' better, allowing for the disagreement which is historically present, and it also has its own plausible basis, in an individual intellectual variation that it could only be pedantic to consider irrational.

### Part I, § 4 Evaluating Kuhn's progress

I have attempted to sketch an account of Kuhn's development in order to display his attempts at responding to the historicist criticisms of epistemology and intellectual history, for which he was largely responsible. "Kuhn III", I have argued, attempts to regain ground conceded by "Kuhn II" to psychological and sociological explanation in *The Structure of Scientific Revolutions,* which was a response to traditional philosophy of science and to "Kuhn I", the orthodox intellectual historian. In an apposite reflection on his development, Kuhn III makes these divisions plain enough:



> Return, finally, to the term "paradigm." It entered *The Structure of Scientific Revolutions* because I, the book's historian-author, could not, when examining the membership of a scientific community, retrieve enough shared rules to account for the group's unproblematic conduct of research. Shared examples of successful practice could, I next concluded, provide what the group lacked in rules. Those examples were its paradigms, and as such essential to its continued research. Unfortunately, having gotten that far, I allowed the term's applications to expand, embracing all shared group commitments, all components of what I now wish to call the disciplinary matrix. Inevitably, the result was confusion, and it obscured the original reasons for introducing the term.[108]

The Kuhn of *The Structure of Scientific Revolutions,* I will argue, was much more the revolutionary, and significantly less confused, than his later self exclaimed: though his later work does show some progress towards his goal of fitting responses to the historicist criticisms of *Structure* into more traditional philosophical and historical pigeonholes, *Structure's* radical arguments are not much the less for that later effort.

The shortcomings of Kuhn I will receive little more analysis here. *The Structure of Scientific Revolutions* presents many sharp criticisms from historical cases that I have mentioned in the previous chapter that suggest that intellectual history's historiography, Like logicist methodology, is inadequate to the phenomenon of science and the historian's goals: extended disagreement, changing intellectual values, and the saltatory movement of ideas that appear to occur in scientific debate are all, apparently, prevalent features of unexceptionable science that, consequently, need explaining. And intellectual history must explain them all *away*, for they appear to be opposed to that framework. Other weaknesses of the historiography of intellectual history (including Kuhn III's later improvements) will also be presented in Chapter 4, where I consider debates of historians and philosophers of history who discuss its plausibility as historian's methodology.

Kuhn III differs from Kuhn II, as I have tried to show in the previous sub-section, in his attempts to reclaim ground for intellectual history and for philosophical explanation from sociological and psychological explanation of science. To some extent, the philosophical and historiographical amendments that he presents are successful: the arguments for rational variation, clarification of the concept of paradigm, and incommensurability's implications for world-view all have some attractions. They do not, however, militate against Kuhn's previously presented psychological and sociological explanations, and do not obviously succeed in covering the explanatory gains provided by those approaches either.

***Rational variation:*** The argument for rational variation can be taken as acceptable so far as it goes, but Kuhn's presentation sometimes suggests that it might be exploited for more than it is really worth; such as for reducing the significance that non-rational features of scientific development were given in *The Structure of Scientific Revolutions.* The prospects for the argument are exaggerated by Kuhn's claim that his position concerning the grounds of theory choice (including, where applicable, theory choice under the conditions of a paradigm shift[109]) is really not very different from that of the philosopher attempting to find a rigorous logic of justification for science. Kuhn sees a general equivalence between his claim that theory choice is guided by rationally variable values of scientists, and traditional philosophical claims regarding rules of scientific method grounding choice:

> It is , after all, no accident that my list of the values guiding scientific choice [i.e., consistency, scope, accuracy, simplicity, fruitfulness] is, as nearly as makes any difference, identical with the tradition's list of rules dictating choice. Given any concrete situation to which the philosopher's rules could be applied, my values would function like his rules, producing the same choice. Any justification of induction, any explanation of why the rules worked, would apply equally to my values. . . . If I now assume . . . that the group is large enough so that individual differences distribute on some normal curve, then any argument that justifies the philosopher's choice by rule should be immediately adaptable to my choice by value.[110]



Kuhn appears to reveal, to everyone's surprise, that he has kept the dream of an historically applicable logic of justification, or at least a universal rationality, alive all along! The universal assent to theory change on the basis of well-defined criteria, which the historian of ideas and the philosopher craved, *remains;* it is merely statistically distributed over a normal curve. The individual scientists may be difficult to plot, but 67% of them, it appears, will remain within one standard deviation of a mean with respect to each value! Kuhn seems to return to endorsing the position of *The Copernican Revolution,* but with a statistical law attached to the universal rationality assumption.

We must be clear on the importance of Kuhn's revelation, however. I think the implication of a universal rationality for scientists is clear, but Kuhn does not make these claims for his argument, and even states later in the article that such a narrow discussion of theory choice is off of the main track of his philosophical approach[111] . It is also misleading: the *prospect* for a clearer account of shared values among scientists may be improved as a result of Kuhn's discussion, but the *role* of shared values insofar as they determine revolutionary development in science should not be considered to be enlarged as a result of this discussion. For Kuhn has provided no arguments in his article to show that shared values present a sufficient guide for scientific debate to overcome the influence of the *un*shared values that presented roadblocks to rational explanation of development in *The Structure of Scientific Revolutions.* Thus, the territory reclaimed by this article for philosophy, and for intellectual history, is easily exaggerated.

***Incommensurability:*** A second of Kuhn's improvements to his earlier position centers on incommensurability and his newer philosophy of language arguments. I argued above that Kuhn expanded on his analysis of the incommensurability thesis after *Structure* in order to construct a more philosophically palatable defence of his "different worlds" hypothesis concerning participants utilizing different paradigms; and the philosophical defense, I suggested, is partly meant to replace the gestalt explanations suggested by *Structure*. Here, unfortunately, Kuhn's efforts appear to be too vaguely presented to be easily assessed. The philosophical soundness of Kuhn's expansion is, on the face of it, debatable: I argued in the previous chapter that the clearest implications of incommensurability are directed against inadequate accounts of the link between language and the world, and, perhaps, the logical empiricists' cumulativity thesis as well. In his latest efforts, however, Kuhn is concerned to show more clearly that incommensurability is more than a problem for philosophy of language to solve: the mismatches of reference manifest in incommensurable terms are utterly irrecoverable, because differences in the ontologies of different paradigms are pervasively represented in the language (in the "*taxonomies"* of the corresponding theories). Kuhn is concerned to develop further his thesis regarding *conceptual* incommensurability, which is a thesis concerning *meaning holism*, as Kuhn admits (see quote, early in the previous sub-section)[112].

The problem with Kuhn's thesis is that it appears to be a clear enough practical and psychological thesis, but not so clearly a philosophical thesis, because he hasn't brought sufficient and appropriate *philosophical* machinery to bear on the problem to seal the philosophical point.[113] As a claim about the psychological difficulty that scientists might face in coming to grips with the models, analogies, exemplars, mathematics, and ontology of a paradigm with which they are not familiar, '*practical' psychological* incommensurability strikes me as a plausible thesis about scientific practice, and about what one can expect from limited beings. And I believe, if it is a historically pervasive psychological roadblock, *it ought to be accounted for in a philosophical study of scientific activity as well,* so that explication stands a chance of approximating the phenomenon it is intended to explicate.



As a philosophical thesis, however, Kuhn's conceptual incommensurability is inappropriately developed because of an incompatibility between meaning holism and practical scientific testing, as Feyerabend, Shapere, Scheffler, and Kitcher all argue against Kuhn[114]. Briskly put: if Kuhn *is* putting forward a thesis about the theory of meaning, then it appears to be a devastating problem for him that he cannot, in principle, account for scientific testing, and the partial comparison of theories in overlapping empirical domains that he allows does occur. For Kuhn apparently intends that there *is* no room for comparison that can be represented in his theory of language—recall the passage quoted above: " . . . the central characteristic of scientific revolutions is that they alter the knowledge of nature that is intrinsic to the language itself and that is thus prior to anything that is quite describable as description . . . ". Kuhn's problem lies in the thesis of meaning holism: if the meaning of language attached to a theory is truly, *entirely* holistic, as Kuhn claims, then one cannot make any comparison among theories regarding their adequacy to the empirical phenomena *except* as wholes. And of course, science *just doesn't* proceed by the wholesale comparison of theories: or at least, it often does not, for it proceeds by testing empirical adequacy at the level of specific instances, in individual experiments. If Kuhn cannot accommodate an account of how competing theories are tested within his theory of meaning, then he must say *either* that his theory presents an inadequate account of meaning, *or* that, in testing competing theories, scientists do not take into account the meanings of their findings (that is, that empirical comparison proceeds without involving meanings: Feyerabend suggests this possibility, see below). And Kuhn clearly does not intend the latter, since, as he suggests, meaning holism is the basis of a breakdown in communications across paradigm shifts.

What Kuhn's holistic theory of meaning leaves us with, then, is a doubt concerning its applicability, because *a strict meaning holism implies that the empirical content of theories cannot be compared in any manner, excepting that the empirical content of each theory in its entirety may be compared.* The incommensurability theses considered in the previous chapters were treated as critical attacks on philosophies of science; this one, unfortunately, appears to suggest a *complete* breakdown in communications across paradigms among scientists. Meaning holism simply gives incommensurability a devastating thoroughness when applied to science, because it does not allow for the comparison of empirical results that plays so great a role in scientific theory change.

That Kuhn's theory of language has problems might also be indicated by comparing his account of incommensurability with Feyerabend's. Recall that Feyerabend also put forward an incommensurability thesis, but finished his criticism by suggesting that the greatest philosophical problems accruing to it could be repaired by thinking of the empirical comparison of theories as not involving meanings at all[115]. Feyerabend instead suggested that Galileo's and Newton's laws were, indeed, incommensurable, and the former was not reducible to the latter, as Nagel would have it, because the two theories predicted different *empirical* results[116]. The similarity between Newton's and Galileo's theories—the intuition which actually based Nagel's attempt at reduction—does not lie in their meaning-structure: it lies in their having empirical equivalence to the best of our ability to measure the two theories' predictions, for falls over short distances near the earth's surface (as Feyerabend says, they are "experimentally indistinguishable"[117]).[118] Feyerabend argued that Newton's and Galileo's laws were incommensurable, but he did not argue that the two frameworks were entirely sealed off from one another, which is what meaning holism appears to imply.

These points of philosophy of language I find difficult to argue, but it appears that, at best, Kuhn's analysis of the importance of language to the "partial communication" and "different worlds"



theses is too vaguely formulated to judge. If Kuhn's argument *were* to go through, his new approach might serve as an alternative to the explanation of these theses in psychological terms, including the explanation from Gestalt theory that I have been highlighting.

***Paradigms:*** Kuhn III differs from the Kuhn of *The Structure of Scientific Revolutions* in many ways, but one very important aspect of his view that has not changed greatly is his endorsement of the centrality of paradigms to scientific practice. Particularly important to historians, I expect, is the modification presented in "Second thoughts on paradigms", which enumerates and explains specific aspects of the paradigms that may be assimilated into a broadened conception of intellectual history. History of science, then, may be profitably studied as a history of scientists following the guides provided by precedent-setting exemplars within their disciplines, or drawing and testing their conclusions because of analogies suggested to them by models. A wealth of examples of this approach have been developed since Kuhn first suggested its merits.[119]

Paradigms present a genuine challenge to traditional approaches in philosophy of science, however, and the challenge should be supported by the above discussion of Kuhn's later work. Towards the end of the first chapter I suggested what I take to be an appropriate goal of philosophy of science, and an appropriate normative position for it with regards to actual scientific practice: philosophy of science should develop towards producing theories that explain the progressive development of actual science, preferably in terms of what actual scientists believe, think, and do, or in terms that are easily translatable into those terms. Kuhn's later work, again suggests just how far traditional approaches need to be extended (or how much they must refute) to achieve this goal. Traditional philosophy of science has focused on the grounds of rational theory-choice, often abstracted into the context of justification. Such theories, however, do not go very far towards explaining why and how scientists develop the theories that they do.

Kuhn's more careful recent analyses of the intellectual aspects of paradigms suggests that paradigms play a significant role in guiding scientists' thought; suggesting through exemplars the experiments that they perform, and through ontology directions for theoretical developments, and through analogy the explanation patterns that they develop. If Kuhn is right, and paradigms are an important and stable part of the *mental equipment* of scientists, then they offer the opportunity for expanding philosophy of science's scope, and incorporating the analysis of these features of theory *construction* as well as those governing theory *choice*.

## Conclusion

There is obvious development through these three works, but Kuhn retains some important affinities to his original history of ideas approach in all of them. In his earliest book, scientific advance was clearly found to be explainable in terms of good reasons and consensus of the community. In his second book, both of these tenets were abandoned as inadequate to the historical facts for cases of what Kuhn called revolutionary episodes, where "good reasons" would not explain conversions to new paradigms, and explicit psychological theory was called in to fill the empty spaces in explanation. In the third work which I have examined, Kuhn regains ground for a modified history of ideas approach: But Kuhn's later development, as is suggested by the quote at the beginning of this subsection, also presents an attempt to draw away from the promise that his analysis of revolutionary science presents for sociological and historical features of scientific development that might deepen the accounts of scientific development of historians and philosophers. In the final chapter of this thesis, I will attempt to indicate ways in which philosophy of science may be reconceived to take advantage of these features.



### Part II. Imre Lakatos' scientific rationality

Along with Paul Feyerabend, Kuhn argued that both philosophy and history of science did not support the consensual agreement assumption of the history of ideas. The 'history' suggested that many of the most important scientific debates were protracted and tangled affairs, with few steps that reshaped disciplines by producing general and immediate conviction among scientists. Feyerabend embraced the doubts which he raised, and challenged the ideals of scientific rationality and scientific method, advancing political and social explanation of science in their place. For Kuhn, the problems appear to have led to a 'crisis' for the approach from the history of ideas, and for the rational explanation of science. The crisis led him to psychological explanations of science, but he appears to have attempted more recently to overcome limitations within the history of ideas by partially reconstituting its historiography. Imre Lakatos also took up the duty of responding to historicist criticism, but he did so by adopting a different rational framework, radically recasting Popper's analysis of scientific progress and scientific rationality, as Kuhn recast that of the history of ideas.

### Part II, §1 Popper on progress and rationality

We begin, then, with Popper and the problems Lakatos saw in his approach. For Popper, scientific progress is achieved through the development of theories that manifest an increase in empirical content. Schematically expressed, that increase is achieved through the construction of newer theories that preserve the corroborated content of the present theory, but differ from it in some other empirical prediction; a test of that area in which the two theories differ provides an increase in corroborated content, and determines which theory survives to take on the next challenger. In its practical realization, progress is achieved through the rational practice of science, according to the process of conjectures and refutations. The rational root of scientific practice rests partly with the scientist who creates theories, and partly with one who facilitates empirical falsifications to accompany those theories. The former's job is to produce new theories of ever greater empirical content; theories which allow, for example, for more precise predictions, or predictions over a broader range of the phenomena. The latter's job is to test the theories of the former, ideally with resounding "crucial experiments" that will conclusively decide the relative empirical adequacy of the contending candidate theories developed within a science. Conjoining the two roles yields a discipline in which ever more detailed conjectures are put to ever more stringent empirical tests; and this is the basis of scientific rationality[120] .

Popper has, then, provided an analysis of scientific progress in terms of falsification and content that is easily convertible into an analysis of scientific rationality, and rules of rationality for the individual scientist. If falsifications are called for by the philosophical analyst, individual scientists may set out with the purpose of finding them in mind: Popper's framework for the appraisal of theories is readily understandable, and applicable to the individual scientists' work; and indeed, I believe that a small though notable fraction of practicing scientists even today continue to think of their work in vaguely Popperian terms[121] . The rational action of the individual—action promoting content-increase and falsification—could served to account for progress, and explain the rationality *of science:* the rationality of science proceeds from the rational individual's actions, a key assumption within the history of ideas, as we have seen.

***Lakatos and Popper:*** Lakatos, by contrast, constructs a very different account of scientific progress, and consequently, a different account of the seat of scientific rationality as well. He develops his account partly in response to the historicist criticisms central to this thesis, and particularly in response to the logical and empirical problems accruing to Popperian infirmation. Lakatos finds that scientific progress can no longer be characterized according to the ideal of science as a field of theories unequivocally falsified and



replaced by others with greater content[122] : a very different approach is called for.

Lakatos' primary trouble with Popper's philosophy of science, as he himself argues in "Falsification and the methodology of scientific research programmes", is that there appears to be nothing unequivocal, and very little that is obviously empirical, about the process of falsification. The problem arises because Popper rejected the approach of confirmation, maintaining that it could not overcome Hume's problem of induction; his solution lay in the most basic falsificationist tenet, that a general theoretical statement (or law) cannot be verified, but only proved false. Falsification, however, cannot be nearly as straightforward a testing process as others had hoped confirmation would be. One source of trouble lies in the breakdown of the theory/observation dichotomy, the absolute distinction between theoretical and empirical statements that positivists were concerned to uphold. The breakdown implies that the empirical basis of falsification is no more secure a foundation than the theories which it is meant to test, insofar as the empirical basis—what Popper called the "basic statements" of a theory—is no less theoretical.

Falsification thus becomes a game largely governed by the rules of maintaining consistency among theoretical statements, rather than those for which Popper was striving, rules of empiricism[123] . One such resulting rule that Lakatos paid particular attention to was what he called the "*ceteris paribus* clause" ('all else being equal') in a disconfirmation:

> [Some] theories never alone contradict a 'basic' statement: they contradict at most a conjunction of a basic statement describing a spatio-temporally singular event and a universal non-existence statement saying that no other relevant cause is at work anywhere in the universe. And the dogmatic falsificationist cannot possibly claim that such universal non-existence statements belong to the empirical basis: that they can be observed and proved by experience.

> Another way of putting this is to say that some scientific theories are normally interpreted as containings a *ceteris paribus* clause: in such cases it is always a specific theory *together* with this clause which may be refuted.[124]

With such philosophical problems standing in the way of falsification, Lakatos faced a crisis in the rational explanation of science much like Kuhn's. Popper presented a very eloquent individual-level analysis of rational activity for the scientist; but like Kuhn, Lakatos found that an individual-level analysis of what he called "intellectual honesty" was *insufficient* for explaining how progress had been achieved within science[125] .

### Part II, §2 Lakatos' solution: Rationality as manifest among research programmes

But whereas Kuhn argued that the significance of rational explanation to scientific development had been overemphasized, Lakatos reconfigured rational explanation drastically, and for similar reasons:

> If we look at the historical details of the most celebrated crucial experiments, we have to come to the conclusion that either they were accepted as crucial for no rational reason, or that their acceptance rested on rationality principles radically different from the ones we just discussed [i.e., brands of falsificationism]. . . . By [the falsificationist's] standards, scientists frequently seem to be irrationally slow: for instance, eighty-five years elapsed between the acceptance of the perihelion of Mercury as an anomaly and its acceptance as a falsification of Newton's theory . . . On the other hand, scientists frequently seem to be irrationally rash: for instance, Galileo and his disciples accepted Copernican heliocentric celestial mechanics in spite of the abundant evidence against the rotation of the earth . . . [126]



History and philosophy both pointed Lakatos away from standard analyses of scientific rationality. His solution to the problem, quite an original one, was to abandon the rational analysis of science at the level of the individual scientist, and reconceive rationality at the level of *interacting individuals and groups of individuals.* The rationality of science is not primarily based on the rational properties of individuals for Lakatos; it is based on the properties of research programmes, and in a dialectic among research programmes.

The standards governing rational progress within a single research programme are reminiscent of those of individual-level analyses of rationality. A research programme is a historically ordered series of theories, which may be labelled 'scientific', or 'progressive' if it meets certain criteria: A theoretical development within a research programme is *theoretically progressive* over a prior state if it leads to an increase in theoretical content (e.g., the changes are not merely linguistic, or *ad hoc,* yielding a decrease in content), and *empirically progressive* (or progressive *tout court*) iff some of that increase is empirically corroborated. With these criteria, Lakatos remains largely within the falsificationist framework (and his adjustments at this level, clever as they are, are not of great concern for this discussion). Lakatos also remarks that the succession of theories which makes up a research programme is also reminiscent of Kuhn's normal science[127] . In the quote above, however, Lakatos points to developments which correspond to Kuhn's *revolutionary* science. To explain such events, Lakatos makes a more radical break with tradition, and enters into the analysis of interactions among groups, announcing "the end of instant rationality"[128] .

Scientists, Lakatos argues, are aligned within research programmes, but at most points in history there are several programmes competing for the allegiance of scientists, and it is often the case that more than one is progressive. At a given time, then, a scientist might have reason to be involved in any of a number of programmes; it just happens to be the case that most scientists will not participate in more than one programme at any given time. The rational individual, then, is limited: with good reasons for entering any of a number of research programmes, individuals' decisions of loyalty will not provide the spur to the one specific programme that will eventually win out, and a problem arises for "instant rationality" solutions. The history of science is a history of overlapping research programmes ("or, if you wish, paradigms"[129]) each driving predecessors to extinction, and such shifts among programmes are the troublesome saltations which stalled Kuhn's project of rational explanation. Lakatos' great innovation is that he proceeds to argue that historical changes of research programmes, to the extent that they fit certain criteria, may be considered rational as well, despite breaks in the continuity of content in a field.

Lakatos' criteria for progressively eliminating research programmes are very much like those he uses for appraising developments within research programmes. A programme may be eliminated if a rival explains its successes, *and proves superior in terms of heuristic power,* which is its promise for forwarding further research.[130] The primary difference between inter- and intra-programme development is that the strict commensurability of all positions within a research programme (governed by the "negative heuristic") is not maintained in conflicts between different programmes. The conflict is consequently bloodier, but by no means a free-for all:

> When two research programmes compete, their first 'ideal' models usually deal with different aspects of the domain (for example, the first model of Newton's semi-corpuscular optics described light-refraction, the first model of Huyghens's wave optics light-interference). As the rival research programmes expand, they gradually encroach on each other's territory and the *n*th version of the first will be blatantly, dramatically inconsistent with the *m*th version of the second. An experiment is repeatedly performed, and as a result, the first is defeated in *this battle,* while the second wins. But *the war* is not over: any research programme is allowed a few such defeats. All it needs for a comeback is to produce an $(n+1)$th (or $(n+k)$th) content-increasing version and a verification of some of its novel content.[131]



Through this process, progressive growth of science which is not manifested at the level of rational decision by individual scientists can arise. Individuals may (rationally) engage in research within one programme or its rival, provided that each shows reasonable signs of being a progressive programme. On this conception of scientific rationality, then, the individual scientist faced with two strong programmes does not bear the burden of determining the rational progress of science: it might be rational for the individual to rest in either camp. When one programme eventually appears to be radically progressive *with respect to developments within the other programme,* however, continued pursuit of that other programme is no longer justified, and is irrational. A rationally justified proliferation and tolerance allow a multiplicity of programmes to exist in order that they may battle each other. Scientists, who might be members of one programme only, might be intolerant and do not provide this rational tolerance. Consequently, the rationality of science does not rest entirely with their decisions.

**Part II, § 3 Critically assessing Lakatos' solution**

***Lakatos' roots:*** Lakatos' solution to the problem of accounting for rational disagreement in the history of science is as brilliant and original as Kuhn's. Where Kuhn responds to the challenge by asserting the importance of psychological explanation, Lakatos radically reconceives scientific rationality, considering it a property of *science itself,* analyzable in terms of the interactions of groups of individuals: on his analysis, rational dissent among individuals is a productive characteristic of science, and does not stand in the way of scientific progress.

Lakatos' analysis is original, I believe, to philosophy of science, but it is not entirely without forebears in philosophy. Expressed in the above terms, the tradition from which Lakatos draws to build his account should be obvious: the Hegelian overtones in Lakatos' approach are clear if we see "rationality" erected as the central feature of the analysis of scientific history, in place of the "*Geist*" of world history. The parallels are quite robust: for example, we see that like Hegel's *Geist*, Lakatos' scientific rationality does not reside in the minds of individuals. For Hegel, world-historical figures such as Napoleon, though perhaps conscious of their great *importance* to history, have no knowledge of their precise *role* in history's development: as Hegel states, such knowledge can only be acquired in retrospect, and perhaps, in its fullest form, only at the end of history—the owl of Minerva only spreads her wings at dusk. For Lakatos, it appears, the same can be said for 'scientific-historical' figures such as Copernicus: the genuine importance of his views for the future of science could only be seen in the long retrospect, after competing research programmes had finally been extinguished.

The relation between Lakatos and Hegel I find a particularly fruitful one to consider, for it clarifies and suggests origins for his innovative conception of rationality. The Hegelian parallels also point the way towards the clearest and most damaging criticisms of Lakatos' philosophy of science, for similar criticisms apply to Hegel's philosophy of history. The criticisms of Hegel's work I will leave implicit henceforth, but I hope that they will be obvious from what I write about Lakatos: they concern problems accruing to his philosophy's relation to analysis of practical human action, and its fidelity to the reasonable requirement that the study and writing of history should be an empirical enterprise. I will focus upon two features of Lakatos' approach that I find troubling, and that suggest to me its inadequacy as philosophy of science and as an account of productive relations between history and philosophy of science. I suggest, at the conclusion, that Lakatos is, in fact, promoting an enterprise—one coherent in its own right—that addresses somewhat different concerns from those we would wish



to consider of central importance to history and philosophy of science.

***Lakatos on the individual and history:*** Lakatos' methodology of scientific research programmes simply shows too little concern for the individual, and for their actions in history, to be useful as a theory of rationality. This is so because his is really a theory about *the nature of scientific progress*, rather than about *rational action.* I will argue below that Lakatos does not make this distinction forcefully enough, nor does he make its implications clear.

If Lakatos' methodology of scientific research programmes were a theory of rationality, one might expect that it would be reducible to a theory regarding individuals' actions. One would expect this especially because a rationality theory should serve to indicate the possibilities of human action and the best possible choice which an agent can make, or the best policy for an institution to maintain, in order to serve the role of a *normative* theory of scientific development. But Lakatos' analysis is couched instead in terms of research programmes, and the rationality of a research programme cannot be reduced to the analysis of the rationality of individuals' actions, nor institutional policy, as appears to be the case for Kuhn, because research programmes have some very peculiar properties. Most notably, in many instances they can be neither identified, nor assessed in terms of progressiveness, *excepting that these tasks may be accomplished retrospectively.*[132] More plainly: when researchers are 'in the thick of it', it may not be possible for them to identify their research programmes, nor to determine the different programmes' relative progressiveness.

I have dealt with the peculiar historical character of Lakatos' research programmes in detail elsewhere[133]; here I will present only a few illustrative comments about Lakatos' tenuous contact with practical action and history. Lakatos suggests that, because a flagging research programme may make a comeback no matter how grim its prospects, it is never irrational for an individual to pursue any programme: it is only irrational for the agent to ignore the risks involved, says Lakatos; but this conclusion in itself provides no basis upon which one could decide whether to choose a high-risk or a low-risk programme.[134] The programme with poor prospects may, with one development, become a progressive one, and it is even possible that developments which *had* been classified as degenerative are, as a result of the new development, reclassified as progressive. So the scientist who in the past was driving a programme into the ground in a degenerative fashion may, as a result of another's discovery, be changed overnight into a hero of progressive development.[135]

For a concrete example, consider what Lakatos has to say in his remarkably convoluted rational reconstruction of the development of a 19th century atomist programme in chemistry, which he dubs "the Proutian programme".[136] Lakatos intends to explain the early rational development of the theory of elementary isotopes; a theory that resolved an anomaly for Daltonian atomism by arguing that chemical elements that do not appear to have unit-multiple atomic weights are physical mixtures of isotopes with unit multiple weights. Thus, for example, isotope theory suggests that Chlorine, with atomic weight of approximately 35.5 atomic mass units, is composed of multiple isotopes of the Chlorine atom, of different unit-weight; and 20th century experiments designed to separate chemically indistinguishable material physically, by mass, suggest that Chlorine is, in fact, composed mostly of two isotopes, of 35 and 36 atomic mass units[137] , in approximately equal proportion.[138]

To begin his reconstruction, Lakatos brings on a character whom he calls "Prout". "Prout" claims, in 1815, that the atomic weights of pure chemical elements are whole number multiples, thus presenting simple relative weight-ratios. These ratios, he argues, are not detected observationally [as with, for example, chlorine] because of weaknesses in his contemporary experimental technique. Lakatos then informs the reader in a footnote that, in fact, the *historical* figure named Prout made no such claims regarding the weakness of experiment; instead, he "lied" and claimed that no such anomalies were to be found. (We have two characters, then, to keep separate: "Prout", the fictional visionary who challenged



the experimentalists, and Prout, the historical figure, and liar.[139] ) Lakatos nonetheless carries on with his narrative, discussing the *impact* upon other chemists of the fictional writings of the fictional character[140] : forty-five years later, according to Lakatos' narrative, "Marignac" and "Stas" continue the debate begun by "Prout"; they have 'picked up' his attack upon experiment. *In fact,* two historical figures, named Marignac and Stas, have framed the debate . . . and Lakatos consequently quotes them in the main text, rather than in the footnotes. The historical figures, Marignac and Stas, then, are quoted in the main body of Lakatos' rational reconstruction, though the historical figure Prout is actively excluded, and a fictional character constructed in his place: The debate of "Marignac" and "Stas", according to the rational reconstruction, had taken its course as a result of an utterance by a fictional character; yet Lakatos links it somehow to the writings of two historical authors, and somehow *not* to the writings of Prout.

***The trouble with Minerva:*** Lakatos' exposition of his methodology of scientific research programmes suggests that the actions of individual scientists are not of central concern to his program. His fabulous history appears to support that contention, since fictional arguments and arguments presented in quotes from actual historical sources are treated *on a par.* In more than one historical reconstruction, Lakatos also deliberately rearranges chronology, reordering the historical development of arguments to show their proper significance in research programmes.[141] It should be clear, then, that Lakatos' research programmes are not closely related to an analysis of actual figures and their actions, and it should also be clear why his analysis can provide little assessment or advice for practitioners: In a very straightforward sense, *research programmes do not exist in the real world;* the most obvious characteristics that separate them from it are that research programmes as represented in rational reconstructions incorporate fictional characters and rearranged time order. Researchers, on the other hand, have to live in the real world. Such is the character of Lakatos' history: from it, what practical conclusions regarding action *could* be drawn? (What

Lakatos' confusing rational reconstructions could possibly mean I will consider in the following sub-section.)

We are left wondering, then, what Lakatos' intentions for philosophy of science are, and also his intentions for rational reconstruction; and he has many more difficult statements to take account of. On occasion, Lakatos gives recommendations regarding rational action for individuals and policy for institutions: he argues that a new research programme which has gained little empirical success should be "sheltered", for example, and that, on the other side of the coin, research foundations should "refuse money" to degenerating programmes.[142] Perhaps for *each and every* recommendation that Lakatos gives, however, there is a footnote providing a retraction. With respect to the first bit of advice, Lakatos writes, "Some might regard—cautiously—this sheltered period of development as *'pre-scientific'* . . . ": on the second, Lakatos writes, "In such decisions one has also to use one's *common sense.*"[143] By contrast, though he may give advice with copious caveats, Lakatos also does not appear to intend that his theory be used for critically examining scientists' actions: he maintains that his methodology is first and foremost a study for the *"appraisal* of ready, articulated theories", and not primarily a repository of "*heuristic* advice". Lakatos makes the distinction clear: "methodology is separated from *heuristics,* rather as value judgments are from 'ought' statements". Appraising individuals' rationality seems to be philosophers' "*hubris"* in his opinion[144].

Lakatos' goals for philosophy of science are, he says, to present an account of scientific rationality, an account of criticism and its role in rational progress, and a system for the appraisal of theories that is not intended to be critical of actors' actual practice.[145] It appears to be to provide a *rational justification* for science—to answer the question, "science: reason or religion"?[146] in favor of the former—rather than to provide a *rational explanation* of science's historical development and scientists' practice. It is the attempt to discern whatever rationality resides in science, as seen over the long run and in retrospect; and this is, of course, the view of Minerva's owl.



Lakatos' goal reflects a return from Kuhn's position to longstanding goals of justification within the philosophy of science; it is a goal that Kant, Whewell, and the logical positivists share with him. It seems odd, however, in that it jars against Lakatos' claim to be motivated by a desire to make philosophy of science adequate to history (see his quote near the beginning of my treatment). Though Lakatos shows admirable restraint in deciding not to attempt to tell scientists their business, in this he also has abandoned a reasonable goal for philosophy of science that *all* of the other authors, to some degree, retain: one which provides a significant mandate for philosophy of science's existence. Framing his discussion in such a way that it divorces his view from any easily accessible practical account of action, or historical reference points, also distances him, on the face of it, from understanding *how science actually proceeds*. And that epistemological project, the project of discerning scientific methodology in the context of discovery, also remains a valid one, even if the prescriptive project is abandoned.

### Part II, § 4 Rational reconstruction and the history of science[147]

Lakatos' views regarding how one can and cannot go about writing history of science will be taken up in the next chapter, but it will be good to finish here with an analysis of the significance of the history written along the lines of his conception of rational reconstruction. The example of the rational reconstruction of a chemistry debate above indicates the strange character of rational reconstruction: it is part fiction and rearrangements of time-order—it embodies a genuine disregard for history (events and discipline) in many respects—but it does not appear to present an attempt to present falsehoods about the *content* of science, to the best of our current knowledge. Lakatos' footnotes, furthermore, prove that he does believe there to be genuinely accessible *facts* regarding the past that are independent of his reconstruction, and can be used to check its fidelity.[148] Lakatos also requires that rational reconstruction proceed out of the application of a philosophy of science to history; he promotes rational reconstruction according to his philosophical methodology of scientific research programmes, but allows that other rational reconstructions may be provided from Popperian falsificationism, or inductivism. So what is the point of the enterprise?

One highly anti-historical aspect about Lakatos' "internal history", or rational reconstruction, which gives a clue as to its true character and purpose, is that Lakatos requires that we take the opinions of élite scientists into account when constructing internal history. That is, Lakatos requires us to consider the opinions of the élite among scientists regarding historical occurrences—regarding what events were examples of good science—when constructing rational reconstructions.[149] But, 'taking their opinions into account' is a rather unclear recommendation, and there is also another element of slop in the system, for different generations of scientists have had different heroes and have found different events to be exceptional: I am not at all sure that Descartes, for example, would be considered a hero, nor that any of his accomplishments could be recounted by many members of the élite among physicists today, though his inclusion into the hall of fame would seem to have been more likely in the recent past. Differences in opinion are not surprising, since Lakatos stresses the importance of hindsight in assessment of research programmes, and it seems likely that physicists, like philosophers, use hindsight. It is also not surprising because most scientists, including the élite, know very little about history[150]. Lakatos also suggests at times that the assessment of the élite regarding "best gambits" in history includes an understanding of the relevant detailed methodology used by the historical actors in their debates[151]—history about which most of the élite are almost *certain* to be ignorant.

We have the existing scientific élite as a great source of knowledge about science: The question is, what, more precisely, are they



knowledgeable about? I expect that what the current élite *are* knowledgeable about is the extent to which some historical theories, and perhaps methods, as far as they know them, have contributed to *growth,* and growth *as seen from the current standpoint*. The élite know their 'textbook history'—i.e., what passes for history in science textbooks—but in good detail, and with a critical eye: Textbook history is often re-written, and the élite form their own opinion, from their working knowledge of a science, of what the really significant developments which led up to the current state of the field were. The stories of textbook history, then, would be concrete instances of the *growth* of the field to its present condition, instances which may have *no* direct relation to actual history and historical actors. In important respects, then, the dependence of internal history on élite textbook history makes it less of a genuine history, and Lakatos' allowance that time-order can be partly ignored adds to its handicaps. But these features also make internal history more of something else that is also interesting. What else?

If the élite can provide information regarding instances of growth, then internal history becomes a retrospective account of the growth and content of scientific knowledge. The élite point out the content of progressive science, and the philosopher tailors a philosophy of science to explain this content: From these sources, an understanding of the nature of science, with respect to both its theoretical *accomplishments* (or instances of growth), and the *process* of its growth, *retrospectively conceived,*[152] is achieved. The methodology presents an analysis of the process of science, and the élite provide a partial list of science's product. The élite's assessment of content presents the necessary historical content which an internal history must account for (or persuasively argue against[153]); and a reconstruction is superior to another if it can otherwise include as internal history more putatively scientific historical activity.

What, then, is the useful contribution that internal history can hope to provide? The textbook history of the current élite, when matched to theories of science which present analyses of growth, can provide retrospective histories of the current content and manner of growth of theoretical scientific knowledge[154]. The purpose of internal history is to reflect and test methodologies: A rational reconstruction provides an interpretation of history according to a methodology. Rational reconstructions, then, provide tests for deciding among general methodologies, *and the extent to which they match 'actual' history tells us the extent to which science has been methodological, to the best of our ability to construct theories of method.* Likewise, the textbook history of an élite group or individual from the past, coupled with methodologies which were also available or espoused, may provide another closely related and useful field of study for history[155].

Of course, such a retrospective history will not tell us *how science has proceeded in the past,* nor *how it arrived at its current state,* nor *how it will proceed in practice in the future:* It will rather tell us *what the current state of science is* to the best of our ability to characterize growth in our theories of methodology, and it may partially answer the question of *what the history of science means and provides for us culturally,* by presenting an *ideal* of science loosely related to history. To the extent that it fails in the first two regards, it is inferior history; to the extent that it succeeds in the last two, it has some claim to being called history nonetheless. Thus it may perform much the same function in science as the "state of the union" address in politics in the United States: it is a composite insider's view of the condition of the field, its accomplishments, current methods, and, perhaps, its plans for the future. It may serve educational purposes as a tool for explaining to young scientists, and perhaps managers of science policy, the methods and current condition of science.

## Conclusion

Thus, it appears that individual rationality has little to do with the rationality of science for Lakatos. Scientists appear to adopt research agendas according to criteria which have very weak links with what makes for progress in science; but they occasionally



produce progressive developments, and so forward the cause. Lakatos' methodology, then, appears to have little relation to the project of explaining *rational human action, and little to do with explaining the rationality of science in terms of rational human action.* Furthermore, in Lakatos' concrete examples of scientific practice, his rational reconstructions of research programmes, he often presents what he calls "caricatures" of actual history[156], re-ordering events, and examining explicitly fictional representations of scientists who could not possibly have existed. Lakatos' scientific rationality is not related to rational action; on the other hand, it is not clearly a normal participant in the context of justification debate: it uses history of science, but in a way that produces a philosophy that does not satisfy some attractive philosophical concerns.

I have suggested that Kuhn has provided challenges to philosophy of science that his later return to a traditional philosophy of science approach could not answer. I have argued that Lakatos, in constructing his own response, produced a philosophy of science in some respects even more abstract than logicism, replacing the context of justification with a context of scientific rationality. We will return to consider other responses to Kuhn's challenge in the fifth chapter; before doing so, however, we should delve deeper into the study of history, historians' methodologies, and the character of historical knowledge. We have already considered positions discussing the advantages that philosophy may or may not gain from history; what, then, are history's needs from philosophy? That analysis, an interesting subject in itself, should also give clues with regard to how philosophy can use history, and what it can expect to learn from history.

# CHAPTER 3: DOES HISTORY OF SCIENCE NEED PHILOSOPHY OF SCIENCE?

. . . philosophy of science provides normative methodologies in terms of which the historian reconstructs 'internal history' and thereby provides a rational explanation of the growth of objective knowledge . . .

Imre Lakatos

It is the historian's intellectual—and even moral—obligation not only to be self-conscious about the kinds of norms he is applying, but also to see to it that *he is utilizing the best available set of norms. . . .* to utilize a half-conscious or less than adequate model of science for writing the history of science, is as intellectually irresponsible as deliberately ignoring the evidence.

Larry Laudan



## Introduction

In previous chapters I have focused upon accounts, primarily authored by philosophers, concerning the importance of history of science to philosophy of science. I will now go on to consider the converse, the relevance of philosophy of science to history of science; a topic that is foreshadowed in the preceding discussions of Kuhn and Lakatos, who both relate the two subjects closely, but very differently, as I will indicate below. Philosophers of science tend to take a very strong position regarding the relevance of their discipline to history: Joseph Agassi, Lakatos, and Larry Laudan variously find that the history of science *requires* a careful adherence to a position in the philosophy of science, in order that a coherent historiography and a workable historian's practice may result—history of science may proceed through no other method. Certainly I do not doubt that history of a sort *can* be written on the basis of these assumptions, nor that it has been so written, particularly by Lakatos and his students; but there is also no doubt that history has also been *criticized because* it has been written in just this way[157] . What to make of such claims and criticisms?

The present chapter will present a general argument against the positions of these philosophers, that historiography for the history of science need not proceed along the lines of their prescriptions. The exercise is largely intended to restore an appropriate 'balance of power' between the two disciplines: I will attempt to argue as strongly as possible against the philosophers' claims, and for the radical independence of history of science from philosophy of science. Finding a productive point of balance, allowing for the advantages that a more restricted use of philosophy of science might provide to history, I will consider in Chapters 4 and 5.

I should make clear from the outset that I do not wish to argue that history writing can coherently proceed without an historiography (an issue at the heart of the fourth chapter); my argument instead is that historians of science may, and in fact do, proceed without a regard for the claims put on their practice by these philosophers *of science:* claims such as those embodied in the two quotations that open this chapter. I shall argue my point, first of all, by attempting to show—partly on the basis of examples—what historians do in fact write about, and how they go about it; and I shall draw conclusions regarding their intentions and the purposes of their research from these examples. I shall then indicate that these philosophers' recommendations actually serve to stultify, rather than promote, the purposes which a historian may have. I shall argue that the features which some claim are needed from philosophy of science by historians in order to write coherent history of science, such as a demarcation criterion, are either not necessary for writing history, or can be and often are acquired through other resources than philosophy of science.

General conclusions of value to a study of the relation between history of science and philosophy of science, I hope, will arise out of my discussion, and should promote a productive engagement between the two disciplines. I shall argue that, though historians may well have a use for philosophical accounts of scientific rationality in their discipline, history of science *need* maintain no closer relation to such theories than to psychological or political theories, and may nonetheless remain a coherent, vital discipline, with a clear purpose and methodology, and an appropriate claim to be called history *of science*, as distinct from other forms of history. I shall also maintain that the claims which philosophers of science put on historiography may show both an uncritical acceptance of weaknesses within their approaches to the study of science and an insensitivity to the distinct and worthwhile goals that a historian may have. Simply put, these philosophers appear to neglect that the subject of history of science is *science*, and *not* especially philosophical conceptions of growth. Nor need history's methodology be particularly closely related to philosophy of science, because historians maintain their own distinct methods: ones more appropriate to historical materials and concerns. How history can be used more appropriately in the service of philosophy of science is a general concern of this



thesis, and will receive the beginnings of a treatment at the end of this chapter.

### Part I. What is History of Science?

***Philosophers' history of science:*** In an earlier chapter I suggested what I take to be a reasonable interpretation of the project of philosophical theories of scientific rationality: They are attempts to explain science's process as approximating an understandable, recognizably rational process. We have seen that for Lakatos especially, and for Feyerabend and Kuhn as well, debate regarding the relative validity of rationality theories may concern which of the available theories best accounts for certain events in history that are taken for granted as rational developments. In this way rationality theories are mediated and judged in terms of their applicability to historical cases, for otherwise there would be no reason to believe that the theories of rationality represent the rationality behind actual scientific debate. So history may be said to test a philosophical theory of scientific rationality, and a clear role for history in the service of philosophy of science is delineated.

But for any historical event which is not taken for granted as rational—events for which, in Laudan's language, we do not have strong pre-analytic intuitions about the rationality of the development—the determination of whether or not the historical event is in fact *rational* is based on the preconceived theory, and so is determined independently of—and effectively *prior to*—a detailed examination of much of the history. Laudan refers to this dual role of history for philosophy of science by stating that philosophy is empirical with respect to some historical cases, and normative with respect to others, and Lakatos has a rough analogue of this descriptive/normative split in the importance which he gives to the judgement of the scientific élite regarding historical cases[158] . As I noted above, some history has been written in just this form, though in practice it has concentrated nearly exclusively on the former role. *Scrutinizing Science,* authored in part by Laudan, embodies an attempt to examine specific theses of rationality theories, and many articles by Lakatos and his students argue these issues. Testing philosophy with history and examining history as written from the standpoints of different philosophies of science, then, are the primary purposes for writing history and represent the goals of historians, according to two of these authors, as they themselves explicitly assert (see the quotations that open the chapter).

***An alternative empirical approach:*** But that is not the kind of history of science engaged in for the most part by historians of science, at least of late. It is rather underrepresented in premier journals of history of science today, such as the *British Journal for the History of Science, History of Science, Isis,* and *Science in Context.* What is history of science today, and what are historians' goals? For an answer, why not examine these journals to discover what the *status quo* in history of science articles is?

In *Isis,* September 1989, there are three articles along with various book reviews, discussions and letters. The first, by Rima D. Apple, concerns the establishment of patents on university research in the 1920's to '30's. It deals especially with a corporation associated with the University of Wisconsin, developed at the time to handle patents and provide a distance between research and the market. The second, by Robert W. Smith, concerns the interest in England surrounding the possibility of an eighth planet (Neptune) in the 1840's. Its concern is to explain why British astronomers, especially the Astronomer Royal, had not searched for the planet on John Couch Adams' calculations a full year before Galle found it on similar data of Leverrier; the conclusion that Smith draws is that the Astronomer Royal and others were, unfortunately, slow to the post for rather mundane reasons, despite everyone's great interest in the subject. The third article, by William Newman, concerns technology and alchemy in England in the late 13th century. Newman argues that alchemy was, like craft technology, generally viewed as an art that imperfectly imitated nature. Its poor reception in academic and theological circles was due to a belief that the products it created were unnatural and artificial; an



alchemical product of a kind was not considered to be pure because it was unnaturally produced; not a *natural* kind[159] .

Such a literal-minded empirical approach to the study of what history of science is perhaps displays a *disingenuous* reply to these philosophers: they are concerned with the philosophical presuppositions of historians, which I have not discussed since I have not worked through the articles in detail. The philosophers may also hold a normative agenda which they would like to impose on history so that it *can* make sense—can be a productive form of study, addressing held concerns—and they may indeed see much of history writing as useless, and perhaps for some good reasons. The historians may be muddled, explaining the wrong things—operating under the guise of history of science but not explaining the *science;* using the wrong form of explanation for the case; using an outmoded explanation form; or choosing an atypical historical example of scientific change without realizing it. A philosopher's work might indeed be clearly laid out: many of these issues will be at the focus of this chapter.

There is also merit, however, in actually looking seriously at what is being done by historians, for the philosophers might not have recognized a legitimate independent activity—what one *might* consider a non-philosophical one—of which these articles may be representative, and one for which their philosophies do not adequately prepare us. This chapter will attempt to identify such an independent activity which some historians appear to pursue, and in this section we may conclude on the first and most central feature of a list of differences in purpose between philosophy of science and history of science which helps to delineate the two as independent activities. The argument for that feature is as follows: Only the second of the three articles considered above could reasonably be construed as an attempt to discuss rationality and irrationality, discovery and missed opportunity—the features of history one would expect a rational reconstruction to draw from such an example—and even that article appears to have a rather distinct purpose from the one I have just suggested, to which a philosopher *might* put it. These articles have the distinct purpose of explaining what occurred, why things were as they were, how they became so, and what they produced; i.e., the historians attempt to research, and the articles attempt to *present and explain the facts*. This purpose does not directly—or at least does not exclusively—concern rationality or other issues traditionally associated with the philosophy of science; indeed, history is not a philosophical enterprise at all in this respect. In the historian's *central* concern for the facts rather than the ideal, history's separation from philosophy is assured.

This distinction between fact and ideal is quite important to make, but our discussion must be more detailed; for the issue to be argued is the extent of history's independence from philosophy of science, as no-one holds that they are identical enterprises. The extent of history of science's independence from philosophy of science will be argued below by similarly sketching an analysis of several more concerns of historians and more recommendations of philosophers of science; and the independence will be shown by arguing that the recommendations of philosophers of science for historiography primarily serve to violate, rather than promote, the historians' clear and legitimate purposes. So, a philosopher of science who does not find the discovery and explanation of the facts to be a central feature of the history of science has *begun* on the wrong foot in characterizing a major goal of historians, and (I believe) one of history, in fact[160]. I will suggest below that just such a position is taken by Imre Lakatos.

## Part II. Does history of science need philosophy of science?

### §1 Explanation of growth vs. explanation of product

My goal is to show not only the independence of history from philosophy of science, but also the *difference of purpose* behind much history which implies that philosophical theories of rationality are



inappropriate guides to writing history. I begin with the subject of rational explanation.

History is not only a discipline which searches for fact; it also concerns itself with explaining the development of those facts, and this explanation may take the form of explanation of the behavior of agents, in terms of reasons which ground their actions. But history can also be seen to be quite at odds with philosophical theories of scientific rationality, because history is usually an attempt to explain the facts, and consequently the genuine causes and reasons behind events, and not *exclusively* their rational basis. Its focus is on investigating *historically significant* perceptions, beliefs, writings and actions of historical agents, and not primarily on determining agents' perceptions as opposed to their misperceptions, nor their rationality as characterized from various standpoints, nor their progressive or unfortunate actions—which, I will suggest, are the goals that philosophical theories of scientific rationality posit for the historian. History usually focuses on how answers were found historically, on the *production* of knowledge, and not on how the right answer, as seen in retrospect, was or could have been produced.

***Lakatos' take:*** We begin with Lakatos, who most clearly exemplifies the view that a grasp of the right answer is of importance in writing history of science. He appears to maintain this because he attaches great importance to a differentiation between mere history and history of *science* , which is supposed to avoid a "blindness" of ordinary history by the use of philosophy of science:

> It will be argued [in my article] that (a) philosophy of science provides normative methodologies in terms of which the historian reconstructs 'internal history' and thereby provides a rational explanation of the growth of objective knowledge; (b) two competing methodologies can be evaluated with the help of (normatively interpreted) history; (c) any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) 'external history'.[161]

A clear purpose for the historian is set out in this quote: the historian's goal is construction of a *rational history of the growth of scientific knowledge* such as I elaborated at the end of the previous chapter, which will require a supplemental 'external' history wherever fact does not conform to the canons of rationality provided by philosophy of science. The rational 'internal' history explains the growth of knowledge on this scheme[162] , and the non-rational external history explains how the genuine historical actors fell short of rationality.[163] A clear need of the historian for the philosopher's theory of scientific rationality is also exposed: For how else could the historian know what is progressive and leads to growth *except* through a theory of rationality, which is a historic (i.e., meta-methodological) statement of what makes for progress towards growth in scientific activity?

The project of explanation that Lakatos presents makes history and philosophy interdependent activities. Because history is reconstructed differently, depending on the philosophy of science used in its historiography, history is dependent on philosophy; and philosophy is indebted to historical reconstruction because the best philosophy of science is that which produces a reconstruction that allows that the most of the best scientific activity in history is rational (that is, the one that produces the internal history most embracing of the élite's judgments is the best). What counts as 'the best' activity is determined by the considered opinion of the scientific élite of the philosopher/historian's time[164] , who presumably have been polled: the project, then, is to develop a theory of rationality which shows as much as possible of this 'best science' as rational activity; to discover what pattern of activity or methodology the best science presents that is both *prima facie* rational and, by implication, the rationality which science follows. Other activity can then be judged rational or irrational by the extent to which it fits the same pattern of explanation, by the extent to which it mimics the best science. Lakatos goes on to consider history as written from the point of view of several philosophies, in an attempt to argue that the one which he is backing in fact does the job best for history.[165]



In the previous chapter I have pointed out various problems that arise if Lakatos' methodology of scientific research programs is treated as a theory of rationality; here I am most concerned to consider the validity of his position as a guide for writing history. In this regard, it is clear that Lakatos argues that the historian has an implicit or explicit goal of providing a rational explanation of science, a goal for which a theory of rationality is necessary. Lakatos holds that the philosophy of science is necessary for two reasons. First:

> Some historians look for the discovery of hard facts, inductive generalizations, others for bold theories and crucial negative experiments, yet others for great simplifications, or for progressive and degenerating problemshifts; all of them have *some* theoretical 'bias'. This bias, of course, may be obscured by an eclectic variation of theories or by theoretical confusion: but neither eclecticism nor confusion amounts to an atheoretical outlook.

And second:

> . . . . One of the most interesting problems of external history is to specify the psychological, and indeed, social conditions which are necessary (but, of course, never sufficient) to make scientific progress possible; but in the very formulation of this 'external' problem *some* methodological theory, *some* definition of science is bound to enter. History of *science* is a history of events which are selected and interpreted in a normative way.[166]

So, because of the apparent impossibility of a history of science which does not use philosophical descriptors, and because of the normative selection of events collected under the term 'science', Lakatos concludes that history of science without an explicitly adopted philosophy of science can be nothing more than "confused rambling"[167] . Presumably, this does not mean that as *history* the presentation would necessarily be incoherent, but perhaps only partial explanation, and not properly explanation of history *of science.* We will return to these two concerns of Lakatos in more detail in subsequent sections.

***A reply to Lakatos; a different pursuit:*** In response to Lakatos' remarks concerning the goals of historians, we might question his assumption that it is a likely goal of a historian to present a "rational explanation of the growth of scientific knowledge". Lakatos implicates the historian in a concern for the *growth* of knowledge; but what importance does the choice of this metaphor have?

Growth can only be seen from particular standpoints: exclusively retrospective ones; and so the issues surrounding retrospective or 'whig history'[168] here rise to the surface. In circumscribing this goal, Lakatos also implies that what is irrelevant to the development of knowledge under a retrospective appraisal is irrelevant to the history of science: he confirms this by writing that "external history is irrelevant for the understanding of science."[169] It is a bit more difficult to determine precisely what is excluded as external, however, for though Lakatos is clear in his disparagement of what he calls sociology of science, and sees a useful but external role for psychology[170] , he is rather quiet about what, from a retrospective standpoint, counts as historical development that contributes to growth: in places, for example, he appears to suggest that *no* historically significant events in a field which were not progressive developments within the specific research program that is presently embraced are contributions to growth[171] ; i.e., that historical challenges from other research programs which may have prompted activity in the program representative of more recent science *do not belong in internal history* [172] . Such sympathies have led Lakatos to what are perhaps his most notorious pronouncements; that actual history's misbehavior might be relegated to the footnotes of an internal historical reconstruction, and that one should criticize actual history for its lack of rationality[173] .



Lakatos' assessment, however, surely does not reflect the goals of many historians, because they may instead take as their province the history of science defined sociologically, *qua* the amateur or professional activity of diverse people, latterly called scientists, and formerly called a variety of other names. History of science need not be the history of activity seen to be conducive to the current state of knowledge: particularly, it could also be the history of scien*tists* and their activity, an examination of what historically occurred and why. What, precisely, such a history is to look like has been discussed in the case of Kuhn's work (Kuhn II) in the previous chapter and will be further considered in later chapters; but the methodological differences between Kuhn's position and Lakatos' prescription that the historian is to provide an account of *growth* can be easily established. It should be clear how this history could differ from Lakatos' ideal: such a historical study need not focus on those who, in retrospect, pushed the field forward or very narrowly missed the opportunity. One might instead, for instance, find it important to focus on the activities of those who were considered by their *contemporaries* to be top performers, or focus on the most important theoretical and experimental triumphs as seen from a *variety* of historical or contemporary theoretical standpoints. Indeed, these purposes appear to be the very ones founding the second and third of the above-mentioned articles from *Isis* (the first article diverges from examination of rationality in different respects).

Why write such history? Historians are not philosophers, and they may have their reasons for differences in their aims, and methods as well. Through the present and the following chapter, I will attempt to flesh out accounts of historians' goals and the general uses and purposes of their enterprise. Here I may conclude that a historian's focus for determining what is the material within the discipline may be upon scientific activity rather than on growth; and consequently, this material may be discovered by looking at the past from a variety of historical perspectives, and not necessarily retrospective ones. In this way, history may be pursued which does not concern the rationality of science and the growth of knowledge as seen in retrospect, because it is concerned with discerning the *process* by which knowledge is produced; the historical process of a human activity. *A historian may be concerned with the production rather than the growth of knowledge*; with knowledge's actual history, rather than its internal history. Whereas a philosopher is concerned with exploring the possibility of the rational growth of knowledge, a historian may be concerned, in contrast, with explaining the rational production of a continuously evolving (perhaps actors') category *also called 'knowledge'*, regardless of whether that activity contributed to the current research program's success.

Why should a philosopher care about such history? This question, too, will have a long development throughout my thesis. For a quick point in support of such an approach, however, recall that in the second chapter of this thesis I produced an argument to suggest that Lakatos' goal of producing a "code of scientific honesty" was not well-served by his rational reconstructions, because reconstruction illuminated the path of historical progress, but could only *indirectly* illuminate good practical method via progress. The shortcoming is due to the separability of success and good method: one could conceivably follow the best of methodologies in some situations, and still produce null results or results which have no impact on a research program; and this would be so because the fault does not lie in the method, but in the purpose of the experiment, or, alternatively, in the structure of the world being not as anticipated. One could, perhaps, learn a great deal about methodology from historical events that, on Lakatos' scheme, would be excluded from the history of science; and this is why the opinion of the actors about who amongst them is performing the best work might be worth looking into.

***A plea for growth:*** Concerns that I wish to separate, however, others might wish to unify: though historians of science do pursue history in this way, Lakatos might still be within his rights in



asking whether they should. One might at this point ask: "would such a pursuit properly be called history *of science?* This may be a worry because the approach might not allow for a differentiation between history of science and other cultural history, since such a project does not appear particularly suited to producing or accommodating any criterion of demarcation: science, under this historiography, is 'levelled' to the status of a human activity, not Platonically demarcated as knowledge-construction. Consequently, science does not appear to be differentiated from other human activity: it remains a cultural feature of some societies, to be studied in its development in ways similar to the study of other cultural features.

I believe that some historians of science, even in their soberest moments, may have no qualms about such a result. They may see science as an interesting social activity, interesting because its influence is currently very prominent in political and economic affairs, perhaps much as the Church was in earlier times. But many, I expect, also see science as very different; perhaps they harbor a belief that science maintains some apolitical and amoral dimensions of significance, captured in the word 'knowledge'. The challenge of a Lakatosian is: if one does hold that there is such a difference, can such an approach do justice to that difference? The study of science as a culture like any other might present a useful methodology for studying the history of science—for avoiding unfortunate methodological errors, such as presentism in explanation—but perhaps it belies the original motivation which many have for studying history of science rather than history of other forms of culture: the importance and use of science's specific products of knowledge, its forms of explanation, and the control of nature which it may promise. *Would one really wish to study the production of knowledge, rather than its growth?*

These worries, then, would appear to support Lakatos' division of the history of the activities of scientists into the internal and the external—or, equivalently, the relevant and the irrelevant to the growth of knowledge and the understanding of science—as defined by a normative philosophy of science. They would also support his claim that "History of *science* is a history of events which are selected and interpreted in a normative way."[174] Without a normative philosophy of science backing our judgments—providing a *philosophical demarcation* for the term 'science'—how can we determine whether or not a field of debate is within the bounds of science, whether or not a change in a social institution affects science, whether or not an individual is a scientist, and whether or not a given activity of that individual is concerned with science? If the goal of a historian of science *is* to study scientists and their activities and institutions, how, except through a normative theory, can the class of relevant individuals and activities be chosen?

### Part II, §2 Historiographical weaknesses of Lakatos' and Laudan's approaches

I hope that I have appropriately presented the quality and the spirit of Lakatos' concern above; it now remains to answer the above objection, and consider in particular the problem of demarcation. The reply will come in two parts. Section 3, below, will discuss how historians do, in practice, solve this problem of demarcation. To begin, however, since Lakatos points out the limitations of history without a philosophical guide, so I should point out the weaknesses of history written under the sway of a philosophy of science. To do this, I will examine both Lakatos and Laudan on the relation of history of science to philosophy of science. I will argue that Lakatos' internal history would behave in a way which I take to be unfitting to the historian's purpose, since considerations of the present state of science weigh too strongly in demarcation, and cause internal history to fluctuate improperly in its compass. I will argue that Laudan, by contrast, commits himself to an unfortunately naive account of our understanding of history in his account of the relation of history of science to philosophy of science; one that shows weaknesses that only an



acknowledged, more sophisticated role for the history of science can strengthen.

***Lakatos' approach and the content of science:*** Philosophies of science are themselves fallible, empirical theories, and Lakatos himself sees limitations to the depth and accuracy of a history of science which takes a flawed philosophy of science into its methodology: certain patently 'rational' historical occurrences fit the partially successful philosophical model which has been adopted, and certain *other* rational events do not. Lakatos writes that each imperfect position in philosophy of science thus has both its exemplary successes in its treatment of important historical cases, and its own embarrassing failings with respect to some others; he illustrates the relative merits of philosophical programs in his review of the Copernican revolution. The inductivist program, he suggests, finds great support in Kepler's derivation of elliptical orbits for the planets from Tycho's data, but can hardly do anything with Copernicus, for the advantages in his system were primarily heuristic, and not mensural. The Duhemian conventionalists, on the other hand, see their brand of rationality in Copernicus, but haven't much to say about ellipses[175]. Lakatos claims that the most success heretofore in accommodating these and other putatively great developments accrues to his methodology of scientific research programmes—but he makes no claim to have eliminated all conflict.

There is a decided weakness in Lakatos' approach, then, for on any imperfect philosophy of science, the written history will be flawed, and will have unfortunate lacunae that can only be described externally; or, alternatively, history may be bolstered with ideal rational reconstructions which fabricate a revisionary internal history that is historically inaccurate to the extent that the philosophy of science is flawed. This makes for incomplete or incorrect historical explanation, but Lakatos feels justified in presenting this solution because he finds that "*History without some theoretical bias is impossible*", and the theoretical bias for history of science he takes to be within the scope of concepts of the philosophy of science (and not merely within philosophy of history, a view that I will promote). And so, Lakatos allows the weakness, and holds that those histories of science which are based on the philosophies of science that produce the most progressive and successful historiographic research programs are the best we can hope for.[176]

So we have a significant weakness in history written according to Lakatos' recommendations, if we do not expect to meet up with a perfect theory of scientific rationality any time soon; but the problems for history become much worse than Lakatos makes them out to be if we also factor in the changes which science undergoes over time, *for the historical development of science itself affects the dicta of these historiographical research programs,* and most notably Lakatos' own program. This is so because, for histories which are, to paraphrase Lakatos, relevant to understanding science, or which serve to provide "a rational explanation of the growth of objective knowledge", what has been conducive to growth is very much dependent on what is currently taken to be knowledge—and *that* changes over time. Growth is linked to the current state of knowledge for two reasons: First, if history of science is supposed to be the history of growth, then historical events which did not contribute to growth to the present state do not belong in the (internal) history of science. Since the relevance of past activity to current theory varies in sympathy with developments in *current theory,* however, historical activity which was once considered relevant may become irrelevant, and some activity which was once considered irrelevant becomes relevant. Second, if the judgment of the scientific élite is used to determine what is 'good science', then the research programs in which they are engaged at the time of appraisal are likely to play a role in determining their choice. This would not be so great a problem if the élite did not maintain different research programs within a field at different times in history, for then the change of viewpoint might be largely a matter of accumulation, but Lakatos allows that different programs are prevalent at different times. *Thus, for one who links history of science and rationality to growth, or to the opinion of the scientific élite, the internal/external divide will be in constant flux, locked to the changes of science.*



Some examples of research programs which have entered into and exited from internal history are appropriate here, to show that a criterion of growth makes internal history very fluid. As debate raged in the 1960's over the existence and nature of complex water-molecules known as polywater, historical developments within the research program would have been considered to be internal history to structures physics; since the research program's clear demise in the mid 70's, however, the entire debate is irrelevant. Perhaps none of the polywater debate is likely to be called 'science' on Lakatos' 'growth' criterion: it is to be excluded from the internal history of science, because the entire debate was non-progressive as seen from the present[177].

Since theories and theory-kinds return to favor in addition to going out of favor, however, revision also is not merely a process of realizing the retrospectivist fruits of philosophical appraisals of growth. It is not merely the case that more and more history, such as the polywater debate, has over time been found to be irrelevant as our knowledge increases: Heliocentrism and the wave theory of light, for example, after long periods in which they were out of favor, were 'rehabilitated' from the perspective of growth, and allowed to re-enter the current canon. Indeed, even if the unrealistic assumption that scientific theory advances in a simple cumulative manner were entertained ('historical inductivism'—see Agassi below), then growth might *still* present this problem: each significant event in science would be another step up the ladder to the present state of knowledge, but new theoretical development might rehabilitate older developments which before were thought to be irrelevant[178].

Lakatos acknowledges the truly chameleon-like character of history written from different philosophical perspectives in his explanations of those positions[179], and to these changes that result from philosophical perspective, I have added changes that result from scientific perspective. So history must be rewritten at every theoretical turn in science for Lakatos. Joseph Agassi, albeit sarcastically, sums up the problem well with regard to the inductivist historiographical tradition, which relied on progress in its appraisals, as Lakatos does: "The great scientist has to foresee intuitively the future textbook of science in order to gain the approval of his inductivist judges"[180]. Agassi's estimation of the worth of such history rings true: Lakatos' internal history, like the inductivist's, is "ritualistic"[181], and primarily serves the purpose of canonizing scientific saints and excommunicating the infidels who gave nothing to present science, excepting, perhaps, the goad to the saints. Since history is on Lakatos' program primarily meant to serve the purpose of explaining scientific knowledge's development up to the present standpoint, and since science in the present is and ought to be continually reformulated and improving, that the historical categorization of science 'improves' along with it may be a virtue in his opinion. But, as I have indicated above, the historian's concern for the facts of actual history suggests a focus on the actual *production* of 'knowledge' as a historically changing category, rather than a focus on scientific growth. Agassi's comical remark suggests the absurdity of defining the methodology for the historian, as well as for the scientist, in terms of a retrospective philosophical analysis of growth. I take it that this argument also serves to indicate another reason why internal history is a flawed tool for gaining another of Lakatos' goals, what he calls the code of scientific honesty.

***Laudan's approach:*** An historian may, then, have some legitimate concerns about writing history according to the guidelines of a theory of scientific rationality which springs from an analysis of the growth, rather than an analysis of the production of knowledge. Laudan, however, ties history to a different horse: on his account, rationality theories which inform history writing are tested according to our "preferred pre-analytic intuitions" about the rationality of certain historical events and possibilities, and so the growth of knowledge is not central to his scheme. How does this account serve the historian? I will suggest that it entails either a



naive understanding of historical knowledge, or a circularity which, when exposed, will allow us to gain a clearer grasp of the importance of history's method and its appropriate role in testing philosophical theories of scientific methodology. All of this should suggest that history needs philosophy far less than Lakatos and Laudan maintain.

Laudan 's view has many similarities with Lakatos': they both, in a similar manner, maintain that history may serve to test theories of scientific rationality, and that history of science can and should be written within the guidelines of a theory of rationality. They do differ in detail, however, and these differences are important. For Lakatos, the judgment of the scientific élite indicates the paradigm events of good science to which a theory of rationality should largely conform[182]. Laudan is a bit more liberal in his method for discovering paradigms: he suggests that a more universal form of intuition about specific historical cases, which is available to any educated person, can guide the appraisal of rationality theories. To make this case, Laudan presents a distinction (somewhat like one which Lakatos makes[183]), between **HOS**$_1$, the actual past of science, and **HOS**$_2$, the writings of historians about the past:

> Within HOS$_1$, there is, I shall claim, a subclass of cases of theory-acceptance and theory-rejection about which most scientifically educated persons have strong (and similar) normative intuitions. This class would probably include within it. . . . (1) it was rational to accept Newtonian mechanics and to reject Aristotelian mechanics by, say, 1800; (2) it was rational for physicians to reject homeopathy and to accept the tradition of pharmacological medicine by, say, 1900 . . .
>
> . . . there is a widely held set of normative judgments similar to the ones above. This set, constitutes what I shall call *our preferred pre-analytic intuitions about scientific rationality* (or "PI," for short).[184]

Given this tool, the preanalytic intuition, which simply replaces Lakatos' use of the judgment of the scientific élite, a method of testing theories of rationality very similar to the one presented by Lakatos is open to Laudan, and is endorsed by him:

> In the extreme case, a proposed model of rationality would be justifiably dismissed out of hand if, when applied to the cases involved in PI, it entailed that all our intuitions were incorrect, for it would have failed to capture the very rationality it was designed to explicate.
>
> . . . we can move beyond the extremal case to claim more generally that *the degree of adequacy of any theory of scientific appraisal is proportional to how many of the PIs it can do justice to.* The more of our deep intuitions a model of rationality can reconstruct, the more confident will we be that it is a sound explication of what we mean by "rationality".[185]

Laudan's story is very like Lakatos' at this stage, excepting the imperative nature of the claim in italics; and so his statement about the bias involved in writing history and his conclusions about moral strictures and the historian's role in writing history are also not surprising:

> . . . wrong or not, it is inevitable that any historian's account of science is going to be colored by his views about how science works. Such 'coloring' only becomes invidious when the motivating philosophy of science is implicit and uncritically utilized, or when its existence is denied by the historian who imagines that he is free from any normative biases.
>
> It is the historian's intellectual—even moral—obligation not only to be self-conscious about the kinds of norms he is applying, but also to see to it that *he is utilizing the best available set of norms.* How can he make that choice? By accepting that model of rationality. . . . which does the greatest justice to our PI's about $HOS_1$.[186]



Laudan's view of the relation of history and philosophy of science is very clear: theories of rationality that best fit our strongest intuitions about the rational acceptability of scientific theories under historical circumstances are the best theories of scientific rationality; and those should be used by historians to guide their historical reconstructions, if those theories are adequate.

I see two problems with Laudan's enterprise. The first is that it does not appear to provide the material necessary for constructing a theory of rationality, and provides a test for such theories which is of little critical power. Laudan's method only allows for the development of rules to be used for determining the rational acceptability of theories: his is an enterprise solely concerning the 'context of justification'. Furthermore, he uses lamentably safe examples of justified theory acceptance in the sample list of pre-analytic intuitions which is partially reproduced above. It may have been rational by 1800 to accept Newtonian dynamics over Aristotelian dynamics, but this is because the Aristotelian program was *long* dead by then, and Newtonian mechanics was in full flower. *Perhaps* a theory of rationality should allow for such intuitions, but do these intuitions serve us as any guide in constructing a theory of rationality, or as any significant test of one? Such a theory is wanted to answer the question: was it rational to accept Newtonian dynamics in, say, *1685*, such as the theory was developed after Halley's question about orbits, or in 1689, two years after publication of *Principia*? More important than this concern, a theory of scientific rationality should provide us with an analysis of grounds for experiment design and *pursuit* of a theory, and not merely for theory acceptance. If any important conclusion accrues to Laudan's example, it is not whether Newtonian mechanics should be believed in 1800, but whether any form of Aristotelian program warranted pursuit at that time.

Lakatos' approach, on the other hand, at least provides us with some recourse for these concerns. Because it hinges on the appraisal of the scientific élite regarding events of 'good science' in actual history, momentous and real achievements are likely to be placed under consideration. The élite are likely to consider the procedures and outcomes of real experiments, such as the Michelson-Morley experiment, and real theoretical novelties, such as the Copernican system in 16th century context, as examples of rational practice and science at its best. Lakatos' approach practically guarantees that *some* living experiments and theory decisions—what William James would call genuine options[187]—are considered, and not just ones hundreds of years dead. Lakatos' approach also allows for and suggests directions for the development of a theory of scientific rationality which is not exclusively concerned with belief and theory choice. The élite can be polled on, and the theory of the methodology of scientific research programs can be extended to cover, issues of pursuit and experiment from history which Laudan's theory has not been developed to cover[188].

The second problem I find in Laudan's approach is perhaps practically less apparent—at first glance a point regarding unclarity in his exposition—but one which, when appreciated, indicates that history's role for the philosophy of science must be quite different from that which both Lakatos and Laudan give to it. The point concerns Laudan's explanation of the "preferred pre-analytic intuitions" about scientific rationality. To explain the substance of these intuitions, Laudan makes the abovementioned distinction between $HOS_1$ and $HOS_2$:

> . . . it will be helpful to remind ourselves of one elementary but crucial distinction which is germane to this discussion: specifically, the distinction between history of science itself (which, at first approximation, can be regarded as the chronologically ordered class of beliefs of former scientists) and writing *about* the history of science (i.e., the descriptive and explanatory statements which historians make about science) . . . I shall use "$HOS_1$" to refer to the actual past of science and "$HOS_2$" to refer to the writings of historians about the past.[189]



Here is the core of the problem: If historians' work is history of the $HOS_2$ variety, what are our pre-analytic intuitions? Laudan claims on the next page that philosophy of science "can dispense with $HOS_2$", but has a "parasitic dependence" on $HOS_1$. Since, as is clear from the second of the above quotes from Laudan, pre-analytic intuitions cannot be dispensed with in producing an adequate theory of rationality, it is apparently the case that Laudan believes that they are not $HOS_2$. In terms of Laudan's presentation they *could* not be $HOS_2$, of course, since the intuitions are beliefs, not inscriptions; but Laudan clearly intends that they have a status different—and in a way perhaps *closer* or more true to $HOS_1$—than the writing of historians: a status which is signalled by Laudan's use of the word "intuition" in this case. The difference lies in Laudan's claim that we can dispense with $HOS_2$ in preparing our philosophy, but cannot dispense with $HOS_1$, nor with our pre-analytic intuitions. This difference between intuition and $HOS_2$ is apparent in Laudan's recommendation that:

> The task of the historian of science, so conceived, is to write an account ($HOS_2$) of episodes in the history of science ($HOS_1$) utilizing as his criteria of narrative selection and weighting those norms contained in that philosophical model which is most nearly adequate to representing PI. [190]

It is less important to determine whether or not Laudan actually maintained that our PI's about history are genuine intuitions[191] (that is, knowledge of a different kind than the ordinary empirical) than it is to see that such a division of historical knowledge into privileged cases and normal history presents a misleading characterization of much of the way in which we come to understand history, and how we may responsibly judge theories of rationality.

For where do those who are "most scientifically educated persons" get their pre-analytic intuitions? There can only be two sources, if we discount genuine intuition: either one goes back to the original texts of the time and understands the field in question well enough to work through the problems as a "virtual witness"[192] —as a historian of science might—and *uncover* intuitions, *or* one repairs to $HOS_2$! The former path is only open to dedicated historians and experts, able to grapple with the languages, concepts, and history of the time; and the path is open to them only for a limited range of fields, since time and energy are limited. And in practice, $HOS_2$ is used by such historians in their work, for how common is it for a historian to actually learn a field in any distant past without recourse to $HOS_2$ in the early years of an education, and without constructive dialogue with peers in the later years? The second path, learning most of history through $HOS_2$, is certainly the far more common way for most of the scientifically educated, such as myself or a modern scientist, to get a grasp on many or all of early 19th century Newtonian mechanics, early 20th century homeopathy, late 19th century thermodynamics. . . . and other backgrounds necessary to develop the Laudanite intuitions. If the only other way to develop these intuitions for events in the historical past is to become a historian and make a historian's judgement, how are these intuitions any different from the most deeply held beliefs about history which one would write down as $HOS_2$?

The trouble with Laudan's argument is that there is no particularly compelling reason to believe that our pre-analytic intuitions, which he claims are our most firmly held beliefs about the rationality of certain events in the history of science, are especially *useful*, since Laudan has not argued that they are also our most *justified and rationally held* beliefs about the rationality of certain events in the history of science. This is a fair conclusion for two reasons: the first is that the writings of historians and scientist-historians about the history of science are likely to have affected the educated person's opinion about the rationality as well as the importance of certain historical events: these 'intuitions' are likely



to be pillars of certain historians' programmes which may now illegitimately be supporting the scientific culture, and may indeed be of mythical events, or be assessments of questionable validity[193] . Secondly, the events which we are most likely to have strong intuitions regarding the rationality of are likely to be very *vivid* ones, but this does not necessarily mean that they are the most rational and the best ones to anchor a theory of rationality to, because their vividness has little to do with the assessment of their rationality.[194] Pre-analytic intuitions are likely to pass a vividness criterion rather than be paradigms of rationality—they will be the events we all remember: historically 'big' events, such as the Copernican revolution and the cracking of the genetic code, but big, perhaps, because historians (or contemporary scientists) have told us that they are big. Such events *might* not be considered to be rational by most of us, or perhaps even by any among current historians, because they have been carefully examined and have proven to be paradigms of rationality; rather, they might be considered to be rational because they were the big, formative events of science on someone's interpretation of the formative events of science (for if science is a rational enterprise, how *could* the big events be anything less than rational)?

A theory of scientific rationality should definitely conform to and explain those events of history which are most rational; I would suggest, however, that our pre-analytic intuitions may not provide us with a good list of those events. This is because, for the scientifically educated person to whom Laudan appeals in setting up his criterion, most of those intuitions are very likely to be based on a very weak grasp of a broad sweep of the history of many fields. Ask the ordinary "scientifically educated person" why Copernican astronomy triumphed over Ptolemaic, or when it did—probably questions which would fit Laudan's intuition criterion, primarily because they are so famous—and you are likely to get a few embarrassing answers which show rather little knowledge of history and the 16th century mind. This is because the scientifically educated person, generally, has a lamentably poor grasp of the history of science.

Finally, it seems that we should deny one of Laudan's assumptions, especially given the possible weaknesses which accrue to relying on our pre-analytic intuitions. Laudan presents a criterion for judging the adequacy of theories of rationality: "*the degree of adequacy of any theory of scientific appraisal is proportional to how many of the PIs it can do justice to.*" I have suggested that our PI's may not provide us with the best cases of rationality; it is instead a much better criterion of a theory of rationality that it conform to the *best* cases in history. Perhaps our PI's do currently give us the best guide we have available to finding the best cases; perhaps a survey of PI's provides the best approximation to the class of best cases available. But the imperative form of Laudan's claim regarding the adequacy of a theory of rationality suggests an inflexible disregard for other plausible ways of determining the adequacy of a theory (e.g., through application of various theories as predictions of scientific development), or for determining the set of best cases of rational science (e.g., through a dialectical assessment of events resulting from a comparison and assessment of intuition combined with an application of theories of rationality). *Ideally, we want a theory of rationality which will allow us the possibility of overthrowing our preferred preanalytic intuitions, not one adequate to them.* A theory of rationality suitable for guiding history writing might not come from an articulation of pre-analytic intuitions.

### Part II, §3 How a historian may solve the problem of demarcation

So far, I have allowed Lakatos and Laudan to subordinate history of science to philosophy of science by imposing philosophy as an activity methodologically prior to the writing of history. Why retain the hierarchy? Lakatos gives two justifications: the first is that historians have the goal of presenting "a rational explanation of the growth of objective knowledge", the second is that "History of science is a history of events selected and interpreted in a normative way". I have argued at length that historians are likely to have a different conception of their field than the first justification suggests, and one that is still clear enough: historians may be concerned with process and production, rather than growth, and I have also argued that conceiving of the history of science in terms of growth and writing history in terms of explicit theories of scientific rationality present problems which the historian is likely to want to avoid as well. Lakatos' second reason for subordinating history to philosophy is that history of science studies a grouping of events ('science') which are selected normatively; and without a philosophical grounding for that normativity, he claims, historical investigation can only be "confused rambling". Presumably, Lakatos would have the historian examine each debate which is a candidate for anointment as a portion of the history of science according to the criteria of his methodology. The historian would judge whether the persons concerned are recognized as members of a progressive research program, and whether the particular event of research, or theoretical accomplishment, serves as a contribution to this progress[195] . The approach appears plausible for a historiography that embodies the goal of explaining the growth of knowledge, but it hardly coincides with the historian's goals suggested above, for reasons which I hope are now quite clear. Nonetheless, it is true that 'science' is a normative term, perhaps suggesting some specific standards of discourse, and the term is certainly useful for including some and excluding others from discourse. How, then, *without a philosophical basis* could an historian determine what is and is not science, and who is and is not a scientist, assuming that the historian does want to demarcate science?

***Another empirical foray:*** How might a historian come across a topic, and how be assured that the concern is history of science? I do not doubt that history *can* be written in the manner which Lakatos suggests, and I also don't doubt that the writing of most history of science is in some way *affected* by philosophy of science. But I deny that this is how most historians determine what is science, what mattered historically, and what they are interested in, for those functions can be served for the historian by much more humble historiographic principles. Events become important to historians, and are flagged as scientific, for a variety of reasons different than those provided by the philosophers above. Though a number of potential markers for scientific activity immediately come to mind, it is appropriate at this point to *avoid* the temptation to adopt the philosopher's mode of presentation—that of an 'armchair historiographer'—and avoid producing a theory of historical sources of science from intuitive reflection. If reflection on sources is wanted, why not return again to the empirical mode, and ask the historian?

> I had been asked to advise on a private collection of the scientific manuscripts of George Bellas Greenough, the first president of the Geological Society of London. When I first saw them they were in chaos, with letters from different correspondents and different decades mixed up without a trace of order. It was therefore all the more striking to come across one bundle of letters carefully tied with red tape, and labeled in Greenough's distinctive handwriting, "Great Devonian Controversy.". . . . it was Greenough's label that first suggested that the Devonian controversy, although less well known than other conflicts in the history of geology, might have particularly deserved the epithet "great," and not only in the eyes of those who took part in it.[196]

Here we have a wonderful sort of story, variants of which are often told by historians[197] . A chance find: an interesting parcel wrapped in red tape—a topic suggested by a long-dead participant, no less!—launches Rudwick's *The Great Devonian Controversy.*

Historians are not entirely guided by chance, however—consider some of Stephen Jay Gould's historical work:

> I am aware that I treat a subject currently unpopular. I do so, first of all, simply because it has fascinated me ever



> since the New York City public schools taught me Haeckel's doctrine, that ontogeny recapitulates phylogeny, fifty years after it had been abandoned by science. Yet I am not so detached a scholar that I would pursue it for the vanity of personal interest alone. I would not have spent some of the best years of a scientific career upon it, were I not convinced that it should be as important today as it has ever been.[198]

So begins *Ontogeny and Phylogeny*, which contains some careful analysis of a debate in embryology of 100 years ago. It seems that the original kernel for Gould's interest was, by chance, found in an outdated school curriculum, and this may even have been necessary for his coming to a positive assessment of a position resembling it a century later. His purpose in re-telling the history, however, is to re-awake and further inform a dormant scientific research tradition.

One more example:

> . . . . we will deal with the historical circumstances in which experiment as a systematic means of generating natural knowledge arose, in which experimental practices became institutionalized, and in which experimentally produced matters of fact were made into the foundations of what counted as proper scientific knowledge. We start, therefore, with that great paradigm of experimental procedure; Robert Boyle's researches in pneumatics and his employment of the air-pump in that enterprise.
>
> Of all the subjects in the history of science it might be thought that this would be the one about which least new could be said.[199]

Stephen Shapin and Simon Schaffer do not, perhaps, reveal an original cause of their research here, but they do clearly expose some reasons as to why they have pursued it: The air-pump experiments are both seminal to experimental method and paradigmatic; the authors also see a challenge in finding something new along one of the most well-traveled paths.

***Normative demarcation:*** Enough of empirical anecdote, for now—I have attempted to suggest that historians have a great variety of sources for determining their fields of research. *Perhaps* Lakatos is right in claiming that history of science is "of events selected and interpreted in a normative way", but it seems reasonable to say that *philosophy of science* need play no role in providing the normativity. For there are many other avenues of inquiry open to the historian, and other ways of determining what has been considered to be science, and what has been important in a field of science. For example, the groups 'scientist' and 'scientific' might be socially defined. As scientists practicing in their respective areas, Rudwick and Gould have first-hand acquaintance with what scientists in their fields believe, and what they believe to have been important in history, in addition to their own opinions; and contemporary scientists' views of history, I expect, provide one resource for many other historians. More relevant from the historian's point of view: a historian might also consider the views of scientists or the scientific élite of another era, and especially of those contemporary to the controversy to be investigated; and such a source is directly suggested by the ribbon-tied parcel mentioned in Rudwick's quote.

My point is that whereas one *might* request of a historian a 'philosophically respectable' practical demarcation of scientific from non-scientific activity—if only to preserve the category of 'history of science' (a category that historians might alternatively decide to *throw off*)—that demarcation, as Lakatos points out, is motivated by considerations in *philosophy of history*, and need not concern philosophy of science *at all.* What scientists say and have said in the past about the importance of actors or events, then, may provide the normative demarcation required by the historian to begin study. And scientists' opinions do provide many springboards for launching historians' projects: a conclusion that is not at all surprising, since scientists' judgments regarding the virtues of the



work of other scientists appear to play a great role in the historical development of science. Whether or not the historian finds at the end of exploration that the supposed important events were *in fact* historically important in production or growth, however, is determined through distinct historical methods; and through historiography that has its own rules, as I will argue in the following chapter, and that has no need of philosophy of science.

There are also many other sources from which the historian of science may work to delineate the field, as suggested by these authors. It is fair to surmise that topics are also chosen because they are personally or currently scientifically interesting (see Gould above), the favorites of other historians (see Shapin and Schaffer), politically significant[200], sensational[201], or perhaps even because they are illustrative, but are historically unimportant, and have been utterly neglected by others[202]. With the few testimonials given above, a further rich variety of causes can be seen to launch historians into their topics. Precedent in the writing of history guides a pair of authors, purpose for current scientific research another, and pure luck another—a fuller treatment of the context of historical research I will leave aside for now.

***Kuhn's solution:*** The advantage of the philosopher's demarcation criterion is that it makes clear what is relevant to the subject of the growth of science, and what is not. Is there a similar tool for the historian to use? We have seen that historians do have certain plans of attack towards their subject matter, but is this a method, or a vague approach to an unclear subject of study? Since historians are not philosophers, it is not surprising that explicit methodological discussion of a demarcation criterion is not prevalent; Thomas Kuhn, however, gives a reasonable statement of one historian's solution to the problem of demarcation:

> Insofar as possible (it is never entirely so, nor could history be written if it were), the historian should set aside the science that he knows. His science should be learned from the textbooks and journals of the period he studies,

> and he should master these and the indigenous traditions they display before grappling with innovators whose discoveries or inventions changed the direction of scientific advance. Dealing with innovators, the historian should try to think as they did. Recognizing that scientists are often famous for results they did not intend, he should ask what problems his subject worked at and how these became problems for him. Recognizing that a historic discovery is rarely quite the one attributed to its author in later textbooks (pedagogic goals inevitably transform a narrative), the historian should ask what his subject thought he had discovered and what he took the basis of that discovery to be.[203]

Here Kuhn suggests that the historian must, insofar as is possible, set aside a concern with present science and instead attempt to consider what scientists of the time thought their discipline was, and what problems they took to be important, by taking cues from contemporary textbooks. The innovators' discoveries and accomplishment should be explained in terms of their causes, and the reasons by which they became acceptable to the actors and their contemporaries, as innovations for the field at the time of their proposal or acceptance. One should forget present science "insofar as possible" so as to be able to understand the material as the actors did; to take the role of a "virtual witness"[204].

The relation of this methodological prescription to the historical studies discussed above appears to be rather close: a goal of many of the historical studies considered appears to be to explain why changes occurred, or why theories were acceptable to proponents, and were later accepted by opponents, or outlived them, or were dropped. The difference between this approach and that of a philosophical methodology which invokes the growth of knowledge as an organizing principle for historical exploration and explanation, on the other hand, is great.

***Production vs. growth revisited:*** These are sources for historians; but the philosopher might once again demand to be heard,



and ask how a historian knows that such history, derived from such diverse concerns, is history *of science.* A historian may be able to locate a socially-defined group called "natural philosophers"—for example, by looking at the lists of the Royal Society, or distinguish a problem by examining their debates—but how, without a philosophy of science, can a historian be sure that any given action of a person in the Royal Society is science, or any given move in a debate is relevant to the history of knowledge conceived in any way, even non-retrospectively? We have returned to the doubts voiced earlier in the chapter: wouldn't a denial of the philosophical demarcation of scientific growth belie the historian's motivation?

To ask these questions once again, after having surveyed these sources of the historian's product, is, I think, to miss the historian's point. I have attempted to show that there are significant conceptual problems with philosophers' criteria for answering these questions, and I have also tried to show that, were such criteria to be clear and perfectly successful, they would *still* not be appropriate for the goals of many historians. For if a historian is interested in understanding the *production* of knowledge, a demarcation of science by means of a standard of the *growth* of knowledge is simply inappropriate. The historian *must* look elsewhere to determine what is and is not science: since the goal is to determine what was in fact effective in producing knowledge—a historical category—preconceptions of what was conducive to the growth of knowledge—a non-historical category, essentially composed in the present time—are inappropriate, or at least must be maintained such that they be subject to revision. My examples have suggested how a historian might proceed: by looking at who scientists talk to, and what they say about each other's work, albeit with a critical eye.

Laudan, in his discussion of historiography, excuses actual historians from using philosophical metamethodologies and models of rationality in their practice, because, he explains, those philosophical constructions are currently too rudely developed to answer the historian's needs as resources for historical explanation.[205]

Again, I think Laudan has missed the point, expecting that historians of science have as their primary goal *historical philosophy of science*, rather than the history of science (and in his discussion, Laudan suggests that "Many historians will doubtless agree [with the ideal he mentions]"). What has Laudan got wrong? Some historians[206] are, I expect, very interested in explaining the development of science and scientific knowledge, and 'development' in a very rich sense of the word, akin to the philosophers' conception of 'growth' considered in this and previous chapters. But what historians find in their accounting of development may be quite different from what philosophers such as Laudan would consider to fall into an accounting of growth. As I will argue in Chapter 5, some historians argue that a study of rationality provides only a very pale understanding of the character of scientific development; these historians argue that a broader analysis, incorporating a survey of broader aspects of a culture, is necessary to explain development. Thus, I think that these historian do not belie their motivation for studying the history of science by denying the centrality of theories of rationality presented in philosophy of science: for their accounts do not ignore philosophical efforts, so much as they are in direct competition with them, as hermeneutically appropriate (or, if you like, sufficient and satisfying) accounts of scientific development. These issues will be at the focus of the fifth chapter.

***A historian's practice:*** We may finish this section by examining what, more precisely, a historian's activity is likely to be. I have argued that a historian is not likely to be involved in explaining growth, and need not appeal to philosophy of science to define the discipline of history of science. Surely, however, an historian does not *only* have the goal of finding out what happened in the past: that might be more properly labelled 'antiquarianism'. I have suggested above that historians of science are likely to be driven to their discipline by a desire to understand science and its methods as much as by an enjoyment of studying history. For the philosopher, such a motive is expressed in the explanation of growth; for the historian, the motive may extend to a concern for the process by which knowledge is attained in fact, socially and practically, rather than ideally; a concern for production, rather than growth. Intimately tied to a historian's concern for the production of knowledge, however, is likely also to be a concern with the *producers*. A historian may be interested in the scientific activity of scientists to develop a clearer idea of the social process of production of knowledge, or for reasons equivalent to those of anthropologists interested in other 'foreign' cultures. As well as being interested in the production of knowledge, a historian might attempt to study the culture: an interest in the activities of any or *all* of a scientific community, not just the activities of the 'heroes', is a fair goal for a historian. Such an 'anthropological' conception of the goals of history of science is a coherent and reasonable concern to couple with the historian's more basic task of finding out what happened, and with the historian of science's goal of understanding the production of knowledge. *A factual anthropological account of the production of knowledge could be considered a realistic historian's approach to the discipline of history of science,* and one which would contrast fairly clearly with Lakatos' claim that the goal of the historian is to produce a rational explanation of the growth of knowledge.

If this conception of a historian's purpose is taken seriously, what, then, remains of Lakatos' basis for subordinating history of science to philosophy of science?



## Part III. The relation of history of science to philosophy of science

In previous sections I have attempted to indicate the independence of the history of science from philosophical theories of scientific rationality by showing that the recommendations presented to historians by philosophers have generally presented blocks to the historian's purposes, rather than aiding them. I have also attempted to argue that those features of history's method which philosophers of science have maintained must be provided by philosophy, such as the historical demarcation of science, can be obtained by the historian in other ways. It now remains to present a clear account of a more reasonable and productive relation between history and philosophy of science, which avoids the problems pointed out in this chapter. My account, which will require the further arguments concerning the methodological independence of history of science that I will present in the following chapter, and a discussion of philosophical goals in the fifth chapter, I will begin to present here, on the basis of the discussion of the past few chapters. The approach that I advocate requires us to examine more carefully the purposes of theories of scientific rationality, in order to suggest that they cannot simultaneously serve all of the purposes for which they have often been intended.

***The variety of purposes of philosophical metamethodologies:*** The theories of scientific rationality and the rational reconstructions built from them that we have studied have been given several distinct purposes, and the desire to make them serve too many roles, I believe, has made them impractical. First, they have been intended to explain the historical **process** of science—to explain, for example, why Copernicus' program *succeeded* Ptolemy's—and to provide a historical analysis in terms of the game that the actors were playing. This is the only truly **historical** role that a theory of rationality might play; the only way in which it is used to *explain* historical events. Lakatos and Laudan appear to be correct in insisting that to the extent that real history follows a rational reconstruction, it can be said to be rational and is (and should be) explained in rational terms; the rest is external history. Second, theories of rationality have been intended to explain the historical **success** of progressive science as a knowledge-acquiring activity; to explain why Copernicus' program *superseded* Ptolemy's, and why the historical actors play the game that they do. This is the **epistemological** role of a theory of scientific rationality, the retrospective explanatory account of the history of knowledge acquisition, representable in a rational reconstruction, and testable by history's consilience with the rational reconstruction. Third, theories of rationality have been intended to provide practical theories of rational **practice** to guide scientists and science policy in the present.



This is an entirely non-historical **methodological** role for a theory of rationality, to provide a normative procedure to practitioners, at the very least in a theory of demarcation, for the sake of pursuit and funding. Rational reconstruction and its consilience with history are used to illustrate, test, and defend a methodology.[207]

***Methodology vs. epistemology:*** The historical, the epistemological, and the methodological I take to be the three traditional roles for theories of rationality. A tension between the historical and epistemological roles of rational reconstruction has occupied us to a great extent through this chapter; I would suggest that the tension is due to the circularity of utilizing history as a criterion of adequacy for a rationality theory's performing its epistemological role, and then returning the favor to history by using the theory of rationality so tested to inform historical explanation. That tension will be considered below; there is another important tension to be considered, however, between the epistemological and methodological roles of theories of rationality. The former role is explanatory, and used to justify past development of science, whereas the latter is practical, and forward-looking: a methodology might itself be justified by showing its prevalence in good science of the past, but that is not the only possible justification for putting it forward, and not even a necessary or sufficient one, as we will see below. A methodology which was only used for a short period of the history of science, or one which has never been used—that is, a methodology which conflicts with the epistemological role of a theory of rationality—might still be presented as warranted.

I have not presented an argument to show that these tensions *cannot* be resolved, and that a fully encompassing theory of scientific rationality cannot be formulated. If science's history is ideally rational, and if good scientific methodology has remained unitary through science's history, then rational reconstruction will fit history precisely, and the game will be won. Hope can always be held out for an adequate theory of rationality to replace flawed and incomplete predecessors[208] . Alternatively, history and reconstruction could be noted to diverge, and separate accounts might be written, as Lakatos suggests; for Lakatos, however, reconstruction essentially ignores the historical intentions and products of the actors, and so the rationality of science on his view has little to do with the rationality of the history of science[209] . The possibility of a divergence between epistemological justification of the history of science and current methodology, however, is one which requires serious consideration: it could be that a theory of scientific rationality that explains the rationality of history is not what is needed for (or at least is not the same as) the best methodology that philosophy of science now has to offer. This may be so for two reasons: The first is that methodology or aims may be evolving, so that current methods or concerns do not match those which are historically operational. If the epistemology of science is evolutionary, a theory of rationality must also indicate this development, and methodological recommendations might then be historically delimited; that is, *all* of science's history might not provide the appropriate illustrations for exhibiting good methodology. The second reason that methodology may not match epistemology is a skeptical one, and may be drawn out of the discussion of Laudan in the previous section. The reliance of an historically adequate theory of rationality on our intuitions (or those of the scientific élite) about good events in the history of science, I have suggested, makes it highly susceptible to our current historical understanding, which is largely given to us by tradition and past writings in the history of science, which may be biased, selective, and peculiar. For these reasons, the epistemological project of justifying history and the methodological project of recommending practical procedures to scientists may differ radically, and might not both be accommodated in a single theory of scientific rationality.

***Abandoning the unified project:*** Such considerations have led to two solutions to the problem of relating the epistemological and methodological projects. The first is to deny the importance of the epistemological role (and so the historical role as well) of theories of rationality, and only consider current science as a useful contributor that may inform the methodological enterprise. The



theory of rationality thus becomes exclusively a theory of methodology, and changes through history of the aims and methods used are no longer a problem because history is excluded from consideration. This line is taken by Giere[210] , who claims that to understand the structure of theories and the process of debate for the purposes of methodology requires only an acquaintance with contemporary science. As the philosopher thus is not dependent on history, so the historian is also not dependent on philosophy for patterns of explanation: the two enterprises are at most in a marriage of convenience, for "even historians would agree that relevance to present science decreases the further back we go."[211] This claim and others suggest that Giere has no desire to engage in the epistemological project of justifying history, and so his solution is clear and simple, though such a crude approach may needlessly deny the possible relevance of history to methodology.

A second approach to resolving the tension between methodology and epistemology is to recognize both the legitimacy and differences among the two enterprises, and allow that a theory of scientific rationality is not a theory of methodology. Laudan adopts this position explicitly in a 1987 article, specifically because of the problem of changes in goals of science over time:

> to the extent that scientists of the past had aims and background beliefs different from ours, then the rationality of their actions cannot be appropriately determined by asking whether they adopted strategies intended to realize *our* aims. Yet our methodologies are precisely sets of tactical and strategic rules designed to promote our aims.[212]

Such an approach to methodology does allow a use for history in the form of examples: the historical track-records of various candidate methodologies are examined to provide data on whether these strategies are progressive or not, and this can be done without a concern for the rationality or irrationality of the historical actors' actions, with regards to their aims[213] . Laudan's advocacy of such a procedure further underscores the new split he sees between the theory of rationality and the theory of method: Since the rationality of history has nothing to do with methodological questions, and history is only to be used piecemeal for examples testing principles relating to a retrospective characterization of progressiveness, the project of methodology and the epistemological project of discovering the history of scientific rationality are clearly divorced. If methodology is the only task of philosophy, then philosophy and history are quite independent.

***The relation of history of science to scientific rationality:*** Since our concern here is to examine history's relation to philosophy of science, however, we should once again pick up the thread that Laudan has just dropped. If the goal of producing methodological recommendations is to be divorced from theories of scientific rationality, then what, in the light of arguments of this chapter, is the resulting relation between the remaining two roles which the theory was intended to handle, the historical and epistemological projects? And what is their relation to history as written by historians?

I have attempted to argue that history writing remains largely independent of philosophy of science. Because of differences between the goals of theories of rational explanation and explanation in the history of science, because of the difficulties attached to taking rationality theories to determine the appropriate format of all historical explanation or even of rational explanation of science, and because the problem of demarcation is solved differently for the two disciplines, history of science is in many ways free of, and may be quite independent of theories of rationality; and so the claims made on it by Lakatos and Laudan need not hold.

Historical explanation of science is also independent because it need not be limited to rational explanation: sociological, psychological, marxist, economic, demographic, and other forms of explanation are available to the historian, and may be wielded to solve the problem of explaining historical events in science. I have tried to argue that there is no special relation between history and



philosophy of science: none more special than that between history and psychology of science, or history and sociology of science. The historian's goal is historical explanation, *tout court.* The philosopher's usual goal, on the other hand, is to determine how much of actual science's development can be explained in terms of rational decisions of actors (or Lakatosian scientific rationality) and especially whether there are a small number of patterns of decision and activity which characterize activity within the scientific community almost all of the time. It is also a historian's goal to explain action rationally, but it is not a foregone conclusion for the historian—and philosophers have provided the historian with no compelling reason why it should be—that *rational* explanation (individual rationality, or Lakatosian scientific-rationality explanation), is the only kind of explanation either operative within or of primary interest within history of science. For this reason, the historian's work stands apart from philosophy of science, and because of the great likelihood that a historian is a superior master of the details of history, and more familiar with historical explanation, the historian may also judge the worth of a rationality theory with respect to its applicability to history, with respect to its worth for historical explanation. Though a common goal for history is to provide explanations of historical developments, *it appears to also be the* ***historian's*** *role to determine for the philosopher whether or not a* ***rational*** *explanation is in order to explain any given historical event.*[214] Ideally, both historians and philosophers should agree on whether a historical event has been adequately explained: there is no duality of truth-for-philosophy and truth-for-history in the explanation of history.

## Conclusion

This brings up the question of who would, then, be a better authority in the case of a disagreement over explanation, or of how each discipline can utilize input from the other. If many philosophers were to see a particular rational explanation of a historical event as plausible, whereas historians did not, or vice-versa, how could such a dispute be settled? It is appropriate to keep clearly in mind why such a question should be asked: it is not asked in this place in order to establish a hierarchy of relations of dialogue between history and philosophy of science, so that one may forevermore retain authority over the other. The purpose of the question is instead to allow for the possibility of useful dialogue between the two disciplines; since dialogue can be useful, given that philosophers admit that history is relevant to theories of rationality.

Perhaps the question can be usefully framed as a challenge to philosophers, to replace the hierarchical relation among the two disciplines proposed by the philosophical authors whom we have considered in this chapter. I have attempted to show the independence of history of science from philosophy of science. What of the historian's position as a critical commentator to philosophy? I think the historian's authority may be reasonably taken to hold with regard to rational reconstruction and the interpretation of history; and this, I expect, is so because of a practical asymmetry between the two disciplines. Science may or may not proceed rationally, in whole or in part, as seen from the non-retrospective standpoint of appraising the actors or their collective activity: this should, I believe, remain an open question unless a reasonable epistemological argument, such as an adequate rational reconstruction, closes it. Who, however, is actually better qualified to close the question in that way, to determine whether philosophical models of scientific rationality are adequate to history, than the historian (or, more accurately, the community of involved historians as a critical body, whom I have fictionalized as 'the historian' throughout)? A historian is likely to have a much better grasp of historical psychology and historical detail than a philosopher, a grasp which takes a great deal of time and work to develop, and which might be desirable for a historical assessment of a theory of rationality. The philosopher's learning is no small task either; however, specific theories of rationality which have been proposed *are*, by reason of their purpose and character, rather brief and learnable schematic



or theoretical structures which a historian might well be able to master with a reasonable amount of work. And this asymmetry in the difficulty of accessing the other's field suggests to me the reason why an appropriately prepared historian's opinion should be important to those philosophers who wish to keep the quest for scientific rationality a unified project, and who wish to gather historical backing for their theories. The historian is not likely to be a master of theories of rationality, or even, perhaps, fully expert at applying any of them, but the grasp of a historical situation by one who is primarily a philosopher strikes me as yet more likely to be suspect. To parody a statement of Lakatos': Philosophy proposes, history disposes.

Such a retort to Lakatos and Laudan, however, presents a pale reply in comparison to that which I wish to develop in the following chapters; for I expect that history of science can provide many more interesting ideas in aid of philosophy of science than those addressed in this chapter. The primary shortcoming of the approach considered in this chapter, I believe, is that historical interpretation is conceived of as radically dependent upon philosophy of science, and is taken to aid philosophy only insofar as it might be used to test preconceived philosophical theories. History, then, plays the role of nature to philosophy's position as science: it is exploited by philosophy of science solely as a source of data, serving to confirm or dis-confirm philosophical theories, in experiments set by philosophers. Though there may be a virtue in modeling philosophy of science somewhat along the lines of its subject, the corresponding relationship constructed by these authors between philosophy and history appears less than appropriate. History of science and philosophy of science are more in a symmetrical relation than a hierarchical one: they are two humanistic disciplines, each of which presents *interpretation*, and neither of which should be conceived of exclusively as playing nature to the other's science. Perhaps history, then, can enter into a productive interaction with philosophy of science; *suggesting* hypotheses concerning the nature of scientific change, rather than just testing those presented by philosophers. I will work towards presenting a more equitable relationship, and a productive dialogue between the two disciplines, in the rest of this thesis.

The authors that I have considered in this chapter, I believe, make the mistake of confusing philosophy of science with philosophy of history. The latter (as a practical philosophy, but not necessarily as a field of academic study) *does* have a special relation of priority to the practice of history; *the former does not.* In the following chapter, I will consider the relation between history of science and philosophy of history, in order to show further history's distinctness from philosophy of science, and to show the independent foundation of history's practice. A better grasp of the practice and the basis of historical research, I hope, can better expose its genuine character, and the ideas that it has to offer to philosophy of science, in a constructive role beyond that conceived by Lakatos and Laudan, of a mere laboratory assistant. In the fifth chapter, I will attempt to indicate the sorts of contributions that history promises to provide for philosophy of science.

# CHAPTER 4: HISTORIANS AND HISTORIOGRAPHIES

## Introduction

Comments from Laudan and Lakatos in the two previous chapters should indicate that these philosophers have specific ideas regarding the uses of history for the philosophy of science, and, related to these ideas, others regarding the characteristics of good or useful history. We have seen that these authors suggest that history can be used to serve the purpose of confirming or disconfirming philosophical theories of rationality and rational reconstructions; and how this history is to be written, and how it serves as a test to philosophy, can be learned from reading the history written by these authors and others.

Certainly there does exist history writing related to these authors' purposes, and perhaps even some that was not written at the instigation of philosophers. I have also suggested that writing in the tradition of the history of ideas has affinities to philosophical theories of rationality, at its 'internalist' focus appears to mirror a vaguely stated theory of rationality that is the cause of historical change in science. A glance at the appropriate bookshelves, however, suggests that these approaches are not representative of the contemporary work of many historians, including historians of science. What is history of science, then? Morrell and Thackray write of the genesis, birth, and early life of the British Association for the Advancement of Science[215] ; Gingerich and Westman write of the appropriation of an itinerant scholar's work by Tycho Brahe[216]; Richard Westfall writes a detailed biography of the life as well as the thought of Newton[217]—these examples are of history of science that is not only history of ideas: history of science can be the history of institutions, of the scientists' viewpoints, of biography, of research schools, of controversies, of experiments.

Kuhn, Feyerabend, and others have called upon philosophers to drop Popperian and logicist abstractions, and examine the original sources and the documents that played historical roles in the development of science; their goal, apparently, is to develop a philosophical approach truer to an analysis of actual change, and of sources of change in science. In the remainder of this thesis I make a slightly different call: I ask that philosophers pay more attention to *historians*—the experts in the field—in addition to 'paying attention to history', whatever that may entail. For history (and only just recently, history of science as well) is a very subtle and well-developed discipline, engaged in debate over both content and method that might teach the philosopher a thing or two. History, I expect, is an enterprise quite able to provide hypotheses regarding the nature of scientific change that could be of interest to philosophers. Rather than attempting to re-make the practice in philosophy's image, philosophers should attempt to draw from history's product *in situ*. A clearer understanding of what historians are up to, of the varieties of historical writing, and of how different these are from those conceived of by philosophers of science, may provide new realms of material for philosophers of science to work on.

Underlying the variety of historical genre, I will argue, is a profoundly important root in the variety of historical *explanations*. We have already considered several interpretations of rational explanation of history; I would like to argue here for the existence of two different paradigms of historical explanation, which have affinities to the *Erklärung / Verstehen* distinction of continental philosophy. The titles that I will give these two projects in historical research are the 'prosopographic' and the 'sympathetic' historical



methods; but to begin, these names are perhaps less revealing than examples would be: I will argue that some historical subjects, such as biographies of scientists and studies of controversies, may be approached, and are likely to be approached by many historians, through a well-defined and essentially different method than subjects of, for example, socio-economic history. Though there does exist overlap in the ways in which the subjects are treated—i.e., one *can* use many methods for many historical subjects—that there are two distinct methods of inquiry, argument, and explanation in history, which are, in addition, separable from the tradition of the history of ideas, will be the focus of this chapter.

This division will have important implications for both of the philosophy of history and the philosophy of science, for it will imply that Hempel's claim that all historical explanation is analogous to scientific explanation is misguided. It will also imply that the conception of the virtues and usefulness of history that is maintained by many in the philosophy of science is biased towards only a small portion of the field of history. Prosopographic explanation, which does appear to be amenable to Hempel's covering-law model of historical explanation, does appear to be quite appropriate for the sorts of use that the philosophers of science discussed above have put history to. I will argue that sympathetic history, however, is not so amenable: it will require different treatment to be utilized in philosophy of science.

My purpose in working out this distinction, then, is dual: to clear up confusions about what history is, and to suggest, once the confusion is cleared, how history can be of use to philosophy of science. In this chapter, I will focus upon the former purpose, and in the fifth chapter I will suggest ways in which the relation between history and philosophy of science should be reconceived in order to take advantage of the greater variety of history writing.

**Part I. The covering-law model and prosopographic history**

To begin, I will lay out a matched pair, of philosophical ideal and historical research that appear to be in quite happy harmony. another, and divergent from of history will be treated in part II.

***The covering-law model of explanation:*** In 1942, Peter Hempel published "The function of general laws in history", which presents the covering-law model of historical explanation. Hempel's concern in the article is explanation, and his central claim is that historical explanation is much like scientific explanation, in that it tacitly or explicitly invokes laws to ground explanation: "scientific historical research", he claims, is concerned with the "search for general laws" which govern historical change, and also with explanation (and perhaps prediction), via the use of these laws[218] .

Hempel's view of historical explanation is auxiliary to a view of scientific explanation; one which he presents briefly in his paper, and which is similar to that which Hempel would discuss in more detail six years later[219] . The essence of Hempel's account is that an occurrence is explained scientifically when it may be *logically deduced* from the conjunction of a set of initial (determining) conditions, and a set of universal hypotheses, which are laws. In full dress, a scientific explanation for Hempel is:

(1) a set of statements asserting the occurrence of certain events $C_1$. . . . $C_n$ at certain times and places,
(2) a set of universal hypotheses, such that
(a) the statements of both groups are reasonably well confirmed by empirical evidence,
(b) from the two groups of statements the sentence asserting the occurrence of event E can be logically deduced. [220]

Hempel presents as an example a sketch of an explanation as to why a radiator in a car has cracked on a cold night: from a knowledge that water expands significantly below 39.2 degrees, and that the night temperature has fallen below 32 degrees, and other such



laws and conditions, a deductive explanation of E can be constructed. In his introductory treatise on philosophy of science, Hempel even presents explanations in three-line syllogisms, making the first lines conditions, the second lines laws, and the third lines E-type (*explanans*) events; thus

x is a metal
metals expand when heated
____________________
x expands when heated

is a (very simple) scientific explanation to the question, "Why did x expand?"[221] .

Natural scientific explanation provides the model for historical explanation on Hempel's analysis, and the historian is concerned to show that the events of history can be explained; so history is much the same as any other branch of empirical science in this regard:

> The preceding considerations apply to *explanation in history* as well as in any other branch of empirical science. Historical explanation, too, aims at showing that the event in question was not "a matter of chance," but was to be expected in view of certain antecedent or simultaneous conditions. The expectation referred to is not prophecy or divination, but rational scientific anticipation which rests on the assumption of general laws.[222]

Hempel appears to be claiming that historical explanation is a *branch* of scientific explanation, that it is unlike divination and prophecy (pseudo-science and pseudo-explanation), and implicitly, that there is no third way by which explanation is to be characterized: historical explanation is bound to laws. Hempel, then, argues that he has characterized a model of explanation which is both normative for historical explanation, and represents the ideal to which historical explanation tends. Though some historians would disagree that this historiographical explication captures the nature of historical explanation, Hempel argues that the way in which history is written and historical argument proceeds suggests otherwise.[223] Their arguments will be considered when we pick up Hempel again in the third part of this chapter; immediately below I would like to elaborate on how accurately Hempel's explication has hit the mark for many philosophers and historians.

***Covering laws suit rational explanation and metamethodology:*** The demerits of the covering-law model as it pertains to history will be considered in later parts of this chapter: Here I would like to point out its merit as a characterization of a good variety of projects in both history and philosophy of science, for some maintain that it does not appropriately characterize the discipline of history at *all*[224] . I see *laws*—causal laws, laws of behavior, laws of rationality—as the objects of historical research and the material of historical explanation in the history of ideas, prosopographic history, theories of rationality and meta-methodologies: Hempel appears to have been on the right track regarding the research of *some* historians; but discerning that the covering-law model is very close to philosophical projects of explaining scientific activity should also suggest that those projects are ill-adapted to exploit history which has *little* relation to covering-law explanation; history of a sort that we will consider in part II.

Among theorist of rational explanation and metamethodology, a hankering for laws of rationality and laws of behavior is quite pervasive, and often explicit. The covering-law model can be seen to be at the root of Laudan's more recent work on metamethodology, and his use of history in *Scrutinizing Science* with Rachel Laudan and Arthur Donovan. *Scrutinizing Science* is the epitome of an approach that aims at determining covering-laws, for it is a volume explicitly dedicated to testing laws of change against the history of science. The three organizers and editors of the work present it as an exercise to test the historical adequacy of 32 theses about how science changes, and sixteen historians of ideas each consider the



applicability of several of these theses to a specific historical incident. Such theses as, 'the appraisal of a theory depends on its ability to turn apparent counter-examples into solved problems', and 'new sets of guiding assumptions are introduced only when the adequacy of the prevailing set has already been brought into question'[225] , are tested against events such as the receptions of plate tectonics and Cartesian mechanics. The editors *clearly* see their project as an attempt at "hypothetico-deductive" historical confirmation of theses that have been suggested by various philosophers of science and that should be considered to be laws. Their explicit goal is to determine explanatory laws for the history of science: "Science may not be a natural kind, and its features may be so various that no general 'laws' govern its history. . . . But such a conclusion would be warranted only *after*, not before, a sustained effort to identify the rule and rhythm of scientific change."[226]

In his recent writing on metamethodology, Laudan also endorses a similar examination of history for the purpose of a hypothetico-deductive testing of theses about what makes for *successful* science with respect to particular goals.[227] Though the project is a methodological (and especially, normative) one, Laudan's goal of determining what makes for successful method towards achieving one's ends is realized by his examining the history of what methods were in the past successful towards achieving similar ends. Laudan makes it clear that this is not 'writing history', but making use of historical events for purposes other than history writing:

> to the extent that scientists of the past had aims and background beliefs different from ours, then the rationality of their actions cannot be appropriately determined by asking whether they adopted strategies intended to realize *our* aims. Yet our methodologies are precisely sets of tactical and strategic rules designed to promote our aims.[228]

Laudan nonetheless *uses* history in this endeavor, for he wishes to discover from history the track-record of the effectiveness of certain means to our present ends, and so history is opportunistically exploited along the lines of the covering-law model. Laudan advocates the construction of testable methodological hypotheses of the form "If you want to achieve X, perform Y"; and he tests such a proposal by considering whether method Y has promoted X in the past. Methodological rules shown to be linking effective means to ends in historical cases I take to be substitutable as causal explanations of those historical incidents: in this way, covering law-history—the search for and application of laws in deductive historical argument—appears to be at the foundation of Laudan's meta-methodology.

***Covering laws suit the history of ideas:*** For Laudan in particular among philosophers of science, then, the covering law model of explanation can be seen to be a favored approach; and it appears to be a fitting methodology exactly to the extent that rational action is characterizable (and so, explainable) in terms of rule-governed activity. For history as it is written by historians of ideas, the same may apply: explanation in terms of rules of rational behavior is a common goal[229] . This should come as no great surprise, given the parallels between intellectual history and philosophy of science that I have sketched in preceding chapters: a quick example from John Worrall should indicate the applicability of the covering-law model to the rational explanation of history. Worrall's approach is to show that a principle concerning the significance of different sorts of evidential support for a theory that could be used to explain the history of behavior of actors is also a rationally compelling rule of scientific behavior. The principle is rationally compelling to us, and scientific (i.e., not a philosopher's fiction) because it coheres with the history of science; presumably, the historical actors themselves found the principle rationally compelling, probably for reasons not unlike those that Worrall gives, and such a rationally-grounded principle guided their actions, and so explains the actions. The rationally-grounded principle, then, underwrites a law of behavior that can be used to explain history.

For an example, consider Worrall's treatment of the reception



of Fresnel's wave optics, in which he argues for a principle of rationality that is normatively acceptable to us and serves to explain the actions of the committee studying Fresnel's proposal. Worrall is concerned to explain why, contrary to popular myth, the diffraction pattern predicted by Poisson's theory of the white spot produced by a point-source at the center of a circular shadow, apparently had little impact on the committee's decision to applaud his paper, after one member of the committee went to the trouble of confirming the prediction. Worrall suggests that the facts suggest that the white spot—despite its being a novel prediction—was of little importance because the committee recognized that it provided no empirical support independent of another well-known effect from a straight-edge shadow that Fresnel mentions in his essay. Worrall then proceeds to argue for the rationality of an account of empirical support that squares with this behavior: He argues that the support accruing to a scientific theory depends little on novel predictions produced by the theory, and much on the *independence* of empirical support from the construction of the theory. Worrall argues for the rationality of treating empirical support in this way, and then uses the principle that he has argued for to explain the historical action that occurred rationally. In his introduction to the argument, the purpose of using the principle as a covering-law for the explanation of a historical incident is clearly signalled: "one of the consequences of the account of support to be defended in this section is that Fresnel and his contemporaries *as a matter of fact* judged the theoretical import of the straightedge and circular screen diffraction results exactly as they *ought* to have done, according to that account."[230] Not only did the committee members' actions coincide with the rule; that the rule also provides an explanation of *why* the committee so acted is implied by Worrall's writing: the committee's action is shown to have occurred because of a principle that the committee members appear to have arrived at through a rational analysis of evidential support.

***Covering laws suit some mainstream history:*** Our inventory of historians interested in laws has grown to incorporate several rationality theorists, methodologists, and historians of ideas; their views appear to embrace the covering-law model, since rules of behavior, method, and rationality—which all may be treated as laws guiding historical events—are at the roots of their explanations. One might not be too surprised by this, given that Hempel's covering-law model of historical explanation was inspired by his view of scientific explanation, and given that all of the above mentioned enterprises are geared explicitly to the philosophical and rational explanation of science itself: an affinity between the methods of these approaches and those used in their subject of study (science) might not be accidental; some philosophers of science even strive explicitly to make their methodologies scientific[231] . But the covering-law model appears to reflect positions within the 'mainstream of history' as well, apart from intellectual history and the study of science.

The project of generating laws for use in historical explanation is one that has been taken up by a good number and variety of historians. Members of the French *Annales* school of history in particular have their hands in such research, as well as some historically-oriented sociologists, such as R. Merton. A useful general term for all of these approaches is *prosopography* ,[232] and one definition which prefaces an article on the topic clearly shows the approach's close relation to covering-law explanation:

> Prosopography is the investigation of the common background characteristics of a group of actors in history by means of a collective study of their lives. The method employed is to establish a universe to be studied, and then to ask a set of uniform questions—about birth and death, marriage and family, social origins and inherited economic position, place of residence. . . . and so on. The various types of information about the individuals in the universe are then juxtaposed and combined, and are examined for significant variables. They are tested both for internal correlations and for correlation with other forms of behavior and action.[233]



From prosopographic research (including research not exclusively focussed on individual humans, as this definition suggests), one discovers lawful correlations and causes; these laws may then be used in the explanation of history. Fernand Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II* is perhaps the work of history that presents the most detailed and convincing argument for the usefulness *and necessity* of a prosopographic approach for achieving some worthwhile goals in history. The work is an attempt to explain historical developments in the region of the Mediterranean in the late 16th Century, and Braudel finds fault with older histories of the region, and suggests the need for a new approach—what we may call *Annales* prosopography—to achieve understanding:

> . . . dare I say it, at the risk of seeming ungrateful to my predecessors, that this mass of publications buries the researcher as it were under a rain of ash. So many of these studies speak a language of the past, outdated in more ways than one. Their concern is not the [Mediterranean] sea in all its complexity, but some minute piece of the mosaic, not the grand movement of Mediterranean life, but the actions of a few princes and rich men, the trivia of the past, bearing little relation to the slow and powerful march of history which is our subject. So many of these works need to be revised, related to the whole, before they can come to life again.[234]

In contrast to the history of 'great men' that he opposes, Braudel presents a different image of history:

> The resulting picture is one in which all the evidence combines across time and space, to give us a history in slow motion from which permanent values can be detected. Ge-

> ography in this context is no longer an end in itself but a means to an end. It helps us to rediscover the slow unfolding of structural realities, to see things in the perspective of the very long term. . . . Here it helps us to discover the almost imperceptible movement of history. . . .[235]

Braudel, then, claims that a history of great men will not expose the reasons behind the changes of history: a history that concerns many other people, and many other factors influencing human action. For these reasons, he claims that he must tell the history of the Mediterranean three times, each one representing a different "general explanation"[236], to do his topic justice. The three explanations are differentiated by Braudel in terms of time-scales; events of long, medium, and short effect or periodicity affect history in different ways: the explanation of the *structure* of history primarily concerns the relation of people to geography; the *conjoncture* concerns social structures and economic systems; and finally, there are the individual *events* of history *("l'histoire événementielle")* : "surface disturbances, crests of foam that the tides of history carry on their strong backs."[237]

A detailed analysis of Braudel's tripartite division and hierarchy of historical causes would be of great intrinsic interest, and of great use to this chapter, but I allude to it here only to point to it as a paradigm of prosopography. Because Braudel finds it necessary to include *structure* in a historical explanation, two important points touching our concerns follow. The first is that he finds the analysis of individuals and their ideas and actions insufficient for his goals. Braudel is interested in explaining the history of a region of the globe, and to achieve this, he finds that he needs certain kinds of knowledge: for example, knowledge of geography, climate, crop harvests, plagues, and prevailing winds. I will argue in the next section that history that has been written with different goals in mind has need of different methods: *perhaps* the *structure* is not even an important concern for other purposes in other history. Differences in concerns, I will argue, make covering-law explanation appropriate to some history writing, and inappropriate to other history writing. The second point is that Braudel's concentration on geography suggests the importance for his project of law-based explanation. Geography, crop harvests, etc. have, on Braudel's approach, a regular impact on the development of human history, which has a place in the explanation of history. In making explicit the causal influence of geography on history, the importance of a specific science and of its laws to history is also made clear.



### Part II. Sympathetic history and the sympathetic model of historical explanation

Braudel is clear in his demands regarding the importance of prosopography, and I have attempted to indicate the relation of prosopography and rational explanation of history to the covering-law model. Braudel finds prosopographic study so important, I believe, *partly* because of his subject—nothing less than a slice of 50 years of life around the entire Mediterranean sea. Clearly climate affects agriculture, and agriculture affects such a history: without geographical science, Braudel's story would certainly be incomplete. Scientific sources and covering-law explanation do suit his goals. On the other hand, he also appears to suggest some plausible arguments against the credibility of historical conclusions based exclusively on *histoire événementielle:* arguments that we will return to later in the chapter.

But what can be said in defense of the biographies of great men, such as Philip II, which Braudel disparages? As histories of the Mediterranean, he can find great fault with them, but if they were written for other purposes—for example, to show the work of an expert warrior and politician—then perhaps the criticism misses the mark. If the purpose of the biography is to convey (or even explain) *how* Philip II acted, or how one could or should act as a ruler, then geography and climate might have little importance: whether it rained on a certain day or in a certain pattern might have affected *what* Philip II did, but it may have had little effect on *how* he did it; it might have had little effect on why or whether he acted as he did.

Though history itself—time and events—is one thing, there are, on the face of it, different ways of writing history, for different purposes, with different methods of explanation and argument. Different goals require different methods and standards, and the goals of much of history writing, I would like to argue, are different from those of prosopography and rational explanation, and their methods and standards of explanation may be different from those reflected in the covering-law model. To argue this point, I would like to present a theory of historical understanding and explanation to contrast Hempel's, and one that I see as prevalent in much of history writing. The relation between prosopographic history, which is amenable to covering-law explanation, and what I will call 'sympathetic' history, which presents sympathetic explanation, will be considered in part III, once the distinction between the two is made clear.

***Re-enactment and other sympathy theories of historical understanding:***[238] The clearest proponent of several aspects of the view of historical understanding that I would like to develop is R. Collingwood, an author helpful primarily because he is more of a philosopher than a historian, and so is inclined to write on method as well as to demonstrate method through his history. Collingwood's *The Idea of History* is also particularly interesting because it is approximately contemporary with Hempel's article (it is published six years later, but largely worked out a decade or more before publication); in it, Collingwood is particularly interested in conveying the nature of a historian's understanding of the subject :

> . . . how does the historian discern the thoughts which he is trying to discover? There is only one way in which it can be done: by re-thinking them in his own mind. The historian of philosophy, reading Plato, is trying to know what Plato thought when he expressed himself in certain words. The only way in which he can do this is by thinking it for himself. This, in fact, is what we mean when we speak of 'understanding' the words.
>
> It is not a passive surrender to the spell of another's mind; it is a labour of active and therefore critical thinking. The historian not only re-enacts past thought, he re-enacts it in the context of his own knowledge and therefore, in re-enacting it, criticizes it, forms his own judgement of its value, corrects whatever errors he can discern in it.[239]



For Collingwood, re-enactment is at the core of all historical knowledge. There is no history without re-enactment, and understanding of living and present people as well is achieved through re-enactment[240] : Collingwood intends that his theory explicate the method of understanding everything related to mind, and all facts of relevance to history also fall into this category[241] .

This is the kernel of Collingwood's re-enactment theory, and it is really rather elementary: Collingwood is stating that we must think through another's thoughts in order to understand them and know that we understand them. It is important for later discussion to note carefully what Collingwood does and does not say here: he does say that one can re-think another's thoughts; he does not say that one does this by 'putting oneself in the other's shoes'; that is, he is not claiming that the understanding is achieved through *empathy,* though he is also not necessarily claiming otherwise (unless the critical approach rules it out). Collingwood's theory is one of a grouping that I will call "sympathy theories"[242] of understanding. The widespread popularity of sympathy theories for historical and social scientific understanding should become evident through the development of this chapter.

***Sympathetic explanation and covering laws:*** We now shift from understanding (the process that must occur in the historian's mind) to explanation (the method used in persuasive writing), and consider how the covering-law model sits with Collingwood's sympathy theory. In his exposition of the covering-law model of history, Hempel states that history is, at base, "analogous" to science in many respects. Presumably, given that laws are to be at the center of both types of explanation, this means that history, like science, is concerned with both of the tasks of generating laws and using them to explain historical events (and perhaps it is also concerned with predicting events). The historian is concerned to show that the events of history can be explained, and this implies a model of explanation; as I noted before, Hempel claims that:

> Historical explanation, [like any other branch of empirical science], aims at showing that the event in question was not 'a matter of chance,' but was to be expected in view of certain antecedent or simultaneous conditions. The expectation referred to is not prophecy or divination, but rational scientific anticipation which rests on the assumption of general laws.[243]

I don't doubt, *pace* Collingwood and perhaps Dray as well, that Hempel is partially right in this: as I have argued in the previous section, the projects of generating and using laws in historical explanation is one which has been taken up by a good number and variety of historians. There is definitely a place for such research; but Hempel wishes to claim that it is front and center in history, representative of all of the best of historical explanation, or at least representative of the ideal for all of the best. But note that the projects listed in the first part of the chapter, and the one that Hempel cites as an example, are either *abstract* intellectual history, or socio-economic history, dealing with large populations and economic forces, and not especially concerned with individuals and their discoveries. Much of history, and much of the history of science, however, consists of studies of individuals of historical importance, of great significance or great ingenuity; and so, much of the history of science written by historians is quite unrepresentative of prosopography, even though there is definitely room for



that kind of research in history of science[244] . There is a good body of history that apparently neither generates nor uses historical laws, at least explicitly, as its central project: though laws and such generalizations may be invoked for some explanation, at least some of, and probably the bulk of explanation in works of history considered in this chapter, proceeds through the sympathetic method, and not through the invocation of laws.

But so far I have done no more than make an assertion. On what basis can one say that the kind of history we are concerning ourselves with is different from science, to counter Hempel's claim that it is analogous? In some respects, history is very much like the sciences: the most relevant similarity is probably that they have one identical goal, of gaining knowledge or learning the truth[245] about events in the world. But theology, hermeticism, and witchcraft also claim such a goal, and Hempel would not claim that they, too, are analogous, so some further detail in the features of the similarity is called for. Surely every knowledge-seeking activity has some similarities: I take it that Hempel has committed himself to calling history analogous to science because they have a likeness in *method* and *standards of argument and certainty* : more generally, there are some important equivalent *ideals* that they share, such as the focus on covering-law explanation, that are not shared by other activities such as religion.

As I will point out below, Hempel does not wish to hold that all historians use laws as scientists do, but he does wish to hold that the ideal of law explanation represented in the covering-law model, though perhaps not often practically achieved, does drive historical research and criticism, as well as that of science. I don't think that Hempel even need make the case that historians and scientists would themselves agree that this is their goal, for the requirement that they need be able to articulate their goals may be too stringent. For example: though the theory is at first counter-intuitive, after a little exposure to Popper, scientists often come to believe that they are in quest of falsifiable theories and falsification; and a little more exposure to philosophy of science, which few of them ever receive, could place some doubt in their minds again about this theory. The same could be true about philosophy of history, or sociology of philosophy, etc.: The opinions of the practitioners of the subject discipline (history in the first case, philosophy in the second) may be mutable or even egotistically slanted; and what is important for Hempel as well as Popper is that the relation between the theory of the discipline and the discipline as practiced be argued for in a convincing manner to members of the meta-discipline, not that it gain the practitioners' assent.

Hempel, then, is charged with the duty of showing that the methods and standards of covering-law explanation are practical ideals for all respectable historical explanation, and a reasonable indicator of respectability I will take to be respect from members of the field.[246] I have already suggested that the sympathetic method differs from one used to elaborate and utilize covering laws; but they are not importantly different methods according to Hempel. The sympathetic method ("empathic method" for Hempel) has the following status:

> the method of empathy is, no doubt, frequently applied by laymen and by experts in history. But it does not in itself constitute an explanation; it rather is essentially a heuristic device; its function is to suggest psychological hypotheses which might serve as explanatory principles in the case under consideration.[247]

Hempel claims that by the use of the empathic method, the historian is essentially putting himself in the shoes of his subject: he "tries to realize how he himself would act under the given conditions". But in the final analysis, this is not a method of explanation, but rather a means of evoking psychology, "which somehow is supposed to be familiar to everybody through his everyday experience"[248] .

In this analysis, Hempel has not done the historians justice,



for his use of the term "empathy" and its explication suggests to his reader too close an association of history with vicarious adventuring; it suggests that a historian believes that the way to understand Napoleon is to become him. Such an approach to the subject of history *would* be only a heuristic device; and the approach is, no doubt, used as a heuristic device at times to achieve some personal understanding[249] . But *no* historian would present such an experience as itself *constituting* an explanation, or as grounding a historical thesis: *writing requires an argument* in the discipline of history; and as Hempel has presented it, the empathy theory suggests that debate among historians be equated, intellectually, with "The secret life of Walter Mitty". He does not fairly characterize sympathy theories of historical explanation: vicarious living is a mischaracterization of a respectable method, though perhaps it is an *aspect* of method which historians as well as philosophers are apt to highlight on occasion, when in a rather romantic frame of mind, in discussing one aspect of the pleasures of the intellectual life of a historian. In discussion of the merits of a work of history, debaters do not, to my memory, brandish as their central critical weapons claims such as "this historical work is flawed (brilliant) because it portrays a character who didn't (did) act as I would have under the circumstances"; and I do not think that historians present thinly disguised versions of this argument either. But they also do not tend to enter into extended debates over law-construction and application. Hempel has side-tracked the discussion, for the important point to address now is: In a good deal of respectable history writing, is there a standardly practiced *method of explanation* (which may also be evident in critical discussion of works) which is separable from covering-law explanation? I will present one that I think fills the bill, and one that is only a slight extension of Collingwood's conception of history, moving Collingwood's analysis of historical *understanding* over to the domains of writing, argument, and explanation.

***The sympathetic model of historical explanation:*** Collingwood's theory of re-enactment was presented above in a quote from his work: the historian, he tells us, attempts to understand Plato's thoughts. "The only way in which he can do this is by thinking it for himself. This, in fact, is what we mean when we speak of 'understanding' the words." Re-enactment is re-thinking, but Collingwood is clear in separating this from vicarious experience, since he also claims that the historian maintains a critical distance from Plato, and judges the quality of Plato's thought in the process of re-thinking it. So much, so far, appears to indicate methods by which the historian comes to personally understand the subject individual; it is only a little more enlightening than Hempel's model, and what we are after here is instead an analysis of methodology for explanation[250]. Since Collingwood in his presentation focuses largely on understanding, I will attempt to extrapolate his view of explanation from that discussion.

To understand a historical actor, Collingwood suggests, one must understand the words of that person as one would be expected to in following statements in a conversation. It seems likely, then, that a historian who comes to understand in this way would also attempt to pass on such an understanding to a reader when explaining the actions of an actor; and at issue here is how that is to be accomplished. Collingwood holds that one model that informs historical understanding is re-thinking; his last sentence above, I think, reveals another model that informs his explication of historical understanding and explanation as well: the model of the comprehension of words and sentences in ordinary conversation. I point out this model as well as the re-thinking model because it is much easier to see how one would go about explaining how to understand the words of an actor than to go about explicitly attempting to create an environment in which the reader re-thinks the subject's thoughts: one explains the meanings of words that have changed in meaning, one explains what kinds of people the individual was conversing with, one pulls out for close attention the most important statements from long lectures and correspondence, etc.

The historian, I suggest, does not present to a reader 'the re-thinking': the historian teaches the reader about the rhetorical situation, edits and calls the reader's attention to what appear to be the most important points at issue, *and expects the reader to perform the re-thinking for himself or herself.* This, I think, is a reasonable appraisal of the nature of much historical argument as Collingwood would have it: it gives a place and characterization for *argument* in history that does not obviously invoke covering laws which Hempel seems to neglect, and it clearly shows the connection of private re-thinking and understanding to public explanation and debate. This I will call the sympathy theory of explanation, which complements the sympathy theory of understanding. The goal of sympathetic explanation is to evoke a 'sympathetic response' within the reader to the explainer's condition of knowledge.[251]

***Does sympathetic explanation tacitly invoke covering laws?:*** I suggest that this activity is not the tacit invocation of covering laws, for a variety of reasons. First, an argument from the ideals and goals of the activity. Herbert Butterfield is famous for a phrase which fits this explication of historical explanation very well: "the art of the historian is precisely the art of abridgement"[252]. The ideal suggested in this quote is that the historian invoke nothing whatsoever; merely edit judiciously and explain nothing, because *the reader* is to make the connections. The reader is steered in the direction of the argument by the historian, but what reason is there to believe that this is a process of tacitly *invoking* empirical laws? It seems rather to be explanation with the goal of *evoking* an understanding like that of the historian presenting the argument: explanation as evoking the *same* re-thinking in the reader.

Hempel holds that this process of historical argument does not, in fact, work according to the ideal I have stated. He maintains that the method used by historians, though they may not be aware of it, is the tacit invocation of psychological laws "familiar through everyday experience". Perhaps historians *do* use laws and not know it—how can we tell? If the laws do not *surface* in discussion it would appear to be just begging the question of how one explains to claim that some such laws are there nonetheless. And since Hempel does allow that some sort of 'empathic perception' is available, since it is used as a heuristic device by historians, why is Hempel certain that the goal of historical explanation is not *just that invocation of empathic perception, rather than genuine scientific explanation?* The analysis of historical explanation developed here is intended to argue for just such a goal, reflected in a coherent project that is different in purpose from scientific explanation.

***More support for a difference in method:*** Collingwood further supports this conception of historical explanation, and the claim for the difference in the character of historical and scientific projects, when he writes of the specific differences between the characters of the method of history and that of natural science. He makes it quite clear that, though the method of natural science is available to the historian, it is different because an extra *source* of knowledge is available to the historian: the 'inside' view provided by re-thinking.

> In the case of nature, this distinction between the outside and the inside of an event does not arise. The events of nature are mere events, not the acts of agents whose thought the scientist endeavors to trace. It is true that the scientist, like the historian, has to go beyond the mere discovery of events; but the direction in which he moves is very different. Instead of conceiving the event as an action and attempting to rediscover the thought of its agent, penetrating from the outside of the event to its inside, the scientist goes beyond the event, observes its relation to others, and thus brings it under a general formula or law of nature. To the scientist, nature is always and merely a 'phenomenon'. . . . [253]

In writing about "the outside and the inside", and "the direction in which [the scientist] moves", I gather that Collingwood is talking about method, and he lays down two quite distinct methods,



one for history, one for natural science. This distinction in method is complemented by a distinction in the goals of historical and scientific activities as well:

> For history, the object to be discovered is not the mere event, but the thought expressed in it. To discover that thought is already to understand it. After the historian has ascertained the facts, there is no further process of inquiring into their causes. When he knows what happened, he already knows why it happened.
>
> They are not processes of mere events but processes of actions, which have an inner side, consisting of processes of thought; and what the historian is looking for is these processes of thought. All history is the history of thought.[254]

Collingwood's bold pronouncement at the end of this passage is, to my taste, excessive (though understandable from the mouth of an idealist): I have indicated that a good number of people called historians are prosopographers with clearly defined goals, and since they do concern themselves with explaining historical activity in some sense (though their activity is not so clearly distinguished from anthropology as that which Collingwood describes) there is no apparent reason to deny them the title "historian". But I hope I have also shown that their activity, which might be related to covering-law history, is clearly separable from history done according to the method of re-enactment: whether or not we choose to reserve the title for those using Collingwood's method, the projects are clearly distinguishable.

From these quotes, Collingwood's enterprise can again be seen to be separable from the natural sciences. He clearly sees a distinction in both method and aim: the method of history is re-thinking, the method of science is 'external', a hunt for correlations; the aim of history is understanding thoughts, the aim of science consists of discerning causes. We have seen that the historian's role is evocative[255] , the method an attempt to artfully abridge the masses of historical material so that the reader is directed towards a conclusion and an understanding without invoking any authority such as a law: it is reasonable, then, to take Collingwood seriously in his claim that "After the historian has ascertained the facts, there is no further process of inquiring into their causes."

***A last word from Hempel:*** The strongest rejoinder which I see in Hempel to this line of argument is the following:

> [Empathy theory] may sometimes prove heuristically helpful; but it does not guarantee the soundness of the historical explanation to which it leads. The latter rather depends upon the factual correctness of the generalizations which the method of understanding may have suggested.[256]

This claim in its context, I believe, can be taken several ways. If it is meant as a bare assertion that explanation *just is* the appropriate manipulation of laws, and any other activity does not qualify as explanation, then the appropriate response is that Hempel is begging the question: if he simply *defines* explanation, and if he does so in this way, then a good deal of history does not provide explanations; but that does not make it any the less a respectable field of the humanities, nor obviously the less a useful knowledge-seeking endeavor. If the claim is a normative one, that historians should live up to this goal of D-N explanation, then I think that I have suggested a response of merit: Collingwood and other sympathy theorists have provided what appears to be a coherent method for reaching a clearly-enough defined goal. What Hempel's normative claim amounts to, then, is an unjustified assertion that historians *should* change *their goal as well as their method.* A normative pronouncement, like an estimation of rationality, is an assertion regarding activity for the purpose of promoting a particular goal; and so I think that Hempel has the goal wrong: he makes his claim because he wants history to present scientific explanation, but history as it is done by many historians is in the business of promoting re-thinking.



Finally, the most sensible interpretation: the claim is an empirical one, that, in the final analysis, historians appeal to laws in their critical discussions of history writing; the claim is that the 'acid test' of any historical explanation in its composition, or in criticism, is support by empirical laws. Hempel has staked his claim nicely, but in his article, he does no digging: if historical explanation receives criticism in terms of empirical laws, then Hempel should show this with a good deal of further argument; in examples, for example. In critical discussions of history written along the lines of sympathy theories, I find little explicit debate centering upon empirical laws of psychology; debate tends to focus on arguments over *facts* and *method* to a greater extent. Hempel might respond that there is little debate over psychological laws because historians greatly agree on their content; I hope that the tack of my argument so far has suggested that such a response is extremely weak: if there is no straightforward, explicit evidence for the use of laws, there is little reason to suppose their use. There is little reason because another apparently plausible method of explanation more evident on the surface of the historian's discussion will do the job.

This use of a different method is further supported by a more careful investigation of the standards of evidence and the robustness of the conclusions that historians draw. I will attempt to show in the next section that historians involved in sympathetic understanding and explanation treat their product, the conclusions that they draw, as significantly less secure and stable, of a rather different nature and crafted for different purposes than they may consider natural science's conclusions to be. This will provide further support for my claim that the goal of the historian is quite different from that of the scientist, and so the thesis that much historical explanation is probably not closely related to the covering-law model.

To close the section, it is important for my case to stress again how neatly the sympathetic conception of explanation hangs together, and how clearly separable it is from the approach of explanation by the invocation of empirical laws. For Collingwood, the historian's method is called re-enactment, and the historian's goal is understanding thoughts of historical actors. Because the goal is the understanding of thoughts, the field is easily demarcated from the natural sciences, including the science of psychology, in its content and goal[257]. Collingwood does not so explicitly discuss how historians construct arguments, but since the historian's primary goal is clearly re-thinking, methods and standards of argument commensurate with that goal, rather than the goal of covering-law explanation, can reasonably be assumed to hold. Butterfield's claim that the historian's art—i.e., the method of historical explanation—is judicious abridgement, is also clearly reflected in Collingwood's words in the quote above. I have called this method one of *evocation* rather than *invocation*: the historian does not argue by invoking empirical causal laws; such argument may be included within the historian's argument, but the historian's goal is to evoke an understanding of the subject's thoughts in the reader that is similar to the historian's own understanding, and though law-explanation may help in this regard, there is little reason to believe that such explanation is the explanatory paradigm that these historians look to. The models of understanding a conversation and pointing (showing), and the methods of evoking and editing are more obviously paradigmatic for Collingwood's re-enactment approach, and sympathy theories in general.[258]

### III. Historical explanation: Forms of history and the explanation of science

***The pragmatics of historical explanation vs. the pragmatics of scientific explanation:*** I have argued that the sympathetic approach to historical explanation differs both in method and goal from those of the covering-law method; and I agree with Collingwood that these also differ from those of natural scientific explanation. We can lay aside the covering-law model, then, and consider further aspects of the relation between science and history. Though an adequate model of scientific explanation might aid in distinguishing between science and history, that would be a difficult task to take on as well, and since my primary purpose here is to elucidate historical explanation, I will concentrate on history and leave the relations and divergences partly implicit. I have suggested above that a kind of explanation much more closely akin to the covering-law model, and also to natural scientific explanation, and, finally, to the expectations of philosophers of science, is available for historians in prosopography; our concern here is, why shouldn't that, then, be taken as the ideal for all of history writing?

The example of Philip II from the beginning of the previous section indicates that an answer lies in the pragmatics of history. I have been attempting to argue that there is a distinct and well-formed project, sympathetic history; one distinct from prosopography. In my discussion of Hempel, I suggested that he probably mis-characterized the goals of sympathetic explanation: Hempel's assessment of the methods of historical explanation differed from that of Collingwood because they had different goals in mind; and it is unsurprising that Collingwood's methods would be inadequate for attaining Hempel's goals because they were not tailored to those goals. Unless we wish to dismiss out of hand a clearly articulatable and very broad aspect of historical study as illegitimate, then, prosopography will not do as an ideal; and so we must come to grips with this other form of history, and either undermine its plausiblity, or assimilate it into our consideration.

Such is an argument from the pragmatics of history, one concerned with the consilience between goals and methods. I take pragmatics to involve all of: the greater purposes behind the enterprise, the goals within the enterprise, the methods, the ideals which these goals and methods reflect, and the standards of evidence and certainty within the enterprise. I have already discussed some specifics of the goals and methods of the sympathetic approach to understanding and explanation in the previous section. I will now proceed to consider the ideals of explanation, standards of certainty, and purposes for engaging in the sympathetic approach. It will also be good to examine the relation between the sympathetic and prosopographic methods, to get a more complete view of the collective compass of these different aspects of the field of history. Some recent discussion put forward by historians suggests a recognition of the split, and also presents attempts to weld together the two approaches in a historical enterprise superior to either alone. Given that philosophy of science has traditionally focussed on only one of these two sorts of history, historians' analyses of the link between the two might help to make the second more accessible to philosophy.

I have already argued that the (internal[259]) goal of Collingwood's re-enactment theory is re-thinking the thoughts of historical actors, and that this clearly differs from goals of scientific explanation such as the construction of laws, the use of laws, and prediction and control of phenomena. This difference, I think, reflects a difference in purpose, intellectual ideal, and standards of certainty: for sympathetic history, laws are not the ideal for grounding explanation, the certainty which goes with laws is not what is striven for, and prediction and control are not *wanted* or sought (not that they are necessarily undesirable, they are simply not actively pursued). These are rather strong claims: it would be good to have an account of the ideals, certainty, and purposes of explanation to hold up against those maintained by science and prosopography, to make the claim credible.

***Ideals of explanation for history, from hermeneutics:*** We begin with ideals for explanation: i.e., characterizations of what can be taken to be good or paradigmatic forms of explanation, as evidenced by discussions about explanation by historians and by those they refer to and regard as authorities; in this case, mostly philosophers and anthropologists. If law-construction and law-explanation are not the ideals that are pursued, which are? We have already touched on the issue in Collingwood; his presentation of the re-enactment theory's goal of re-thinking of past actors' thoughts and the related method by which this is achieved suggests an ideal for explanation that appears to reflect the *hermeneutic* theory of



explanation; a theory developed with the intention of making explicit the contrasts between the natural sciences and social and historical sciences.

The theory, though alive today through the Frankfurt school and succeeding generations, still has Dilthey and Heidegger as its most articulate and challenging exponents. We see Collingwood's explanation of the differences between the "external" method of the natural sciences and the "internal" method that is also available to the historian reflected in Dilthey:

> . . . for the natural sciences an ordering of nature is achieved only through a succession of hypotheses. For the human sciences, on the contrary, it follows that the connectedness of psychic life is given as an original and general foundation. Nature we explain, the life of the soul we understand.[260]

Dilthey's use of the term "foundation" is important in that it marks the root difference between methods of natural and social sciences: an intuition regarding "psychic life" is available to the social scientists *in addition to* those methods available to the natural sciences. This method is clearly not available in other sciences, where the existence of a psychic life in the subject matter is found to be lacking by, or is not evident to, the practitioners.

Dilthey's "connectedness of psychic life" is, I think, the unargued basis of justification underlying Collingwood's belief that a historian is able to re-think another's thought. Heidegger presents an analysis of hermeneutic understanding and knowledge which might even avoid the necessity of assuming such a principle in *Being and Time*, his long argument for a truly anti-foundationalist analysis of knowledge. Going into the full depth of Heidegger's analysis would be too long a foray for this discussion, which is intended merely to argue that a discernible ideal, separate from law-based explanation, is available to and alluded to by historians. It will be useful, however, to note the very strong commitment which Heidegger shows in his work to proving that the separation of hermeneutics from natural science is principled and defensible.

***Heidegger and standards of explanation:*** Heidegger argues that hermeneutic explanation (or "understanding"—*verstehen*—as Dilthey would have it) is not scientific explanation, and he also considers the ramifications that arise because hermeneutics does not live up to the supposed standards of natural science. The difference in standards is based particularly in the assumption of an understanding of a person's place in the world in the interpretation of meaning. According to Heidegger's view of interpretation, the interpreter of another's words or meaning is required to assume beforehand some 'situation' for the individual who is behind interpreted, relating to what the person knows about the world, or what the person's concerns are: more bluntly, an interpreter must assume *some* knowledge of the meaning of what the other is attempting to say before even listening to what is said[261] . The apparent problem for hermeneutic interpretation—and the main difference between science and hermeneutics—is this assumption of some content of meaning prior to the interpretation of meaning, for "In a scientific proof, we may not presuppose what it is our task to provide grounds for". Science would seem to suggest that hermeneutics is circular:

> *But if we see this circle as a vicious one and look out for ways of avoiding it, even if we just 'sense' it as an inevitable imperfection, then the act of understanding has been misunderstood from the ground up.* The assimilation of understanding and interpretation to a definite ideal of knowledge [e.g., natural science] is not the issue here. Such an ideal is itself only a subspecies of understanding. . . . Mathematics is not more rigorous than historiology, but only narrower, because the existential foundations relevant for it lie within a narrower range.[262]

Heidegger's claim here is that a scientific or mathematical ideal of argument or knowledge may be an ideal within the compass of that field, but should not be confusedly taken as an ideal for all



knowledge. Even in the unlikely event that, on the basis of some criterion, we considered mathematics to constitute 'the best' of our knowledge, a demand that other inquiry should strive for the ideals that have been developed for mathematics might be unjustified, since the mathematical ideal need only be ideal for a particular limited task, and may not represent an adequate ideal for all knowledge.

A weaker line of argument than Heidegger's, and one more clearly directed to the purposes of our argument might be the following: the fact that historical argument may not fit the canons of science does not imply that it is a poor way of gaining knowledge, since the ideals of science are themselves only fallible, empirically based and lately-gained ideals. That historical knowledge does not conform to those ideals does not mean that it is faulty, for it only shows that it does not conform to those ideals: to show that it is not a worthwhile pursuit, one must show that it doesn't attain a worthy goal or the goal that the ideals are meant to ensure, *viz.*, producing knowledge. That one route to knowledge *does* work does not mean that others *won't* work: That hermeneutics does not promote the ideals of natural scientific argument does not serve well enough to impugn its credibility; it is independently grounded, and without further argument, the virtues of the ideals of science are not strong enough to show that hermeneutics ought not to be pursued.

***Another ideal from art-history and anthropology:*** Alongside the hermeneutic ideal for understanding and explanation, another which fits nicely with it and would seem to sit poorly with natural science has been more recently articulated by Clifford Geertz in anthropology, and Carlo Ginzburg in history. The ideal is the evidential paradigm of the sign, or symptom, and its corresponding standard of explanation. Where Hempel finds an ideal for historical explanation in science, Ginzburg traces this paradigm, present in all of the humanities and medicine since the 19th century, to a late 17th century development of art-history. The paradigm is rooted particularly in an important paper by Morelli, which introduced a very successful method of discerning between the paintings of great artists, and copies, fakes, and those of their pupils. Morelli's method required a comparison of the appearances of the smallest and least significant features of paintings, such as the details of portrayed ears and hands, for identification: though another would attempt to copy the grosser subject matter, the finest features and general brushwork of a great artist's technique, the very smallest of details—which from an aesthetic point of view really didn't matter—would invariably be original and regular to each artist. The least significant from an artistic point of view became the most significant for identification, and 'symptoms' of the artist, rather than aesthetics of the art, became a key for art identification, and for the explanation of such judgments[263] .

The method, then, is to use signs, and the goal of the enterprise is primarily identification, rather than inquiry into causes. Ginzburg points out that both Sigmund Freud and Arthur Conan-Doyle (in the character of Sherlock Holmes), specifically invoke the symptomatic paradigm, and Morelli's contribution. This method and goal are also apparent in medicine. Because the ultimate goal of medicine, as opposed to pathology, is curing the sick, the primary categories of discussion might be taken to be 'symptom' and 'cure', rather than 'cause' and 'remedy': the physician inquires into causes to the extent that it will aid him in discovering a cure, and failing scientific knowledge, symptoms may often be used equally as well to steer a physician to the desired treatment. Though from a causal or scientific point of view, knowledge of symptoms and cures is inferior to knowledge of causes and remedies, it requires further argument to establish that in an important sense, this paradigm is inferior *to the purposes of the practice of medicine*, and this point, concerning the inappropriateness of scientific standards of inquiry to some ends, is precisely Heidegger's point above. Though I believe that such a case can, in fact, be argued for medicine, since medicine's adoption of scientific procedures appears to have done it good service, only a dubitable induction could extend the argument to art-history, or anthropology, as we will see below.



Holmes and Freud also had pretensions to scientific theory and analysis of cause, but Ginzburg sees Morelli's concern with symptoms rather than causes as presenting an important and distinct evidential paradigm, which was evident in psychoanalysis, and neither a half-measure, nor a groping towards causal analysis:

> A discipline such as psychoanalysis came into being, as we have seen, around the hypothesis that apparently negligible details could reveal profound phenomena of great importance. The decline of systematic thought has been followed by the success of aphoristic reasoning—from Nietzsche to Adorno. . . . Aphoristic literature is, by definition, an attempt to formulate evaluations of man and society on the basis of symptoms and clues. . . . [264]

Clifford Geertz provides a fuller analysis of an approach to ethnography that eschews causes for the analysis of symptoms and meanings; what he calls a "semiotic concept of culture"[265] . I note his contributions here especially because his theoretic statements about ethnography have achieved a wide familiarity, and are avowed by many historians to represent quite faithfully what they themselves are up to (most notably Ginzburg, Roger Chartier, and Robert Darnton). Geertz does not consider the ontological characterization of culture as a system of forces *causing* or constraining the behavior of its participants to be an appropriate one to the study of anthropology. Instead, because the variety of possible significances attaching to any given piece of behavior is so diverse, an analysis of *symbolic* relations attached to behaviors appears to him the more appropriate. The project of analysis of significances—Geertz's recommended method towards the goal of ethnography—he calls "thick description": an attempt to relate with the greatest possible fidelity the meaning-relations of actions and words of the subject culture in terms understandable to the analyst's culture.

Geertz explicitly compares his project with that of medicine, and explains his purposes and method in terms that should be very familiar by this point:

> . . . the essential task of theory building here is not to codify abstract regularities but to make thick description possible, not to generalize cases, but to generalize within them . . . rather than beginning with a set of observations and attempting to subsume them under a governing law, such inference begins with a set of (presumptive) signifiers and attempts to place them within an intelligible frame. . . . In the study of culture, the signifiers are not symptoms or clusters of symptoms, but symbolic acts or clusters of symbolic acts, and the aim is not therapy but the analysis of social discourse.[266]

Geertz apparently maintains that ethnography is an empirical, knowledge-seeking enterprise, but he also sees it as, at its most basic level, *a semiological field of study* ; a study of signs and significances. For reasons similar to those that Heidegger holds to differentiate sciences from hermeneutics[267] , Geertz does not believe that culture is characterizable otherwise: because of the profusion of possible significances to any behavioral manifestation, significance outstrips behavior to a great degree; and because of the nature of understanding, in ethnography "what we call our data are really our own constructions of other people's constructions of what they and their compatriots are up to."[268] For Geertz, culture is "a context within which [people's actions] can be intelligibly—that is, thickly—described".[269]

I have pointed out several intellectual ideals that I see as related to the sympathetic approach to history, and that do not obviously have a close association with ideals in physical science: the assumption of "the connectedness of psychic life", the hermeneutic response to the scientific ideal of knowledge, and the symptomatic and semiotic paradigms of evidence and explanation. I have tried to suggest that these ideals do go against some which, though perhaps not universal, are prevalent within science, e.g., empiricism as opposed to the assumption of psychic unity, and the importance of causal or law-grounded explanation as opposed to semiotics.



***History of the Annales school:*** To further deepen our discussion of what the historians which I see as using sympathy theories are trying to accomplish today, I must now dig a little into the history of the field of history. By 1950, or what Lynn Hunt calls the tenure of the 'second generation' of the *Annales* historiographical school (and so named because its central organ was the journal *Annales d'histoire economique et sociale*), Fernand Braudel's focus on long-term geographic and demographic changes in history became the paradigmatic approach to the study of history. As I noted earlier in the chapter, Braudel's approach fits a tradition that I have characterized as prosopographic: the analysis often concerned the collection of volumes of rather mundane data, careful tracking and tabulation of variables, and the presentation of hypotheses regarding the causes of these changes in conditions. History was studied in this way because of an underlying commitment that the geographic and demographic "structure" of the world was a fundamental basis of change in history, and one quite available to historians: political and social movements were likely to collect too much emotional and propagandistic baggage in their reporting to be presentable as secure bases of knowledge of history[270] . Braudel has even been read as suggesting that the geographic and demographic are the prime movers of history; the short time scale of politics is swamped by the *longue durée* of the structure.[271]

At present, the fourth generation of *Annalistes* , and others who can be seen to be associated with the school and journal, have radically changed the focus of investigation to microhistory and cultural history, and have changed their methods as well. My impression, from Hunt's article and other sources, is that the current leading figure—to replace Braudel—might be Carlo Ginzburg, with his little book *The Cheese and The Worms* replacing *The Medi-*

*terranean* as the paradigm work of the school; other leaders are Robert Darnton, Natalie Davis, Roger Chartier, and Edmund Burke.

*The Cheese and The Worms* might be taken as paradigmatic because it illustrates the goals of both micro- and cultural history so well. Briefly, the work is the story of the inquisitorial trial of Menocchio, an insignificant 15th Century European miller who proposed a cosmogony equating the earth with a ball of whey, and a variety of other views which were deemed heretical in an inquisitorial trial, and cost him his life. The work might be considered the epitome of its form because it delves very deeply into the trial records and the few other resources available, and uses them to develop an elaborate presentation of the trial and the way of life of a common villager, as well as the intellectual and social possibilities for living available to an extraordinary miller under the contemporary economic and political conditions. The discussion has a strong flavor of Geertz's thick description, and still maintains a close tack to many traditional concerns of historians, by proceeding through an ingenious indirect approach to high culture *by way of* popular culture.[272] Ginzburg provides many details in his attempt to 'thicken' the presentation. He claims to dissect the sources of Menocchio's ideas, by arguing that Menocchio appeared to have had access to a few rather widely circulated books, and perhaps not many more books, from which a little of his own ingenuity and a little experience of station (as a miller) led him to his views—and by this route, Ginzburg presents a discussion on the history of publication in Europe at that time. Ginzburg is yet more insightful in his discussion of the trial, which considers the purposes and social impact of the inquisition as it affected the common population of Europe, and through them, European history.

The foci of cultural and micro-history represent approaches in many ways diametrically opposed to those of the first generation of *Annalistes*. In microhistory and cultural history, the study is not demographics and the *longue durée,* but rather the history and development of the smallest, and in a straightforward sense often



the *least significant* of cultural institutions. Parades and carnivals receive particular attention, not because they are productive of historical change, but because they are particularly reflective of cultural features and social tensions (or 'refractive', as these events often magnify some features of society in caricature). The search in history is less for the causes of demographic shifts and historical development, and more for deeper and more illuminating characterizations of any specific single cultural *status quo*. In the second *Annales* generation, agricultural and geographical sciences were intimately woven with historical research; in the fourth, Geertz and Heidegger are acknowledged as important intellectual forefathers, and the semiotic ideal that Ginzburg acknowledges is primary. The material used by the fourth is, obviously, very 'micro'-oriented, but the fruits of the previous prosopographic tradition are often used to frame and provide evidence for the micro-discussion: the first and fourth generation are linked though they have very different concerns, and conscious attempts to further bind them together will be noted in the next section.

***Purposes and standards of certainty in historical approaches:*** For the second generation—and for other prosopographers—because of their 'scientific' approach and goals, what is a problem, what is evidence, and appropriate standards of are in some respects easily understandable: for they are partially dictated by the relevant sciences. Finding data appears to be a major task, and statistical tools to aid in the analysis of results are used as they are in other statistical approaches to the social sciences. The method is clearly present in *The Mediterranean,* but is front and center in another prosopographic work, *Little Science, Big Science,* in which Derek Price attempts to explore the demography and economy of scientific activity as it has operated in recent years, and this goal is pursued primarily by counting and sorting entries from *Chemical Abstracts.* These works are by no means simple in their construction—I am not attempting to suggest that any geographer, given the right libraries, could have come up with *The Mediterranean,* which might be considered one of the greatest work of 20th century history. I am suggesting that, to the extent that we (philosophers, scientists, and historians) have come to understand methods and explanation in the sciences of demography and geography, and to the extent that we lack understanding of the creative process, we may have a parallel understanding of this kind of history, primarily because these sciences play such a large role in its construction.

But to the fourth generation, standards of certainty differ, reflecting the different intellectual ideal of interpreting symbolic significances. Because characterization of *meanings* is at issue, what certainty and significance result from sympathetic understanding is often at the center of debate within the field, and quite problematic. The root source of the concern, which Geertz recognizes, is the involvement of the analyst's culture and meanings in the interpretation of those of the other: as he writes, "what we call our data are really our own constructions of other people's constructions of what they and their compatriots are up to." For historical and ethnographic knowledge, it may be important that the analyst must go about the job of *making sense* of the other culture or historical situation; and to the extent that making sense is *itself* a culturally conditioned activity, with culturally defined standards, the analyst's knowledge, experience, and culture are themselves caught-up essentially in interpretation of the other culture. The historian/ ethnographer cannot exit his or her own culture in order to explain the subject culture; and the history of science suggests that, at least to some extent (though perhaps not in the respects necessary to seal this argument), making sense, understanding, and explaining are all categories that change in some ways through history.

So how does the dialectic between analyst's and subject's culture affect the knowledge acquired through sympathetic understanding? The extent to which the analyst's culture does and should invade explanation is very much at issue, in both anthropology and history. One critic of Geertz, Vincent Crapanzano, argues that such anthropological explanation is inherently problematic, for



Geertz claims on the one hand that his interpretation is provisional, yet he settles on a single 'thick description' regardless. Geertz is further charged with tainting his 'description' with rhetorical tactics that hinder description, and instead pander to the traditional purposes of article-writing in the field of anthropology, namely, gaining the confidence and conviction of a western audience. Crapanzano argues that Geertz's best known article, "Deep Play", is more rhetorically centered around the goal of convincing his fellow anthropologists that he is a credible witness than around the purpose of thick description—Geertz's self-identification as an anthropologist in the article inhibits the purpose of thick description[273] .

In the field of history, Roger Chartier has criticized Robert Darnton (who explicitly takes Geertz's thick description as his model for explanation) as providing explanations too 'thick' to be true. In his article "The Great Cat Massacre", Darnton attempts to examine the personal motives and cultural tradition that produced a mock-trial and execution of a few alley-cats in an 18th century French town. Darnton argues that the legal 'joke' was intended by its perpetrators as a symbolic rape, a cuckolding, a carnival festival, and a political revolution, all situated within the rhetorical context of the mockery of an initiation rite of a secret printers' society. Chartier feels that description has passed beyond thick, and become smothering: Darnton has produced too complex a world for the subjects, "an entire set of beliefs, rites and behavior difficult to imagine as inhabiting the mind of urban printshop workers of the eighteenth century."[274]

I have included these brief examples of critical debate within cultural history and within a related branch of ethnography to show that there are concerns about the *practice and status* of such explanation in the disciplines which would not arise in natural sciences, or even in prosopographic history and most of the social sciences. It is not surprising that these debates about explanation and certainty are foreign to most other areas of science, since in most sciences semiotic (or sympathetic) explanations *are not sought*.

Their presence within the disciplines which use semiotic and sympathetic explanation, however, suggests the presence of a genuine, developed theory of explanation which differs essentially from other scientific (e.g., causal) explanation.

***Unifying historical explanation:*** I have argued for a separation between two kinds of historical explanation, prosopographic and sympathetic, a separation that I think can also be roughly equated with the split between natural scientific and semiotic explanation. These are two kinds of explanation that concern causes and ideas respectively, and I would not be surprised if another's analysis would turn up more. There is, however, only one past to be explained, so how might these two explanations be related? To further indicate the presence of the explanatory divide, and in aid of its resolution, I will close by indicating that historians have begun to address this problem.

Recall that the second and fourth generations of the *Annales* school appeared to have little in common. The first generation searched for causes of large-scale change in *longue durée* history, and the *structure* was used to explain the *development* of history; the fourth generation have searched for an intimate interpretive understanding of cultures, but it appears that they have also shown less concern for discerning how those cultural conditions *came to be.* Aleeta Biersack recognizes the shortcomings of Geertz's program, a paradigm for the fourth generation, when she writes that thick description does not allow for the exposure of the mechanisms of formation and change of a culture. Geertz presents his analysis of individuals and their thoughts, but this understanding she finds to be too "local": it gives no analysis of the global historical process.[275] This is not *too* surprising a fault for an anthropologist, who might usually study rather stable cultures; and it is perhaps less of a fault for an anthropologist than a historian. Such local and personal understanding as is provided by the anthropologist's paradigm, however, need not lead only to knowledge of individuals *per se*, or purely static cultures; for from information about what moves one person to act, an understanding of



historical process would seem to be available, if one wishes to develop the local knowledge in that way. That Luther said, "I must stand here, I can stand nowhere else," tells us something about the effective historical significance of religious faith as well as something about Luther.

No-one, I think, has done a better job of using the tools of microhistory and cultural history to draw conclusions regarding historical *change* than Roger Chartier[276] ; and none have presented superior theoretical treatment of the integration of the static (in the 'micro' of microhistory) and the dynamic (*longue durée*) than Carlo Ginzburg[277] . Ginzburg and Carlo Poni reflect, and begin to answer, Biersack's concern with the semiotic approach in "La micro-histoire"; they suggest the virtues of combining the 'micro' of micro history with elements of two traditions of prosopographic history:

> Voici quelques années, traçant un bilan des recherches prosopographiques, Lawrence Stone distinguait deux courants: l'un, qualitatitif, attentif aux élites (politiques, culturelles, etc.); l'autre quantitatif, préoccupé d'analyser des agrégats sociaux plus massifs. Nous proposons de combiner la perspective non élitaire du second courant avec le souci d'individualisation qui est au coeur du premier. Une prosopographie de la masse, donc, analogue à celle que proposait E. P. Thompson, et qui devrait déboucher sur une série d'études de cas, sans pourtant exclure, on l'a dit, les recherches sérielles.[278]

The non-elitism of mass prosopography is here combined with the focus on the individual associated with history written around the apparent influences of great and powerful men: here we see the focus of the new cultural history rests on non-élite individuals, but *as individuals*. From the study of many of these individuals, some of the virtues of prosopographic, serial history can be gleaned, and the causes of historical change may be learned. Ginzburg and

Poni suggest that an indiscriminate study of *every* individual in history would be impractical, and so choice must be made; but that choice may seem to suggest the installation of a new élite once again. What is needed, they suggest, are case-studies of the "exceptional normal": representative cases of frequent occurrences, or less frequent but probably historically important occurrences: Menocchio's cases, that of a free-thinking peasant, and that of an inquisitorial victim, appear to fall respectively into both of these analyses of sub-categories of the exceptional normal. From collections of such micro-histories, a history containing an element of duration might be constructed.

So we come back to Braudel's concern, that history without an account of *structure* is inadequate; and also to the more general concern, that historical methodologies based in laws and semiology must in some way be brought into accord, since these historians all do share the goal of explaining the past. Braudel and the microhistorians do, indeed, share that general concern; but they rest at some distance from each other in their detailed concerns: Braudel's focus is on the forces of historical change, and the microhistorians' is in the shapes of specific cultures and societies. Ginzburg and Poni attempt to stretch microhistory through time in strings of case-studies; though they do not explicitly mention the role of the *structure* and *conjoncture* in these histories, a place for them, as well as for other sorts of law-explanation, has clearly been provided.

## Conclusion

In this chapter, I have attempted to show that philosophy of science favors a particular kind of history of science: one which is productive of laws, or uses laws in explanation. I have also pointed out the close affinity of such history to science. There is another form of history writing, however, that appear to have little relation to the ideals of law explanation because it has quite different goals. I have tried to present an alternative model of explanation to



complement Hempel's, reflecting a form of explanation produced as a result of different purposes, goals, methods, and standards than those operative in covering-law explanation. The form of explanation can be seen to reside in a flourishing, and critically aware tradition in the social sciences and the humanities.

I have argued that philosophy of science traditionally neglects this history—the writing of many historians. The challenge facing me, then, is to elucidate the way in which philosophers can use it, now that I have characterized the enterprise. Laudan and other rationality theorists have a clear sense of ways in which historical material and covering-law explanation can be exploited for philosophical conclusions, mostly in the philosophers' own history; my task is to make equally clear ways of using more of the history historians produce in philosophy of science.

# CHAPTER 5: A PRODUCTIVE ENGAGEMENT

## Introduction

The primary task that remains for me in this thesis is to provide an account of a relation between philosophy of science and history of science that allows for a productive engagement between the two disciplines. The relationship that I envision will require a rather radical departure from the philosophical approaches to science considered above, because it will embody a significant re-orientation of the philosophical project of explicating science. Specifically, whereas the other philosophical approaches to the study of science that we have considered in this thesis conceive of philosophy of science's goal as the explication of the concept of scientific *rationality*, I will argue for the relevance of the study of a broader conception of scientific *development* to general philosophical concerns in a program of philosophical study that I will entitle *methodological relativism*.

I argue that traditional philosophical accounts of scientific rationality still stand in the way of a productive study of history, especially because the writings of many historians suggest that scientific methodology may be significantly affected by larger, non-intellectual *historical* variables. Lakatos, Laudan, intellectual historians, and the logicists would certainly agree that historical factors—'the social', as opposed to 'the intellectual'—affect the process and the progress of science, *but only in unimportant ways, or to*



*its detriment.* I will attempt to argue that features that are not 'rational' (on a narrow construal of rationality) might nonetheless *contribute positively and crucially* to the shaping of argument in science, with regard to theory choice, and perhaps also with regard to methodology and goal-formation; consequently, these features might be profitable foci for study in philosophy of science in addition to the study of rationality. Others may argue that such a position concerning the facts of history and the importance of history to a study of science makes me a relativist regarding the product, methods, and goals of science. I largely agree with such an assessment—but just what this relativism entails, and how destructive it must be to epistemological concerns, I will consider towards the end of this chapter.

I will argue, then, that an account of the *historicality* of science, which allows for the possibility of changing aims and methods and of significant constructive characteristics derived from the social world, is needed to support an account of rationality in order to adequately explain scientific development. I suggest that a general study of all factors contributing to scientific development, not just an account of a stable scientific rationality and a rational account of growth, is a relevant topic to the philosophy of science.

### Part I. The case for studying scientific development

#### §1 Prolog: a note concerning this project and science studies

***Philosophy of science and histories of science:*** 'History' is, of course, a word of many meanings as it is used in this thesis. In the first chapter, I attempted to argue for the relevance of the history of science to philosophy of science as a consequence of philosophy's generally accepted goal of explicating science; and in that chapter, the focus was particularly on 'history' as, roughly, the fact of the matter about what happened in the past. The facts of the past, however, are not immediately accessible, and so history as a *discipline* governed by *methodologies* entered the discussion in Chapter

2, in the form of attempts at interpreting history authored by Kuhn and Lakatos. Their attempts, however, exhibited both philosophical and historiographical weaknesses; they also failed to address the possible philosophical relevance of the vast product of many historians which does not conform to the model of intellectual history[279] . To discern what those historians were up to, I launched a much more direct analysis of historians' writing and historiography in the third and fourth chapters, and argued for the methodological independence of history from philosophy of science.

Looking back over the previous chapters, then, there may appear to be something of a gap between the purposes of Chapters 1 and 2 on the one hand, and 3 and 4 on the other; and the gap has to do, once again, with a change in the use of the term 'history'. Lakatos, Kuhn (especially 'Kuhn III') and Laudan appear to have been looking for a method for writing and reading history that would serve the purposes of philosophy of science and of the allied approach of intellectual history; and to the extent that logical positivists and empiricists utilize history, their concerns, too, are similar. I have gone on in the third and fourth chapters, however, to consider another history, the studies produced by a group of academics called 'historians', pointing out the independence of their practice from philosophy of science, and the great distance between their practice and the philosophers' conception of good history. The two diverging programs of study represented in the first and second chapters on the one hand, and the third and fourth chapters on the other, then, appear to provide *radically* different answers to the question of how to foster a productive engagement between history of science and philosophy of science: the abovementioned philosophers would *write their own history* or have it written as they wish, and I would foster an engagement between historians and philosophers, and mine the ideas of professional historians.

I have, then, the task of making the link between these two 'histories' stronger in my thesis, and of showing the philosophical



relevance of history, and specifically of history of science, of the sort that I analyzed in the third and fourth chapters.

***Philosophies of science and history of science:*** The abovementioned divide might appear to be more representative of differences in taste, and in assessments of the relative competences of philosophers and historians, than of a disagreement regarding philosophy of science or the character of science. I think there are significant philosophical disagreements between the approaches, however; and ones of great significance to science studies—the attempt to relate or integrate the efforts of different humanistic approaches to the study of science.

I see two fundamental disagreements between the approaches. One concerns the character of science studies and the relative roles of philosopher and historian. On the one hand, it is a respectable, healthy practice that philosophers look at history, learn the history of science, and design their own model of an ideal type of historical writing for their purposes—as do those who have been considered above. Philosophers *can*, I expect, test and prove philosophical conclusions on the basis of the idealized historical research that they conduct and direct, and I do not wish to suggest that their efforts to that end are fruitless or unimpressive. But such an approach, as I have argued, presents an extremely impoverished history of science, and neglects the industry of most historians of science. At *worst*, it may be considered an imperialism: an attempt, mostly by philosophers, to dictate to historians a normative conception of their work—and it is sometimes misunderstood, *and sometimes correctly understood* to be just such an enterprise by many historians of science. At *best*, however, it is not much better: it indicates a tacit admission that nearly all of the work done by historians of science is practically irrelevant to philosophy of science and science studies.[280]

This 'end of science studies' solution—and I see it as just that, the end of interdisciplinary exchange—may be held, as it was for logicism, on the basis of some fairly plausible principles. I have attempted to call into question a few of those principles in this thesis, particularly in my discussion of the historiographies of Lakatos and Laudan in the third chapter. Instead of *directing* historians, or isolating philosophers' history from that produced by historians, I wish to incorporate the work of historians into philosophy of science *in situ*, judging and using the insights they have provided for philosophical purposes. What follows is my attempt.

The second disagreement that I find between the two philosophical approaches follows partly as a result of the first: to engage in science studies in the way in which I propose, the self-conception of philosophy of science must differ from that proposed by the philosophical authors who have been examined above. The change centers around a difference of opinion concerning the central purposes of philosophy of science, and the general potency of rational explanations of science. I wish to argue for the importance to philosophy of science of a study of the historical process of scientific development, as opposed to an exclusive study of scientific rationality, which is clearly representative of the logicist position, and is also promoted by Lakatos and Laudan. This alteration should not be considered an attempt to *adapt* philosophy to the concerns of historians, which would be as inappropriate a move as that considered above, of adapting history to philosophy. I will attempt to argue below that the study of development is conducive to a variety of purposes that may be considered to belong within the purview of philosophy of science, and is required to provide a philosophically adequate account of the process of scientific growth.

My methodology for philosophy of science, then, is as follows, and will be outlined in detail in section 2: rather than logic, rather than foundations, and rather than rationality, I tout the value to philosophy of studying scientific growth or development very broadly construed. The study of growth I take to be the explanation of historical change in science—in essence, this entails a full embrace of the context of discovery for the study of philosophy of science. Such a study strikes me as inadequately addressed by the views which have been surveyed in this thesis, very much in accord



with philosophy of science, and a fine candidate for fulfilling a variety of useful purposes.

***Origins:*** Why do I think a broader study of scientific development than an account of scientific rationality affords is desirable for philosophy of science? The stumuli, I believe, come from philosophy, and from an acquaintance with history of science. The philosophical stimulus derives particularly from Friedrich Nietzsche, and is succinctly expressed in one of his epigrammatic conclusions: "The living are only a very small fraction among the dead . . . and not the most lively fraction"[281] . What Nietzsche was expressing was a view concerning illusion and human freedom. In our actions we feel that we are in control of a great deal; we *feel* that we have freedom, and rational control of our choices. But whether freedom may or may not be available to us, often we are clearly mistaken in our appraisals of what is driving our decisions: we think that we know why we act as we do, but from a historical distance, other causes become more obvious, and glaring inconsistencies in behavior suggest that we are often fooling ourselves in our 'rational' appraisals of our own actions. A large measure of Nietzsche's meaning is much more easily expressed in terms of Freud's categories of the conscious and the unconscious, which followed Nietzsche's work by only a few decades: That of which we are conscious in our decision-making is only one factor governing choice; that in us of which we are not aware has a great deal of influence as well.

All philosophers of science, I expect, are very concerned with understanding, explaining, and also improving upon the degree to which scientists have rational control over their scientific decisions; and the last concern is surely the rationality theorist's ultimate goal, and is expressed in attempts to discover and elucidate the best of scientists' epistemologically justifiable tacit methodology. That is one approach for putting the unconscious under conscious control, and it has its virtues; but I suggest that another, very different one has virtues as well. If we also make efforts to understand other features of personal psychology and scientific development over which practitioners have little conscious control, and of those over which they *can* have no control at all, we arrive at a more profound understanding of science, and very different methods for improving scientists' methodology. As well as studying the practitioners' *surface* methodology through epistemological analysis, in hope of elucidating and improving methods of testing theories that are perhaps already half-understood by practicing scientists, I see advantage in putting science through *deep analysis,* to bring the *least* exposed factors—those furthest from conscious control—to the light of understanding, and perhaps, to conscious control. In science, I expect, the rational is only a small fraction of what a philosopher might wish to understand.

My concern with factors of scientific development far from conscious control, of course, explains why I have studied contemporary historians of science and the mettle of their methods, and why I find their work relevant to philosophy of science. Many professional historians of science focus on just these deep associations in their analyses of individual scientists and controversies. They provide important psycho-historical analyses: of the psychology of scientists (Dobbs: *The Foundations of Newton's Alchemy*, Westfall: *Never At Rest*); of the social psychology of groups of scientists (Morrell and Thackray: *Gentlemen of Science,* Rudwick: *The Great Devonian Controversy*); and of the impact of culture upon scientists and their work (Ginzburg: *The Cheese and the Worms,* Gingerich and Westman: *The Wittich Connection*). Many historians, including some of those just mentioned, also provide analyses of the *non-rational historicality* of scientific development that are separable from psychological analysis, and serve to further indicate the shortcomings of rational analyses, which do not have the scope to cover such matters. I will now turn to one such case of historicality in scientific development for a short example of the sort of history and historical conclusions I see as relevant to my focus in philosophy of science.



### Part I, §2 Historicality and contingency in science: one historical example[282]

I will attempt to argue for the philosophical relevance of the writings of practicing historians by considering closely the work of two figures—Lorraine Daston, in *Classical Probability in the Enlightenment,* and Ivo Schneider', in "Laplace and thereafter"—concerning early theoretical developments in Classical probability. I will compare their analyses of the historical development of classical probability with those that I think would be available on an intellectual historian's methodology, or to a rationality theorist; in doing so, I hope to indicate the shortcomings of those other views in coming to grips with intelligible features relevant to explaining the history of science and scientific development.

Daston and Schneider probe the early development of probability theory, and focus particularly upon the rise and fall of the Classical interpretation of probability—the view that the probability calculus should be developed so as to provide an index of reasonable belief. Pierre Laplace put the goal most succinctly in the late 18th century as "good sense reduced to a calculus"; and the same goal is also voiced clearly by the earliest theorists in the line, such as Blaise Pascal and Jakob Bernoulli.[283] The goal was clear, and much work was done in its pursuit; but Classical probability became a *repudiated* topic of research during at least thirty years of the 19th century, approximately between 1840 and 1870; for example, it was deemed by Siméon-Denis Poinsot "an aberration of the intellect, a false application of science"[284] . The goal did not gain popularity again until the third decade of this century, with the work of Keynes, Carnap, and Ramsey; and now it is quite heavily studied, under the heading "Bayesian inference".[285] The question to be considered here, then, is: *Why was this topic simply dropped, since it was a legitimate enough one to be picked up again in the 20th century?*

Historians (and the actors) tell a variety of stories about plausible factors contributing to Classical probability's decline; and the variety is what particularly draws my attention, for I think that *a broad variety of causes* contributed to Classical probability's development and decline: and this variety serve to indicate the character of scientific growth. I will not attempt to settle the issue here, and present an argument to close debate over the variety of causes and their relative significance. I will instead argue from this case that a narrowly rational account, such as the paradigm of intellectual history allows, goes nowhere near as far towards presenting an adequate explanation of change as would be available from a different methodology.

***Intellectual history:*** Many of the analyses of the fall of Classical probability, discussed by both historians and actors, fit well into the intellectual historian's division of intellectual and nonintellectual sources of change: we can see clear sources in both areas. Classical probability was a successful program in the 18th century, but among 19th century probabilists, an often-voiced opinion was that the Classical approach had been misconceived: it applied mathematics into domains where variables were too difficult to analyze, and where the *judgment* of skilled individuals was required. This was particularly the concern with regards to the moral sciences; a review of Poisson's approach, written by Charles Gouraud in 1848, gives a representative analysis of the basis and character of the Classical probabilists' supposed blunder:

> The objects of nature as a whole, in the moral as well as in the physical world, are according to [Poisson] subject to a universal law that is approximately as follows: if one observes a very considerable number of events of the same kind that depend on constant causes, causes that, as happens in the moral world, vary in an irregular fashion . . . the amplitude of these irregular effects produced by the variable causes will increasingly contract proportionally as the series of experiments becomes larger . . .



[For future reference: the 'universal law' described above is a rough and ready gloss on Bernoulli's law of large numbers[286] ]

> Armed with this principle, [Poisson] did not even shy away from the determination, however dangerous it might have been, of the mathematical probability of every human decision. . . . In the *Recherches sur la probabilité des jugements,* one finds numerical expressions that are obtained from an analysis of a large number of earlier judgments whose aim is to determine the exact future probability for each citizen of being charged, convicted, or acquitted. This is an almost incredible audacity, which exceeds that of Laplace and Condorcet.[287]

Other, more clearly mathematical problems certainly arose for the Classical interpretation: counterintuitive, though not inconsistent results arose from the theory, for example[288] . Though "audacity" in the application of the Law of Large Numbers may strike one as a weak and unsatisfying criticism of little mathematical merit, however, Daston and Schneider suggest that it nonetheless was a very important one.

Such a conclusion is not, I expect, fitting to the methodology of intellectual history: how, then, might an intellectual historian explain these developments? There does exist a traditional story of the decline, and Daston and Schneider draw some analyses that do, I expect, fit well with intellectual history. The prevalent 19th century interpretations of Bernoulli's law of large numbers suggest a clearer intellectual reason for the Classical interpretation's failure: the unified account of probability broke up when probabilists developed a distinction between subjective (epistemic) and objective (resident in nature) probabilities; and this division is often understood as a primary reason behind the change.[289] As the account goes, a new and very fruitful *objective* interpretation of probability had arisen, which supplanted the subjective interpretation of Classical probability. Physical scientists went on to apply statistical reasoning to gas theory, though not until late in the century, and mathematicians and astronomers went on in mid-century to produce further developments out of Laplace's work in the objective direction, in the development of error theory.[290]

Such development is of great relevance to the history of probability, but the rise of the objective interpretation does not explain the *devaluation* of Classical probability, nor the devaluation, beginning in the 1830's, of subjective probability, which was the natural heir to many developments in the Classical approach: still to be explained is this *loss* of interest in the study of rationality. The intellectual historian might at this point turn to *external* explanation, for a non-intellectual source of the Classical interpretation's decline: Schneider suggests that the fall was partly due to philosophers' hostile reactions to the inroads that mathematicians were making into their discipline, the moral sciences. The Classical theory may have fallen victim to broader 'turf debates' between the *Académie des sciences* and the *Académie des sciences morales et politiques*; and there was similar political factioning within mathematics. Finally, on the boundary *between* intellectual and non-intellectual sources—an area perhaps within the purview of intellectual history, where the differences between the intellectual and the social become less isolatable[291] —Schneider suggests a variety of intriguing contributory causes. Many mathematicians and philosophers also objected to Classical probability on the basis of broader intellectual concerns, for Classical probability eroded other intellectual ideals: in matters of law, for example, subjective probabilists focused on the "convictability" of charged individuals, rather than their guilt. Poisson's analysis of the probability of a citizen being convicted of a crime is, in itself, merely a statistic: its subtle implications regarding human predictability as opposed to freedom may have been a contributing factor, making the enterprise objectionable. Divisions among mathematicians also had to do with the professionalization of disciplines: a gap opened when, in universities, debate moved towards pure mathematics, and government commissions took on research in applied mathematics,



focusing more on social application and less on practical problems of individual reasoning.

***Beyond intellectual history:*** The paradigm of intellectual history, then, with its divide between intellectual and non-intellectual (primarily social) forces on development, allows for some interesting analyses of causes of Classical probability's fall. But there are further candidates for causes of change that do not easily fit into intellectual history's scheme, nor that of Lakatos, nor that of the logicist. For example: Daston makes much of the historical character of the chosen concept of *rationality* founding the Classical interpretation. She argues that it had a great deal of influence on the path of development of Classical probability: subjective probability was intended to be "good sense reduced to calculus", but that good sense was originally gathered from intuitions of particular men of particular station, and from good sense codified in culturally prevalent juristic and economic ideals.[292] The tug-of-war between competing conceptions of reason is particularly obvious in the debates over the St. Petersburg paradox—Classical probability's most protracted debate, spanning centuries partly because of the variety of intuitions about *good sense* that theorists attempted to reconcile.[293] If the Classical probabilists' conception of rationality was largely grounded in features of their culture, then this aspect of the development of the theory does not appear to fit into intellectual history's scheme: for there appears to be a significantly and irreducibly social element in the *development* of the theory.

Perhaps philosophical rationality theorists might be content to live with such shortfalls in characterization and explanation, even though intellectual historians could not; for the logicists concern themselves with the context of justification, and not of discovery, and Lakatos' reconstruction has a different, but similarly abstract relation to actual historical development. As I suggested in the first chapter, there are good and principled reasons available for constructing the divide between context of justification and context of discovery; and so, the cultural sources of the theorists' rationally held ideas might be considered to be 'external' to the problematic of justification and of appraisal of those ideas. But it is very important that we be aware of how much is lost in the move to the exclusive study of rationality in the context of justification. Rationality theorists might be made to feel discomfort at their restricted field yet, I suggest, for their context of justification appears to provide little purchase on the concept of *rational pursuit* (or methodology) in science.

Rationality theory, like intellectual history, is limited in its analysis of rational pursuit because each draws a similar internal—external divide concerning rational appraisal and intellectual sources of change. Consider the juridical and economic ideals appealed to by the Classical probabilists, which, according to our historians, appear likely to change according to political and social concerns as well as narrowly rational concerns. If we take it as given that they serve a significant role in the historical development of Classical probability, they would appear to fall under the non-intellectual category of sources of change for the intellectual historian; and they would be non-rational aspects of change excluded from the rationality theorist's context of justification. But I expect that these ideals *should not* fit into the categories of non-intellectual and non-rational—the external, to both the intellectual and rational—for sources of change that are classified into those categories also carry the implication that they have no positive bearing on the growth of knowledge: the non-intellectual (often read as 'social') and the non-rational (often read as irrational) are at best neutral, at worst retarding forces on the intellectual and rational. But some of the social and political factors affecting development, and especially the juristic and economic ideals, are clearly *constitutive features of the positive development of the science,* and so should not be classed as *external* sources of change. If I may adopt Kuhn's framework for a moment: we see that the actors' conception of rationality appears to be governed partly by a juridical *paradigm*, and this paradigm's ascendancy is to be partly explained in terms of culture and the actors' positions in history. If one attempts to



maintain an unchanging, universal conception of intellectual sources of change, then this example suggests that some non-intellectual social forces contributed positively to the shaping and assessment of a scientific theory.

The actors' conception of rationality appears to be a feature that is both a historical happenstance, to some degree, *and* one that plays a significant, productive role in the historical development of the theory of Classical probability; and for this reason, it doesn't *really fit* into the intellectual historian's classification system, nor that of the rationality theorist, for it is not entirely *rationally* grounded. The actors' conception of rationality is partly rationally grounded—for instance, in an understanding of the efficacy of law and economics—and partly culturally grounded—in an understanding of specific legislation and economics tied to cultural behavior and cultural norms. This presents a strong argument, I believe, that scientific *pursuit*—what scientists actually do and should do—is underdetermined by a narrowly conceived scientific rationality or scientific methodology.

***Historical influences?:*** I have argued that the Classical probabilists used historical features of their culture to build and gauge their theory of rationality; but taking the case on behalf of the intellectualist opposition, I should ask: How central were those features to the theory's development in the long term? Might they only be *ephemera* that the calculus of good sense, in its normative development, overcame?

Note first of all that these two questions differ greatly, both in intent and import. If the former is misconstrued as the latter, half of my point is lost, for something ephemeral can nonetheless be a crucial feature of theory development in the context of discovery; and my point is that an account that can teach us about science should include all that is crucial. Consider now the second question, which doubts that cultural influences have a long-term effect in science. Recall that the project of Classical probability collapsed—Daston argues that this failure, like the launching of the program, *also* had to do with cultural norms. By the early 19th century, the probabilists' faith in their sources of standards for a calculus of good sense was waning. The calculus in the 18th century was mostly developed as a descriptive theory, provided to accommodate intuitions in line with the élite's conception of good sense; it took on a stronger normative status in the 19th century. But development in the 19th century also proceeded towards *doubt* that individual rationality could be characterized by calculus. Did the change arise because the theorists were getting at the truth, and zeroing in on better conceptions of rationality?

Perhaps so—I expect so—but note that even if this were the case, it would not get the intellectual historian off the hook for explaining the *rise* of Classical probability before its demise; and Daston's history suggests that such an analysis will not suffice to explain the fall of Classical probability for the rationality theorists either. Classical probability, she argues, was not only grounded in culturally prevalent ideals of rationality; it also arose and fell partly due to historical shifts in conceptions of rationality, and changing perceptions of stability within the world. The break with the Classical model appears to have been accomplished on two fronts, both indicating a general erosion of the enlightenment ideal of the *rational individual;* and the fall of the model, she suggests, was partly a result of that more general social movement.[294] First, as interest in the interpretation of probability theory in terms of rationality waned in the early 19th century, interest picked up in the objective interpretations mentioned above. Adolphe Quetelet introduced "*l'homme moyen*" as his topic of study, a statistically average man, whom he apparently considered an ideal, and the acme of civilization. Quetelet's theory was not, however, intended to aid the individual in decision, but to aid social planners: thus, interest in rational thinking was replaced by interpretation of society along the lines of the objective ideal, of the sort soon to be seen in the new approach developing in the social sciences.[295] Second, Daston also presents the (sparsely defended) hypothesis that the rise and fall of the Classical theory, and the changing conception of rationality, had much to do with the theorists' environment and their conceptions of its stability. Social environment had great effect on the *kind* of theory done: *Increased* stability at the end of the 17th century led to first attempts at probabilistic calculus, beyond the more modest projects of constructing rules of judgment in the 17th century. Towards the end of the 18th century, decreased social stability, as represented in the French revolution, led to distrust of the enlightenment ideal of the individual and a further change in standards for probability, away from subjective, and towards objective analyses. Daston suggests, then, that the broadest of political tableax, and consequent conceptions of man and of useful social action, had roles in the subsequent rise of the objective ideal, which supplanted the rational ideal.[296]



Enough of hypotheses on sources of change. I have laid out a variety of historical hypotheses concerning the rise and fall of the Classical interpretation. I certainly do not have the space here to adequately judge the relative merits and interrelations of all of the causes introduced; my purpose is to provide them as illustrations, and indicate the improved philosophical perspective that such history allows for.

***Philosophy, reasoning and historicality:*** The history that I have reviewed has already gone most of the way towards indicating the character of the sort of philosophical study and philosophical account of scientific development that I wish to promote: I will go on to outline such a philosophical program in the following section. The example suggests that a great multiplicity of factors played productive roles in the growth of Classical probability theory and the later emergence of the approach of objective probability for the social sciences: intellectual developments such as the law of large numbers; intertheoretic support in psychology; metaphysical concerns in ideals of freedom and responsibility; institutions such as law courts and government commissions; and broad cultural and political developments in Europe. A convincing, connected account of the sources of growth over a long range of history and a broad field of concerns that would allow for such historicality would be the philosophical ideal for the study of growth.

How does this project tie to more traditional philosophical projects? I think the relevance is only glancing, as my goal is to produce an adequate account of scientific growth which goes beyond, but need not entirely reject, theories of scientific rationality. To compare: Philosophers such as Colin Howson and Peter Urbach have attempted to argue for the applicability of Bayesian standards to scientific reasoning. They have shown the Bayesian approach's consilience with intuitions, and have alluded to logical arguments that establish the appropriateness of applying the theory of probability to fair betting. What history has further provided is argument that the intuitions which Howson and Urbach appeal to may also be historically conditioned, and subject to future change. The historians' analysis suggests that the very subject of Classical probability, good sense that is reducible to a calculus, came into doubt for an extended period, and partly for reasons which cannot clearly be classified as *detrimental* nonintellectual ones: the rise of objective probability, social revolution and changing conceptions of human character and psychology. The current 'fit' of Bayesian reasoning with the data of intuition, then, may be only temporary; the ideal of a normative normal standard for reasoning, in some ways reminiscent of Quetelet's *homme moyen*, appears historical.

This argument for the relevance of history to philosophy of science, which indicates the extent to which my position might be a relativist one, is not intended to undermine methodological projects such as that of Howson and Urbach. In suggesting the historicality of their enterprise, I do not wish to argue that it is, consequently, the less legitimate; but historicality does suggest that their project should be construed less as a strongly normative one, and perhaps more as an exercise in clarifying concepts. Is the history sufficient to support such a conclusion?—I expect that as Daston and Schneider argue it, it is sufficient to be worthy of consideration. Doesn't the history suggest instead that our understanding of reasoning has improved?—Whether or not our understanding has improved, here we have a case in which a field of research was dropped, and then picked up again, many years later. The case suggests that this science, the study of rationality, is itself historical; and a non-historical conception of progress with regard to our ability to explain the phenomenon seems either inappropriate, or only productive of a limited form of explanation, not adequate to one sort of analysis of scientific development.



### Part I, §3 Why and how to study growth

***Rationality theories and scientific growth:*** I hope that my historical example has argued well enough to indicate the attractions I find for a philosophy of science that studies scientific development on a larger scale than an account of scientific rationality allows. I will pick no more quarrels with rationality theorists here, but will instead press on to present a quick characterization of the philosophical program of studies that I envision. To begin, I should point out the advantages that I see such a study as presenting.

Indeed, I need have no quarrel with rationality theories at all. One advantage that I see in my approach is its flexibility with respect to theories of scientific rationality: it may accommodate any or none, I expect, *if* they are construed as empirical theories, to be examined within historical research programs. I do not intend to suggest that a rationality theory must be a useless enterprise, and to the extent that one may play a role in historical explanation, it would be of great utility to *my* program of research. My point in this chapter has been that a rationality theory is insufficient to explain scientific development; and this suggests an important problem for rationality theorists, such as Laudan, who intend to test their theories on the field of history. Lakatos suggests that good rationality theories tend to have both clear successes with some historical cases, and other cases that do not fit well at all; and the advocates of each program crow the success in cases of the former kind, and maintain the latter set under the heading "unresolved anomalies". Rationality theorists, I expect, require a general theory of scientific development such as I propose to *enshroud* their rationality theories, allowing them to stand up to broader historical testing situations. An account of development should not be looked upon as an *ad hoc* adjustment to a rationality theory: it provides legitimate explanation of scientific development, to the quite significant extent that it is not entirely rationally governed.

I have taken no position regarding the relative virtues of rationality theories, here or in the historical example I have developed, primarily because my intent has been to display the shortcomings of such theories. I see no reason why the projects *cannot* be unified, or would not be unified in a fuller explanation of scientific development, but my interest lies in promoting the study of other features of growth for the purposes of this thesis. My writing and hedging probably indicate that I remain agnostic, and somewhat confused about the character of an appropriate theory of rationality for science, and I do feel much more confident in the successes to be found explaining scientific development along other paths. By *not* endorsing any specific rationality theory, I might appear to be endorsing none at all, in an epistemological relativism, or anarchism. My nescience does bend to those directions, I must admit; and those epistemological approaches will be considered in Part 2 of this chapter.

***Advantages of a study of growth:*** Why else study growth over rationality and logical foundations? The broader understanding of the process of science afforded to the study of growth and of historical variables will allow philosophers many *practical* gains. A growth analysis is more likely than other philosophical approaches to adequately address questions relevant to science education and science policy; such as:

*What is the significance of rationality in science?*

*What perceptions of their role in producing knowledge do scientists have, and is it accurate or appropriate?*

*What are the advantages of different methodologies?*



*What is the variety of aims, methods, and arguments accepted in a science at a time, and at different times? What are the limits of rational variation at a time?*

*What practical effects on scientific development do various institutions have?*

*What activities constitute scientific advance?*

All of these questions belong to science policy, and some may *look* like they belong to history or sociology—and they do. I am an advocate of tight interrelations among disciplines in science studies; but one can put a specifically and significantly philosophical focus on one's answers to these question as well, as I will suggest below in "Philosophical goals for a study of growth". One particularly philosophical project in the study of growth is assessing the significance of traditionally philosophical classifications of science: What is rationality, and what role does it play in scientific advance? What is scientific reasoning, and what role does it play? These questions clearly shatter the distinction between context of discovery and context of justification; but might it be of no use here?

***Baconian philosophy of science:*** How does one go about determining the character of scientific development without using a rationality theory? Simply "looking at the history" will not do: an account of growth will not easily arise out of a naive induction from a broad sweep of stories from "the history of science". But the traditional philosophical approach of formulating prior hypotheses and *not* looking with care at the history first—the problem with logicism, and to a great extent also with the so-called "historicists"—will not do either. If an explicitly articulated methodology is needed, my position and argument regarding the philosophy of science, as I see them, are very much like Francis Bacon's regarding the practice of science, according to an account recently provided by Peter Urbach; and so I would like to advocate a Baconian approach to the philosophy of science.

Bacon has traditionally been associated with a naive view of the process of scientific discovery: the view that individual scientists develop theories on the basis of a bare induction from observed features of the world. Urbach suggests that Bacon held positions quite different from those attributed to him according to the standard view: Bacon was a "hypothetico-inductivist"[297], rather than the naive, and ultimately inconsistent inductivist that many consider him to be.

Bacon, according to Urbach, held that the careful collection of evidence that he became so famous for advocating was intended as a *prelude* to a second sort of activity in science, the formulation of hypotheses *beyond* the data. Collecting evidence was intended to be followed by the "commencement of interpretation", or the "first vintage" of knowledge.[298] These features of Urbach's interpretation justify his calling Bacon a "hypothetico-inductivist", but the importance and character of his approach can be seen only in reference to the approach that Bacon *opposed*, the "anticipation of nature", reflected in astrology and astronomy:

> For hitherto the proceeding has been to fly at once from the senses and particulars up to the most general propositions, *as certain fixed poles* for the argument, to turn upon . . .[299]

Anticipation was a method of constructing propositions "by a scanty and manipular experience"[300], tending to fit the hypothesis to the fact, and situate it immovably at the level of first principles. Bacon, Urbach asserts, does not criticize anticipation because of its *speculative* (i.e., hypothetical) character: he argues that it is weak because it tends to address the facts, rather than going on to produce novel predictions.[301] The tendency to jump to first principles, Bacon suggests, also leads to a tendency towards conventionalism in theory, rather than towards a search after causes and mechanical explanation.[302] Bacon suggests his method as a remedy to these weaknesses; a method which does not deny the value of hypotheses, but asserts the value of prediction, causal explanation, and particularly, investigation over a wide variety of contexts, to maximize certainty.



My inclination regarding philosophical methodology is very much the same, I believe, as Bacon's is for scientific methodology. I have criticized some approaches in philosophy for a similar tendency to jump to 'first principles' and explain scientific growth in terms of prior theories of rationality. To understand growth, then, a closer examination of history is necessary because, I expect, growth does not proceed in lockstep with rationality.

I advocate to the philosopher the merits of an openness to the study of history verging on *vacancy;* and this recommendation does not ultimately present much of a methodological guide to study. I suggest that historical features play significant constructive roles in scientific growth, and in the production of scientific knowledge. The epistemological position that I embrace in my methodology for historical investigation is, then, conspicuously relativist: It is brazenly non-foundationalist, and begins to look like the last refuge of a philosopher; for what is left of philosophical criticism and elucidation of science without a separable epistemological basis from which to launch the analysis? Some authors, such as Larry Laudan and Dudley Shapere, present attempts at constructing non-foundationalist epistemologies that do not endorse relativism: both authors argue that, though no global conception of scientific rationality governs all of the profound changes in aims or methods that science undergoes through history, on a more local level, the details of change can be seen to be rational under the circumstances. I will return to explain this conception of local rationality in the final part of this chapter; my position, by contrast, is that non-rational sources of change abound in history, and guide scientific development to a great extent. So far, I have attempted to argue for the plausibility of the relativism that I expect to find in scientific development, and the appropriateness of my methodological historicism as a philosophical approach; but I have not given much of an indication of just how history is to be treated by philosophers, and what the product of such an approach would look like.

***Philosophical goals for a study of growth:*** What I have suggested may appear very like a hybrid of two historical projects, intellectual history and cultural history: a program partly intended to examine intellectual change, like intellectual history, but with a broader methodology, maintaining a regard for technological, social and cultural bases of intellectual change as well. But I do not think that the pursuit is quite the same as any of those approaches, for the goals which I will suggest are not quite the same. I do recommend that intellectual and cultural history be studied, and considered central sources of ideas for philosophers—ones as important as primary historical sources—because they may provide ideas that, in my opinion, *just would not occur* to philosophers of science, trained up on rational dialectic and eternal verities, rather than the development of institutions, and human biography.[303] It is certainly the case that historians know more history and on the whole write better history than philosophers: they also produce ideas valuable to a natural account of scientific growth.

My method in philosophy is very close to methodologies in history because, as I indicated above, methodologies in traditional philosophy of science have presented too narrow an explanatory base to provide an explanation of historical development, which I hope to achieve through a study of growth. It shouldn't be surprising that my project looks like history, since I have, in several chapters, provided different attacks on the intellectual—non-intellectual and context of discovery—context of justification distinctions that pervade philosophical and intellectual history accounts. I do see some differences, however, if not in form, then in content. The difference in content, in brief, is that my philosophical account, and the philosophical history that might accompany it, will be much more schematic, linear, and *longue durée* than what most historians would produce: I am interested in the wheels that turn in history and result in growth, rather than the historical accounts themselves (among historians, and for the purpose of historiography, perhaps Fernand Braudel expressed similar concerns).

I have not much more to say in general about what growth *is,* however, because at present my position puts me on the near-end



of a very complex historical project. I presented some tentative philosophical conclusions concerning development in the particular historical example that I chose; but this thesis sets out the project, it does not set out on the project. I believe that I retain some decidedly philosophical goals, then, which differ from most historians' goals in content, if not in principle. Those goals, in order of increasing difficulty and depth, are as follows:

1. *Discern the sources of change in science: intellectual, social, technological, . . . etc.* This is the project of determining the breadth of variables affecting scientific change. Emphasis on variety counteracts the philosopher's tendency towards setting an unduly restrictive limit concerning the historical factors responsible for growth. Though it remains reasonable to distinguish between change in general and growth (perhaps retrospectively), a survey of causes of change will allow for a more responsible evaluation of aspects of growth.
2. *Delineate the limitations, relative importance, and interconnections among features affecting growth.* Though philosophy of science might be primarily concerned with intellectual causes of growth, other historical circumstances set limits to intellectual possibilities, or determine conditions for the possibility of development in knowledge. In the case above, for example, I indicated that culturally available ideals of rationality played a part in determining scientific development. This project specifically helps in providing answers for the science policy questions mentioned earlier in the chapter.
3. *Discern the history of scientific methodology.* My philosophical methodology allows for the construction of a history of the development of methodology in science; a historical study of the rise of *objectivity* in science, as well as the rise of objective knowledge (which is the traditional philosophical project). The history, for example, might include for the seventeenth century the rise of experimental method, hypothetico-deduction, and community practice. A detailed and extended

history of scientific methodology would be the goal, a project I oppose to Lakatos' retrospective history of methodology, in which change of method appears to be suppressed.

### Part II. Historicism, epistemological relativism, and order in science

In adopting a Baconian method, I have committed myself, at least temporarily, to a view of philosophy of science with effectively no substantive normative content with respect to the character of science; and particularly, here I have also endorsed *no* specific theory of scientific rationality. I appear, then, to have nothing to say about good versus bad method, science versus pseudoscience, or growth versus degenerative development. Isn't such a position philosophically odious? The charge has now been addressed to me many times, approximately along the following lines: "You have given up epistemological foundations for science, and you have given up the intellectual—non-intellectual distinction; so what, then, holds the position of methodological historicism apart from commitment to a radical epistemological relativism? You have no basis upon which to distinguish productive from damaging activity; so why doesn't your position just collapse into Feyerabend's epistemological anarchism, which is to say, epistemological nescience? What conception of growth could you possibly maintain?"

A fast response to the charge is a qualified capitulation: I have no reason to believe that science does not proceed anarchically. Because I have attacked the traditional distinction between the intellectual and the non-intellectual, and have also attacked standard foundationalist epistemology of science, and because I have suggested that we have to look at history to determine just how scientific methodology proceeds and changes, *I cannot legitimately present an epistemological account of scientific knowledge*—for I haven't worked through enough history yet. I have clear reasons—historiographical scruples—for having little to say about science, since this thesis has concerned itself with issues in historiography and



the viability of various *philosophical* methodologies, rather than scientific methodology.

On the other hand, *some* intuitions about the nature of scientific change, such as were discussed previously in this chapter, prompted my line of inquiry: at the very least, a vague worry regarding the satisfactoriness of logicist and historicist attempts to explain scientific growth stirred up my concern, and I do, in fact, have some ideas about scientific method and success, and the sources of change in science's method. Historiographical scruples may conflict with epistemological intuitions, and especially with philosophers' fears: I can responsibly write something to calm those fears. Given that I have not as yet any well-founded concrete ideas about the basis of scientific change, I can instead assuage doubts about methodological historicism and the nonfoundational approach to philosophy of science by showing in a more abstract way why a historicist antifoundationalism *may allow* for a reasonably stable and traditional conception of science and of growth—if the history bears one out—and *needn't* point to epistemological anarchism.

***A methodological position:*** The general fear to be addressed is that *any* historicism, including methodological historicism, is forced (because of its intellectual libertinism in forsaking foundations) into the position of a particularly radical epistemological relativism: the view that there exist no rational standards by which knowledge claims can be judged. Embracing such relativism, Paul Feyerabend named his normative position regarding standards of scientific debate "epistemological anarchism", and adopted the motto "anything goes" for scientific procedure.[304] If epistemological or methodological standards in science are, as historicism allows, historically variable, then all assessment of knowledge claims and explanation of the growth of knowledge becomes *doubly* problematic. For if all standards of knowledge are mutable, there remain no standards by which to judge any historical development of knowledge as rational or irrational, progressive or regressive, or growth or degeneration: all one may say is, "beliefs change". Furthermore, historicists appear to be hindered on *two* fronts, because of reflexivity: for they are attempting to make judgments and assertions which involve knowledge claims, and those claims are also about the history of science, a separate knowledge-gaining activity. This is precisely the two-fold problem that supposedly reduced Cratylus to silence in his attempt to express Parmenides' metaphysics coherently.

First, to clarify, one should not assume that the historicism which I promote as a methodology for the study of science *must* imply epistemological anarchism, relativism, or even a substantive historicism concerning scientific practice; nor that the motivation for taking this approach need lie in a belief that science goes through radical shifts. As I argued in Chapter I, anarchism is a substantive position: construed descriptively, it presents the argument that no reasonably constant patterns are discernible in history; construed normatively, it maintains that "anything goes" is the appropriate methodological stricture for science, which is no restriction at all. Methodological historicism, on the other hand, I have presented as a methodology for philosophy of science, rather than a substantive position, and one intended to allow for the possibility of *detecting* radical shifts in scientific methodology (i.e., historicism) and in the character of scientific knowledge (i.e., epistemological relativism). On the basis of methodological historicism and the Baconian methodology, one may build a substantive position such as epistemological anarchism, or a moderate relativist historicism; but one might instead return to an assertion of epistemological foundations: *a survey of history and the Baconian method are intended to provide the basis for a substantive position*; and history might point the inquirer towards anarchism, historicism, or foundationalism. Methodological historicism, then, allows for relativism: but it does not assert it, nor deny the existence of norms or stability in scientific practice.

The history of science has *suggested* the presence of shifting epistemology and methodology in science to me, but I am most interested here in producing a methodology for inquiry that does



not automatically beg the question *for* shifting *or* stability. The motivation for methodological historicism, then, lies in a belief that science *may* undergo such changes: I wish to allow that science *may be* a *historically open* activity (or what Wittgenstein has called an unbounded concept[305]), a game in which the rules may be changed, for one reason or another, since there is no good reason to exclude that possibility. And I suggest (below) that even a historically changing methodology in science, with no underlying stable foundations, can nonetheless exhibit order and growth.

***Epistemological relativism assumed:*** My discussion has been very abstract up to this point: some assurance that my allowance of the *possibility* of relativism will not rob science of all stability is needed. Let us assume, then, for the balance of the chapter, that my methodological historicism, as the result of a survey of history, *has* pointed me in the direction of a relativist position. Not only have I found a need for a study of a broader topic than rationality to explain development (the point of the first part of this chapter); I also find no reason to assert the presence of a continuously forward-marching rational progress occurring through history. Suppose I adopt a position in epistemology reminiscent of the Kuhn of *The Structure of Scientific Revolutions* such as Gerald Doppelt suggests, and names "moderate relativism":

> On this view, while scientific development is rational (there are typically some good reasons for the theory-changes which occur), new theories often fail to be demonstrably more rational than their predecessors, on any standards which are mutually acceptable and applicable to both. As a result, the thesis of moderate relativism is that scientific change is often or typically underdetermined by good reasons. It opens the way onto a 'sociological relativism' which claims that the explanation of scientific development or agreement requires an ineliminable sociological component to explain why scientists agree to make theory-changes which are underdetermined by the good reasons in their favor.[306]

The affinities of the sociological relativism that follows in the wake of this moderate relativist position with my arguments concerning the historical contingency of scientific development should be obvious. Under the assumption of such a relativism, then, the key issues of our discussion are: what conceptions of *continuity, order, stability, normativity, and growth* in science, *could* remain to moderate the relativist tendencies? How far back can the anarchistic tendencies of relativism be pushed, and how far forward can order and some conception of rationality venture, in a science not governed by foundations, nor by a determining theory of scientific rationality? Science is an ordered activity, but what order could remain to a game in which the rules change?

***Continuity without essence:*** The game metaphor used above, of course, suggests Wittgenstein's work, which gives us several plausible and by now familiar accounts of the relation among varied activities that fall under the same name. The category 'science' might be like Wittgenstein's 'game'[307]: a collection of activities *related* in many ways, but not connected by a core concept, a single 'essence' of scientific rationality or method, or a foundational epistemology for science. The game of science might be many games, each exhibiting a 'family resemblance' to others—the importance of Wittgenstein's examples and analogies is that collections can be made to form one specifiable grouping in which there need be no elements that all members share: just substantial overlap. The radical diversity of method and radical nature of change allowed in historicism needn't rule out a recognizable family of activities, each called "science", even if foundations are not present.

***Ordered development:*** Such a simple non-essentialist account of science will probably not suffice for any reasonable historicist account of *growth*, however, since the picture constructed is one of a *non-ordered* bag of related tricks called "science"—this is Feyerabend's view, and allows for the most severe relativism.

Historicism allows for some order in time; and history, *prima facie,* suggests that some ordered development does occur: for certain kinds of method become obsolete, such as appeals to the Bible, and it seems safe to bet that they will not gain power again; certain innovations arise which apparently could not have been presented much earlier in scientific development than they were; *etc.* If we are attempting to account for the possibility of growth and order without foundations, a different account is necessary. Consider, then, another of Wittgenstein's analogies, the thread:



> And for instance the kinds of number form a family. . . . Why do we call something a "number"? Well, perhaps because it has a—direct—relationship with several things that have hitherto been called number; and this can be said to give it an indirect relationship to other things we call the same name. And we extend our concept of number as in spinning a thread we twist fibre on fibre. And the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres. [308]

The thread analogy is similar to the family resemblance analogy; but slightly altered, it can serve better to elucidate a historicist conception of scientific development. If different 'games' (or, alternatively, specific methodologies) of science are considered to be the fibres of the thread, and the thread itself is stretched through *time* rather than space, then a continuity of science through history is represented, without appealing to a connectedness of practices and methods separated through a long stretch of time. (Picture the thread, an overlapping collection of fibres, *coming towards you* out of the past.) Science *en bloc* is then understood as a history of limited, continuously interconnected methods or practices; but innovation and obsolescence are also allowed, as new fibres begin and old fibres end.

***Stability:*** Continuity in scientific method, then, might be accounted for without foundations. A complementary stability of development in the discipline (again, anticipating historical backing for the phenomenon) might also be achieved by the replacement of aspects of method or practice in science, but replacement in a *piecemeal* fashion.

Laudan's reticulated model of scientific change in *Science and Values* suggests such an approach to ensuring stable change without foundations. Laudan presents a non-foundational approach to accounting for scientific growth in his book, in response to two other approaches, a "hierarchical model" and Kuhnian historicism. According to the hierarchical model, "disagreements about factual matters are to be resolved at the methodological level; methodological differences are to be ironed out at the axiological level", and differences in aim—in cognitive goals, such as truthseeking—"are thought to be either nonexistent . . . or else, should they exist, irresolvable"[309]. Laudan argues that this hierarchical model, which he sees as representative of logicist approaches, does not fit the history of science: disagreements regarding cognitive values occur and are resolved, and the state of the facts commonly leads to changes in methodology. Laudan proposes a more complex, reticulated model of scientific change to replace the hierarchical one: though aims still justify methods, and methods justify theoretical developments, as in the old hierarchy, influences work in the opposite direction as well: facts and fertility on the theoretical level *constrain* methods, and the realizability of aims at the theoretic and methodological levels has effect on the choice of cognitive goals.[310] Aspects of scientific practice at all levels—theory, method, and practice—are thus mutable, but an ordered structure of constraints limits their changes: thus Laudan presents an account of change that he considers to be non-foundational.

Reticulation provides for an account of historical change in science that is foundationless and might nonetheless remain stable; and so far, I would follow Laudan. But Laudan takes the issue further by tying his model to a thesis concerning rationality: having addressed foundationalism, Laudan turns to consider Kuhnian claims that scientific change is an irrational process. If a scientific practice has no foundation, profound and abrupt changes of char-



acteristics at *all* levels of scientific practice become possible: in other words, radical relativism is possible. Kuhn's *Structure of Scientific Revolutions*, as I argued in chapter 2, suggests that such fundamental, sudden change does occur in history, in cases of scientific revolution, where rational explanation is forced to make way for psychological and social explanation. Laudan argues for the stability and the rationality of science in his non-foundational approach by relying on its properties of articulation, and by making a historical claim about the piecemeal character of scientific change. The wholesale changes in all aspects of scientific practice which may occur, he suggests, occur over extended periods of time: a scientific revolution, when looked at more closely, is likely to show two aspects of practice as relatively stable when one is shifting. Thus Kuhn's view of revolutionary change, which would be an abrupt change between two unrelated states:

a) *theory , method, axiology* to
a') *theory' , method', axiology'*
might, on Laudan's account, proceed through four states, such as:
a) *theory , method, axiology* to
*theory', method, axiology*, then
*theory' method', axiology,* and finally, to
a') *theory' , method', axiology'*.

In simple language: Laudan argues that a scientific revolution is a process of several changes that look, from a distance, like a saltation. On closer historical examination, however, the jump is shown to occur through piecemeal change, and two aspects of the practice remain as a rational base, to adjudicate adjustment of the third.[311]

Laudan provides a good case for the possibility of relative stability without foundations, even in the face of profound change. His position is also fortified with strong historical defenses that I will not detail.[312] The use of history as a crucial element in his arguments suggests a great affinity to the project that I have been pursuing, and a good deal of his historical citation in *Science and Values* appears to be relevant to my project. His attempt to account for scientific change in terms of rationality, and especially in terms of three canonical and asocial features of scientific practice, on the other hand, goes against the liberalism of the historicist analysis that I have argued for above.

Apart from my methodological disagreement with Laudan's focus on rationality, based in the position that I have argued for in previous sections of this chapter, I and many others see a crucial sticking point for Laudan's foundationless approach to rationalism. Laudan gives us an account of fluid, stable change; but how may he then found the claim that that change is *rationally based?* The historical examples that follow Laudan's presentation of his model argue for cases of rational change; and I will not here go into a detailed consideration of the virtues of the arguments he presents there.[313] But one might ask: on what basis does he find that they are rational? It would be difficult for Laudan to argue that the scientists' long-run agreement over developments bears the hallmark of rationality, for that puts the cart before the horse; for Kuhn could accept such agreement, but argue that it is arrived at through conversion.[314] What rarefied rationalist foundations of science Laudan may admit to might be tied to comments such as the following:

> Before a purposive action can qualify as rational, its central aims must be scrutinized—in ways outlined above, to see whether they satisfy the relevant constraints. But beyond demanding that our cognitive goals must reflect our best beliefs about what is and what is not possible, that our methods must stand in an appropriate relation to our goals, and that our implicit and explicit values must be synchronized, there is little more that the theory of rationality can demand.[315]



Laudan, then, backs away from entertaining the possibility of relativism at the last chance. He attempts to allow for the fluidity of change suggested by Kuhn by claiming that there are no rational foundations for scientific change, yet claiming that change is, nonetheless, rational. He denies that change is irrational on the basis of the stability of change afforded by his reticulated model, but he also appears to require a much reduced, but nonetheless solid, universal foundation for rationality.

***Normativity:*** Conceiving of science as a thread of changing elements through time also allows for a normative conception of scientific practice, but one which is historical, rather than absolute. Despite Feyerabend's arguments, which have received substantial damaging criticism[316], there appear to exist standards in science: certain activities are currently countenanced (e.g., experiment, fossil dating of strata), and certain are not allowed (e.g., appeals to the Bible, entrail reading). At any time, a specific variety of approved and proscribed methods constitutes the practice of a science, allowing for normative argument. The historicist, however, allows that the rules may change over time: uncountenanced developments (experiment, fossil dating) may enter the practice, approved methods (appeals to Bible) may change to proscribed ones. Of course, even non-relativists attempt to accommodate the history of science, and are 'relativist' concerning scientific methodology to the extent necessary to accommodate these changes: the changes are merely methodological, or means; they illustrate unchanging aspects of rationality applied in practice, to the foundationalist.

I would just like to point out here, however, that one does not require foundations to forestall an anarchistic analysis of rationality in the face of such changes. Feyerabend's anarchism may be limited: science may have an indefinite variety of moves available to it—in this sense, "anything goes" may hold true—but the full variety of moves may not be held to be available at a single time in a field's development: they might be partly restricted in their historical situations. Historical changes of norms might proceed through narrowly rational procedures, as Laudan argues, but broader aspects of society and practice might also have their effects: historical argument should decide this question. Thus, there may be rules to the game of science, but a relativist might allow that they are *only local rules*, which history suggests do not apply to science throughout the entirety of its history. The historical *change* of local rules consequently becomes a subject for research, and precisely the locus at which relativists and non-relativists engage in debate over whether those changes are rationally controlled, partly rationally and partly socially and historically controlled, or generally anarchistic.

I will not engage that debate here: my purpose is to point out that relativism need not lead immediately to a radical anarchism: rationality may act to constrain change in science, so why not society, and history also? Normativity in science may be historicized, but it may remain as a discernible feature of scientific debate nonetheless.

***Growth:*** I have, I hope, argued that a fair number of likely features of ordered activity in science could be accounted for on a relativist framework, and without appeal to a foundationalist epistemology. The toughest candidate is growth.

A weak concept of growth is certainly available to non-foundationalists. That is the conception of *progress* promoted by Kuhn in *The Structure of Scientific Revolutions,* and by Laudan in *Science and Values,* in which progress is conceived to be scientific development only insofar as it is development *to* the present *from* an earlier condition. Kuhn suggests this re-orientation of our conception of progress as follows:

> We are all deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance.
>
> But need there be such a goal? Can we not account for both science's existence and its success in terms of evolution from the community's state of knowledge at any give time? . . . If we can learn to substitute evolution-from-what-we-do-know for evolution-to-what-we-wish-to-know, a number of vexing problems may vanish in the process."[317]



This position tacitly denies that any significant conception of progress is available, and appea3rs a faulty interpretation of evolution, if the metaphor is taken literally. It also produces an extremely weak conception of growth, because it makes no reference to the relative *content* of theories, nor to *sophistication* of method.

Non-foundationalism can, however, lead to a stronger conception of growth than Kuhn provides: growth towards greater methodological sophistication and greater knowledge in science; but growth "towards" need not be precisely what the foundationalist conceives it to be. For the foundationalist, both aspects of growth might be measurable against absolute standards, either of rationality or of attainment of truth; such an account of growth could lead to a vigorous account of *progress* as well, wherein progressive development is seen as tracing a path of development that was intended by all of those travelling along the path, throughout the history of science. Such a conception of a unified goal of progress throughout history, however, might cut against the grain of historicists and relativists, who believe that they see variety in the goals pursued by important scientists through history. What greater than Kuhn's conception of progress is open to the historicist who does not wish to embrace progressivism? More is available, but a dialectical argument is necessary: an argument that shows historical improvement in relation to a standard, though not necessarily in relation to an absolute or a goal sought by all. Two possibilities for standards which nonetheless maintain a distance from theories of rationality suggest themselves.

One is contained in David Stump's very recent reply to charges that Laudan's and Shapere's projects border upon relativism.[318] Stump presents a persuasive case for the reasonableness of a non-foundational science by turning doubt back upon the skeptic: "Why", he asks, "can the demand to ground 'the overall epistemic credibility of science' not be met by looking at lots of specific reasons in limited domains?"[319] In other words, let piecemeal justification lie: that a universal story of progress through the ages may be difficult or impossible to construct does not imply that compelling reasons for change did not occur at every stage of development. Such an *incremental* account of rationality, however, does not allow for an overall analysis of the rationality of the whole of science, or of scientific knowledge: it does not allow a "cartesian" overview of the overall rationality of science, or of the validity of all knowledge claims (which Doppelt and Worrall suggest that Laudan and Shapere endorse). It only allows for justification from our present point of view. Stump replies:

> What does Worrall mean by "our present point of view?" . . . In the case we are discussing here, it is assumed that we have good reasons for our beliefs. Worrall apparently has an extremely high standard for rationality. He says that the methodological beliefs supported by Laudan and Shapere's model are not well-founded enough simply because they may change! To say that any belief which could change "just happens to be our point of view" implies that all of empirical science is subjective and that it is automatically not well enough justified. This line of argument leads me to wonder what Worrall has in mind that is stronger.[320]

Stump's reply strikes me as quite strong, though argument earlier in this chapter should suggest that I find his non-foundationalist tendencies ultimately unrepresentative of Laudan's view. Skeptical doubts about the grounding of scientific knowledge may lead us to deeper analyses of the character of knowledge, but it is quite conceivable that the foundationalist picture of knowledge that grounds the skeptic's worries is in fact false: science may have a different structure than the skeptic demands. One is *justified* in worrying seriously about putative knowledge that just happens to be one's point of view, with no pragmatic support in its favor; one is a fastidious skeptic, on the other hand, if one worries about the solidity of science in general. Even if we have problems articulating epistemological justification for science, we have pragmatic reasons for believing in some conception of scientific growth; that is, in believing science to be a good source, possibly the best source of knowledge concerning nature available.[321]



The second possibility for a stronger conception of growth arises out of Lakatosian retrospective history. Methodological growth could be considered to be present in science just to the extent that actual history of science recapitulates the best philosophical conception of progress that we have. Of course, Lakatos' retrospective history as constructed on his methodology of scientific research programmes is not very close to science's history: science does not manifest progress very well on his account. One might, however, develop a superior philosophical methodology which fits history better; and this is, in fact, the goal for what Lakatos calls his methodology of *historiographical* research programmes.[322] The approach to developing a conception of growth, then, might be considered to be *backwards* with respect to traditional conceptions of such a project. Growth (or progress) has traditionally been considered to be a feature *of epistemology* understood in philosophical terms, and manifest to a greater or lesser extent in actual science; on the Lakatosian conception, it might be considered instead to be an inherent characteristic *of scientific practice* (or praxis) that philosophical methodologies may expose to a greater extent as those philosophical methodologies improve. Growth, then, on this account is redefined as a property of science rather than epistemology; and so far, I am an orthodox Lakatosian, if this position is the one that Lakatos was suggesting.

How to improve philosophical methodology? Here I break with Lakatos, for reasons laid out in the second chapter of this work, and because, as I argued at the beginning of this chapter, I expect that a *rational* accounting is insufficient to explain scientific development: growth is underdetermined by rationality. Also, as I have argued in the past three chapters, *pace* Lakatos, history is likely to provide us with clues to improving our understanding of rationality, because of its independence from philosophy of science. This keeps Lakatos' historiographical programme, but turns Lakatos on his head in another area: for he argued that one must maintain a stable, prior conception of rationality not learnable from history in order to write history of science.

The conception of progress that I envision, then, includes something like a narrow, traditional conception of rationality, but it also includes analysis of traditionally social features as well. To explain scientific growth, a more comprehensive grasp of the world than a rationality theory provides is necessary, in order to account for alterations in what is considered to be 'rational' practice. What is rational in a given situation may be learned through a combination of intuition and historical research: through the hermeneutic process, a methodology I attempted to identify in the third chapter. *If* methodological growth occurs in science, then history, told in hermeneutically adequate fashions, and a hermeneutically adequate rational reconstruction, should be *convergent.* If they are not convergent, growth is in doubt.

*Once again:* I have not attempted to argue that science *has* the features continuity, order, stability, normativity, and growth; I have only indicated that they are available within the historicist framework. To decide whether those features are present, I would have to look at the history.

***The problem of football:*** The resulting conceptions of normativity and growth might present one more worry: if normativity in science is historicized, and growth—the external standard of normativity for development—is also detached from any absolute standard, then how are we to know how science will develop, or should develop in the future? Our understanding of development in the very near future appears reasonable, though not unexceptionable, but as the standards of science and perhaps of philosophy of science also may slowly change, there appears no reason to expect that what is currently considered good practice, progress, or growth will have a similar status in the far future.



How does a relativist know that the game of science will continue in its search for truth, and in its socially and technologically productive aspects? One might call this "the problem of football"[323]: What reason have we to believe that science's goals and methods won't shift to ones similar to those of football (or knitting, or advertising . . . )?

My answer is that, of course, one doesn't know this, and one shouldn't expect to, or even hope to. An essence, or an eternal demarcation criterion could anchor science, and stop it from wandering off in the direction of football. Perhaps science does have some goals now that it has continuously had from its earliest stages—perhaps a search for truth, or for an explanatory framework. Attempting to elucidate such features, and show their relation to science, I take to be an appropriate endeavor; I feel no great compulsion at this point, however, to construct an account of science which *explains* features which no-one before me, in my opinion, has articulated with great success.

What stability science has arises not from an essence, I expect, but from its *inertia*, the continuity of its historical development that I have argued for in the past few pages. As for the hope that science will continue to respond to the social needs of some social groups in its production of technology: science does, currently, respond to social needs to some extent, but certainly only to a very limited extent. But why shouldn't we expect that that relation arises only to the extent that a relation between scientific institutions and society's funding sources has been perceived as mutually advantageous?

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# ENDNOTES

1 See esp. Suppe's (1973) extended review of the project, and Salmon (1989).

2 The notable exception in this regard among logical empiricists, we will see in Chapter 4, is Hempel, who sees a role for philosophy of science at the foundation of all philosophy of history.

3 One might take logical empiricism to be the late development of logical positivism. There appears to be no widespread agreement on the distinction between positivist, which I take to be instrumentalist or phenomenalist, and empiricist epistemological principles. Feigl, for example, appears to consider empiricism to be a sort of positivism (Feigl (1949)); Reichenbach calls his field "logistic empiricism". I will attempt, where possible, to cite both early and late authors and note differences where they arise. The collective term "logicism" I take from McMullin (1970).

4 Reichenbach (1938) provides a clear statement of the formal-empirical divide. Nagel, in work after Quine's attack on the divide, takes a different tack: he appears to advocate what has more recently been called a *reliabilist* account of epistemology (1960), p. 13.

5 This is the role assumed in, for example, Carnap (1936-7)"Testability and meaning", *Phil. Sci.* 3, pp. 419-471 *&* 4, pp. 1-40. ; see pp. 420 f. Also Nagel (1961), p. 14.

6 Esp. Suppe (1973) for structure, Salmon (1989) for confirmation.

7 Reichenbach (1938), pp. 4-5.

8 See esp. Carnap (1928), Carnap (1936-7).

9 Nagel (1961), p. 14. For Nagel on axiomatization and rational reconstruction, see pp. 90ff.

10 An analysis is dubbed "external" if its critical basis or justification is not to be found within the compass of the subject matter that is being examined. This terminology will be used quite often throughout the thesis.

11 McMullin gives a similar analysis of the relation between philosophy and science:

One appeals to an intuition based on a shared language: "anyone who knows what 'confirms' means would say that . . . " The only experience involved is the unspecified experience required to allow one to come to use the term, 'confirms', correctly. This need not be an experience with the technicalities of scientific method, since the term has a much wider scope. To construct a "logic of confirmation", one has to articulate a set of intuitive principles of this kind and weave them into a single deductive system...It does not require any references to instances, any induction over (say) various types of confirming case. It is assumed that these would bear out theorems derived from the intuitive first principles; if they did not, we would simply conclude that they were being incorrectly applied to the instances in question, not that the principles were wrong. (1976), p. 586-7.

12 Carnap (1950), Ch. 1.

13 Consideration of the origins of a theory would fall into the *cognitive* assessment of a theory, which has nothing to do with its empirical value, its applicability to the facts of nature, according to Carnap (1939), p. 68.

14 McMullin (1970), pp. 57-8.

15 McMullin (1970), p. 55-7.

16 Carnap (1936), p. 67.

17 See, e.g., Suppe (1973), "Introduction".

18 A general account of the rise of successor theories as well as the fall of logical empiricism, and one to which I will refer frequently, is Suppe (1973).

19 See, e.g., Grice & Strawson (1956), "In Defense of a Dogma", *Phil. Rev.* 65, 141-58.

20 Quine (1951), p. 31.

21 Quine (1951), pp. 36-7.

22 Quine (1951), p. 43.

23 Quine (1968), pp. 82-3.

24 Ihaven't the space for a justifiably careful account of Nagel's analysis, so I will leave the exposition here. For Nagel's account, which uses the Newton-Galileo example of a case of "homogeneous reduction", and then goes on to discuss cases of "partial reduction", in which results disagree because of progressive theoretical development, see Nagel (1961), pp. 338f.



25 See Hempel and Oppenheim (1945/ 1965)

26 Feyerabend (1962), pp. 46-7, 80.

27 Feyerabend (1962), p. 34.

28 Feyerabend (1962), p. 47.

29 Both of these points are made by Feyerabend, pp. 48, 92.

30 Though I should note that Feyerabend does attempt to argue that "a formal account of reduction and explanation is impossible" (p. 28), a claim which perhaps goes beyond what he has shown.

31 See, for example, the solutions suggested in Suppe (1973). Feyerabend himself offers something of a 'syntactic' theory of explanation and prediction, which, he hopes, will avoid the problems accruing to meaning accounts by replacing reference with a criterion of success in action:

Now our theories, apart from being pictures of the world, are also instruments of prediction. And they are good instruments if the information they provide, taken together with information about initial conditions characterizing a certain observational domain $D_0$, would enable a robot, who has no sense organs, but who has this information built into himself (or herself), to react in this domain in exactly the same manner as sentient beings who, without knowledge of the theory, have been trained to find their way about $D_0$ and who are able to answer 'on the basis of observation,' many questions concerning their surroundings. (p. 94).

32 See Nagel (1961), pp. 345ff. It would be desirable to go from here into Nagel's account of non-homogeneous reductions—reductions in which it is not the case that "the laws of the secondary science employ no descriptive terms that are not also used with approximately the same meanings in the primary science" (p. 339 ). This line of argument would, however, take our discussion so deep into the details of the problems of logical empiricism that we would not emerge from them before very many pages are written. Feyerabend does, I believe, present some argument against non-homogeneous reduction as well: specifically, insofar as Nagel requires that the theories be formally axiomatized, Feyerabend's general attack on the theory-observation distinction applies.

33 The weak interpretation of incommensurability that I have outlined, I expect, needs a new name because it is a different and less radical thesis than any of those attributed to Kuhn, usually named conceptual, method-

ological, and observational incommensurability. The line of improvements to theory of language that I have in mind are represented in Kitcher (1983).

34 ***Please Note:*** Kuhn's views from *The Structure of Scientific Revolutions* will be introduced 'gradually' during this and the following chapter, in a way that might, at times, make the presentation odd sounding and circuitous to one who already has a grasp of Kuhn's views. The plan is as follows: In this section and Part II, §2.1 below, I present Kuhn's historically-based *criticisms* of logical empiricism and traditional approaches in the philosophy of science. Those criticisms I attempt to divorced as much as possible from Kuhn's constructive efforts at explaining the basis of the historical facts' existence. Those *explanations*, which detail the importance of scientific paradigms and revolutions in scientific practice, and the psychology of science, will be treated in Part II, §2.2 below, and the following chapter, respectively.

35 Kuhn (1962/1970), pp. 101f.

36 Theses regarding data loss and shifts in explanatory adequacy may be found in Doppelt (1978). They are distinctions that, I shall argue, are related at bottom: changes in standards and concepts that accompany scientific development, founded in paradigm changes, *result in both* data loss and explanatory loss. Doppelt (1978) has similar arguments concerning explanatory and data loss pp. 42f; the originality of my discussion I take to lie in highlighting the priority of paradigms as the source of these shifts (Part II, §2.2 below). Kitcher (1991) presents explanation and criticism of the explanatory loss thesis, §5.9, which I will consider briefly below.

37 Doppelt (1978), pp. 47ff.

38 Kuhn (1962/1970), pp. 104-5.

39 See Doppelt's account (1978), p. 48; and Kuhn (1962/1970), pp. 124-5.

40 Attempts at such work are made in Kitcher (1991); see next section.

41 Kuhn (1962/1970), p. 42.

42 Kuhn (1962/1970), p. 43.

43 Kitcher (1991), Ch. 4.

44 Kitcher (1991), §5.9.

45 One trouble I have with Kitcher's argument here is that it asserts that there has been progress without arguing for the presence of progressive shifts in history; that is, it is too strongly rooted in the context of justification to make any point regarding rational *development*. Though "we" may not regard natural motion as correct because of arguments we may know about composition, Kitcher hasn't actually claimed that such arguments, or anything like such arguments, were responsible for progressive development in the historical development of science. So Kuhn's argument in "The function of measurement", which is about historical development, is only partly answered by Kitcher's defense of his own conception of progress. Elsewhere in the work, Kitcher does appear to *concede* the point of data loss in another case-study, and he adjusts the logicist project accordingly (§ 4.5, mss. pp. 74-5). I fear that time compels me to leave out of my discussion detailed treatment of later work by Kitcher, Salmon, and others in the tradition that I consider to be recent extensions of logicism. Those works certainly present some relevant challenges to historicist criticism to which I have not begun to reply, excepting, perhaps, my short treatment of arguments concerning globality of aims, in this section and below.



46 Kuhn (1987), p. 10. I use this example from a later work of Kuhn's because it fits the example that Kitcher has brought up. The same point, regarding changes of aims, is brought up with regard to 19th century chemistry in Kuhn (1962/1970), pp. 132f.

47 Newton to Richard Bentley, 10 December 1692, in H. W. Turnbull, J.F. Scott, A. R. Hall, L. Tilling, eds., *The Correspondence of Isaac Newton,* 7 vols. Cambridge: Cambridge University Press, 1959-77, vol. 3, p. 233. Called to my attention by Gabbey (1990), p. 494.

48 See esp. the articles in Lindberg & Numbers (1986), Lindberg & Westman (1990).

49 See, e.g., Kitcher (1991), § 5.6, mss. p. 82.

50 In response to Laudan's views regarding shifting goals, Kitcher concludes: "...the history can be understood in terms of enduring commitments to the aim of achieving as much significant truth as one can get and shifting ideas about significance and what is possible for us to know." § 5.6, mss. p. 90.

51 Kuhn (1962/1970), p. 132.

52 Kuhn (1962/1970), p. 133.

53 See esp. Masterman (1970) for a review of the variety of conceptions in Kuhn's book.

54 Kuhn (1962/1970), p. 10.

55 Kuhn (1962/1970), p. 11.

56 Kuhn (1962/1970), pp. 23-4.
57 Kuhn (1962/1970), p. 42.
58 Kuhn (1962/1970), p. 103.
59 I use Goldman's term here because I take reliabilism to be the current strongest heir to Quine's naturalizing maneuver. See Goldman (1985).
60 Doppelt (1978).
61 Kuhn (1962/1970), p. 204.
62 Kuhn (1962/1970), p. 44.
63 Kuhn (1962/1970), p. 46.
64 Kuhn (1962/1970), p. 44; concerns Wittgenstein (1952), §§ 65ff..
65 The term 'historicism' has an involved history, maintaining at the same time, from different mouths, a variety of positive and polemical connotations (see Iggers (1973))
66 Burian (1977), p. 9.
67 This is a pretty robust phenomenon, not local to UCSD: I have tried the experiment with historians stationed at three other institutions (Berkeley, Toronto, Simon Fraser), and have received similar responses.
68 Kuhn (1962/1970), pp. 2f.
69 Kuhn (1957), p.212.
70 Kuhn (1957), p.226.
71 The questions that Kuhn has not addressed show his methodology even further. Several that would be at the focus of other historiographies are as follows:

a. *Why was Kepler led to address the problem of the geometric shape of the orbits?* —Many historians wish to examine the construction of a problem situation in a field, rather than holding that it is a matter internal to the field. On occasion, actors discern problem situations which others have not: Many historians are interested in exploring the construction of problem situations

b. *How, in detail, was the solution arrived at?* —The reader of Kuhn's book is not likely to have any experience calculating the geometry of planetary orbits, yet Kuhn passes over the detail of the discovery of elliptical orbits as though it were a trivial piece of mathematics, within the reach of any reader. Other authors would focus more precisely on the methods used to calculate and certify the discovery.



c. *How did Kepler convince other scientists that he had indeed found a good solution to the problem?*—This is part of the area of the social construction of knowledge. The problem is symmetrically linked to that of the denial of others' claims as false and irrational, in the case of Galileo's opponents above. How does Kepler establish to the community that his conclusions are true (or rationally arrived at), and how are the conclusions of Galileo's opponents determined to be false and irrationally arrived at?

Of course, we cannot expect (and would not want) any historiography to answer all questions about a historical event: Different authors will have different foci, depending on their concerns and the scope and *tempo* of their work.

72 Kuhn identifies himself as a member of this grouping: (1957), p. viii. Note that I am not attempting to characterize all of the tradition of the history of ideas here, but only the tradition as it is represented in recent history of science; and in a particular methodological approach that appears to me to be at the heart of intellectual history, but may not be *invariably* followed. Intellectual historians will, *on occasion,* lapse into other methodologies, but will *feature* the one I analyze here: see Shapin (1980) for a similar opinion. For more detail on the general state of affairs for the history of ideas, see King (1983), *The History of Ideas,* Barnes and Noble, Totowa, N.J.
73 Gilbert (1971) argues that similar assumptions are basic to, and suggest the basic faults of, intellectual history's methodology in the past; Toews (1980) presents similar criticisms. These authors and the general problems accruing to intellectual history and internal historiography will receive further treatment in Ch. 4.
74 Conant (1957), "Introduction", in Conant (ed.) *Harvard Case Histories in Experimental Science,* Harvard Press, p. vii.
75 See, e.g., Kuhn (1957), p. 227.
76 For the generally accepted scope of variation away from internalism in the history of ideas, see Shapin's (1980) critical article, which examines some inherent limitations of the internalist methodology. Shapin also considers the ways these shortcomings are subverted by practitioners in such moves as "footnote contextualism", in which the social element of historical change

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is mentioned, but also isolated from the main structure of the argument by its placement in a footnote.

77 Kuhn (1957), p. 37.

78 Kuhn (1957), p. 265.

79 Thanks to Robert Westman for pointing this aspect of *The Copernican Revolution* out to me.

80 So, for example, Kuhn writes that conceptual schemes provide "hints" and "guidance", driving forward the internalist problematic assumed in the history of ideas, and reflecting the philosopher's search for a logic of discovery (Kuhn (1962), p. 40). Kuhn also uses conceptual economy to explain agreement regarding developments among scientists: see quote which follows in the main body of this thesis, and accompanying footnote.

81 Kuhn (1957), pp. 226-7. Kuhn's reference to conceptual economy, and to external causes for dissent from other *intellectual* disciplines, is reminiscent of Shapin's discussion of footnote contextualism: it only takes prominence *in the final three pages of the discussion* of the Copernican Revolution's assimilation (see the footnote four previous to this one).

82 Kuhn (1962/70), Ch. 3-4.

83 One might even consider that Kuhn does have a supra-paradigmatic universal conception of scientific rationality, for he mentions "commitments without which no man is a scientist" (1962/70), p. 42), and the goal of solving anomalies. There are rational and irrational reasons for change; in Kuhn's opinion, however, rational universals serve only as a limited guide, and are usually not substantial enough to *determine* paradigm choice among a set of rivals for even the most rational and capable among practitioners (pp. 110ff.)

84 Kuhn (1962/1970), p. 150.

85 Doppelt (1978), p. 73, presents a similar point: Kuhn is allowing a wider latitude to rational scientific action than his positivist opponents, who argue that an account of rationality should be sufficient to determine scientific development.

86 Kuhn (1962/1970), pp. 151f. Kuhn does not support this generalization; it is, perhaps, prima facie plausible, but I believe that it is largely based in anecdote (particularly from Planck's *Autobiography)*, and contradicted by the statistical research of Frank Sulloway.



87 Kuhn (1962/1970), p. 93; Lakatos (1970), p.9.

88 "how an individual invents (or finds he has invented) a new way of giving order to data now all assembled—must here remain inscrutable and may be permanently so." (1962/70), p. 90).

89 Kuhn (1962/1970), p. 151; the final quote occurs on p.204 (a re-affirmation in the 1969 postscript).

90 Kuhn (1962/1970), pp. 96-7.

91 Kuhn (1962/1970), pp. 111ff.

92 For a general introduction to gestalt psychology, particularly as it applies to human *action* as well as perception, see Merleau-Ponty (1962), *The Phenomenology of Perception.*

93 Kuhn (1962/1970), pp. 116-7.

94 Lakatos (1970), p.9.

95 The above passages—especially the denial (in 1969) that good reasons constitute conversion and the claim that the process of conversion should be "explicated", and not explained further along the lines of rationality—and many others in *Structure* suggest that Kuhn did go through the "crisis" of rational historiography which I refer to. Kuhn has plenty to say concerning accusations regarding his conversion, most notably in Kuhn (1970), pp. 236ff., (1973), and (1983). The point is best served up as a challenge: to the extent that critics can find straightforward global conflict and local coherence among writings, different and separable positions should be admitted, unless the offending statements are mis-statements (and not mistakes).

96 Kuhn (1987), p. 20. This I take to be Kuhn's reply to Kitcher's criticism ((1983), p. 697): Kuhn acknowledges in this article that conceptual change occurs during normal science as well as revolutionary science (p.19), but suggests that for revolutionary change, a good proportion of the central concepts of a field must be unseated all at once. As a philosophical argument, such a statement is weak; on the other hand, the historical example backing the argument has more vigor.

97 Kuhn is, perhaps, more precise regarding incommensurability in his (1983), but that position is not as strong with respect to the argument discussed in the previous footnote.

98 E.g., (1962/1970), p. 148.

99 "If I am right, the central characteristic of scientific revolutions is that they alter the knowledge of nature that is intrinsic to the language itself and that is thus prior to anything quite describable as description or generalization, scientific or everyday....Violation or distortion of a previously unproblematic scientific language is the touchstone for revolutionary change." (Kuhn (1987), p. 21.)

100 Masterman (1970).

101 Kuhn (1974), pp. 297f.

102See esp. Kuhn (1984), (1987).

103 Kuhn (1974), pp. 318f.

104 Kuhn (1973), p. 329.

105Kuhn (1973), p. 331.

106 Kuhn (1973), p.325.

107 To be charitable to intellectual history at this point, perhaps we could consider the universal rationality "assumption" to be merely a "simplification".

108 Kuhn (1974), p. 319.

109 See Kuhn (1973), p. 321: Kuhn's choice of quotes from *Structure* indicates that he is concerning himself with issues in revolutionary science.

110 Kuhn (1973), p.333.

111 Kuhn (1973), pp. 334 ff. "Throughout the paper I have implicitly assumed that, whatever their initial source, the criteria or values deployed in theory choice are fixed once and for all, unaffected by their participation in transitions from one theory to another. Roughly speaking, but only very roughly, I take that to be the case....But little knowledge of history is required to suggest that both the application of these values and, more obviously, the relative weights attached to them have varied markedly with time and also with the field of application." (p. 335).

112 Apparently this is the very thesis that Doppelt attempted to defend Kuhn's earlier position *against* in his (1978), pp. 37ff.

113 The arguments in philosophy of language that Kuhn attempts to bring to bear on this problem are quite out of my area of expertise—philosophy of language in general is rather opaque to me—so what follows may be stated in an unorthodox fashion. I hope the point is clear enough, however.

114 For Feyerabend, see my discussion in Chapter 1. For Shapere and Scheffler, see the digest in Doppelt (1978); for Kitcher, see (1983).



115 The primary criterion used in comparing scientific theories, "is the criterion of predictive success, and it is seen not at all to involve reference to the *meanings* of the reactions [of experimenters judging empirical adequacy]". (Feyerabend (1962), p. 94)

116 "It is therefore impossible, for *quantitative reasons*, to establish a deductive relationship between T and T' [the reducing and reduced theories], or even to make T and T' compatible." Feyerabend (1962), p. 47.

117 Feyerabend (1962), p. 47.

118 Kitcher presents a similar solution utilizing very different apparatus, in order to allow that scientists can judge the empirical content of competing theories: he argues for a more elaborate theory of meaning that allows for distinguishing between correct and erroneous uses of a referring term, both of which add together to make the term's "reference potential":
"...my articulation of the notion of conceptual incommensurability shows why this phenomenon is epistemologically innocuous. Even when matters go awry, *even when the assumed connections among the events in the reference potential of an expression are faulty, later workers can recognize that this is so*, and, by attending to context, they can disentangle what their predecessors are saying." Kitcher (1983), pp. 698-8: (my italics indicate the area of similarity between Kitcher and Feyerabend).

119 See especially the development of analysis of analogies in Hesse (1974), Nersessian (1987), and a variation of the approach in Holton (1980).

120 I have, indeed, produced an extremely abbreviated caricature of Popper's view here, since Popper is only to be considered to the extent that Lakatos' view grows out of his. For more detail on the ideas of content and growth, see Popper (197x), ch. 10. For the basics of falsification, see (1959), ch. 4.

121 For example: A. G. Greenwald *et al.* (1986), "Under What Conditions Does Theory Obstruct Research Progress?" *Psych. Rev.* 93, 216-229.

122 Lakatos (1970), pp.17-27.

123 Lakatos (1970), pp. 14-15, 21-22.

124 Lakatos (1970), pp. 17-18. A similar problem lies in wait for Lakatos' sophisticated falsificationist, on p. 26.

125 Lakatos (1970), p. 9.

126 Lakatos (1970), p. 30.

127 Lakatos (1970), p. 46.

128 Lakatos (1970), p. 68 (italicized in original).
129 Lakatos (1970), p. 89 (italicized in original).
130Lakatos (1970), p. 69.
131 Lakatos (1970), p. 71-2.
132 Lakatos (1971), p. 119.
133 Palmer (unpublished), "What is Lakatos' internal history?"
134 Lakatos (1971), p. 117.
135 "...especially if the defeated programme is a young. fast-developing programme, and if we decide to give sufficient credit to its 'pre-scientific' success, allegedly crucial experiments dissolve one after the other in the wake of its forward surge. Even if the defeated programme is an old, established and 'tired' programme, near its 'natural saturation point', it may continue to resist for a long time and hold out with ingenious, content-increasing innovations even if these are unrewarded with empirical success. It is very difficult to defeat a research programme supported by talented, imaginative scientists." (1970), p. 72.
136 Lakatos (1970), pp. 53-5.
137
138 An atomic mass unit is (I believe) defined as the mass of one free hydrogen atom, or is approximately the mass of one proton. The history, or rational reconstruction, that *I* have constructed just above is meant to clarify the problem-situation that Lakatos is describing, by giving an example and an account of further development. I expect that Lakatos would agree with it, as the example is a 'textbook' account of a case of isotope theory, and the development is alluded to later in Lakatos' passage.
139 Lakatos (1970), p. 53. The square brackets in the previous sentence indicate my addition for clarification: in the footnote, Lakatos claims that the actual historical character lied and claimed that Chlorine weighed 36 atomic units.
140 "The champions of "[Prout's"] theory, therefore, embarked on a major venture . . . " (p. 53).
141 Lakatos (1970), p. 61; (1971), p. 119.
142 Lakatos (1970), pp. 70-71; (1971), p. 117. 143 See also, for example, (1970), p.69, where Lakatos gives criteria for eliminating a research programme, and the footnote retraction: "Nevertheless there is something to be said for at least *some* people sticking to a research programme until it reaches its 'saturation point' . . . " Even Lakatos' invective against *ad hoc* adjustment can be questioned. One person's *ad hoc* may be another's revolution, a re-ordering of the central theoretical tenets of a discipline; thus, what is the support accruing to the Copernican shift? The advantage to the newer program in early years may have been only in relation to Lakatos' 'novel fact' criterion: But I suspect that, even with Zahar's improvements, the novel fact has a retrospective interpretive element not available to the working scientist.



144 Lakatos (1971), p.103, text & fn.1; p. 117; p. 137.
145 Lakatos (1971), p. 103; Lakatos (1970), pp. 10, 92.
146 Lakatos (1970), p. 8.
147 Portions of this sub-section are gathered, almost intact, from my (unpublished).
148 Such checking against the facts, for example, is the main purpose of Lakatos & Zahar (1973).
149 Lakatos (1971), pp. 124, 131.
150 If anecdotal evidence is wanted, consider Feynman's popular exposition of quantum electrodynamics, *QED* (1985, Princeton, USA). Feynman unabashedly states his ignorance of history, and advocacy of 'textbook history': "By the way, what I have just outlined is what I call a 'physicist's history of physics,' which is never correct. What I am telling you is a sort of conventionalized myth-story that the physicists tell to their students, and those students tell to their students, and is not necessarily related to the actual historical development, which I do not really know!" (p. 6) In the book, Feynman begins the European history of optics with Newton, excluding Descartes, as is the convention. See also Feyerabend (1976), pp. 208-9.
151 C.f. Lakatos (1971a), p. 126: "That in their choice of problems the greatest scientists 'uncritically' ignore anomalies...offers, at least on our metacriterion, a further falsification of Popper's methodology." Lakatos may remedy the problems of ignorance of the details of history and variety of opinion with the assertion that there is "considerable agreement over the last two centuries concerning *single* achievements"; and perhaps some of the élite along the path to the present did know the arguments—e.g., participants, such as Newton. I am not convinced that the unity which

Lakatos considers present among scientists is really there: It is a reasonably safe bet that the "best gambits" are not remembered because they exhibited good method, since later scientists remember so little of that method. I argue for reasons why great achievements are remembered in the following paragraph of the main body of this thesis.

152 Such a conception does indeed provide a retrospective or 'whiggish' historical story, since events currently out of favor with the élite, or not in keeping with a methodology, are to be considered non-progressive. Once this is understood, however, it should present no problem, since internal history is not meant to assess the actions of individuals: It is intended to assess contributions to growth retrospectively considered. Thus, charges that Lakatos provides for poor historical explanation in his internal history because he presents criticism according to methodologies not available to actors, and that he allows the 'winners' to retrospectively define what is rational action, miss the point of the enterprise: History with a regard for the actors is external.

153 Lakatos (1971), p. 131.

154 Of course, a fuller theory of the content and growth of science which would include more aspects of the practice of science, such as experiment, questions explored, etc. is also conceivable. For fuller conceptions of scientific practice, see Kitcher (forthcoming), Laudan (1984).

155 Garber ((1986) pp. 95-6) argues that Lakatos' internal history does not provide the tools necessary for appraisal of historical actors' actions. The position developed here suggests that Garber is correct, since that is not the purpose of internal history; however, the *related* approach mentioned just above does fulfill Garber's requirement, and appears to be essentially the same as the project which he suggests, of a normative "internal history grounded in an historical conception of rational scientific procedure."

156 Lakatos (1971), p. 138.

157 See especially Agassi (1963). "Towards an Historiography of Science", *History and Theory*, Beiheft 2; Rudwick (1985) Ch. 16, Latour (1987).

158 Laudan (1977), p.163; Lakatos (1971), pp. 125, 131-2. Lakatos' analogue is somewhat more sophisticated, in that the judgement of the scientific élite may change when those individuals are presented with rational reconstructions.



159 Apple (1989). "Patenting University Research: Harry Steenbock and the Wisconsin Alumni Research Fund", pp.375-394. Smith (1989). "The Cambridge Network in Action: The discovery of Neptune", pp. 395-422. Newman (1989). "Technology and Alchemical Debate in the Late Middle Ages", pp. 423-445. All in *Isis,* 80.

160 Immediately the problem of the wide variety of real and possible positions which historians are likely to take regarding methodology confronts us. I would not be surprised if there were historians who explicitly claimed that they had no interest in facts of certain kinds, or facts at certain levels of ontology: marxist historians, for example. What I hope to sketch is a fictional program of methodology which is likely to be largely agreed upon by a large number of historians: a defensible position independent of philosophy. The shorthand expression "the historian" as I use it here expresses an attempt to sketch such a position, not an attempt to present tenets that all members of the field of history would agree to. The expression "the philosopher", however, is very specific as it is used here: it is meant to denote positions held generally by a great proportion of theorists of scientific rationality, and specifically by both Laudan and Lakatos.

161 Lakatos (1971), p. 102.

162 Lakatos (1971), p. 102.

163 Lakatos (1971), p. 114.

164 Lakatos (1971), p. 131. Note that on Lakatos' metamethodology the élite may give up their original judgments of what events are the best science as a result of examination of various reconstructions; presumably, it is allowed that they may change their mind on a few cases if a reconstruction fits most but not all cases. The opinion of the élite, then, is in a dialectical relation with historical reconstruction as well.

165 Lakatos (1971), p. 132.

166 Lakatos (1971), pp. 120-1.

167 Lakatos (1971), p. 121.

168 On the subject of whig history, see Ch. 1 and especially Ch. 5; Butterfield (1931), Wilson and Ashplant (1988).

169 Lakatos (1971), p. 102. Here some of the Hegelian element of Lakatos' treatment of history which was mentioned in chapter I of this work is apparent.

170 On sociology, see, e.g., footnote 5, p.120 of Lakatos (1971); on psychology, see, e.g., p. 121.

171 On the autonomy and autocthonous nature of the development of separate research programs, see Lakatos (1970), pp. 51-3.

172 On the other hand, in places he appears to suggest the opposite, that the battles between competing programs have great importance, since each provides challenges to the other, and often challenges that will eventually upset one program entirely. Presumably, the reason for Lakatos' contesting the significance of interaction has to do with his thesis that a research program dies not because of challenges from the outside, but because of internal problems, specifically a collapsed positive heuristic.

173 Lakatos (1971), p. 120. Lakatos (1970), p.53.

174 Lakatos (1971), p. 121.

175 Lakatos (1971), pp. 103-7.

176Lakatos' presentation of this weakness in his approach actually serves to suggest an alternative that would allow much more history to be accommodated as internal, and explanatory for the growth of scientific knowledge. That alternative would be to allow history to be written from a multitude of philosophical standpoints; that is, from any historiographical research program one chooses. Thus, Kepler would fall under inductivist historiography, Copernicus under conventionalist...etc. Since no historiographical research program is likely to be adequate to the whole of history, it seems only a matter of taste that Lakatos chooses to uphold consistency by restricting viewpoints, and in so doing reduce the amount of science accommodated (the content of explanation). This makes the concept of the growth of objective knowledge rather unruly because of inconsistency; but again, this inconsistency is only solved by Lakatos in the first place by assuming that the actual rationality of history and of actors in history is flawed: that history, instead of theory, is inconsistent (Lakatos admits this much, p.131).

177For information on this debate, see Franks (1981).

178 Stephen Jay Gould's many arguments regarding the virtues of past 'dead' theory are an attempt to present just such revised history, though for persuasive reasons in live scientific debate, rather than rational reconstruction. If his efforts are successful, then they will find their place in 'official' inductive history. See Gould (1977), (1989).



179 E.g., Lakatos (1971), p.116: "All these examples show how the methodology of scientific research programmes turns many problems which had been *external* problems for other historiographies into internal ones. But occasionally the borderline is moved in the opposite direction."

180 Agassi (1963), p. 44.

181 Agassi (1963), p.5.

182 The basic value judgments of the scientific élite provide the anchor points at which Lakatos' theory is attached to history, but his historiographical research programme allows some principled variation from those judgments: See Lakatos (1971), pp. 131-132.

183 Lakatos (1971), p. 121, footnote 1. Note that Lakatos' application of the distinction differs from Laudan's, in a way which will not allow Lakatos to be open to the charges aimed at Laudan presented towards the end of this discussion.

184 Laudan (1977), p.160.

185 Laudan (1977), p.161.

186 Laudan (1977), p.165. Laudan goes on to say that "Many historians will doubtless agree that this is the ideal . . . ", but allows that historians cannot often use these philosophical theories because they are at present not well-enough developed to justify a historian's attention.

187 James (1896). "The Will to Believe".

188 See, for example, the Zahar-Worrall criterion, a Lakatosian thesis, as it is developed in Worrall (1989) Also see the volume of essays *Progress and Rationality in Science*, Radnitzky and Andersson eds., (1978).

189 Laudan (1977), p. 158.

190 Laudan (1977), p. 165.

191 It is difficult to determine whether Laudan does believe in the privileged nature of PI's: of course they can be differentiated from $HOS_2$ because they are beliefs, not writing, and they are not dependent on HOS either, since, as Laudan says, $HOS_2$ can be dispensed with in philosophy, whereas PI's cannot. Are PI's, then, equivalent in nature, but not persuasive force or quality, to the beliefs responsible for the historian's writing $HOS_2$? Laudan's

use of the words "intuition", and "archetypal" (p.162) to describe them suggest otherwise.

192 See the previous chapter of this thesis, where Shapin & Schaffer's expression for the rhetorical stance of Boyle's pneumatics texts is applied to the historian's art. This stricture might be weakened: a "textbook" understanding of the history of science might suffice, such as how a current practicing science might know, for example, how to derive the kinetic theory of heat from kinematic principles. Such a knowledge would not, however, be likely to available to most scientifically educated people in the extreme breadth of field which Lakatos suggests is covered in the pre-analytic intuitions about science, which covers physics and chemistry, and medicine, and geology.

193 For paradigm events of scientific rationality which may instead be myths, see, for example, Worrall's de-bunking of the myth of the persuasive power of Fresnel's bright spot prediction from Poisson's wave theory of light in (1989), "Fresnel, Poisson, and the White Spot"; see also Feyerabend's attacks on many fronts of myths regarding Galileo's ability to view objects through his telescope and convince others that what he saw was of theoretical import in *Against Method*, (1975), especially ch. 9-11; see also the reappraisal of the debates between Hobbes and Locke in Shapin and Schaffer (1985), *Leviathan and the Air Pump*.

194 On evidence for the existence of a 'vividness heuristic'—that information is often weighted in proportion to its vividness, rather than its relevance to the task at hand—which greatly affects human psychology, see Nisbett and Ross, (1976), ch.3.

195 Lakatos claims that historians perform this function from the standpoints of other philosophical-historiographical research programs as he advocates their doing it from his own:

Some historians look for the discovery of hard facts, inductive generalizations, others for bold theories and crucial negative experiments, yet others for great simplifications, or for progressive and degenerating problemshifts; all of them have *some* theoretical 'bias'. This bias, of course, may be obscured by an eclectic variation of theories or by theoretical confusion: but neither eclecticism nor confusion amounts to an atheoretical outlook. Lakatos (1971), p.120.



196 Rudwick, (1985), pp. xxii-xxiii.

197 Cf. Gingerich & Westman (1988), P. Redondi (1985).

198 Gould, (1977).

199 Shapin and Shaffer, (1985), pp.3-4.

200 E.g., Gould, (1989).

201 E.g., Pietro Redondi, (1983).

202 E.g., Carlo Ginzburg, (1985/1989), which is an attempt to show how an obscure cosmogonical theory was the product of a sixteenth century miller's culture.

203 Kuhn (1968) p. 110.

204 Agassi presents a prescription similar to this in his (1963), pp.51-4.

205 Laudan (1977), p. 165.

206 I do not wish to fall into Laudan's error of speaking for 'the historians' here; *unlike* Laudan, I will go on (in chapters 4 and 5) to back up my claims about what some historians wish to accomplish with their work: I will consider the goals of some historians, by examining some writings of several historians.

207 For the historical role, see Laudan (1977), pp.164-5, Lakatos (1971), p. 102; for the epistemological role, see Laudan p. 161, Lakatos p.102; for the methodological role, see Laudan p. 6, Lakatos p. 103.

208 On the other hand, several authors have argued rather persuasively for the real existence of changes in aims throughout the history of science: see Laudan (1984), pp. 83-5; Shapere (1984), pp. 208-214.

209 An alternative account of the relation of reconstruction, history, and rationality not discussed here is presented by Nickles in his (1985), where he argues that scientists themselves use reconstructibility, or cleaned-up history, as justification and practically effective argument in championing their programmes. Thus, science may proceed irrationally in the context of discovery, but be rationally 'repaired' in the context of justification.

210 Giere (1973), pp. 282-97.

211 Giere (1973), p.294.

212 Laudan (1987), pp. 19-31; p. 21.

213 Laudan (1987), p.27.

214 Note that at this point of the discussion, we are no longer considering accounts of scientific rationality which are retrospectively defined, i.e.,

those which do not attempt to explain why history happened as it did, in terms of actors' intentions and activities, but show how past developments in science support current research programmes.

215 *Gentlemen of Science.* Oxford, England, 1981.

216 *The Wittich Connection.* American Philosophical Society, 1988.

217 *Never at Rest* (1985).

218 Hempel (1942), "The function of general laws in history", sections 1, 2.1, *Journal of Philosophy*. Reprinted in Hempel (1965).

219 Hempel and Oppenheim (1948), "Studies in the logic of explanation", *Philosophy of Science* 15, pp. 135-75. Reprinted in Hempel (1965).

220 Hempel (1942), section 2.1.

221Note that this is also only one of two kinds of scientific explanation that Hempel identifies: in addition there is inductive-statistical explanation, which we will not consider in detail here: see Hempel & Oppenheim (1948).

22 Hempel (1942), section 1.

223 Hempel (1942), sections 5.2, 6.

224 See, e.g., Dray (1950) *Laws and Explanation in History.* See esp. pp. 19 & 49.

225 Laudan, Laudan, & Donovan (1988), pp. 378-9.

226 Laudan, Laudan, & Donovan (1988), p. 12.

227 Laudan (1987), "Progress or Rationality?", *American Philosophical Quarterly,* v. 24, p. 25.

228 Laudan (1987), "Progress or rationality? The prospects for normative naturalism," *American Philosophical Quarterly*, v. 24, p. 21.

229 I take the rules to be implicit in Conant, for example; Kuhn makes them explicit in writing regarding an "algorithm of theory choice" in "Objectivity, value-judgment and theory choice", esp. p. p. 330-1 in Kuhn (1977).

230 Worrall (1989), "Fresnel, Poisson, and the white spot", in Gooding *et al.* (1989) *The Uses of Experiment.* C U P, Great Britain, p. 147.

231 See, e.g., Laudan (1987), p. 25.

232 the philosophical category of explanation that arises out of the practice of prosopographic history I will call *prosopographic-nomological* explanation, because of its similarity to Hempel's deductive-nomological explanation—a similarity that will become evident shortly.



233 Stone (1971), "Prosopography", *Daedalus*, v. 100, p. 46.

234 Braudel, (1966), p. 18.

235 Braudel, (1966), p. 23.

236 Braudel (1966), p. 20.

237 Braudel (1966), p. 21.

238 The distinction that I make here between understanding and explanation is as follows: understanding is personal, explanation is interpersonal and persuasive. Understanding is a personal act, of comprehension; explanation is an interpersonal act which concerns the engagement of a rhetorical path or skill, with the purpose of bringing about understanding in the individual who hears the explanation. What sorts of processes constitute historical understanding and historical explanation are the matter at issue in parts II and III of this chapter respectively. I have brought this up to steer readers away from the idea that I use "understanding" and "explanation" to mean humanistic and social-scientific explanation, and natural scientific explanation respectively (which is the standard English terminology for the German *Verstehen/Erklarung* distinction).

239 Collingwood (1946), p. 215.

240 Collingwood (1946), p. 219.

241C.f. the discussion pp. 282-3, in which Collingwood explains that reenactment is necessary for any historical knowledge, including knowledge of facts (c.f., p. 303). Collingwood takes this position, I believe, because he maintains that historical knowledge *can* only be of the intellectual, the thinkable: a bare *fact* such as "the Theodosian code was . . . ", is not a historical fact; "the Theodesian code affected people thus . . . " *is* a historical fact for Collingwood.

242 A term used by Biersack (1989), "Local knowledge, local history: Geertz and beyond", in Hunt, ed., (1989), *The New Cultural History,* U. of California, USA. (Why I prefer this to others such as Hempel's term will become clear later in the chapter.)

243 Hempel (1942), sect.5.1.

244 See, for instance, Morrel and Thackray, *Gentlemen of Science* (1981) Oxford, Clarendon; Price (1963), *Little Science, Big Science*, New York, Columbia.

245 Or what have you. It appears unimportant to me that some historians will be interested in watering down this claim, diluting it to a belief in "critical understanding" of history, or instead something else, because the truth is not accessible, or an ideal construction; equivalently idealistic and anti-metaphysical positions can also be found in the philosophy of science, and are motivated by similar reasons: parallels could, I believe, be drawn regardless of the reasoned positions one chooses to take.

246 Once again, the relation between opinions of members of subject and meta-discipline might be raised at this point. I will forge ahead without addressing the issue further now—in favor of the criterion I have proposed, I will point out that, at least, it has the virtue of a clear empirical basis.

247 Hempel (1942), pp. 239-40.

248 Hempel (1942), pp. 240, 236.

249 It is, in fact, used in a similar way as a heuristic device in scientific circles as well: see, e.g., Barbara McClintock's intimate camaraderie with corn and chromosomes in Keller (1985), *Reflections on Gender and Science,* pp. 164-5.

250 (If "understanding" and "explanation" are not clear as I use them here, see the extended footnote on these terms at the beginning of this section).

251"Sympathetic response" I have used to suggest a parallel with physiology, not romance: the method might alternatively be compared to harmonic excitation in physics.

252 Quoted in Hall (1983), "On Whiggism", *History of Science* 21, 45-59.

253 Collingwood (1946), p. 214.

254 Collingwood (1946), pp. 214, 215.

255 Or perhaps that of a midwife, like Socrates.

256 Hempel (1942), p. 240.

257 More on demarcation from psychology: since the norm in much of psychology *is* clearly the framing and using of laws, (though perhaps not precisely D-N explanation), and psychology is pursued for the purpose of manipulation as well as the purpose of understanding, the disciplines have different goals. Further, because of these goals, psychology might be conceived of behavioristically—in terms of the 'outside' of a mental event—but history is re-thinking, and so necessarily an approach from the 'inside'.

258 Further analysis of the principles grounding evoking, pointing, and judicious editing—the study of *rhetoric*—I leave aside at this point, for my purpose here is more to differentiate projects than to explicate them thoroughly.



259 Passages above should indicate that I am reserving the term 'goals' to designate the immediate goals *within* the enterprise of history: what the historian is trying to do. The term 'purposes' concerns the external reasons motivating the pursuit of history in the first place: in asking about the purpose of history, one considers *why* anyone would entertain an enterprise with those goals, why those goals are wanted at all.

260 Dilthey, *Gesammelte Schriften* v. 5, translated in Howard (1983).

261 This assumption is for Heidegger precisely the assumption of the unity of the existential constitution of *dasein*. A large part of *Being and Time* concerns an argument which is an attempt to provide empirical support for the validity of the assumption.

262 Heidegger (1927/1962) pp. 194-5.

263 Ginzburg (1985/1989).

264 Ginzburg (1985/1989) p. 124

265 Geertz (1973). I hope my discussion below indicates that 'semiotic' and 'sympathetic' explanation are essentially the same: both concern explanation in terms of *meaning*, the former term suggesting that meaning is key, the latter reminding us of the method by which the understanding is achieved.

266 Geertz (1973), p. 26.

267 In Heidegger, this component of hermeneutic understanding is the fore-structure of interpretation: see pp. 192-4.

268 Geertz (1973), p. 9.

269 Geertz (1973), p. 14.

270 Braudel (1966/1972).

271 Hunt (1989b).

272 To expand on the comparison to Geertz and his article "Deep play": both authors attempt to present their analyses of the broadest of social relations (church and inquisition to villagers, community relations in Balinese village) as reflections of individual relations (inquisitors vs. Menocchio, cockfighters in Bali). The process of the trial, and the process of the cock-

fight, are intended to provide significant reflections of their respective societies in the rituals and interactions that they present.

273 Crapanzano, (1983).

274 Chartier, (1988).

275 Biersack (1988).

276 See especially "On the relation of history and philosophy" and "Time to understand: the frustrated intellectuals", reprinted in (1988b).

277 See especially *Clues, Myths, and Historical Method,* and Ginzburg & Poni, (1981).

278 Ginzburg & Poni (1981), p.135.

279 Note that by 'intellectual history' I intend to mark out a specific historical methodology—an intellectual internalist dynamic and a universal rationality assumption, as discussed early in Chapter 2—and not particular writings of any actual historians. Intellectual history, as opposed to other kinds discussed in this thesis, tends to be a methodology present to greater or lesser *degrees* in the writings of those called intellectual historians and of those that go by other names.

280 Such a sentiment, I believe, was expressed by Rachel Laudan in the title of her presentation at the History of Science Society meeting in Seattle, in October of 1990. The title, "More than a repository for anecdote and chronology", referred to the methodology utilized in the compilation of *Scrutinizing Science* (Laudan, R. Laudan, and Donovan (1987)).

281 This quote I have pulled from memory; I cannot recall where in Nietzsche's work it is written. The argument that follows it is representative of much of Nietzsche's work, and most particularly *Beyond Good and Evil* (first part), and *The Genealogy of Morals.*

282 I call this an "example" rather than a case-study because it should not be considered of central importance to my philosophical argument, and can only receive short development here—I work from only two books, one by a historian, and one by a pair of philosophers of Bayesian reasoning, and several articles by historians. I will restrict the development in the main body of the text to accounts by Daston and Schneider; other authors will be relegated to footnotes, to maintain order and brevity. I have done only a little historical work here because this thesis is primarily concerned with philosophical methodology and historiography of science; and with respect to history writing, that is a logically prior topic.



283 Daston (1987b), p. 297. Degrees of reasonable belief can, I expect, be related to another common formulation of the goal of classical probability, the principle of indifference, by the assignment of ratios in the scenario of gambling: where one might be indifferent to a bet on two mutually exclusive outcomes, the odds accepted would represent degrees of reasonable belief in each of the possibilities, knowledge of the outcome pending. Equating different formulations of the goals of classical probability was not so easily accomplished for the classical probabilists, however: see Jorland (1987).

284 Daston (1987) pp. xi, 368; Schneider pp. 197, 203. Similar indications of a lull in interest are provided in Jorland (1987). Poinsot's remark was published in 1836.

285 Howson & Urbach (1989), pp. 45ff., acknowledges Classical probability as the ancestral trunk of Bayesian reasoning.

286 For a more detailed reconstruction of Bernoulli's law, see Howson & Urbach (1989) pp. 34 ff.

287 See Schneider (1987), pp. 195-8, where this response to Poisson and the Classical approach by Charles Gouraud is quoted and discussed in detail. Other, similar views and reasons from Poinsot, in 1837, and John Stuart Mill are quoted in Daston (1987).

288 For a rigorously internalist account of classical probability's problems, though not its failure, see Howson & Urbach (1989), pp. 45ff. Some of the features there discussed may have had a role, but the account, unfortunately, is too much of a rational reconstruction of problems to allow me to pull a historical story of causes of failure, fixed to historical events around or before the time of 1840. Given more time, and if one were available, I would wish to include such an account to deepen the internalist story, and to check the importance of an external account. The mere existence of intractable intellectual problems, however, was certainly not sufficient cause to block the classical probabilists' research programme, as Jorland's (1987) history of over 100 years of connected, lively debate of the St. Petersburg problem shows.

289 See Daston's survey of other historians' focus on the rise of the new approach, pp. 188-90, and an account of one contributing factor to the new approach in developments of psychological theory, Ch. 4.

290 Schneider (1987), presents an account of this branching of later 19th century approaches.

291 Shapin calls this the area of "footnote contextualism" for intellectual history; see his (1980).

292 Daston (1987), Ch. 2.

293 See Jorland (1987), Daston (1987) Ch. 2-3.

294 Daston (1987), Epilogue.

295 Daston (1987), p. 108. for an account of the rise of the objective approach in the social sciences, see Marcus and Fisher (1987).

296 This thesis of Daston's, because profoundly broad, could use more defense in her book; but see 113 ff, 183 ff.

297 Urbach (1987), p. 15.

298 Urbach (1987), p. 178-80.

299 Bacon, *The Plan of the Great Instauration,* in Urbach (1987), p. 36.

300 Bacon, *Novum Organum I,* in Urbach (1987), p. 27.

301 Bacon, *Novum Organum I,* in Urbach (1987), p. 28: "we must also examine and try whether the axioms so established be framed to the measure of those particulars only from which it is derived, or whether it be larger and wider."

30 Urbach (1987), pp. 32-4, 60-3.

303 I attempted to give more specific reasons why historians are likely to come up with different hypotheses regarding causes of historical change in the final part of Ch. 4.

304 Feyerabend (1975).

305 Wittgenstein (1952), § 68.

306 Doppelt (1986), p. 225.

307 Wittgenstein (1952), §§65, 67.

308 Wittgenstein (1952), § 67.

309 Laudan (1984), p. 26. This hierarchical model I take to relate closely to the foundationalism which has been at one of our focuses: epistemological goals reside at the axiological level.

310 Laudan (1984), pp. 63-5.



311 Laudan (1984), pp. 68-78. Historical cases to seal the argument begin p. 81.

312 Kitcher (1986 unpublished) presents an account of piecemeal change similar to Laudan's, including a finer account of a scientific "practice", a term I have used here for convenience which is not in Laudan's text. Kitcher's account is in many ways much closer to my ideal, in that it allows for a much broader analysis of change; it differs in application from my ideal, as Laudan's does, in the express promotion of analysis of rationality and progress, rather than development and growth. Shapere (1980) also introduces piecemeal change, but provides no detailed model.

313 One quick note on the issue: Laudan's argument against Kuhn and for his reticulated model of pp. 81-7 does not suffice to prove the point for his model. Though he argues well for the plausibility of aim and method changes in non-revolutionary normal science, and so causes problems for Kuhn's approach, Laudan does not attempt to apply his reticulated model in any detail to situations that Kuhn would consider to be revolutionary (Kitcher (1991) shows more promise in this area).

314 See especially Doppelt (1986) for a complete presentation of the charge that Laudan drives himself to a foundational conception of rationality.

315 Laudan (1984), p. 64.; this quote was pointed out to me by McMullin (1988) pp. 14 f.

316 See esp. McMullin (1970).

317 Kuhn (1962/1970), p. 171.

318 Stump (1991), "Fallibilism, Naturalism, and the Traditional Requirements for Knowledge", read at APA Pacific Division, 1991; to appear in *SHPS.* A response was presented at the same colloquium by Doppelt, "Relativism and the New Objectivity in the Philosophy of Science."

319 Stump (1991), p. 15.

320 Stump (1991), p. 16.

321In his paper, Stump is attempting to defend *rationalist* nonfoundationalism. Doppelt, in his reply, suggests that a rational account will inevitably be inadequate: a sociological account will be necessary, for "good reasons" are not enough to explain change. I hold a view similar to Doppelt's, as other arguments this chapter should indicate.

322 Lakatos (1971), pp. 134ff.

323 See discussion in Nickles (1980), pp. 102 ff.