Chapter 2: Science, Philosophy, and the Subject at Hand – Some Methods

One central issue in this work is to examine the interrelationships, and possible distinctions, between science and philosophy in the context of early nineteenth century British work on the capacities of the human mind. It will be critical in this endeavor to avoid essentialism. Falling into traps such as claiming that 'science is X' and 'philosophy is Y', and thereby attempting to draw divisions regarding the significance of the subjects under investigation, simply will not do. Nonetheless, I think that there *are*, and must be, some ways of establishing distinct perspectives about how the work I am investigating should be viewed. Even if it is impossible to achieve an absolute demarcation between science and philosophy, it *is* still feasible to develop an idea of what the work in question was like, and how this fits with reasonable models of what science and philosophy are, were, and can be.

This chapter will examine some methods for the analysis of science, philosophy, and their interrelationship, in order to establish an historiographic focus for the investigation to come. I will begin with a review of one of the seminal works in science and technology studies – Kuhn's *Structure of Scientific Revolutions* [1970] – and try to show how this work can provide a model for the present study. I will look closely at the details of Kuhn's portrait of scientific development in *SSR*, in an attempt to draw out something more than the quick 'paradigms are either disciplinary matrices characteristic of a tradition or exemplars used in the transmission of such a tradition' snapshot that characterizes most invocations of Kuhn in the literature.

I will then move on to discuss two different ways of extending Kuhn's work. On the one hand, I will address several instances where scholars have enlisted notions of 'paradigms' and 'normality' to develop a 'Kuhnian' version of the history of philosophy. Such attempts include work by Richard Rorty, Patrick Heelan, Paul Wood, and Alisdair MacIntyre, who are each concerned with the analysis of episodes of philosophical change that might be rendered as exhibiting 'normal' or 'revolutionary' philosophy. These constitute extensions of Kuhn in terms of subject matter – specifically from science to philosophy.

On the other hand, I will consider several recent observations about how to extend Kuhn's basic notions to create a more robust, accurate, or comprehensive understanding

of processes of scientific change. Here, I will attend to the work of Peter Barker, Andrew Pickering, and Ian Hacking, who each identify weaknesses or omissions in Kuhn's original model of scientific change and proposes ways of rectifying these concerns. These constitute extensions of Kuhn in terms of analytical resources.

Next, I will turn to alternate models of the relationship between science and philosophy, to contemplate possible non-Kuhnian resources for the illumination of the work I will be analyzing. First, I will examine Wilfrid Sellars' characterization of philosophy as the study of 'how things hang together', and his closely related portrait of the symbiotic relationship between science and philosophy. Second, I will consider Andrew Cunningham's conception of science and natural philosophy as two game structures related by family resemblance and descent. Third, I will look again to the work of Richard Rorty, and examine his ideas about genres in the history of philosophy and instances of 'science-envy' in philosophical communities. These additional points of view will, I hope, add further resources to those provided by Kuhn and his commentators.

Finally, and somewhat distinctly from the preceding perspectives, I will address more social scientific ways of dealing with issues of the relationship(s) of science to philosophy. My repeated allusions to such relationships (between these two areas of human activity) have, I suspect, already brought to mind the notion of boundaries and 'boundary work'. I would be remiss not to take note here of Thomas Gieryn's influential ideas about institutionalization and boundary-enforcement practices in science. Gieryn's work adds dimensions of rhetoric and social competition to my picture, while reinforcing the contingency and locality of any realistic portrait of science and its commerce with other (equally contingent and local) fields. In addition, some recent work has begun to open up a space for explicit discussions of the sociology of philosophy as distinct from the already multi-dimensional field of sociology of science. In particular, Randall Collins' monumental study of the course of world philosophical schools in the very long view [1998] provides some instructive notions of how we might view philosophy as an enduringly unique human enterprise.

Having then inspected these various possible perspectives on how to write about science and philosophy, I will try to identify the set of historiographic tools that will be useful to the story I am developing. This emerging hybrid of the above methodologies

will provide me with techniques to use in the subsequent, more directly historical chapters of the present work.

2.1 The standard Kuhnian portrait of science

Kuhn's *Structure of Scientific Revolutions* [hereinafter *SSR*] provides an appropriate starting point for the discussion here for two reasons.[1] First, of course, its influence on science studies looms exceedingly large, even if this influence has sometimes been reduced to mere lip service to Kuhn's actual work. Second, given the subject matter I am trying to address, I have to contend with an existing historical record that is colored quite strongly by a particular interpretation of Kuhn. The work of my subjects – figures such as Thomas Brown and Alexander Bain – has been largely unrecognized in the history of either science or philosophy. However, when it has surfaced at all, it has been interpreted primarily as representing a 'pre-paradigmatic' stage in the history of psychology. I take issue with this characterization, and feel that it is best disputed by means of a head-on attack on its own terms. For both these purposes, to do justice to both the general and the specific relevance of Kuhn, it will be necessary to look more deeply at the model of scientific change developed *SSR* than is perhaps usual. Here, I will take in turn the Kuhnian notions of paradigms, normal science, scientific revolutions, and pre- and post-paradigmatic science, and examine what Kuhn *actually says* about each, and what this adds up to as a model of science. I will then change tack and see what relevance these same notions might have for the history of philosophy, first by considering what Kuhn says in *this* regard and then by considering whether we might translate Kuhn's model of scientific development and change – either wholesale or in part – into a model of *philosophical* development and change. As I will detail in forthcoming sections, this shift in perspective is motivated by several intertwined factors, including a desire to turn existing Kuhnian analyses of British associationism on their head, a concern with developing a fuller conception of the interrelations between science and

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[1] As a pre-emptive clarification, the reader should know that subsequent references in the text to 'Kuhnian' perspectives or claims, unless otherwise explicitly noted, are to those of *SSR*. I recognize that this single work does not represent anything like the full scope of Kuhn's work, and I will address some other developments in subsequent sections.

philosophy, and an interest in following up on a call made by Kuhn himself for comparative analyses of non-scientific disciplines.

2.1.1 Kuhnian paradigms

Needless to say, Kuhn's preeminent contribution to science studies is the popularization of the notion of scientific paradigms. His most celebrated statement regarding the nature of these structures appears near the end of *SSR* (in fact, in the postscript added to the second edition):

> [I]n much of the book the term 'paradigm' is used in two different senses. On the one hand, it stands for the *entire constellation of beliefs, values, techniques, and so on* shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the *concrete puzzle-solutions* which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science. [*SSR,* p.175; emphasis added]

Here, then, we have the famous two-fold definition of paradigms as both general 'disciplinary matrices' and specific 'exemplars'. In the more general sense, Kuhnian paradigms consist of, among perhaps other things, (1) symbolic generalizations, (2) beliefs in particular models, (3) values, and (4) exemplars. [*SSR*, pp.182-186] In Kuhn's view, it is the last of these – the paradigm in the more specific sense of a set of exemplars - which is most significant, since this class of characteristics provides a necessary level of specificity to the subject matter of a particular scientific enterprise.[2] Important roles for both senses of the term, however, appear throughout *SSR*, and Kuhn usually identifies clearly enough which sense of paradigm he intends to invoke in the development of his model of scientific activity.

To the paradigm-as-exemplar are attributed a number of important practical characteristics. An exemplar or set of exemplars is distinguished first by its *novel and*

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[2] "More than other sorts of components of the disciplinary matrix, differences between sets of exemplars provide the community fine-structure of science… The paradigm as shared example is the central element of what I now take to be the most novel and least understood aspect of this book." [*SSR*, p.187]

remarkable nature, and second by the *inherent limitlessness* of their applicability.[3] Hence, a science – in Kuhn's terms – must be born of a set of defining moments, representing the overcoming of obstacles to progress, from which spring seemingly inexhaustible resources to tackle subsequent obstacles of the same class. While such defining moments might be replicated, for example as a laboratory training exercise, they are more frequently utilized as *precedents* (explicitly akin to legal precedents) from which it is possible to extrapolate in new cases.[4] The primary means of transmitting such standard examples, aside from direct replication, is the *codification* of them in textbooks and classroom exercises as 'givens'.[5] Once such a code is established, the discipline is effectively ready to proceed on a course of 'normal' scientific activity.

Before moving to consider Kuhn's characterization of normal science, though, we should consider the other aspect of his deployment of the notion of paradigms. It turns out that the 'disciplinary matrix' is the less developed aspect of paradigms in *SSR*. While it is evident that this institutional structure is intended as the glue that binds like-minded scientists together in their mutual quest, the details of its operation are given shorter shrift than that of the exemplars that Kuhn regarded as his singular contribution. Interestingly, though, the interrelation he *does* describe between exemplar and matrix provides a revealing hint about how we should view these disciplinary matrices. Consider two brief quotations:

> The study of paradigms…is what mainly prepares the student for membership in the particular scientific community with which he will later

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[3] "[These] achievement[s are] sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity…[and] sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve. Achievements that share these two characteristics I shall henceforth refer to as 'paradigms,' a term that relates closely to 'normal science.' By choosing it, I mean to suggest that some accepted examples of actual scientific practice – examples which include law, theory, application, and instrumentation together – provide models from which spring particular coherent traditions of scientific research." [*SSR,* p.10]

[4] "[A] paradigm is rarely an object for replication. Instead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions." [*SSR*, p.23]

[5] "Close historical investigation of a given specialty at a given time discloses a set of recurrent and quasi-standard illustrations of various theories in their conceptual, observational, and instrumental applications. These are the community's paradigms, revealed in its textbooks, lectures, and laboratory exercises." [*SSR*, p.43]

practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. [*SSR*, p.11]

and

In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture. [*SSR*, p.109]

In both these cases, it seems clear that Kuhn intends "paradigm", as used in the text, to mean *exemplar*. However, what he is describing as being *achieved* is the acquisition of the paradigm as *disciplinary matrix* – membership in the community, with its accepted values, beliefs, theories, *etc*. What I think we should take away from these two telling comments is an additional role for the paradigm-as-exemplar – that of *catechism* – and a consequent image of the paradigm-as-matrix as that of a community of belief analogous to a religious denomination.[6] It is by this means that science achieves the degree of exclusivity and cohesiveness that provide it the character of normality that I will address in the next section, allowing Kuhn to claim plausibly that "a new paradigm implies a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation or attach themselves to some other group." [*SSR,* p.19] This role for the disciplinary matrix, I think, exhausts the fundamental features that Kuhn attributes to paradigms – in either sense – and which he claims distinguish, at least in part, science from all other human activities or institutions.

2.1.2 Kuhnian normal science

Most of Kuhn's description of normal science proceeds directly from the details of his explication of paradigms. As he straightforwardly puts the matter, "'normal science' means research firmly based upon one or more past scientific achievements,

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[6] Note that I have carefully said 'religious denomination' rather than 'religion'. I am not trying to say that science is just like a religion (I am actually leaving such a judgment open). Rather, I *am* trying to say – and I think Kuhn too is trying to say - is that scientific communities arise through rights of passage analogous to those utilized in organized denominational religious communities – by using learned and performed ritual (catechism; the paradigm-as-exemplar) as an avenue to develop a sense of commonality that transcends the performed ritual and implicitly includes a much broader set of institutional features (values, beliefs, theories; the paradigm-as-matrix).

achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice." [*SSR,* p.10] That is, normal science results from work that *primarily follows examples* (paradigms-as-exemplars). Furthermore, since these examples have the character that they are open to endless variation and limitless potential applicability, an *ongoing tradition* of normal science is able to develop, and this is *what most scientists do*.[7] And, as far as the intrinsic characteristics of normal science go, that is the matter in a nutshell. If that were all there were to say about normal science, it would appear to be of little interest.

However, there are some other features to keep in mind here. First, as Kuhn recognizes, playing out the implications of a paradigm through normal scientific work should not be regarded as *mere* following of examples. Rather, there remains in such activity – especially since it is engaged in by an ever-changing cadre of scientists – a level of interest comparable to that involved in the achievement of the original exemplar itself.[8]

Beyond this, Kuhn's discussion of normal science includes several telling observations that provide more specific insight into (how Kuhn sees) the contingent historical character of science. For one, science is portrayed as *distinct from engineering*, insofar as the former retains the aforementioned quality of open-endedness while the other involves, in effect, closed solutions.[9] Also, Kuhn presupposes an acknowledged *object* for normal science in his discussion: normal science is *about solving problems* (or – at least – the normative constraint on scientists is that their merit is judged by their

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[7] "[W]hat we have previously called the puzzles of normal science exist only because no paradigm that provides a basis for scientific research ever completely resolves all its problems." [*SSR,* p.79]

[8] "Few people who are not actually practitioners of a mature science realize how much mop-up work of this sort a paradigm leaves to be done or quite how fascinating such work can prove in the execution… Mopping-up operations are what engage most scientists throughout their careers." [*SSR*, p.24]

[9] The very few [paradigms] that have ever seemed to [completely resolve all their problems] (e.g., geometric optics) have shortly ceased to yield research problems at all and have instead become tools for engineering." [*SSR,* p.79]

solution of problems).[10] Finally, and perhaps most interestingly, the standard vehicles for the transmission of scientific paradigms, and thus the propagation of normal science (namely, textbooks and related materials) have a peculiar trait. That is, they tend systematically to conceal the process by which the structure of present paradigm has been achieved.[11] The effect of this characteristic is to reinforce the previously identified analogy of science to religion – normal scientists are the inheritors of a *great mystery*. While Kuhn is unclear whether he believes this to be a truly necessary element of the process of creating normal science, he indicates that it has empirically been the case that such obfuscation of the past is characteristic of science, and normal science in particular.[12]

Thus, while the in-principle characteristics of normal science are simple and few in number, the entailments of normal science are much richer. They include particular relationships to activities judged to be of lesser merit or lesser potential, a specific

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[10] "Because the unit of scientific achievement is the solved problem, and because the group knows well which problems have already been solved, few scientists will easily be persuaded to adopt a viewpoint that again opens to question many problems that had previously been solved." [*SSR,* p.169]

[11] "Both scientists and laymen take much of their image of creative scientific activity from an authoritative source that systematically disguises – partly for important functional reasons – the existence and significance of scientific revolutions. [T]he analysis…will begin to indicate one of the aspects of scientific work that most clearly distinguishes it from every other creative pursuit except perhaps theology… As to the source of authority, I have in mind principally textbooks together with both the popularizations and the philosophical works modeled on them. All three of these categories – until recently no other significant sources of information about science have been available except through the practice of research – have one thing in common. They address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are written... All three record the stable *outcome* of past revolutions and thus display the bases of the current normal-scientific tradition." [*SSR,* pp.136-137, emphasis in original]

[12] There are, of course, reasonable justifications available for this pattern to a certain extent, foremost among them being that such concealment of the past is a matter of practical efficiency. Since the circumstances under which a paradigm arose, and the situation preceding it, are irrelevant to the process of normal scientific progress, they can simply be elided. These are Kuhn's "important functional reasons". However, Kuhn goes farther here – he notes that these are only 'part' of the story. The rest, one might argue, is related to the maintenance of the quasi-religious disciplinary cohesion needed to keep scientific activity unified.

presumed objective that is external to the institutional characteristics of the enterprise, and a pattern of concealment and forgetting that shore up the discipline and encourage (enforce) continued normality.

2.1.3 Kuhnian scientific revolutions

The contrast class to normal scientific activity, for Kuhn, is the "extraordinary" science that is characteristic of periods of scientific revolution, or at least attempted scientific revolution. As he describes such episodes, they begin when existing *theoretical constructs* are unable to provide resources for continued progress along a normal scientific path. Recognition of these inadequacies opens the door for replacement of an existing theory.[13] The new theory, once incorporated into a cohesive paradigm, resolves – or at least purports to resolve – these issues, and provides a previous-unavailable means for scientific activity to proceed.[14] Struggles for supremacy between old and new theories may result in a supercession of the existing paradigm or not, but what is perhaps most interesting in such clashes is that (Kuhn claims) the proponents of different paradigms have *incommensurable* conceptions of the way the world works – that is, that the worldviews characteristic of each paradigm are mutually exclusive; one cannot conceivably maintain both positions at once. Incommensurability has been one of the most contentious aspects of Kuhn's model of science, and one on which Kuhn's own position evolved significantly over the course of his career. I will leave a detailed consideration of the implications of incommensurability aside for the moment, and take it up again in the context of Peter Barker's extensions of Kuhn in section 2.3; as we will see there, Barker provides an economical gloss of Kuhn's changing conception of incommensurability, while also advancing his own method of analyzing such cases.

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[13] "If awareness of anomaly plays a role in the emergence of new sorts of phenomena, it should surprise no one that a similar but more profound awareness is prerequisite to all acceptable changes of theory." [*SSR*, p.67]

[14] "Probably the most prevalent claim advanced by proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis." [*SSR*, p.153]

For now, rather than take up such properties of scientific controversy, I will restrict my discussion to a summary of the *characteristic symptoms* of the onset of scientific revolutions. These latter are neatly summarized by Kuhn as follows:

> The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. It is upon their existence more than upon that of revolutions that the notion of normal science depends. [*SSR*, p.91]

So, on this view, the onset of periods of scientific crisis can be identified by several salient features: First, and unsurprisingly, the existence of *argument and disagreement* – it would be a strange 'revolution' indeed that did not exhibit this. Second, unrestricted experimentalism – that is, a *no-holds-barred approach* to resolution of existing issues. Third, and also predictably, expression of *dissatisfaction with the status quo*. Fourth and finally, a "*recourse to philosophy*", by which Kuhn apparently means debate over first principles which are regarded as background assumptions in the course of normal science. The most interesting aspect of this nosology, insofar as it bears on the characteristic relationship of science to other fields rather than on quite general features that might typify any conflict, is this last reference to scientists retreating to a philosophical plane.[15] This will have a special bearing on how we address the next aspect of Kuhn's depiction of scientific change – the transition from 'pre-paradigmatic' to 'post-paradigmatic' periods (that special subclass we might call *inaugural* scientific revolutions) – since the historical record suggests that sciences, more often than not, spring from developments in philosophy.

2.1.4 Pre- and post-paradigmatic science

While Kuhn describes changes of scientific paradigms in general as the replacement of one paradigm by another, there are certain special episodes in which a paradigm arises where none (in the strict sense) previously existed. Examples of these

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[15] If this is the first time we have seen Kuhn refer to the relationship between science and philosophy, it will be far from the last. Section 2.1.5 will examine his opinions in this regard in full.

transitions would include the development of Darwinian biology and Lavoisian chemistry, among many others. Kuhn recognizes that he does not provide, in *SSR*, a sufficiently full discussion of the distinction between activity before and after such a change. In particular, there appears to be something of a contradiction inherent in the use of the term "pre-paradigmatic" to describe the earlier period here.[16] It is not that work occurring prior to the mature development of a science does not involve a sort of disciplinary matrix or exemplars, but rather that these do not (Kuhn says) have the specific character of a *scientific* paradigm.

We have seen already (in section 2.1.1) what is involved in the achievement of a Kuhnian scientific paradigm. Kuhn reinforces the specificity of this structure in his discussion of the nativity of a science from a 'pre-paradigmatic' precursor.[17] Prior to the coalescence of a true science, multiple traditions or schools are usually in competition with one another over the subject matter of a particular field while after a science forms this is rarely the case. At any rate, a significant *reduction of controversy* is evident once a science becomes mature and properly paradigmatic. The specific outcome of this change is an *increase in efficiency*, as activity (in the normal scientific mode) turns to the

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[16] "My distinction between the pre- and post-paradigm periods in the development of a science is, for example, much too schematic. Each of the schools whose competition characterizes the earlier period is guided by something much like a paradigm; there are circumstances, though I think them rare, under which two paradigms can coexist peacefully in the later period. Mere possession of a paradigm is not quite a sufficient criterion for the developmental transition discussed in Section II [*i.e.*, the route to normal science]." [*SSR*, p.ix]

[17] "It is…worth noting…a series of issues that require reference to community structure alone… Perhaps the most striking of these is what I have previously called the transition from the pre- to the post-paradigmatic period in the development of a scientific field… Before it occurs, a number of schools compete for the domination of a given field. Afterward, in the wake of some notable scientific achievement, the number of schools is greatly reduced, ordinarily to one, and a more efficient mode of scientific practice begins. The latter is generally esoteric and oriented to puzzle-solving, as the work of a group can only be when its members take the foundations of their field for granted… The members of all scientific communities, including the schools of the "pre-paradigm" period, share the sorts of elements which I have collectively labeled 'a paradigm.' What changes with the transition to maturity is not the presence of a paradigm but rather its nature. Only after the change is normal puzzle-solving research possible." [*SSR*, pp.178-179]

solution of problems indicated by *accepted exemplars*. Furthermore, the subject matter of the mature science develops a particular *hermetic quality* as a result of the subsumption of certain foundational concepts underneath the veil of the paradigm into which practitioners are indoctrinated. Again here, we see in bold relief the characteristic features which Kuhn thinks make a field of inquiry into a science proper: in short, *the efficient working out of the implications of a set of archetypal puzzles by a class of initiates laboring largely in ignorance of the background assumptions of their discipline.* The pre-paradigmatic discipline(s) upon which a given science is historically based will, by contrast, be seen to lack one or more of these characteristics.

2.1.5 Kuhn and paradigms in philosophy

As I have already intimated, the question of how well *specifically philosophical* activity (whatever that might be) can be described as paradigmatic has a special significance given the close historical relationship between philosophy and science. Sciences, more often than not, appear to be born from philosophical programs. Also, sciences in crisis (if we believe Kuhn) will find themselves in need of recourse to philosophical resources. Yet further, that branch of philosophy known as philosophy *of science* has, for obvious reasons, a particularly close (often even incestuous) relationship with science itself.

For at least these reasons, the question arises what the paradigms of philosophy might be like – what specific disciplinary structures and exemplary modes might be reasonably attributable to science. In the next section, I will pursue this issue by considering philosophy as one possible contrast class to Kuhnian science. Here, I will first review what resources we might find in *SSR* itself to answer questions about the distinction between science and philosophy.

On the one hand, Kuhn quite conspicuously deploys the notion of philosophical paradigms himself, in a consideration of the concept of incommensurability.[18] In this

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[18] "[T]his very usual view of what occurs when scientists change their mind about fundamental matters [*i.e.*, a transformation of vision in which one interprets the same object differently; a gestalt switch] can be neither all wrong nor a mere mistake. Rather it is an essential part of a *philosophical paradigm* initiated by Descartes and developed at the same time as Newtonian mechanics. *That paradigm has served both philosophy and*

context, he suggests that one particularly stable, but ultimately problematic, aspect of the modern epistemological paradigm (a subset of the considerations of professional philosophy) is the depiction of scientific change as leading to differences in world-view. That is, scientists literally live in a different world when they internalize a paradigm shift; in Kuhn's own words, "though the world does not change with a change of paradigm, the scientist afterward works in a different world" [*SSR*, p.121]. He goes on, immediately afterward, to say "I am convinced that we must learn to make sense of statements that at least resemble these. What occurs during a scientific revolution is not fully reducible to a reinterpretation of individual and stable data" [*SSR*, p.121].[19] This is all something of a briar patch, into which I will now throw myself in order to try to clarify.

What Kuhn seems to be indicating is that the philosophy of science has its own paradigm(s), which are deployed in the analysis of scientific activity. The treatment of scientific paradigms and shifts among them calls upon one philosophical paradigm in particular, which helps codify how changes in knowledge and conceptual structure occur. This philosophical paradigm, as it happens, is inadequate to the task of deciphering the process fully, since it leads to statements (such as the one above about the world being the same, yet different) that are apparently paradoxical. Failing a revolution in philosophy, there will remain certain anomalies in how Kuhn can speak about science and *its* paradigms. Here, then, we see Kuhn being hoist upon his own paradigm petard. So much for the internal sense of this passage of *SSR*.

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science well. Its exploitation, like that of dynamics itself, has been fruitful of a fundamental understanding that perhaps could not have been achieved in another way. But as the example of Newtonian dynamics also indicates [in light of its supersession by relativistic theory], even the most striking past success provides no guarantee that crisis can be indefinitely postponed. Today research in parts of *philosophy*, psychology, linguistics, and even art history, all converge to suggest that the traditional paradigm is somehow askew… None of these crisis-promoting subjects has yet produced a viable alternate to the traditional *epistemological paradigm*, but they do begin to suggest what some of that [successor] paradigm's characteristics will be." [p.121, emphasis added]

[19] In a later section, we will see Ian Hacking trying to make sense of a very similar issue regarding the distinction between the natural and social sciences. This neighborhood of discussion, too, points to some very deep philosophical issues in which the subjects of my later chapters are implicated. One outcome of my analysis will be, I hope, to encourage a reflexive discussion about how certain of these entrenched philosophical assumptions bleed back into contemporary science studies.

But let us now look at how the *philosophical* paradigm is characterized therein. First and foremost, this paradigm is one of long standing (Kuhn attributes its origins to Descartes) and apparently devoid of competitors (though not, perhaps, of detractors). Second, it is implicated in a wide array of fields, both scientific and non-scientific (Kuhn lists philosophy, psychology, linguistics, and art history; by direct implication, we can also add science studies). Third, it is directly compared, in its utility, to a paradigm of paradigms (Newtonian mechanics). From this evidence, then, we can identify some points of similarity and difference between this philosophical paradigm and its scientific cousins. In this case at least, the epistemological paradigm appears to be a singleton of the sort that Kuhn has previously reserved to the structures of science. Its similarity to a scientific structure is also suggested by the analogy with Newton's physics and by the analytical leverage that is attributed to it. By contrast, unlike a scientific paradigm, it does not appear to be strongly subject-specific; rather, it has broad applicability over a divergent set of subjects. Nonetheless, on the evidence provided by Kuhn's invocation of the term here, the 'philosophical paradigm' would appear to be a species very much like the scientific one.

That there should be *some* similarities between these two structures is, as Kuhn himself points out, unsurprising.[20] The concept of paradigms – as structures for the transmission of tradition; in other words, as disciplinary matrices – is a broadly applicable one that Kuhn received from the social sciences. But what is under consideration in the use of 'philosophical paradigm' above is, it would seem, *not* disciplinary matrices but exemplars – a concept that Kuhn does regard as his original

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[20] "A number of those who have taken pleasure in [my book] have done so less because it illuminates science than because they read its main theses as applicable to many other fields as well. I see what they mean and would not like to discourage their attempts to extend the position, but their reaction has nonetheless puzzled me. To the extent that the book portrays scientific development as a succession of tradition-bound periods punctuated by non-cumulative breaks, its theses are undoubtedly of wide applicability. But they should be, for they are borrowed from other fields. Historians of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way… If I have been original with respect to concepts like these, it has been mainly in applying them to the sciences, fields which had been widely thought to develop in a different way. Conceivably the notion of a paradigm as a concrete achievement, an exemplar, is a second contribution." [*SSR*, p.208]

contribution, and one that he strongly connects to the sciences as a particular class of human disciplines. This makes the attribution of quasi-scientific characteristics to philosophy somewhat more of a conundrum.

Elsewhere in *SSR*, Kuhn is fairly insistent on a disjunction between science and philosophy in terms of their paradigmatic character. In particular, he draws a division in terms of the relative tendencies of the two fields to progress. Philosophy, along with a host of other types of 'non-science', is held to be essentially non-progressive.[21] In large part, this identification may be circular, since the attribution of progress is said to be an identifying characteristic of those areas of inquiry that we are willing to call science in the first place. But there is more to it than just that. Kuhn calls particular attention to the winnowing of possible paradigms as leading to progress, using debates over the scientificity of the 'social sciences' as an example. Recalling that pre-paradigmatic sciences are often (historically-speaking) termed 'philosophies', we might find a more substantive reason to call philosophy non-progressive – namely, its failure to quash controversy. This contrast with normal-paradigmatic science is highlighted in the following passage:

> "Ask now why an enterprise like normal science should progress, and begin by recalling a few of its most salient characteristics. Normally, the members of a mature scientific community work from a single paradigm or from a closely related set... No creative school recognizes a category of work that is, on the one hand, a creative success, but is not, on the other, an addition to the collective achievement of the group. If we doubt, as many do, that non-scientific fields make progress, that cannot be because individual schools make none. Rather, it must be because there are always competing schools, each of which constantly questions the very foundations of the others. *The man who argues that philosophy, for example, has made no progress emphasizes that there are still Aristotelians, not that Aristotelianism has failed to progress...* With

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[21] " Why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or *philosophy* does not? Why is progress a perquisite reserved almost exclusively for the activities we call science?... To a very great extent the term science is reserved for activities that do progress in obvious ways. Nowhere does this show more clearly than in the recurrent debates about whether one or another of the contemporary social sciences is really a science. These debates have parallels in the pre-paradigm periods of the fields that are today unhesitatingly labeled science." [*SSR*, p.160, emphasis added]

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respect to normal science, then, part of the answer to the problem of progress lies simply in the eye of the beholder." [*SSR*, pp.162-163, emphasis added]

Kuhn, again, draws upon the specific features of scientific disciplines to explain why progress is a feature of science but not philosophy. Progress, as a matter of definition, is relative to a paradigm. Since a given science characteristically has a single paradigm, it has a standard by which to judge development. Philosophy, on the other hand, is characterized by different schools and therefore cannot achieve the same result.

There are, though, at least two apparent problems with this picture. First, as Kuhn's reference to the power of the epistemological paradigm in which his discussion of incommensurability is trapped shows, there *are* evident cases in which philosophy seems to exhibit ongoing unitary consensus. Such cases would, on the terms just discussed, appear to be capable of 'progress'. Second, there is a sleight of hand involved in the comparison of 'science' to 'philosophical schools'. It is an established truth that there is no such thing as science, but that there are rather many specific sciences each with their own paradigm. Kuhn knows this; he is careful, in the above quote, to refer not to the fictive 'science' but to "mature scientific community". If there are, similarly, many specific philosophies each with their own paradigm – as Kuhn's example of the progress of Aristotelianism indicates – then the distinction would seem to disappear. Why, if we are willing to admit that there is no such thing as science-writ-large, should we still maintain that there is such a thing as philosophy-writ-large instead of considering particular philosophies?

One plausible answer to this latter concern is provided by reference to another aspect of the distinction drawn by Kuhn in the quote above. That is, it is not the multiplicity of philosophies itself that is problematic, but that it is characteristic of a philosophical school that it "constantly questions the very foundations of the others". This does *not* happen among the various sciences; biology does not question the foundations of physics, nor does chemistry question the foundations of psychology. There is, then, a more fundamental difference between the branches of science and those

of philosophy that needs to be emphasized.[22] While sciences can remain effectively autonomous by virtue of their special subject matters, philosophies appear by their very nature to aspire to the role of comprehensive world-systems and cannot avoid clashing with one another in the process of fleshing out their paradigms. An illuminating distinction now becomes evident: There can stably exist many sciences, because each attends exclusively to a particular province of knowledge. The multiple scientific paradigms of different fields do not effectively interact. Philosophies, properly so-called, do not generally have the same opportunity, since it is in the nature of philosophies to be jealous of the territory covered by the paradigms of others.[23]

The possibly unique singularity of paradigms is not the only marker used in *SSR* to distinguish science from philosophy. On another front, Kuhn addresses the role of the special form that acquisition of a paradigm takes in the sciences.[24] The degree to which scientific practitioners are insulated from the primary sources from which their paradigms

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[22] Wilfrid Sellars, as we will see later, does a good job of cashing out this difference.

[23] Contemporary professional philosophy, to some extent, has attempted an end-run around this dilemma by a compartmentalization analogous to the sciences. The division of philosophy into such subdisciplines as aesthetics, epistemology, ethics, *etc.*, however, merely masks the essential issue. A particular school of thought might gain ascendancy in one of these subdisciplines and thereby create an environment of quasi-scientific progress. This, for example, would be a fairly effective characterization of the community of logical empiricism within twentieth-century epistemology. Without being too essentialist, though, I think that such compartmentalization is an impoverishment of philosophy. Later sections will examine in more detail some ways of handling this tension between comprehensive philosophical schools and specialized philosophical subdisciplines.

[24] "Other aspects of professional life in the sciences enhance this very special efficiency still further… Some of these are consequences of the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life… In history, *philosophy*, and the social sciences, textbook literature has a greater significance [than in art]. But even in these fields the elementary college course employs parallel readings in original sources, some of them the "classics" of the field, others the contemporary research reports that practitioners write for each other… Contrast this situation with that in at least the contemporary natural sciences… Many science curricula do not ask even graduate students to read in works not written specially for students… Why, after all, should the student of physics, for example, read the works of Newton, Faraday, Einstein, or Schrödinger, when everything he needs to know about these works is recapitulated in a far briefer, more precise, and more systematic form in a number of up-to-date textbooks?" [pp.164-165, emphasis added]

are drawn, and from the achievements of eclipsed paradigms, is – he says – unique. That is, the reliance in the sciences on 'up to date' textbooks for the transmission of tradition is almost complete. While philosophy sometimes approaches the same level of textbook encapsulation as observed in certain borderline social sciences, there is still a characteristic recourse to original texts that would never occur in natural science. This, too, can be taken to indicate a lack of progress in philosophy, or at least a lack of progressive ideology. In addition, Kuhn proposes that there are several other criteria for distinguishing science from philosophy (or any other enterprise).[25] These are: the degree of closure and exclusivity of scientific communities, which regard their practitioners as the sole judges of work within the field (even to the exclusion of competent scientists from related fields); the particular goal of puzzle-solving within normal science; and a specific set of values regarding theories, including predictive accuracy and precision, internal and external consistency, and simplicity. [*SSR*, p.185] In sum, Kuhn argues not just that fields other than science differ from it in terms of one or another characteristic of their disciplinary structure (as described at the end of the last section) but effectively that the sciences differ from other fields in *all* of their characteristics.

Kuhn does, however, make a concession in this regard, indicating that he takes his findings to be tentative. At the very end of the postscript to *SSR*, he calls for studies parallel to his in fields other than science, to establish whether his conclusions about the uniqueness of science are justified.[26] He suggests that the appropriate issues to address in

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[25] "Though scientific achievement may resemble that in other fields more closely than has often been supposed, it is also strikingly different. To say, for example, that the sciences, at least after a certain point in their development, progress in a way that other fields do not, cannot have been all wrong, whatever progress itself may be… Consider, for example, the reiterated emphasis…on the absence or, as I should now say, on the relative scarcity of competing schools in the developed sciences. Or remember my remarks about the extent to which the members of a given scientific community provide the only audience and the only judges of that community's work. Of think again about the special nature of scientific education, about puzzle-solving as a goal, and about the value system which the scientific group deploys in periods of crisis and decision." [*SSR*, p.209]

[26] "I shall close by underscoring the *need for similar and, above all, for comparative study of the corresponding communities in other fields*. How does one elect and how is one elected to membership in a particular community, scientific or not? What is the process and what are the stages of socialization to the group? What does the group

distinguishing (or not) philosophy from science, for example, include questions about the process of achieving membership in a community, the collective goals of such groups, and the control mechanisms available to them in policing their disciplinary matrices. These are precisely the kinds of questions I am trying to ask here about philosophy in early nineteenth century Britain. Having thoroughly surveyed Kuhn's depictions of science and philosophy, I am now in a position to pose a serious question of my own.

2.1.6 Does philosophy exhibit Kuhnian characteristics?

It should be clear by now that we should expect philosophy to have disciplinary structures of some kind, or – in other words – paradigms. It should also be clear that philosophy is likely to exhibit exemplary problems or challenges, or – in other words – paradigms. It should be equally clear that these are entirely insufficient to make philosophy an enterprise that is Kuhnian, in the sense of Kuhnian normal science.

The following sections will proceed to outline extended (post-Kuhnian) or alternative (non-Kuhnian) methods for the investigation of the relationship between science and philosophy. Before proceeding to these, though, I will summarize the set of features that we might use to classify, in Kuhn's terms, an activity that might be scientific or might be philosophical. I take it that any enterprise matching this description, to a greater or lesser extent, will be Kuhnian, to a greater or lesser extent. These characteristics, in the order I have introduced them in the preceding sections are:

- Paradigms based on novel and remarkable exemplars, ostensibly limitless in their applicability.
- Codification of these exemplars, as precedents, in standard texts.
- Ritual use of these exemplars to facilitate indoctrination into other aspects of the disciplinary matrix.
- Circumscription of the activity of most workers in the field by these exemplars.
- Provision of a distinction from otherwise related fields by the open-endedness of these exemplars.
- Solving problems as the primary objective.
- Conscious obfuscation of the past of the field, creating a hermetic community.

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collectively see as its goals; what deviations, individual or collective, will it tolerate; and how does it control the impermissible aberration?" [*SSR*, p.209]

- Onset of changes in disciplinary matrices indicated by disagreement, disaffection, trial or adoption of multiple or novel methods, and questioning of background assumptions.
- Disputes characterized by incommensurability between adherents of different paradigms.
- Closed standards of initiation, creating an effectively autonomous and self-judging community.
- Specialization of subject-matter allowing for singularity of paradigm structures without conflict over foundations between particular subfields.

I will not attempt now to resolve the question of how well philosophical enterprises (or, for that matter, scientific ones) fit, or have fit, this depiction. For one thing, I have hardly addressed at all what philosophy could be considered to include. For another, I think that – even given an acceptable pocket-description of philosophy - such a decision would need to rely on specific historical documentation, which (again) I have hardly begun to address. However, these criteria do at least set the stage for my next step, which will be to consider the plausibility of instances in the literature where other scholars have tried to deploy Kuhnian notions to argue for the existence of 'normal' traditions in philosophy. Having closely examined what this would entail – what it would take for philosophy to be 'normal' in the sense that Kuhn uses the term – we can, I think, be properly skeptical of claims to this effect.

2.2 Attributions of 'Normal' Philosophy

This section will trace four examples in the literature on history of philosophy. Each of these, in its own way, has had recourse to Kuhn's model of scientific development to describe the development of philosophical programs. More specifically, each attributes to philosophy a paradigmatic character resulting in 'normalized' philosophical discourse within identifiable disciplinary constraints. My primary intention here is to draw out the extent to which these extensions of Kuhn retain the level of specificity regarding such characteristics that are evident in Kuhn's original work. In addition, I will examine them for possible new resources to analyze philosophical work in a way parallel to that of Kuhn's account of science.

As I will demonstrate in what follows, these four sources exhibit widely divergent approaches to the question of whether philosophy is, or can be, 'normal'. Richard

Rorty's [1979] work represents a sweeping generalization of Kuhn, in which any human enterprise can be construed as 'normal' or 'abnormal' (this latter quality being similar, but not identical, to Kuhn's 'extraordinary' or 'revolutionary'). Rorty does have a particular target in mind, namely the philosophical status quo within which he himself is laboring, but the scope of his claims far exceeds this particular environment. Patrick Heelan [1983] makes a more modest claim for the paradigmatic nature of philosophy. His main concern, similar to Rorty's, is the limitations of the logical empiricist program in philosophy, which he characterizes (citing Rorty in this regard) as a problematic 'normality'. Paul Wood [1995], too, limits the scope of his attribution of normality to philosophy, but has a different context in mind. Wood argues, using quite specific historical evidence, for a normal tradition in the eighteenth century British empiricist movement that included both Hume and Reid.[27] Last, Alisdair MacIntyre [1984] – while not explicitly invoking the term 'normal philosophy' as the former three scholars do – provides an in-depth argument for identifiable differences between philosophy and science. Using Kuhn's analysis as a backdrop, MacIntyre develops a case for a new methodology in the history of philosophy that takes into account specific features of the philosophical enterprise. I will now consider these four in turn, and then try to extract some useful methodological guidelines for considering the relationship between the history of science and the history of philosophy.

2.2.1 Rorty and philosophical 'normality'

Rorty's *Philosophy and the Mirror of Nature* [1979] is a polemic against the status quo in professional philosophy. It is also a revealing example of how one might analyze philosophical schools, traditions, or paradigms, and of how we might regard the scope and role of philosophy in general. In what follows, I will try to draw out three different aspects of Rorty's work: First, what he thinks philosophy is, or can be. Second, what he thinks philosophy should be. And, third, what analytical tools he brings to bear in developing the case for those first two aspects. Owing to the density of Rorty's

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[27] The topical connection of Wood's claims to the subject matter of the present work is not insignificant, as will become more clear in subsequent chapters.

account, it may not be possible fully to detach these from one another within my gloss of Rorty's work; nonetheless, I will try to effect such a separation after the fact.

Quite early in his development, Rorty first indicates that he will be taking a quasi-Kuhnian perspective on philosophy. Interpreting the work of Wittgenstein, Heidegger, and Dewey as 'revolutionary', he aligns himself with this (heterogeneous) trio in the quest to reorient philosophical discourse.[28] This passage is significant not just for the explicit allusion to Kuhn's work as portable to the analysis of philosophy, but also for the implicit definition of the scope of philosophy that Rorty presupposes: Philosophy's appropriate terrain, he suggests, is "the whole panorama of human activities" - philosophy, for Rorty, is a comprehensive enterprise.[29]

Soon afterward, Rorty vastly extends both his image of Kuhnian analysis and his image of philosophy. This passage is worth considering in full, for it cuts to the heart of Rorty's case:

> [I interpret] the traditional distinction between the search for "objective knowledge" and other, less privileged, areas of human activity as merely the distinction between "normal discourse" and 'abnormal discourse." Normal discourse (a generalization of Kuhn's notion of "normal science") is any discourse (scientific, political, theological, or whatever) which embodies agreed-upon criteria for reaching agreement; abnormal discourse is any which lacks such criteria. I argue that the attempt (which has defined traditional philosophy) to explicate "rationality" and "objectivity" in terms of conditions of accurate representation is a self-deceptive effort to externalize the normal discourse of the day, and that, since the Greeks, philosophy's self-image has been dominated by this attempt. [Rorty, 1979, p.11]

So, it appears that – for Rorty – it is not merely philosophy to which Kuhn's model can be extended. *Any* discourse can be treated as 'normal', if it involves a set of agreed upon criteria for arriving at a consensus. Note too Rorty's rhetorical reorientation of Kuhn's 'extraordinary' or 'revolutionary' into 'abnormal', with the evident implication being one

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[28] "Wittgenstein, Heidegger, and Dewey have brought us into a period of "revolutionary" philosophy (in the sense of Kuhn's "revolutionary" science) by introducing new maps of the terrain (viz., of the whole panorama of human activities) which simply do not include those features which previously seemed to dominate." [Rorty, 1979, pp.6-7]

[29] Of course, Rorty is not alone in this. A similar point arose in my discussion of Kuhn's characterization of the non-progressiveness of philosophy, and it will appear again when we turn to other commentators on the philosophical project.

of disapprobation (on the part of the 'normals', not on the part of Rorty; Rorty clearly aligns himself with the outsiders). It is easy to see how one can move from Kuhn's model in *SSR* to a position like Rorty's (it took Rorty only three sentences), but – and I emphasize this strongly – this position is not Kuhnian at all! First, while we have seen that Kuhn insisted vigorously on a specific identifiable set of criteria for the establishment of 'normal science', Rorty has only one criterion for creating his 'normal discourse' – the generation of agreement.[30] Furthermore, and despite accusations to the contrary, Kuhn's characterization of normal science (vs. 'extraordinary' forays into new territory) was intended as purely descriptive. By contrast, Rorty's normal discourse appears to have the sinister character of branding dissent as 'abnormal'.[31] Leaving completely aside the question whether Rorty is right or wrong, he is not talking (here) in terms that we can regard as closely related to Kuhn's. Put more directly, the removal of identifiable standards and the reorientation of terminology from descriptive to prescriptive combine here to gut the Kuhnian model of any useful historical application.

Or so it seems thus far. In fairness to Rorty, let us look further into his account to see if anything helpful can be extracted from it despite the initial violence he does to Kuhn. The same passage quoted above begins to point to some specifics about philosophy that may lead in the right direction. I have already suggested that Rorty views philosophy as an essentially all-encompassing endeavor (one whose province is the full range of human activities). In his introduction of quasi-Kuhnian (we might now say *pseudo*-Kuhnian) terms, he adds to this the notion that philosophy is (has been, since Plato) defined by an attempt to find representational conditions for the rational and objective. Rorty views this, effectively, as characteristic of a particularly long-lived paradigm – one that can, and should, be shifted. So, at least, Rorty begins to develop a particular depiction of the parameters of the discourse he is challenging.

More specificity - and more Kuhnian apparatus - appears when Rorty turns to an explication of the role of Descartes in the history of philosophy. In particular, this

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[30] And, he is *against* it.

[31] To give a fuller flavor of the rhetorical difference, I note that *Roget'sInternational Thesaurus* includes 'abnormal' under the following categories: 'anomalous', 'eccentric', 'inconsistent', 'insane', and 'wrong'. In the same reference, 'extraordinary' leads one to 'distinguished', 'exceptional', 'remarkable', 'special', 'unexpected', and 'wonderful'.

provides Rorty an occasion to introduce Kuhn's notions of incommensurability and social constraint. He portrays Descartes' work as revealing the rhetorical pattern typical of all revolutionary shifts of conceptual paradigm.[32] Since a new paradigm must accomplish its revolution in the rhetorical environment of an existing one with which it is incommensurable, Rorty observes that (surreptitious) reorientation of the meaning of existing concepts is a prerequisite to the accomplishment of paradigm change (in philosophy, as well as in science).[33] Later on in his work, Rorty returns twice to the case of Descartes to make connections with Kuhn. In the former of these revisitations, he observes generally that Kuhn's example provides ammunition for Rorty's assault on internalist philosophy.[34] In this respect, Rorty suggests that a 'Kuhnian' turn to social context would impel a return to authenticity in philosophy, allowing us to do Descartes one better by resolving matters that he could not settle. Later, this suggestion is brought home to its Kuhnian origin when Rorty reminds us of the analytical dilemma in which Kuhn found himself trapped by the "philosophical paradigm initiated by Descartes." [*SSR*, p.121] Rorty intimates here that he is following in Kuhn's footsteps when he seeks

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[32] "[Descartes allowed] most of the work of changing the notion of "mind" to be done under the table, not by any explicit argument, but simply by verbal maneuvers which reshuffled the deck slightly, and slightly differently, at each passage in which the mind-body distinction came to the fore." [Rorty, 1979, p.58]

   As an aside, Rorty's belief that this pattern is typical and necessary may go some way toward explaining why he twists Kuhn's model in the way that he does, and also suggests that we should watch his words very carefully if we are to avoid being victims of legerdemain. Since Rorty is proposing philosophical revolution in this work, one is reminded here of the old 'all Cretans are liars' paradox…

[33] "In "Kuhnian" terminology, no revolution can succeed which employs a vocabulary commensurable with the old, and thus none can succeed by employing arguments which make unequivocal use of terms shared with the traditional wisdom. So bad arguments for brilliant hunches must precede the normalization of a new vocabulary which incorporates the hunch. Given that new vocabulary, better arguments become possible, although these will always be found to be question-begging by the revolutions victims." [Rorty, 1979, p.58, n.28]

[34] "To understand the matters which Descartes wanted to understand...we need to turn outward rather than inward, toward the social context of justification rather than to the relations between inner representations. This attitude has been encouraged in recent decades by [among other developments] Kuhn's *Structure of Scientific Revolutions*." [Rorty, 1979, p.210]

to overcome this limitation, and further claims that he has resolved the issue himself.[35] It is here that we see revealed most clearly not just the degree to which Rorty's image of philosophy might be comparable to Kuhn's image of science, but also the specific philosophical revolution that Rorty supports. While, for my purposes, the former is more important than the latter, I will need to say a few words about both in order to clarify the role Rorty's work might play here.

Philosophy and The Mirror of Nature is, by and large, advocating a shift from the pursuit of epistemology (as demarcation of the rational and objective) in philosophy to the pursuit of hermeneutics (as perspectival interpretation of discourses). In promoting this shift, Rorty reduces the differences among various human discourses and activities to a bare dichotomy between those that display 'normal' commensurable modes of discussion and those that do not.[36] This division is couched as a generalization of Kuhn, and the suggestion is made that Kuhn's particular subject matter – science – is an area where replacement of one normal discourse simply results in the establishment of another

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[35] "[Kuhn] called for a "viable alternate to the traditional epistemological paradigm"…Kuhn was right in saying that "a *philosophical paradigm* initiated by Descartes and developed at the same time as Newtonian dynamics" needed to be overthrown, but he let his notion of what counted as a "*philosophical paradigm*" be set by the Kantian notion that the only substitute for a realistic account of successful mirroring was an idealistic account of the malleability of the mirrored world…[W]e can get rid of this notion by being behaviorist in epistemology rather than by being idealist. *Hermeneutics does not need a new epistemological paradigm,* any more than liberal political thought requires a new paradigm of sovereignty. Hermeneutics, rather, is what we get when we are no longer epistemological." [Rorty, 1979, p.325, emphasis added]

[36] "The pragmatic approach to knowledge suggested by epistemological behaviorism will construe the line between discourses which can be rendered commensurable and those which cannot as merely that between "normal" and "abnormal" discourse – a distinction which generalizes Kuhn's distinction between "normal" and "revolutionary" science. "Normal" science is the practice of solving problems against the background of a consensus about what counts as a good explanation of the phenomena and about what it would take for a problem to be solved. "Revolutionary" science is the introduction of a new "paradigm" of explanation, and thus of a new set of problems. Normal science is about as close as real life comes to the epistemologist's notion of what it is to be rational…More generally, normal discourse is that which is conducted within an agreed-upon set of conventions about what counts as a relevant contribution, what counts as answering a question, what counts as having a good argument for that answer or a good criticism of it. Abnormal discourse is what occurs when someone joins in the discourse who is ignorant of these conventions or who sets them aside." [Rorty, 1979, p.320]

normal discourse of similar intrinsic character.[37] In philosophy, however, things – Rorty claims - can be different. He proposes a transition in philosophy away from what might be called a quasi-scientific approach ('epistemology') to a process where common standards of agreement are neither assumed nor sought ('hermeneutics'). In this latter hermeneutic mode - in effect - philosophy examines, perhaps interprets, but does not conclude.[38] Thus, I take it, Rorty is claiming (significantly) that philosophy can escape from being a paradigmatic enterprise at all. While any discourse *can* be normal and paradigmatic, and science may perhaps *necessarily* be, philosophy (on this view) is the sort of enterprise that can *opt out* of the process of coalescing into communities of agreement. Furthermore, Rorty believes it should.

Rorty's bottom-line position, then, amounts – I think – to this: Philosophy, like all other discursive enterprises, can be paradigmatic. However, it is possible – and desirable – that philosophy can avoid this and instead be *non*-paradigmatic. Unlike science, the essential aims of philosophy require that 'normality' be avoided. Furthermore, Rorty believes that he has identified a means of achieving this desideratum in what he characterizes as a pragmatic, hermeneutic behaviorism open to ongoing non-normal discussion.

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[37] On this latter point, Kuhn would likely agree, but for reasons external to Rorty's argument. The resources used in *SSR* to make claims of this sort have been jettisoned in Rorty's 'generalization' of Kuhn.

[38] "[H]ermeneutics is the study of an abnormal discourse from the point of view of some normal discourse – the attempt to make sense of what is going on at a stage where we are still too unsure about it to describe it, and thereby to begin an epistemological account of it…[T]he line between the respective domains of epistemology and hermeneutics is not a matter of the difference between the "sciences of nature" and the "sciences of man," nor between fact and value, nor the theoretical and the practical, nor "objective knowledge" and something squishier and more dubious. The difference is purely one of familiarity. We will be epistemological where we understand perfectly well what is happening but want to codify it in order to extend, or strengthen, or teach, or "ground" it. We must be hermeneutical where we do not understand what is happening but are honest enough to admit it, rather than being *blatantly* "Whiggish" about it. This means that we can get epistemological commensurability only where we already have agreed-upon practices of inquiry (or, more generally, of discourse) – as easily in "academic" art, "scholastic" philosophy, or "parliamentary" politics as in "normal" science." [Rorty, 1979, pp.320-321; emphasis in original]

Unfortunately, and perhaps necessarily given his position, Rorty provides few specific methodological guidelines for the historical analysis of philosophy here. His depiction of philosophy in its paradigmatic mode has none of the specificity of Kuhn's elaboration of the disciplinary character of science, and relies instead on a sweeping distinction between communities of agreement and communities of debate. While Rorty's other work (as we will see later) provides some better clues in this regard, these are not couched in Kuhnian terms. If we are to understand the history of philosophy in a way analogous to that of Kuhn's history of science, we will need to look elsewhere.

Nonetheless, this consideration of Rorty's position has not been fruitless. However much he may take Kuhn's name in vain, he has provided us with some possible markers for the interpretation of philosophy as a human activity. These include: the notion that philosophy is intrinsically comprehensive in scope; the idea that philosophy can (given certain conditions) be either paradigmatic or not; and a depiction of incommensurability in which transitions between paradigms (in philosophy, as well as in science) require reorientations of fundamental *existing* concepts in ways that prove indefensible on one side and unquestionable on the other. I will add these suggestions to my analytical toolkit (for what they are worth), and move on to consider some other inroads Kuhn has made into the history of philosophy.

2.2.2. Heelan and philosophical 'normality'

Patrick Heelan's *Space-Perception and the Philosophy of Science* [1983] makes more modest use of Kuhn's example than Rorty does. Conversely, Heelan's work suggests a particularity of application that may provide a better clue to the applicability of the term 'normal philosophy'.

In fact, Heelan's reference to Kuhnian notions is limited to a single prefatory statement in the opening passages of his work. In introducing the methodology he employs in discussing the acquisition of modes of visual interpretation, Heelan says:

> It may well be that the latter [phenomenological and hermeneutical] methods are more flexible [than standard methods of analytical philosophy and epistemology], because they have not yet reached the "perfect" stage of constituting a paradigm for normal philosophy. [Heelan, 1983, p.xiii]

In regard to this statement, Heelan footnotes Rorty's *Mirror of Nature*, as well as a 1980 collection edited by Morick (with contributions from Carnap, Sellars, Putnam, and others). While this explicit statement is brief, and does not even provide any emphasis to the notion of 'normal philosophy', the context of Heelan's citation of the phrase helps to flesh out its significance.

In the passage in which this quote appears, Heelan is describing the process he employed in pursuing the work that his book details. He says that he began with the intention of using a variety of methods, including both 'standard' analytical ones and hermeneutical ones, to approach the different aspects of his topic each according to their own particular character. In the end, he was "forced to adopt" an exclusively hermeneutic and phenomenological approach by the nature of the work itself. [Heelan, 1983, p.xiii] His best explanation for this is that his specific subject matter – issues of perception – required a methodology that was flexible, and therefore not constrained by a rigid paradigm in the manner of analytical philosophy and epistemology.

Heelan's remarks are loose and ahistorical, but they are suggestive in several useful ways. His attribution of 'normality' to analytical philosophy and epistemology is a common one (shared, for example, by Rorty but also the kind of remark that could easily be elicited from any number of contemporary professional philosophers).[39] The profession of philosophy in twentieth century America was largely dominated by a school of thought (logical empiricism or logical positivism) that construed itself in ways quite similar to that of a paradigmatic science. Standard methods, modes of argumentation, examples, and theoretical constructs were the rule, and these were propagated by a tradition of thought recognizing relation by descent within a closed community with high standards for admission. In short, if philosophy were ever normal in a Kuhnian sense, Heelan's 'analytical philosophy and epistemology' would provide the most conspicuous exemplar. The Morick collection cited by Heelan (and entitled *Challenges to Empiricism*) provides a clear picture of some of the limits of this paradigm. Leaving aside the issue of whether such an enterprise ceased at some point to be philosophy and became instead an aspirant science, Heelan's remarks underscore the close familial ties that often

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[39] Off the record, I have myself gotten professional philosophers to say exactly this quite easily – analytical ones as well as more post-modern types.

exist between science and philosophy. If such normality can exist in philosophy at one time and place, it can – in principle – exist in another (or, of course, might not). Further, note that Heelan quietly disagrees with Rorty's suggestion that an alternate method can guarantee the avoidance of normality. If the hermeneutic techniques Heelan employs are relatively free from paradigmatic constraint, this is - for him - not a matter of an intrinsic quality of this method but rather because it has 'not yet' become enveloped in such a structure, an historical contingency.

Nonetheless, Heelan's gesture to Kuhnian terminology, incisive though it may be, is too fleeting to provide any concrete assistance from a methodological standpoint. While he accurately indicates a case for which the term 'normal philosophy' might apply, and appears to regard the development of such normal character as a contingency that might descend upon any philosophical movement, this is the extent of his concern with the matter.

2.2.3 Paul Wood and philosophical 'normality'

Paul Wood's work, unlike either Rorty's polemic or Heelan's reminiscence, is geared specifically toward using a Kuhnian framework for the historical analysis of philosophy. Wood, in his "Hume, Reid, and the science of the mind" [1995], considers the existence of 'normality' in the work of two eighteenth century philosophers, keeping an eye firmly on aspects of Kuhn's original use of the term. I will deal with the detailed results of Wood's analysis in a later chapter, in the context of my own historical narrative, but let us now consider the methodological basis he uses to develop his case.

Wood begins his essay with the suggestion that, despite the near-universal acknowledgment of *SSR* as a landmark in philosophy of science, there has been little serious historical application of Kuhn's proposals therein.[40] Specifically, Wood finds that Kuhn's important recognition of the role of exemplars in transmitting traditions has been

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[40] "[I]t is arguable that scholars have largely ignored the historical implications of his analysis of the nature and function of disciplinary matrices, which for Kuhn were embodied in paradigmatic works such as Newton's Principia and what he called "exemplars", that is, those concrete solutions to specific technical problems found in journal articles which serve to shape the individual scientist's perception of his or her field." [Wood, 1995, p.119]

under-utilized. He goes on to propose that the Kuhnian framework, and especially the paradigm-as-exemplar, has applicability outside science proper. According to Wood, texts - as vehicles for the transmission of knowledge traditions – have played a large role in the historical development of philosophy, in just the way that Kuhn described them to operate in the sciences. Usefully for the present project, Wood emphasizes the applicability of this analytical insight to the early modern period in general and to the study of the human mind during this period in particular.

Since my project involves an investigation of the study of the human mind in philosophical circles in the period immediately following that considered by Wood, it would be exceptionally convenient to claim that he has provided a fully fleshed out framework with which to proceed. Alas, this is not the case. What Wood *has* done is to suggest one particular respect in which we might use Kuhn's work in the historical analysis of philosophy (and further, as we will see, proceeded to demonstrate the concrete utility of this approach). And this in itself is quite useful. However, as has been the case with each of the other works we have seen that have claimed philosophy as paradigmatic in Kuhn's name, there remain significant holes. Recalling once again the multifaceted and specific representation that Kuhn developed of the character of normal science, we see that much work remains to be done. Even if philosophy (somewhat unsurprisingly, we must admit) appears to proceed by the transmission of paradigmatic tropes in canonical texts, this by itself is insufficient for the attribution of Kuhnian character to philosophy (or, alternately, for the refutation of it). Wood's work is inspirational as the first we have seen to even begin to take Kuhn seriously on his own terms, but more remains to be done.

2.2.4 Alisdair MacIntyre and philosophical normality

In a similar vein to Wood, Alisdair MacIntyre's "The relationship of philosophy to its past" [1984] takes up the issue of how we might best view the course of philosophical inquiry in historical context. Like the rest of the authors I have just discussed, MacIntyre has recourse to the example of Kuhn in this endeavor, although he (like Wood) does not explicitly utilize the term 'normal philosophy'. Like Wood, too, MacIntyre explicitly introduces the work of Hume and his contemporaries to illustrate his

concerns, making this analysis of dual significance here. As I will now show, MacIntyre's work provides the fullest account yet of how the history of philosophy might need to differ in detail from the history of science, owing to differences in what we might call the disciplinary structures and goals of the two subject matters.

First, MacIntyre observes that the scope and intentions of science and philosophy (among other endeavors) are contingencies that must be situated in place and time to be properly understood. Furthermore, he suggests that the relationship between science and philosophy in particular is such as to make these two areas frequently parasitic upon one another.[41] That is, in cases like the development – following Hume – of the 'moral sciences', we can observe the boundaries of science and philosophy *mutually* rearranging themselves in what is effectively a zero-sum game. On this view, the pursuit of a comprehensive understanding of human social and moral behavior – in origin a wholly philosophical enterprise – has, over the course of time, experienced a kind of diaspora. The contemporary intellectual scene reflects this in the existence of a circumscribed philosophical ethics, and a set of similarly compartmentalized social sciences. This process has left philosophy, science, and human affairs in general irrevocably altered. In the history of philosophy, MacIntyre claims, such dynamic reorganization of disciplinary boundaries has often been disregarded. The end result of neglecting this aspect of the history of human institutions has been distortion of the situated intent and significance of, in particular, the philosophical enterprise.[42]

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[41] "We now characteristically distinguish philosophical problems and enquiries from scientific or historical or theological problems and enquiries; but it has not always been so. Hume's ambition [for example] was to be the Newton of the moral sciences… What we are apt to treat as the history of philosophy, properly so called, involves often enough the abstraction of what *we* now take to be the genuinely philosophical parts from larger wholes. But in so doing we cannot avoid distortion… The subject matter of the moral philosophy of the seventeenth and eighteenth centuries provides a telling example. Its twentieth-century intellectual heirs and beneficiaries include not only the attenuated and impoverished discipline that modern philosophical ethics has become at the hands of most of its modern practitioners, but psychology and the other social sciences. And this has not of course simply involved a reallocation of topics and issues. The very process of reallocation has been a transforming one, and the transformations have extended beyond the academic disciplines to the idiom of everyday life." [MacIntyre, 1984, pp.31-32]
[42] Quentin Skinner's observations, discussed in the previous chapter, amount to a similar point.

Having thus argued for a more historically sophisticated account of philosophy, and especially its association with science, MacIntyre proceeds to consider some particular implications of the changing face of the field. For one, he is concerned with the possible use of this observation as a means of discrediting philosophy as a worthwhile pursuit.[43] Taking to heart the example of theology's fate in the European Enlightenment, MacIntyre remarks that theological claims to rationality were discredited in that period largely on the grounds of the inability of different schools of thought to defeat one another's claims. In Kuhnian terms, theology was brought down by the enduring existence of multiple paradigms – something the new Enlightenment sciences were able successfully to avoid. Given the shifting ground on which philosophy too apparently rests, and the evident existence (remarked by Kuhn) of durable disagreement over philosophical fundamentals, it might be expected to follow the same path.

It is precisely such a fate for philosophy that MacIntyre is concerned to avoid. In that pursuit, he turns to the particulars of Kuhn's analysis of science to attempt the construction of a similar model for the history of philosophy that reveals in this history a rational pattern. He notes that the primary concern here is the accommodation of issues of incommensurability.[44] This recognition provides him with the first critical distinction

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[43] "[I]t was in part, at least, the discovery of rival theological modes of enquiry embedded in rival forms of religious practice similarly unable, and for similar reason, to defeat each other's claims at a fundamental level by rational argument that led to the Enlightenment and post-Enlightenment discrediting of theology as a mode of rational enquiry. So the question necessarily arises: why should philosophy not suffer the same discredit?

The question has force for two distinct reasons. The first is that it is a more sophisticated version of a question that is often already posed by non-philosophers. Philosophy, so it is sometimes claimed, differs from the natural sciences in its inability to resolve fundamental disagreements; since philosophers address the world in varying and discordant voices, what should any attention be paid to them? And secondly this crude but common attempt to discredit philosophy is reinforced by the whole family of considerations that I have been adducing [*i.e.*, the evident changes in academic division of labor, internal structuring, literary genre, and relationships among concepts that are characteristic of the history of philosophy]." [MacIntyre, 1984, p.35]

[44] "The problems of how issues can be rationally resolved when they divide the adherents of large and comprehensive points of view…are the problems of incommensurability… The characteristics of the natural sciences, identification of which led Kuhn to his initial claims about incommensurability, were of course more limited in scope than the characteristics of philosophy which generate our present problem. For one thing…the conception of natural science which informs his whole argument is one in which modern

he will make between science and philosophy. Proposing that the two enterprises differ in scope (a general position that has already been suggested here in earlier sections), he offers one concrete example of such a difference – the essential presentism of the history of science. That is, MacIntyre maintains that, in historical inquiry into science, the subject matter to be investigated is circumscribed (at least in Kuhn's account) by the contemporary definition of what science is and how it functions. Furthermore, he implies, this is a more-or-less acceptable restriction. The history of philosophy, on the other hand, cannot sensibly be written in such a fashion. Despite this difference, though, MacIntyre suggests that the essential problem in both cases is the same: to explain how clashes of rival, incommensurable claims, and resolutions of such clashes, can be dealt with historically.

Since he believes that what is foremost at stake here is the recovery of rationality in cases of theory change, MacIntyre finds it necessary to contend with a (for him) troubling interpretation of incommensurability. Philosophers of science, according to MacIntyre, have generally fallen into one of two camps: those who disagree with Kuhn about the applicability of incommensurability to the history of science, and those who have drastically extended the concept of incommensurability beyond Kuhn's original intent. He suggests that these two camps nonetheless agree that, *if* incommensurability is a useful concept for history of science, it follows that choices between bodies of theory cannot be analyzed on rational grounds. In other words, incommensurability is generally held to entail an essentially non-rational (not to say *ir*rational) relativism in instances of theory change.[45] MacIntyre takes exception to this view:

> "This entailment I wish to challenge. The argument which I want to deploy requires an initial emphasis upon two points to which philosophers of science have so far perhaps paid insufficient attention… The first is that theories in the natural sciences as elsewhere have an essentially historical

conceptions of natural science are largely allowed to determine which theories and activities in pre-modern societies are to be accounted precursors of the history of modern science… Nonetheless the form of Kuhn's initial problem was exactly the same as ours: how is it possible to treat rival claims, claims embedded in contexts so different that no neutral criterion or standard of argument is available." [MacIntyre, 1984, p.40]

[45] Implicit here is that scientific change *is* theory change. This is MacIntyre's implication, not mine.

existence… [The other is that p]articular small-scale theories come to us for the most part embedded in larger bodies of theory." [MacIntyre, 1984, pp.41-42]

The upshot of MacIntyre's characterization here is also twofold. First, regarding the historicity of theories, he argues that the flaws and lacunae in theoretical systems should be regarded not just as problems but also as driving forces for change. This point is obviously congruent with Kuhn's depictions of "normal" scientific activity as the fleshing out a healthy (but nonetheless incomplete) paradigm and of "revolutionary" scientific activity as serving to replace an unhealthy one. Second, the embeddedness of theoretical system within theoretical system, on an ever more comprehensive scale, provides a necessary means for the recognition of disagreement between theories. As MacIntyre puts it, "there has to be such a framework, for without the conceptual resources which it affords we could not understand the two theories as rivals." [MacIntyre, 1984, p.42] Together, these two points add up to a methodological direction for the history of theory change in science (and philosophy) that jibes well with a recent example set by Peter Barker (as we will see in the next section).

Recall, though, that MacIntyre's focus is on the history of philosophy rather than on the history of science (however much these two may be intertwined). He uses Kuhn's template to demonstrate the ways in which the natural sciences have been colored by the course of their historical development, but then proceeds to consider how this same model might apply more directly to the history of philosophy instead. In this light, he observes that:

> "to have recognized [the historicity of science] is to have reached a point in which it is possible to turn back from the history of the natural sciences to that of philosophy and to enquire whether the relation of past and present in philosophy can be understood, if not in the same, at least in a closely analogous way… A condition of being able to do this would be that the questions raised by three crucial differences between the problems posed by the natural sciences and those posed by philosophy can be answered in at least a minimally satisfactory way." [MacIntyre, 1984, pp.44-45]

MacIntyre characterizes these three differences as being: first, that delimitations of what counts as history of science are based on a modern conception of science, while this is

clearly inappropriate for philosophy; second, that it is generally easy to assume that scientific discontinuities occur within a *Weltanshauung* that can be assumed constant, while in philosophy this may not be the case; and third, that more recent developments in science can almost always be assumed to have overthrown earlier views, while philosophy does not show this same kind of linearity. He resolves these discrepancies, to his own satisfaction, as follows: First, he provides a standard for the recognition of 'philosophical matters' that is based not on a presentist conception, but on an originary one. That is, in his words, "[n]obody is to count as a philosopher who does not have to be judged in the end against standards set by Plato." [MacIntyre, 1984, p.45] Second, he simply denies that wholesale discontinuity of worldview exists in philosophy any more than it does in science. Here he has recourse to the notion that "even the most radical of philosophical conflicts occur within the context of not dissimilar continuities. The philosophical truth, and it is a truth, that not everything can be put in question simultaneously is relevant[.]"[MacIntyre, 1984, p.45] Finally, he notes that the recognition that philosophical change does not commonly involve linear progress is a sufficient methodological caveat for purposes of beginning historical analysis. He concludes that it is then possible to rationally adjudicate, on an historical basis, among rival philosophical claims so as to provide an account of philosophical change that mirrors the Kuhnian picture of scientific change.

Taking the elements of MacIntyre's solution in reverse order, I believe we can rank them in terms of descending usefulness. It seems essentially correct that we can assess the confrontation of rival traditions, whether in science or philosophy, without needing to assume that the one emerging as ascendant in more recent times is the superior. It also seems to be *practically* valid to assume that there will be some common ground from which to assess the confrontation of two philosophical traditions, even if they purport to argue for completely divergent worldviews. However, I contend that this practical sufficiency is a weaker one than MacIntyre thinks (if still one that is "minimally satisfactory").[46] The notion that the delimitation of properly philosophical matter needs

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[46] One might take issue with the 'in principle' claim - made by MacIntyre here - that this is a consequence of it being impossible to put everything into question simultaneously. This ignores another important historical aspect of theory change. To enlist a metaphor

to redound to a Platonic standard, though, seems to be both unnecessary and eccentric. As I will discuss, there are *many* ways of marking off such a boundary, and even if MacIntyre's is "minimally satisfactory" it provides a certain idiosyncratic coloring to the history of philosophy that I find no more (or less) appropriate than the idea that the standard of scientificity should be that of the present day. Many authors have argued against presentism in the history of science under the rubric of "anti-Whiggishness." Even if this perspective has its own flaws, it is clearly not unassailable to maintain either that science should be judged by its current manifestation or philosophy by its original one. Nonetheless, even if MacIntyre's solutions here are of differential merit, the three issues he identifies as separating the method of the history of science from that of the history of philosophy at least provide a solid starting point for considering how we might need to frame a story that incorporates both scientific and philosophical elements. We begin, with MacIntyre's work, to see how a set of specific criteria – similar to those which Kuhn proposes for the special nature of science – might be constructed for the analysis of the history of philosophy. While more work remains to be done to bring this analysis up to Kuhn's level, a framework for the consideration of philosophical paradigms is beginning to emerge. I will return to this problem later, after having considered three extensions of Kuhn along other dimensions, and some additional viewpoints on the relationship between science and philosophy.

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generated by Otto Neurath, let us imagine two identical boats putting out to sea, and each being reconstructed part by part while afloat. Upon returning to port, there is no need to assume that the boats will have any common features, especially if they have access to different sets of flotsam and jetsam in their travels. Neurath's original boat metaphor (with one boat only) would support the idea that not everything can be put into question at once, and this is supportable in the context of scientific change, in which – as MacIntyre notes – we can usually assume a certain linearity of development and further assume that problems, once solved, remain solved. In philosophy, however, given the possibility of confrontation between perennial issues having undergone fundamental permutation in structure along independent paths, we need not have 'put everything into question *at once*' in order for there to be clashes between utterly divergent points of view. If this seldom occurs, it is not because it cannot occur.

2.3 Methodological and conceptual extensions of the Kuhnian framework

The Kuhnian framework has not gone unmodified since the publication of *SSR*. We have already seen how it might be stretched (sometimes with a certain amount of distortion) to the purpose of studying the history of philosophy. But even within the history of science, Kuhn's successors (and, as will emerge, even Kuhn himself) have had occasion to reconsider the details of the model proposed in *SSR*. This section will consider three such modifications of standard Kuhnian history of science before I proceed in the following sections to move away from Kuhn's model altogether to examine some alternate methods from the history of science, the history of philosophy, and the sociology of science.

First, I will review Peter Barker's method of comparing concept structures to analyze the differential degrees of incommensurability that might exist between (partially) divergent scientific models of the same subject matter.[47] Barker argues that this method provides a means for the rational analysis of theory choice that is consistent with both the state-of-the-art in cognitive science and the modifications suggested to the *SSR* depiction by Kuhn's later work. Following this explication of Barker's approach, I will give a brief overview of limitations ascribed to the standard Kuhnian model by Andrew Pickering and Ian Hacking. Each of these three scholars introduce to the paradigmatic analysis of science new issues that were not part of Kuhn's original conception.

2.3.1 Barker's post-Kuhnian analysis of conceptual structure

In a recent paper entitled "Kuhn, Incommensurability, and Cognitive Science" [2001], Peter Barker proposes a new methodology for the assessment of scientific theory change that is couched squarely in Kuhnian terms. Barker begins with the assertion that contemporary developments in cognitive science indicate that there are problems with the way conceptual change has been treated in mainstream philosophy of science. He further

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[47] This 'sameness' of subject matter is one that I hope is not question-begging. What I intend to indicate is the sort of 'sameness' that might exist when two scientists of differing opinions examine a single table of data. Irrespective of the difference in interpretation, it seems uncontroversial that these data would be 'the same' for each scientist in this respect. Skeptical relativism beyond this threshold seems pig-headed.

indicates that this recognition, rather than invalidating Kuhn's model of science in *SSR*, actually provides resources for the clarification and substantiation of Kuhn.[48] Moreover, he details Kuhn's own expansion on the concepts in *SSR* in his later writings, which are congruent with the results indicated by the cognitive scientific work.

As Barker documents, Kuhn elaborated and revised his treatment of paradigms and incommensurability in *SSR* over the course of his subsequent career.[49] Beginning with the Postscript to the second edition of *SSR*, and continuing in a series of papers (published and unpublished) from the early 1970's until shortly before his death in 1996, Kuhn continually rethought his treatment of conceptual change. With persistent attention to the state-of-the-art in scientific treatments of human mental function, he gradually shifted from a 'gestalt switch' model of incommensurability to a linguistic-translation picture to a model based on the rearrangement of conceptual structures.[50] Nonetheless, Barker observes, this work of the 'later Kuhn' has been under-recognized and under-utilized.

Barker himself maintains that Kuhn's revisions of the notion of incommensurability point toward a sophisticated new methodology for the assessment of clashes between different paradigms. Specifically, they indicate that paradigm shifts must be treated *not* as the wholesale replacement of an existing conception (or set of conceptions) by a newer incommensurable one, but rather as a rearrangement of the *relationships* among concepts. The relevant unit of analysis, then, must be relocated from the individual concept to that of conceptual structures. This reorientation has important consequences. First, incommensurability can exist *without* there being any significant differences in the individual concepts being deployed in conflicting paradigms. Instead, we should look to differences in the ways in which concepts relate to

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[48] "The demonstration by cognitive scientists that human beings cannot and do not use concepts in the way philosophers expected is a piece of bad news that has yet to be generally assimilated by the philosophical community. Kuhn's work, however, and the extensions of it made possible through the application of methods from cognitive science, shows how scientific change and conceptual revision can take place in a world where family resemblance concepts are the usual case." [Barker, 2001 p.434]

[49] See Barker, 2001, p.436.

[50] Importantly, Kuhn's progress in this arena is anticipatory of the later influential work of Peter Galison on 'creoles' and 'trading-zones'. It is even possible that Kuhn has outpaced Galison in this regard.

one another within the two (or more) systems.[51]   Second, since the characteristics of incommensurability are – on this view – a result of fundamental cognitive processes, the applicability of incommensurability to particular scientific theories is only a special case of a general property of human thought.   Thus, Barker suggests "that although incommensurability is created locally, and has local effects, it is the result of the operation of mechanisms that are universal… Incommensurability is always a possibility in the development of any human conceptual structure." [Barker, pp.436-437]   Third, from a methodological standpoint, this view of the nature of incommensurability lends itself to concrete visual-structural modeling to demonstrate rearrangements of conceptual networks.[52]   Fourth, in Barker's view, the conceptual structure model can provide a means for the rational assessment of theory choice, insofar as the simpler conceptual network can be objectively viewed as the more scientifically valuable.[53]

For present purposes, the most significant aspect of Barker's development is the modeling procedure that he introduces for assessing differences between conceptual structures. Barker's extension of Kuhn uses a "dynamic frame" notation to exhibit instances of local incommensurability and incremental paradigm shifting. Effectively, this notation provides a way to visually map the associations among concepts in particular scientific theories.   Again, in this model, it is not concepts but conceptual *structures* that are in play in the process of comparing scientific paradigms. The shifting *relationships* among concepts, and the differing *entailments* of concepts, play a much

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[51] "An important consequence of the family resemblance account made explicit by Kuhn is that individuals using the same concepts may nevertheless use wholly disjoint features to successfully classify objects as falling under those concepts." [Barker, pp.435-436]

[52] The conceptual structure model of incommensurability differs significantly, in this way, from Kuhn's earlier gestalt switch or linguistic translation models.   While the gestalt switch model famously employs the 'duck-rabbit' depiction of how interpretations of the same stimuli can be disjunct, the transference of this depiction to clashes of scientific ideas is strictly one of analogy. The conceptual structure model, as we will see shortly, provides a much more direct way of assessing differences between features of paradigms.

[53] I find this a contentious element of Barker's argument. The attribution of rationality to the simpler model appears to be fraught with hidden assumptions about the nature of science and reason. I will, however, leave this issue aside here. Suffice it to say that *Barker* believes it to be the case that simplicity is a rational scientific virtue providing a decision-making procedure within the conceptual structure model.

greater role here than do wholesale *replacements* of concepts. Structures of this sort can be represented diagrammatically and compared to other ways of modeling the same neighborhood of concepts. Barker shows how this method can clarify the role of *reorganizations* of our understanding of phenomena in cases of 'conceptual change' in science. The resulting picture is one in which incremental shifts in conceptual structure are much more commonplace than dramatic 'revolutionary' rearrangements of structures. Incommensurability, then, becomes a matter of degree rather than an all-or-nothing proposition. This technique provides an extremely valuable extension of Kuhn's conceptions – both in *SSR* and later work – and can be utilized to compare both the structures of scientific theories and those of philosophical systems as well.

2.3.2 Pickering's observations on Kuhn and on Kuhn's limitations

In the same journal issue in which Barker's reassessment of incommensurability appears, Andrew Pickering provides his own reflections on the value of Kuhn's work. Pickering's "Reading the Structure" [2001] is largely a personal rumination on how Kuhn and *SSR* have influenced his own work. While Pickering's assessment of Kuhn is largely charitable, he identifies a number of failings in the depiction of science evident in *SSR*. These include an overemphasis on the unitary nature of scientific paradigms, and undercurrents of internalism and humanism thoughout Kuhn's work.[54] Pickering's main bones of contention with *SSR* are that Kuhn ignores the material aspects of science in favor of conceptual ones, and that Kuhn overestimates the degree of autonomy that scientific enterprises are typically invested with (not surprising given the origins of *SSR* in the *Encyclopedia of the Unified Sciences*). In effect, Pickering finds Kuhn's picture of science too abstract and not local enough.

However, for the purposes of investigating the possible paradigmatic character of philosophy, these two biases may be just what is called for. Philosophy after all, while perhaps – like science – best described as a practice or activity, has nonetheless been both relatively independent of instrumental changes and relatively cosmopolitan over the

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[54] 'Humanism' here, in Pickering's usage, means an overemphasis on the degree to which human interests dictate scientific progress. Pickering distinguishes his own views as a species of 'post-humanism'.

course of its history. It would not be surprising, from this perspective, if Kuhn's model of the progress of science ended up as a fair approximation of the progress of philosophy instead. Thus, while Pickering's criticisms – in the context of the history of science – may be entirely on the mark, they also accentuate reasons why Kuhn's model might be readily exportable to the history of philosophy in a way that would be less problematic. In fact, these suggest that *SSR* – in some respects – provides a *better* depiction of philosophy than it does of science.

2.3.3 Ian Hacking and natural vs. social science in Kuhn

The idea that the history of philosophy may provide an equally, or more, receptive home for Kuhn's ideas than the history of science is reinforced by elements of Ian Hacking's "Five Parables" [1984]. Appearing in a volume devoted to historiographic issues in philosophy, Hacking's essay identifies some limitations of Kuhn's work that have direct bearing on the case I will be considering in later chapters here. In large part, Hacking's comments parallel those of Pickering – insofar as both emphasize the important role played by material aspects of scientific activity – but Hacking introduces another important element to the discussion, namely the possible distinctions between the natural and social sciences. He argues that the applicability of Kuhn's work might best be limited to the former regime and that even there the real significance of Kuhn's contribution has been misunderstood.

Hacking's discussion of *SSR* begins with an assessment of the widely held view that, therein, Kuhn established a 'historicized' model of the development of (natural) science. Hacking's position on this view is strongly negative. Furthermore, he indicates that the relevance of history to the natural versus the social sciences is quite distinct.[55]

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[55] "There is a time-honoured opinion that history matters to the very content of the human sciences while it does not matter much to the natural sciences. If Kuhn had succeeded in historicizing our understanding of natural science, his achievement would have been revolutionary. I want to show why he did not succeed, and to give a new twist to the old idea about a difference between natural and social science…

My contrast…is as follows. In natural science our invention of categories does not 'really' change the way the world works. Even though we create new phenomena which did not exist before our scientific endeavors, we do so only with a licence [sic] from the world (or so we think). But in social phenomena we may generate kinds of

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Hacking's own work, of course, has accentuated the materiality of natural scientific activity.[56] The controlled generation of phenomena through instrumental means is, on Hacking's view, the *sine qua non* of natural science. In "Five Parables", he introduces the related insight that – despite the degree to which we can create new phenomena in the laboratory – natural science is strongly bounded by pre-set limitations of the world in a way that social science is not.

In effect, then, conflicts between natural scientific paradigms can be adjudicated through reference to a relatively stable 'real world' in such a way as to make Kuhn's model of scientific change broadly applicable. However, this applicability itself is thus an indication that natural science is not strongly dependent on its own history for its results. Kuhn, then, has not historicized natural science (in the way that many have taken him to have done). By contrast, the social sciences demonstrate a much more comprehensive historicity; in Hacking's words, "we 'make up people' in a stronger sense than we 'make up' the world." [Hacking, 1984, p.115] In creating - and adjudicating between - social scientific theories, the very fabric of what individuals and societies *are* is at stake. Here Kuhn might have had an opportunity to inject the strong historicity that is often ascribed to his work. But here Kuhn's model of scientific change does not fit the facts of the case nearly so well. Hacking contends that the progress of the social sciences has been dealt with far more effectively by Foucault than by Kuhn, and that we might best regard these two figures as having – respectively – the social and natural sciences as their special provinces.

For Hacking, though, Foucault's contribution does not indicate a path forward as clearly as does Kuhn's. He concludes his essay with the less-than-sanguine observation that "I think that we shall lose ourselves in confusion and obscurity for some time yet, in the so-called social and human sciences, because in those domains the distinction between word and thing is constantly blurred." [Hacking, 1984, p.124] Pessimistically, we might read this comment as suggesting (along with many others) that the term

people and kinds of action as we devise new classifications and categories. My claim is that we 'make up people' in a stronger sense than we 'make up' the world." [Hacking, 1984, pp.114-115]

[56] See especially *Representing and Intervening* [1983] and "The Self-Vindication of the Laboratory Sciences" [1992].

'science' be reserved for those laboratory-based investigations of the material world that fit the Kuhnian framework. More optimistically, we might simply take Hacking to be indicating that the social realm provides special challenges, from scientific and historical as well as philosophical perspectives.

The significance of Hacking's comments to the present discussion is two-fold. First, they bring back to the surface Kuhn's problem about the extent to which, and the sense in which, theories remake the world (see section 2.1.5). The problems Hacking recognizes were already anticipated in *SSR*. There, we see the issue being couched in terms of the role of the scientist herself in interpreting the world, and the inadequacy of existing philosophical systems to deal appropriately with this phenomenon. The scientist might 'see the same world as different and thereby live in a different world' upon grasping a new paradigm. Kuhn suggested that the resolution of this issue was contingent on the development of not a new *scientific* paradigm but a new *philosophical* one. And this brings us to the second point.

Hacking may be essentially correct about the distinction between the natural and social sciences, and yet still have missed the significance of his observation. The social sciences may remake the world in more comprehensive ways than the natural sciences, but the natural sciences – as human practices – have an essential dependence on the social that is neatly captured by Kuhn's philosophical paradox. Furthermore, the development of adequate means for discussing such issues in the first place is reliant on a sort of feedback loop from the social sciences to the philosophy of science that has been rearing its head throughout my development here. Even if the significant aspects of this feedback loop are taken to be those parts of human nature that are essentially timeless and physical (such as Barker's appeal to cognitive science – see section 2.3.1), or if material factors are taken to dominate scientific activity (as in Pickering's post-humanism – see section 2.3.2) it remains the case that the human element cannot be eliminated from an understanding of natural science. Even more to the point here, it is precisely this sort of feedback system running through natural science, social science, and philosophy that –

I contend – informed the activity of the early nineteenth century British scholars who I will be discussing in later chapters.[57]

2.4   Some Non-Kuhnian conceptions of the science-philosophy relation

Having now looked extensively at the usefulness of Kuhn's work – and ideas flowing from it – for the analysis of science and philosophy, I will now turn to the investigation of several alternatives to the Kuhnian framework. These other views may – or may not – be compatible with the Kuhn's picture of science. Either way, all share a common concern with Kuhn, namely the relationship of scientific work to philosophical inquiry. I will actually divide my attention to such alternative systems between two basic categories of work. This section will address work that is primarily philosophical or historical in nature, while the following section will consider some insights to be taken from recent work in the sociology of science.[58]

Here, I will focus on the contributions of three figures, each of whom develops a vivid portrait of how to distinguish between science and philosophy on the basis of criteria quite different from those of Kuhn. First, I will review the ideas of the philosopher Wilfrid Sellars on the subject, as expressed in his "Philosophy and the Scientific Image of Man" [1962]. Sellars contends that the two fields differ primarily in terms of the intentional scope of their practitioners and that an essential continuity exists between the two enterprises. Next, I will return to the ideas of Richard Rorty (whose *Mirror of Nature* was considered in the previous section) to see how some of his other work might provide resources for development of a fuller picture of the history of philosophy. Rorty's "Historiography of Philosophy: Four Genres" [1984] and his "Introduction" to the edited volume *The Linguistic Turn* [1967] each suggest some novel

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[57] An absolutely essential point here, and one that I will be unable to take up adequately until the conclusion of the present work: The philosophy of science has been, and remains, colored fundamentally by the background assumptions of the human sciences. Hume was right, and I will say this again, that "Human Nature is the only science of man." That is, what we call science exists only through humans and their constitution – whether that constitution is fixed and constant or fluid and changeable.

[58] In some cases, this distinction reduces simply to the professional identification of the scholars being considered. All, in the end, fall within the pale of STS and I don't mean to overemphasize any fundamental differences that might be assumed on the basis of disciplinary affiliation.

avenues for the analysis of the history of philosophy. Last, I will investigate an historical argument made by Andrew Cunningham in two papers [1988,1991] on the nature of science and the distinction between science and natural philosophy. Cunningham's work is reminiscent of Sellars' idea that the difference is one of intention, but he also emphasizes that both constitute what might be called 'game structures' and goes into greater historical depth than Sellars regarding the peculiar development of science as a recognized field. I will conclude this section with some remarks based on George MacDonald Ross' [1990] review of the historical relationship between science and philosophy, which encapsulates the mainstream scholarship on the issue.

2.4.1 Sellars and the missions of philosophy and science

Sellars "Philosophy and the Scientific Image of Man" [1962] opens with what is perhaps one of the most dramatic statements on the nature of philosophy ever made. According to Sellars,

> The aim of philosophy, abstractly formulated, is to understand *how things in the broadest possible sense of the term hang together in the broadest possible sense of the term*. Under 'things in the broadest possible sense' I include such radically different items as not only 'cabbages and kings', but numbers and duties, possibilities and finger snaps. To achieve success in philosophy would be, to use a contemporary turn of phrase, to 'know one's way around' with respect to all these things…in that reflective way which means that no intellectual holds are barred. [Sellars, 1962, p.1; emphasis added]

Thus, for Sellars, the scope of philosophical inquiry is to be defined in the broadest possible terms. Philosophy, on this view, is an umbrella discipline with a sort of dominion over all others; it is the philosopher's business to take the entire realm of the possible into account.[59] Taken the other way around, this means that for Sellars there is no *special* domain of philosophy.[60]

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[59] I have already introduced this notion in section 2.1.5 as a possible solution to a dilemma raised by Kuhn's depiction of the difference between scientific disciplines and philosophical schools. Note too, here, that it is perhaps understandable for a philosopher to advance such a position, since it gives pride of place to his own enterprise. However, the fact that a claim like this is ideologically convenient does not necessarily make it

Special domains, or special subject-matters, are instead characteristic of sciences – which appear in Sellars' account as circumscribed endeavors that develop from philosophical roots.[61] As philosophy generates special areas of study in its more sweeping quest, it sheds them and allows them to flourish independently. Sellars' quasi-historical sketch of this process suggests that it represents a pattern that has remained stable over millennia, and can thus be taken to be an essential characteristic of the science-philosophy relationship. Thus, he would have us believe, sciences are by nature the children of philosophy.

However, he finds this distinction not quite sufficient by itself, since there is an ongoing interaction between the two that complicates the picture. First, any special discipline must know its place within the larger whole of which it is a part.[62] This is part of the process of defining a specialized domain. Second, philosophy must delve – to some extent - into the specifics of *all* the particular disciplines, since it is only through the recognition of such details that a portrait of the whole can be created.[63] Given this overlap between the role of a holistic view in science and the role of specifics in

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false. We will bear with Sellars' portrait a bit longer, since it is not his alone but rather represents a common and time-honored sentiment (dating at least to Plato).

[60] "*Philosophy in an important sense has no special subject-matter* which stands to it as other special subject matters stand to other special disciplines. If philosophers did have such a special subject matter, they could turn it over to a new group of specialists as they have turned over other special subject matters to non-philosophers over the past 2500 years, first with mathematics, more recently psychology and sociology, and, currently, certain aspects of theoretical linguistics." [Sellars, 1962, p.2]

[61] While Sellars introduces such activities not as 'sciences', but rather as "disciplines", his examples of such specialized disciplines include only areas of inquiry that might be viewed today as scientific in nature (see above note 60).

[62] "Now the special disciplines know their way around in their subject-matters, and each learns to do so in the process of discovering truths about its own subject-matter. But each special discipline must also have a sense of how its bailiwick fits into the countryside as a whole." [Sellars, 1962, p.2]

[63] "There is much truth to the Platonic conception that the special disciplines are perfected by philosophy, but the companion conception that the philosopher must know his way around in each discipline as does the specialist, has been an ever more elusive ideal since the scientific revolution began. Yet if the philosopher cannot hope to know his way around in each discipline as does the specialist, there is a sense in which he can know his way around with respect to the subject matter of that discipline, and must do so if he [or she] is to approximate to the philosophic aim." [Sellars, p.2]

philosophy, serious questions about demarcation of the two enterprises appear, even if we accept the essential characteristics that Sellars attributes to them. For one, if the philosopher has recourse to specific subject matter, and the scientist to the 'big picture', how are we to ever tell them apart? Sellars resolves this issue by relying on the intentionality of the practitioners – the *purpose* for which each enters the other's domain. While the scientist approaches the broader landscape of things for purposes of orienting his own enterprise, the philosopher approaches special subject-matters for purposes of examining the whole of which they are a part.[64] Without reference to such intentions, says Sellars, the practical differences between the two may be neglible: "Otherwise, there is little to distinguish the philosopher from the persistently reflective specialist; the philosopher of history from the persistently reflective historian." [Sellars, 1962, p.3] To this explicit statement, we might add that there would be little to distinguish the philosopher of science from the persistently reflective scientist as well.

It is easy to raise some objections to Sellars' account. Foremost, he has little recourse to real historical detail in his treatment.[65] It is hard to take his claims too seriously if one relies simply on the evidence he offers for them. Also, he makes a significant slip between the characterization of a discipline and the characterization of its practitioners. While it may be the case that a scientific discipline requires a degree of self-conscious orientation within the whole of things, it is less clear that individual scientists need achieve this.[66] We might skirt this concern by observing that any given discipline is likely to have *some* self-consciously reflective practitioners whose role it is within the field to provide a holistic orientation even if their colleagues cultivate specialist tunnel-vision.

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[64] "[T]here *is* a place for specialization in philosophy. For just as one cannot come to know one's way around in the highway system as a whole without knowing one's way around in the parts, so one can't hope to know one's way around in 'things in general', without knowing one's way around in the major groupings of things. It is therefore, the 'eye on the whole' which distinguishes the philosophical enterprise." [Sellars, 1962, p.3]
[65] His development appears to be an example of what we will see Rorty, in the next section, call *Geistesgeschichte*.
[66] It is also unclear that disciplines are historical actors that 'know', 'learn', and 'have a sense' in the way that Sellars suggests. A charitable reader might gloss over this concern.

Further, the proof being in the pudding, it is worth observing that Sellars fundamental point – that science and philosophy both are matters of orientation and intent – appears to be borne out in practice. There are many examples of professional scientists contributing to philosophy by reflecting on the conceptual foundations of their fields.[67] But rarely do these contributions carry much further than the sub-branches of philosophy that bear directly on the specialty itself – we have physicists and chemists who serve as philosophers of science, artists who serve as philosophers of art, and statesmen who serve as political philosophers, but seldom do these boundaries bleed into one another. By contrast, even in today's highly disciplinary context, there are numerous cases of specialist philosophers crossing such boundaries – philosophers of science contributing significantly to aesthetics, ethics, or political theory; political philosophers having an impact on scientific method or on theories of art; *etc.* So, in a general way, Sellars appears to be on the mark here.

However incomplete Sellars' account may be (and it was not intended to be a complete argument for his position, but only a statement of it), he does throw into bold relief two aspects of the relationship between science and philosophy that we have not seen as clearly until now. One is the notion that sciences are born of philosophy.[68] The other is that the only reliable demarcation criterion between the scientist and the philosopher is the scope of their intentions in pursuing their work. While there may be clear-cut cases of the scientist in a clean white laboratory and the philosopher in an ivory tower, it is more often the case that the roles display a certain amount of overlap. Once again, I will add these insights to the growing box of tools that might be employed to interpret my historical case and come back to assess their usefulness later.

2.4.2 Rorty and the historiography of philosophy

While, in one mode, we have seen Richard Rorty employ a quasi-Kuhnian approach to the analysis of the history of philosophy, other aspects of his work show a different style of reasoning that is oftentimes in conflict with Kuhn. Here I will address

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[67] Kuhn, Laudan, and many others have been insistent on this point.

[68] And perhaps, as Kuhn would have it, attempt to return to the womb during personal crises – this being the upshot of the 'recourse to philosophy' he describes during revolutionary episodes.

two examples of this second mode, both of which concentrate on questions specific to the historical character of the philosophical enterprise.

The first of these is a 1984 essay entitled "Historiography of Philosophy: Four Genres", published in the same collection of meditations on the history of philosophy as Alisdair MacIntyre's work already discussed in section 2.2.4. Here, Rorty is concerned to identify the most appropriate methods and questions to apply to the study of the philosophical past. He begins by posing an apparent dilemma, which he maintains is a false one.[69] Opposing two basic approaches – a presentist and a contextualist – Rorty argues that each has a certain range of applicability but that neither is sufficient in itself.[70] He suggests that a problem arises when the two approaches are conflated, but that the pursuit of each in its own right remains valuable. In this, Rorty claims, the proper method for historiography of philosophy is similar to that of science.

However, in Rorty's view, philosophy also differs in significant ways from science, and therefore requires additional care in reclaiming an appropriate historical perspective.[71] First, he introduces the notion that philosophical history, like philosophy itself, is open to a more diverse set of interpretations. This point, though, he sees as less significant than another issue regarding the differentiation of the philosophical enterprise itself. Rorty's concern is basically the question of what philosophy is, and has been. A solution to this problem, he maintains, requires writing the history of philosophy using a different approach from either of the two he has initially identified.

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[69] "There seems to be a dilemma: either we anachronistically impose enough of our problems and vocabulary on the dead to make them conversational partners, or we confine our interpretive activity to making their falsehoods look less silly by placing them in the context of the benighted times in which they were written… Those alternatives, however, do not constitute a dilemma. We should do both of these things, but do them separately. *We should treat the history of philosophy as we treat the history of science.*" [Rorty, 1984, p.49; emphasis added]

[70] In this, Rorty echoes the sentiments of Quentin Skinner discussed in chapter 1.

[71] "The only difference I have mentioned [between histories of science and those of philosophy] is that, because philosophy is more controversial than [*e.g.*] biology, anachronistic reconstructions of great dead philosophers are more various than those of great dead biologists. But my discussion so far has ignored the problem of how one picks out who counts as a great dead *philosopher*, as opposed to a great dead something else. So it has ignored the problem of how one picks out the history of *philosophy* from the history of 'thought' or 'culture'." [Rorty, 1984, p.56; emphasis in original]

As a solution to this issue of the demarcation of philosophy from other related enterprises, Rorty advocates the pursuit of *Geistesgeschichte* – a history of spirit, in the spirit of Hegel. As characterized by Rorty, this endeavor

> works at the level of problematics rather than of solutions to problems. It spends more of its time asking 'Why should anyone have made the question of --- central to his thought?' or 'Why did anyone take the problem of --- seriously?' than on asking in what respect the great dead philosopher's answer or solution accords with that of contemporary philosophers. [Rorty, 1984, p.57]

Thus, rather than take for granted a stationary subject matter, the history of philosophy must be centrally concerned with how philosophy itself is framed in any particular moment or period. This broader historiographic enterprise of dealing with 'problematics' itself mirrors what Rorty takes to be an enduring characteristic of philosophy itself, namely its penchant for working at a grand scale. Since philosophers themselves, in addition to being contentious, have also tended to apply themselves to issues of considerable scope, the history of philosophy must follow suit. In so doing, the kinds of questions that arise are those concerning the relevance or significance of the topics considered to be philosophical in any given circumstance.[72]

When appended to the presentist and contextualist genres, the pursuit of historical problematics forms – in Rorty's eyes – a classic Hegelian dialectic.[73] Serving as a

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[72] "[P]hilosophers…need to justify their concern with semantics, or perception, or the unity of Subject and Object, or the enlargement of human freedom, or whatever the philosopher who is telling the big sweeping story is in fact concerned with. The question of which problems are 'the problems of philosophy', which questions are *philosophical* questions, are the questions to which *geistesgeschichtlich* histories of philosophy are principally devoted." [Rorty, 1984, pp.57-58; emphasis in original]

[73] This quasi-Hegelian genre of the historiography of philosophy, Rorty says, "takes responsibility for identifying which writers are 'the great dead philosophers'. In this role, it is parasitic upon, and synthesizes, the [aforementioned] two genres – historical reconstructions and [rational] reconstructions. Unlike rational reconstructions, and unlike the history of science, it has to worry about anachronism, for *it cannot regard the question of who counts as a philosopher as settled* by the practices of those presently so described. Unlike historical reconstructions, however, it cannot stay within the vocabulary used by a past figure. *It has to 'place' that vocabulary in a series of vocabularies and estimate its importance by placing it in a narrative which traces changes in vocabulary*. It is self-justificatory in the way that rational reconstruction is,

synthesis of the two more focused forms, the *geistesgeschichtlich* approach opens up issues that neither of them easily permit. Specifically, again, these issues relate to the situated identification of philosophers and of philosophy itself. Here, we begin to see appear a new level of specificity about the tools required to study philosophical history; these were conspicuously absent from most of the accounts in section 2.2 that attempted to base a history of philosophy on Kuhnian principles.

First, in Rorty's picture, the historiography of philosophy must leave open the question of how to distinguish legitimate practitioners. In this way, it contrasts sharply from (most of) the historiography of science. Second, the identification of philosophical questions must proceed by tracing the dynamic changes in vocabulary that occur in the pursuit of developing issues. Both of these indicate a non-essentialist approach on Rorty's part; in his words, he maintains that "philosophy is not a natural kind." Furthermore, this claim indicates a partial point of contention with Wilfrid Sellars' depiction of philosophy discussed in the previous section.[74] While Rorty observes that there is some truth to the notion that philosophy serves as an umbrella over the special subjects, a 'meta-discipline' *a la* Sellars, he insists that this should not obscure the fundamental historicity of the philosophical enterprise. That is, philosophy-as-meta-discipline does not deal (has not dealt) with the same set of overarching concerns throughout history, but rather has undergone continuous changes in its interests over time.

Moreover, the adoption of the *Geistesgeschichte* genre of historiography, whatever its value, does not satisfy Rorty's requirements for a proper depiction of the

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but it is moved by the same hope for greater self-awareness which leads people to engage in historical reconstructions" [Rorty, 1984, p.61; emphasis added]

[74] "My claim that philosophy is not a natural kind can be restated with reference to the popular notion that philosophy deals with 'methodological', or 'conceptual', meta-issues thrown off by the special disciplines, or more generally by other areas of culture. Such a claim is plausible if it means that, in every period, there have been questions which arose from the clash between old ideas and new ideas (in the sciences, in art, in politics, etc.) and that these questions are the concern of the more original, dilettantish and imaginative intellectuals of the day. But it becomes implausible if it means that these questions are always about the same topics – e.g., the nature of knowledge, or reality, or truth, or meaning, or the good, or some other abstraction sufficiently fuzzy to blur the differences between historical epochs." [Rorty, 1984, p.64]

past of philosophy. The identification of historical problematics through the work of exemplary figures still, he says, skirts the surface of the intellectual landscape that we should attempt to reconstruct. This project should thus be supplemented by a fuller 'intellectual history' that examines the activities not just of the 'greats' but also of average practitioners.[75] By moving to this level, we will open up another set of questions, including what roles philosophy (or intellectual inquiry more generally) played in a particular social context, what institutional arrangements made these roles possible, and how the significance of such philosophical work – as intentional contributions to a particular milieu – might best be interpreted.

Rorty maintains that questions of this type mitigate the importance of inquiry about the historical identity of philosophy, and similar concerns of the *Geistesgeschichte* style, although he clearly regards the four genres he has outlined as complementary to one another. For the purposes of the present work, I will take to heart Rorty's exhortations toward both the *Geistesgeschichte* and the 'intellectual history' approaches. The questions he has identified as central to each of these historiographic modes will from the core of my investigation of studies of the mind in early nineteenth century Britain.

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[75] "I should like to use the term 'intellectual history' for a much richer and more diffuse genre – one which falls outside this triad [of rational reconstruction, historical reconstruction, and *Geistesgeschichte*]. In my sense, intellectual history consists of descriptions of what the intellectuals were up to at a given time, and of their interaction with the rest of society – descriptions which, for the most part, bracket the question of what activities which intellectuals were conducting…

I should want to include under 'intellectual history' books about all those enormously influential people who do not get into the canon of great dead philosophers, but who are often called 'philosophers' either because they held a chair so described, or for lack of any better idea… Discussion of these 'minor figures' often coalesces with thick description of institutional arrangements and disciplinary matrices, since part of the historical problem they pose is to explain why these non-great philosophers or quasi-philosophers should have been taken so much more seriously than the certifiably great philosophers of their day…

Once we drop below the skipping-from-peak-to-peak level of *Geistesgeschichte* to the nitty-gritty of intellectual history, the distinctions between great and non-great dead philosophers, between clear and borderline cases of 'philosophy', and between philosophy, literature, politics, religion and social science, are of less and less importance." [Rorty, 1984, pp.68-70]

This delineation of the possible genres of philosophical historiography amplify earlier statements made by Rorty in the context of twentieth century linguistic philosophy. His "Introduction" to the collection of essays included in *The Linguistic Turn* [1967] highlights a few other useful recommendations. There, Rorty opens his discussion by characterizing the history of philosophy as the record of a series of 'failed revolutions' in each of which philosophers attempt to execute a transition into scientific practice (in the sense of developing authoritative and unitary decision-making procedures).[76] These revolutions, he says, have consistently been failed ones because they have encapsulated significant question-begging background assumptions that have crippled their efforts to develop universal agreement around the principles they have proposed. Thus far, this characterization appears more or less congruent with the Kuhnian distinction between science and philosophy. However, Rorty goes further in his discussion of the ways in which philosophy might be said nonetheless to progress.

While the persistence of philosophical inquiry – despite its failure to achieve a quasi-scientific (or fully scientific) consensus – might be taken to indicate that philosophy reduces merely to matters of opinion, Rorty maintains that this conclusion is undeserved.[77] Instead, while recognizing the perils of presupposing a notion of 'progress', Rorty suggests that philosophy is and has been, like science, a progressive

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[76] "The history of philosophy is punctuated by revolts against the practices of previous philosophers and by attempts to transform philosophy into a science – a discipline in which universally recognized decision-procedures are available for testing philosophical theses… In all of these revolts, the aim of the revolutionary is to replace opinion with knowledge, and to propose as the proper meaning of "philosophy" the accomplishment of some finite task by applying a certain set of methodological directions… In the past, every such revolution has failed, and always for the same reason. The revolutionaries were found to have presupposed, both in their criticisms of their predecessors and in their directives for the future, the truth of certain substantive and controversial philosophical theses." [Rorty, 1967, p.1]

[77] "A philosopher who has idiosyncratic views on criteria for philosophical success does not thereby cease to be accounted a philosopher (as a physicist who refused to accept the relevance of empirical disconfirmation of his theories would cease to be accounted a scientist)… Confronted with this situation, one is tempted to *define* philosophy as that discipline in which knowledge is sought but only opinion can be had. If one grants that the arts do not seek knowledge, and that science not only seeks but finds it, one will thus have a rough-and-ready way of distinguishing philosophy from both. But such a definition would be misleading in that it fails to do justice to *the progressive character of philosophy."* [Rorty, *Ling Turn*, p.2; first emphasis in original, second added]

endeavor. He avoids the issue of defining a goal for philosophy by suggesting that we consider 'progress' as simply meaning "movement toward a contemporary consensus." [Rorty, *Linguistic Turn*, p.2]. Besides, he says, the more interesting historical question to consider is what criteria philosophers themselves have used to identify progress within their enterprise, and how the pursuit of such goals has led to change within the field.[78] This proposal falls perfectly in line with the other suggestions outlined in his 1984 essay. Together, I think, these provide a broad and sensible platform for investigation of how philosophy has developed alongside science in the past two centuries. They also go much further toward the goal of providing a methodology for the history of philosophy than Rorty's co-option of Kuhnian terminology (described in section 2.2.1).

2.4.3   Cunningham and science vs. natural philosophy

Yet another perspective is provided by Andrew Cunningham's work on the disjunction between modern science and its predecessor field, natural philosophy. Cunningham's main object of disdain is the frequent historical ascription of the term 'science' to activities that he claims predate its use as a meaningful referent.[79] Unlike many other of the commentators already reviewed here, Cunningham is insistent that it is inappropriate to use present-day categories to describe past work as scientific.[80] This

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[78] "It is more interesting to see, in detail, why philosophers *think* they have made progress, and what criteria for progress they employ… Uncovering the presuppositions of those who think they have none is one of the principle means by which philosophers find new issues to debate. If this is not progress, it is at least change, and to understand such changes is to understand why philosophy, though fated to fail in its quest for knowledge, is nevertheless not "a matter of opinion"." [Rorty, *Ling Turn*, p.2; emphasis in original] Rorty then proceeds to apply this principle to a discussion of linguistic philosophy (logical empiricism).

[79] "[W]hen we set out to study the history of science, are we properly equipped to identify *science* in the past in order to study it? When we decide to put some episode into our account of the history of science, have we identified it correctly: *was it science* in any meaningful sense?" [Cunningham, 1988, p.365; emphasis in original]

[80] "[W]hat is actually happening is that we – the historians – are the active agents: that we are actually taking to our investigation a ready-prepared set of finding guides to identify past science… It is our concept of 'what science is' which is going to determine all the history that we write in our discipline… We need to distinguish between on the one hand that set of *assumptions and techniques by which* we find out about the past (let us call it

approach, he says, obscures a set of assumptions that we cannot safely rely upon, including the authority of science as a means of describing the world and the objective nature of scientific claims.[81] In his view, these are indicative of a reification of science as a particular kind of body of knowledge.

In place of this approach, he proposes that we focus intently on science as a particular human activity being practiced in a particular historical time-frame.[82] Only then can a properly historical viewpoint on science – as distinct from other, perhaps similar, human activities – be obtained. To this end, Cunningham's specific recommendation is that we view science as a game-structure.[83] As such, he suggests that science will have an identifiable set of characteristics; he lists six features of games that he also contends will prove applicable to science:

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our 'historiography') and, on the other hand, what we use them to find out, i.e. *what happened in the past.*" [Cunningham, 1988, pp.366-367; emphasis in original]

[81] "[I]t is because we live in a world which has been profoundly structured by the achievements of science and scientists, that we treat it as 'natural' that science is the authentic way of looking at the world… [W]e also take for granted a certain kind of claim about the nature of science: that it produces 'objective' findings, and hence that it is itself 'objective'. It is in this that its specialness supposedly consists." [Cunningham, 1988, p.368]

[82] "[If, as I contend,] the practice of science is a human activity, wholly a human activity, and nothing but a human activity, then we should hardly be satisfied with setting out to produce history of science which merely makes just some concession to this… The thing of which we [typically] write is science – taken not as a human practice but as something else, such as a series of discrete 'ideas', or as a 'body of knowledge'. Our usual historical theme is the career of science over time… There is nothing in principle wrong with this turning of human activities into things other than human activities. The problem comes when we mistake the one for the other… Conventionally, we have treated science as if it is some kind of material object, and we have given it the history over time appropriate to a material object… But science is not a material object. It is a human activity. It therefore deserves the history appropriate to a human activity… [W]hat we should be exploring and trying to reconstruct is human activity in investigating Nature – even including asking why man should ever have seen (or demarcated) 'Nature' as a thing separate from himself and needing investigation." [Cunningham, 1988, pp.370-372]

[83] "I will be claiming that the practice of science is just like a game. In the same breath I must add that in making this parallel I am not seeking to disparage science, its practitioners or its findings in any way: my claim is that science is 'just like' a game, not that it is 'just' a game (deadly serious though many games are)." [Cunningham, 1988, p.373]

If we wanted to draw up a simple check-list of some of the features typical of games, in order to compare them to science, we might settle for the following:

(i) They are structured and disciplined ways for man to behave in, invented by man, and played only by man.

(ii) Every game has a point, it has rules, it has procedures and conventions. In playing a game we put all these into practice at the same time.

(iii) A given game is something that (at a given moment) one is either playing or one is not; and if you are playing a particular game you know you are doing so – because that is part of what playing a game is.

(iv) The identity of any game is not affected by which particular individuals are playing it at any time, nor how well or badly.

(v) The experts on the identity and rules of a game, and the judges of competence at it, are those skilled in the game.

(vi) Any game must have been invented before it can be played.
[Cunningham, 1988, p.375]

We might take issue with any or all of these presumed characteristics in principle, but for present purposes it will be more relevant to examine how Cunningham deploys them in actual practice.

The two proposed features that he emphasizes for his own analysis are the third and sixth, the notions that science (like a game) is effectively defined by the intentionality of practitioners and that practice of it must necessarily postdate its codification. Cunningham intends both of these to be taken in a very strong sense. That is, he will exclude from the regime of science both any activity that was not explicitly identified by those pursuing it *as* 'science' and also any activity that preceded a recognizable moment of invention of 'science' *as such*. Note here that Cunningham is not being intentionally obtuse – he recognizes full well that the name 'science' has had a referent since ancient times (as the *episteme* of the Greeks and *scientia* of the Romans) and also that the invention of what we now call science occurred in steps and in an evolutionary manner from other activities. To be more precise about his claim, he wants to reserve the term for that modern activity that is pursued by people called 'scientists'

working under a set of procedural assumptions that are roughly congruent with those of today.[84]

Having mapped out this claim on Cunningham's part, I would add that it is not just the set of six features he has used that we could take issue with, but also his specific notion of science itself. It wanders quite close to a new kind of essentialism, ignoring changes in the character of science since it was conceived as such and relying heavily on a feature that is nominal at best (the explicit use of a term to designate an activity).[85] However, this may be more of a feature of his rhetoric than his intent, as will become more clear as we consider the particular historical case he is trying to make using this theoretical construct.

Cunningham's worry is this: Much of what we now consider to make up the history of science was in fact , he says,a part of quite distinct enterprises properly called 'philosophy' or 'natural philosophy', and a conflation of these obscures the intentions of practitioners and distorts history.[86] Furthermore, the relationship between natural philosophy and science, as intentional activities, is one of supercession – the latter strictly postdates the former, and emerged from it as a sort of mutation.[87] That is, the rules of science were based upon, and replaced, the rules of natural philosophy but were in part different from it. Furthermore, this transition reflected a broader shift in the intellectual landscape that involved a whole host of disciplinary fields, so that a new mapping of

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[84] In a nutshell, he wants science to postdate the work of Whewell, although there is more to it than just that, as we will see.

[85] Case in point: I take it that 'draughts' and 'checkers' are the *same game*. In fairness, Cunningham begins to address this issue himself, as shown by the following remarks: "I want to stress that my whole argument here is built on something other (and stronger) than on the usage of particular *words*: my case does not stand or fall on whether particular past people used the term 'science' (or *scientia* or an equivalent). My argument is about certain human *practices* and their intentionality." [Cunningham, 1988, pp.384-385; emphasis in original]

[86] "The general enterprise that most people in the past would have usually described themselves as engaged in when they produced what we normally take to be their 'scientific' findings, was either *Philosophy* or *Natural Philosophy* (which for many centuries was treated as a large branch of Philosophy)." [Cunningham, 1988, p.379; emphasis in original]

[87] "Men did not start doing Science until they stopped doing Natural Philosophy. They did not start doing Science until they had invented it as an activity to do." [Cunningham, 1988, p.383]

interrelationships among these fields became required.[88] Cunningham identifies the salient difference between the two enterprises as being that natural philosophy was a specifically Christian enterprise, with subvariants dependent on religious denomination, while the science that succeeded it was and is atheistic (in the sense of *setting aside* religious commitment).[89] Such differences, Cunningham contends, matter very much to the history of intellectual inquiry, and he uses his model of science as game-like activity to reinforce this point.

For the case at hand in my work, two aspects of Cunningham's work emerge as particularly significant, one less developed than the other. First, and briefly, he alludes to a fundamental difference between the activity of philosophy (full-stop - not *just* natural philosophy) and that of science. The former, he seems to assume, is contemplative while the latter is active.[90] This off-handed distinction of essences is, again, problematic as an in-principle claim, but does provide a perfectly good issue for historical investigation. Unfortunately, Cunningham does not follow up on this contention, nor on the related

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[88] "I am not implying by this that the new Science comprised (new versions of) all and only the disciplines which had constituted Natural Philosophy, for it is obvious that several disciplines which had been considered part of Mathematics were also now put under the umbrella of Science. But it would also be premature of us to assume that the pre-Science mathematics and its disciplines had the same identity as the later Science versions." [Cunningham, 1988, p.383, note 10]

[89] "[P]erhaps the single greatest difference between Natural Philosophy and Science is that Natural Philosophy was an enterprise which was about God; Science by contrast is an enterprise which (virtually by definition) is *not* about God." [Cunningham, 1988, pp.383-384; emphasis in original]

"[P]articular religious attitudes went into *constituting* each and every variety of natural philosophy, because natural philosophy was itself about God and His universe. The religious commitments of certain people led them to engage actively in natural philosophy in the first place (beyond what they were taught in the universities), and the *particular* forms of their religious commitments led them to pursue it in particular ways." [Cunningham, 1991, p.385; emphasis in original]

[90] This is indicated within the following passage where he reiterates the natural philosophy vs. science distinction:

"[O]ver and above any other defining feature which marks natural philosophy off from modern science – for instance, that *it was philosophy, and therefore primarily for the purposes of contemplation rather than action* – natural philosophy was about God and about God's universe. Indeed, this was the central pillar of its identity as a discipline, both with respect to its subject-matter and to its goals, its purposes, and the functions it served. This is what, more than anything else, distinguishes it from our modern science." [Cunningham, 1991, p.381; emphasis added]

expansion of his concerns to include theology as a third distinguishable field alongside science and philosophy. Nonetheless, it will be useful to keep such possible institutional distinctions in mind.

Cunningham more completely develops a second relevant point, namely the particular features of the birth of what he is willing to call modern science in the first half of the nineteenth century (precisely my period of interest).[91] He fully admits that this depiction is an interpretation rather than an established fact of the matter, but contends that this is a feature of *all* history. The value of his interpretation, he claims, is that it provides a more comprehensive and nuanced narrative than the conventional ones he has criticized. In addition to the already-discussed metamorphosis of natural philosophy into science, he attributes to the birth of science a particular intentional process that helps explain why he has had to raise procedural concerns in the first place. Specifically, Cunningham's contention is that science *was* – in a very strong sense – invented, and invented in such a way as to enact the erasure of natural philosophy.[92] Put another way,

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[91] "Our historiographic concern…has brought to light an historical event of the period c1780-c1850, which I am calling 'the invention of science'. But is it an artefact or a fact: is it merely a mirage given visibility by this historian's procedure, or did it actually happen?… [This] is as incapable of proof or disproof as is our customary view of science as either having been practised somewhat intermittently for centuries but in a cumulative and progressive way, or as something (or things) which became increasingly and progressively visible over time. All one can present…is a historical story presented to be as convincing as possible… Presumably an historical story is the better for explaining or accounting for more. Now, the finding that science was 'invented', and in this particular period, cannot be explained within our conventional portrayal of the past of science: indeed, it contradicts it." [Cunningham, 1988, p.385]

[92] "[L]et us here take it as fact that science was indeed invented in this period [c1780-c1850], and that a particular claim – that it produced 'objective' knowledge, knowledge independent of men – was made about it… It should now be possible to see why people might first have introduced the 'whiggish' tradition of conceiving and writing the history of science, and also why the invention of science might have become historically 'invisible'… Writing the history of science was an early nineteenth century innovation (as it could only be, if the practice itself had only just been invented). The inventors of science and their immediate successors unselfconsciously rewrote the past in a way which showed themselves to be the heirs to a grand tradition… The most general form that this history-of-science writing took was to write histories which were in practice actually making novel *assertions* about where the 'natural' subject-boundaries of knowledge now lay: for instance, histories of the 'inductive' sciences, of the 'exact' sciences, or histories of 'biology', of 'geology', or of 'physics'; sometimes too people

conventional history of science – that depiction which Cunningham has argued against – was invented in conjunction with the invention of science itself. It is therefore unsurprising that natural philosophy *per se* has remained largely uninvestigated – it was not just replaced by science but buried by it, if we are to believe Cunningham. This claim has a two-fold significance here. On the one hand, it constitutes a strong historical claim for the initiation of one characteristic of the Kuhnian scientific paradigm. That is, Cunningham's case supports the view, developed in *SSR*, that science is distinguished by its deliberate concealment of its own past. On the other hand, it bears importantly on questions of the science-philosophy relationship. *If* we believe Cunningham that science is an invention of the early nineteenth century, and one attended by a wholesale rearrangement of intellectual categories that did not conserve boundaries or affiliations, it follows that *philosophy too must be an invention of the early nineteenth century* in interesting and non-trivial ways. That is, it would not merely be significant that disciplinary philosophy, as a profession residing within academic settings, dates from this period. In addition, the identity of philosophy itself – as a specific human intellectual activity – must have been constructed anew at this time. I take it that both these points will be important to keep in mind throughout the development I will pursue.

2.4.4 Ross and some final views on the history of science and philosophy

At this point, it might appear that I have said all that could be said about the dimensions of the issue at hand. However, there remains more to consider. The next section will address a significant lacuna in my methodological development, turning away from academic history to address some counsel we might take from recent work in the sociology of science. Before moving on to that task, though, it will be worthwhile to give a brief nod to one standard review of the science-philosophy relationship available

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wrote histories about the development over time of one or another aspect of the 'method' of science. But, in the very process of writing such histories of science, our nineteenth century historians gave science itself a new identity. That is to say, they presented what was a *human practice* as if it was a series of *autonomous concepts*." [Cunningham, 1988, p.386; emphasis in original]

in the *Companion to the History and Philosophy of Modern Science* [1990]. This will provide an opportunity to sum up and highlight some broadly recognized issues.

George Macdonald Ross' contribution to that volume, simply entitled "Science and Philosophy", breaks the topic down as follows: First, he addresses etymological concerns about the development and interaction of the terms by which these fields have been designated. Here, a long-standing distinction – beginning with *episteme/sophia* and *sapientia/scientia* and proceeding to the present designations – is developed, noting the fluid boundary separating the two over time. Next, he turns to institutional interactions, focusing primarily upon the conflicts over authority between universities and scientific societies in the seventeenth century. Then, recognizing the tension between the long-standing dipolar interaction between the fields, he examines in turn attempts, on the one hand, to demarcate the two, and on the other, to characterize their continuity. Each of these enterprises has a long-standing history and the two viewpoints have significant overlap. However, very broadly speaking. Ross identifies the demarcationist camp as comprised primarily of those we might call 'empiricists' and the continuist camp as including mostly 'rationalists'.

Ross' overview of the issues adds little in the way of historical detail to the problem, but does provide a handy framework for considering relational questions in terms of language, institutions, and practical characteristics indicating points of contact and divergence between science and philosophy. On one specific historical point, though, Ross account is perspicuous and interesting. This, worth noting in full, is as follows:

> The Scottish Enlightenment of the mid-eighteenth to the mid-nineteenth centuries provides a striking instance of how a continuity between science and philosophy can be preserved at the level both of practice and of education. A number of illustrious Scottish philosophy professors, such as Thomas Reid, Adam Ferguson, and Dugald Stewart, started their careers as teachers of science or mathematics; and the main thrust of their work was to develop a methodology and framework of concepts common to science and philosophy. Reid, for example, sought to refute scepticism by means of a number of *a priori* principles basic equally to science and to philosophy, while at the same time recognising that the empirical methods normally characteristic of natural science could equally be applied to traditional problems of mental philosophy. Their general emphasis was on epistemological issues which arise from science, such as those relating to

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causality and to the nature of scientific method, and some scholars have seen this as having had a crucial influence on the development of British physics as a whole during the relevant period. The debate between Sir William Hamilton and William Whewell in the 1830's highlights the Scottish resistance to the Cambridge ethos of drawing a sharp line of demarcation between the emergent natural sciences and traditional arts subjects; and the eventual erosion of the Scottish ideal of continuity is one of the sadder episodes in the history of education. [Ross, 1990, p.812]

There are two major points to be made regarding this passage. On the one hand, this is a ringing endorsement of the present project. A neater encapsulation of the reasons why one interested in the relationships between science and philosophy might turn to early nineteenth century Britain is hard to imagine. Furthermore, I suggest, the relative absence, from Ross' enumeration, of later figures such as Thomas Brown, James Mill, and Alexander Bain only accentuates the need for further investigation in this area. On the other hand, Ross tows too close, I think, to a common set of presuppositions about the role of philosophy with respect to science. We have already seen, in Chapter 1, Larry Laudan's claim that the 'action' in nineteenth century philosophy centered on methodological issues. I take exception to this view again here, where it is advanced more closely to my specific concerns. While it is certainly true that this area represents one significant point of contact between science and philosophy, I will try to show in later chapters that there are other (even, perhaps, more important) aspects to be taken into account, especially within the domain of Scottish and English work on the mind during the period Ross identifies. That said, I will turn now to the last element of my methodological development.

2.5   Sociological insights into the science-philosophy relationship

Thus far, my search for methodological insights has effectively been limited to the traditional regimes of history and philosophy. Thus, the approaches I have identified have been primarily historiographic and conceptual in nature, focusing on questions about which aspects of science and philosophy are most salient in different periods and what the fundamental distinctions between the two enterprises might be. However, such approaches leave significant holes in the fabric of the story. In particular, we have heard

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relatively little about the social aspects of science and philosophy so far.[93] I think several specific elements are significantly absent from the account being developed here. These include (a) the methods used by scientists and philosophers in establishing and maintaining distinctive niches in the broader society, (b) the interactive processes that have characterized these two institutions in particular, and (c) the 'internal' characteristics and dynamics of the two fields (in particular, that of philosophy). While the need for including such elements has perhaps become conspicuous by attending to the work of historians and philosophers of science, we have thus far only raised questions and provided little in the way of answers. To patch these holes, I suggest that we must turn to concepts available in the sociology of science and the sociology of philosophy.

The sociology of science is, of course, a huge multifaceted field of study, and I could no more address all of the relevant concerns raised in it than I could address all of the possible historiographic elements of the subject in question. While the sociology of philosophy is less developed as a professional enterprise, its dimensions – in principle – are just as numerous. I will thus concentrate here on two particular approaches that speak directly to the three conspicuous lacunae I have already identified. First, I will discuss the important work of Thomas Gieryn on the boundaries of science and their rhetorical and institutional enforcement. This, in turn, will open up a number of related concepts – including those of 'boundary work' and 'boundary object' – that will assist in treating the interaction and co-development of the scientific and philosophical enterprises of the early nineteenth century. Second, I will turn to Randall Collins' recent opus, *The Sociology of Philosophies: A Global Theory of Conceptual Change* [1998]. Collins' extensive treatment of the social past of philosophy serves two purposes. As a general contribution, it helps to map out a new field of inquiry – specific sociological attention to philosophy – that also includes a handful of other scholars of note. More specifically, it provides an explicit argument for distinctive characteristics of the philosophical enterprise comparable to those forwarded by Kuhn and others for science. Taken together, the work

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[93] To be sure, Kuhn, Cunningham, Rorty and others have made frequent use of analytical criteria that we might term social in nature, but these allusions and insights are haphazard and anecdotal ones. None of the figures addressed so far has taken anything like a systematic approach to identifying possible social determinants of the progress of science and philosophy.

of Gieryn, Collins and associated social scientific scholars fills in several important gaps in the historiographic apparatus I have been constructing.

2.5.1   Gieryn and the notion of boundary work

Thomas Gieryn's attempts to characterize the progress of science through the examination of rhetorical practices have spanned the past twenty years of his career. I will highlight salient aspects of this work by reference to two contributions: the seminal article "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists" [1983], in which he first introduced his notion of boundary-work, and his 1995 review – simply entitled "Boundaries of Science" – of the current usage of 'boundaries' in the social analysis of science in the *Handbook of Science and Technology Studies*. The former provides a program statement for a particular approach to considering science as social practice, while the latter considers more recent extensions of this work by Gieryn and several like-minded scholars.

For Gieryn, the most important distinguishing feature of science is its ongoing and flexible struggle to maintain for itself a position of social authority. This campaign can be discerned most clearly in the rhetorical practices of advocates of science, which constitute what he dubs "boundary-work" – the ideological policing of a domain of scientific influence. To elucidate such practices, Gieryn "examines both *style* and *content* of professional ideologies of scientists." [Gieryn, 1983, p.782] This approach, he believes, provides insight into science because science is, foremost, a fundamentally social activity. As such, like all social activities, it must compete for resources and influence within the broader society and ideology is its chief instrument in this process.[94]

Recognition of the role of ideology in science, however, is insufficient to explain very much in Gieryn's view. Most considerations of the scientific ethos gloss over the fundamental question of how science achieves the degree of ideological leverage that it

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[94] "[I]deologies are "social levers" or "weapons" used by groups to further their political or economic interests amidst universal struggles for power and advantage." [Gieryn, 1983,p.782] Note here that Gieryn implicitly endorses a fairly straightforward Marxist view of social conflict.

does.[95] Even when this question arises, typical explanations simply incorporate reference to the 'objectivity' or 'utility' of science as ultimately self-justifying – scientific ideology is effectively validated on its own terms. But these accounts miss the mark for (at least) two reasons. First, they leave unquestioned the reasons why we might accept such claims in the first place. Second, they ignore the differential strategies that have been employed historically to prop up scientific authority relative to other competing social domains. The first of these objections is, I think, fairly self-evident, the second requires a bit more illumination.

Gieryn provides a number of examples of the contingency of the approaches used to justify scientific authority with respect to different possible competitors. In the case most closely related to my own concerns, he details how John Tyndall, Victorian popularizer of science fought rhetorical battles to shore up the reputation of science versus institutional opposition presented by engineering and mechanics, on the one hand, and religion, on the other. With respect to the former, science faced struggles for economic resources. Tyndall's arguments for a reallocation of funds to disciplinary science centered on the notions that science – unlike mechanics – was theoretical, systematic, and devoid of self-interest, and represented both a foundation for practical developments and a self-justifying cultural achievement. With respect to religion, by contrast, science faced struggles for cultural prestige and control over educational institutions. Tyndall's response to this situation was to present science as an empirical, practical, skeptical, and objective enterprise. The differential strategies presented in these two cases demonstrate the specificity involved in carving out a space for science with respect to particular antagonists.

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[95] "A common thread runs through…descriptions of the relationship between science and ideology: all *assume* that science carries its own intellectual authority… Yet none of the perspectives asks *how* science acquires that intellectual authority. Part of an answer to this large question will come from investigations of professional ideologies of scientists: What images of science do scientists present to promote their authority over designated domains of knowledge?… [H]ow do scientists construct ideologies with style and content well suited to the advancement or protection of their professional authority?" [Gieryn, 1983,p.783]

In his 1995 review, "Boundaries of Science", Gieryn goes into further detail concerning the study of ideological boundaries and the value of this methodology over more conventional approaches to science. In particular, he calls Kuhn to task as an essentialist on two counts: First, because of the degree to which Kuhn defends the possibility of objective demarcation of science. Second, because Kuhn's work minimizes the significance of rhetorical-ideological practice as a constitutive element of science.[96] While it is not clear that Gieryn's attack on Kuhn is entirely justified, he does raise some valid concerns (also raised elsewhere by others) about the empirical accuracy of Kuhn's standard account.[97] For example, Gieryn notes that detailed studies of the reception of new theory have called into question the extent of consensus that is ever achieved in science. Issues including the limits of recognized membership in a scientific community, the dynamic changes that occur in opinions of scientists within a community, and the interpretive flexibility involved in determining actual cognitive content of theory combine to challenge the accuracy of Kuhn's model of unitary paradigms.

Given the range of options open to scientists in marking out and defending their territory – as seen in the example of Tyndall above – Gieryn ultimately asks whether there can be anything actually characteristic of science, and only science, at all. Science, he instead suggests, is something like a blank symbol of authority.[98] Attempts to 'define'

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[96] "Kuhn is an essentialist not only because he offers paradigmatic consensus as a demarcation principle but because he dismisses as unimportant, merely "semantic," those questions that animate constructivist studies of boundary-work… What Kuhn chose not to consider is that the degree of consensus in science itself might be a matter of interpretation, negotiation, and settlement – by scientists and sometimes other involved bodies." [Gieryn, 1995, p.403]

[97] In fairness to Kuhn, ideology is far from absent in his picture. We recall from earlier in this chapter that Kuhn does speak of the role of policing consensus in science, and speaks of science as a self-judging enterprise. Furthermore, Gieryn's phrasing in labeling Kuhn an essentialist in the previous note is infelicitous, amounting to a denunciation on the grounds that Kuhn ignores the questions Gieryn finds interesting. This, in itself, is not an argument.

[98] "Pragmatic demarcations of science from non-science are driven by a social interest in claiming, expanding, protecting, monopolizing, usurping, denying, or restricting the cognitive authority of science. But what *is* "science"? Nothing but a *space*, one that acquires its authority precisely from and through episodic negotiations of its flexible and contextually contingent borders and territories. Science is a kind of spatial "marker" for

science will always fail because science distinguishes itself among other social domains in a purely pragmatic and occasion-specific manner. Having thus argued for the abandonment of demarcation, Gieryn proposes the boundary-work viewpoint as providing an alternative means to make meaningful statements about what distinguishes science from other activities. He uses a cartographic metaphor to make his case – doing boundary-work is like making maps.[99] When scientific ideology is put forward in a conflictive social situation, this occurs in such a way as to highlight specific useful features relevant to the occasion. Of course, such gestures are not random but constitute a catalog of potentially effective strategies for maintaining scientific authority.[100] However – and this is Gieryn's main insight – the picture of science that we would glean from a survey of boundary-work episodes would be one of inconstancy rather than some characteristic set of attributes.[101] Science, for Gieryn, is an empty space, and it is to this chameleon-nature that its success can be attributed.

Other scholars have followed in Gieryn's path, examining science on its social boundaries. Perhaps the most significant extension is that of Star and Griesemer [1989] who reorient the issue toward interaction rather than separation. Here, the divalency of the notion of boundary emerges in an interesting way – boundaries are both walls and interfaces. While Gieryn has been primarily concerned about how science maintains its

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cognitive authority, empty *until* its insides get filled and its borders drawn amidst context-bound negotiations over who and what is "scientific." [Gieryn, 1995, p.405]

[99] "'Unique' features of science, qualities that distinguish it from other knowledge-producing activities, are to be found not *in* scientific practices and texts but in their representations. Boundary-work stands in the same relationship to what goes on in laboratories and professional journals as a topographic map to the landscape it depicts; both *select* for inclusion on a cultural or geographic map those features of reality most useful for achieving pragmatic ends (legitimating authority to knowledge claims or hiking through wilderness)." [Gieryn, 1995, p.406]

[100] "[T]hat idea could easily be exaggerated into a silly conclusion that…there are no patterns at all from one episode to the next. Scientific practices and antecedent representations of it [*sic*] form a *repertoire* of characteristics available for selective attribution on later occasions… [S]ome maps of science are easier than others to defend as bona fide representations." [Gieryn, 1995, p.406]

[101] "A survey of historical instances of boundary-work would turn up a science with no consistent shape, no necessarily enduring features… It is precisely the emptiness of science – a space waiting for edging and filling – that best accounts for its historically ascendent cultural authority." [Gieryn, 1995, p.407]

own territory, Star and Griesemer concentrate on how sciences share territory with other social domains.[102] They deploy the notion of a "boundary object" as something – or someone – facilitating interaction between different social fields. Effectively, this notion presages – and generalizes – Galison's [1997] treatment of 'trading-zones' and 'creoles' in science. Boundary objects serve as mediating totems that bind together otherwise disjunct communities.

Gieryn, reviewing his own work and that of others like Star and Griesemer, suggests that the socio-cultural usage of boundary-terms has made several significant contributions to science studies. He realizes that the notion of 'boundary' by itself is not among them – the concept of disciplines, fields, social domains, *etc.*, having boundaries is almost unavoidable, and certainly predates his own work and transcends his own field. Nonetheless, he proffers four more specific ways that attention to 'boundary-work' and 'boundary-objects' have supplemented our understanding of science.[103] First, he highlights the use of cartographic metaphor in the boundary-approach as distinctive.[104] Next, this approach accentuates the issue of how a place of *authority* for science is

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[102] "How do people from distinctive social worlds – with commitments to activities and interpretations different from those across the border – come together to get something done?… The useful concept of "boundary object" begins an answer, defined by Star and Griesemer (1989) as objects "which inhabit several intersecting social worlds and satisfy the informational requirements of each of them… Boundary objects may be ideas, things, people, or processes; the requirement is that they be able to span boundaries separating social worlds, so that those on either side can "get behind" the boundary object and work together toward some goal." [Gieryn, 1995, pp.414-415]

[103] "What do these anthropologies and histories of cultural classification add to our boundaries-of-science soup? Four ingredients, mainly: First, cartographic metaphors offer a robust language for thinking about relations among cultural phenomena such as science… Second, this perspective does not privilege "what-has-become-science"…but instead compels analysis of how such a distinctive space was carved out… Third, this orientation shifts attention…[to] the shifting borders and territories of science… Fourth, cultural historians offer help with a befuddling theoretical problem…Why are some characterizations of the borders-and-territories of science more portable through space and time than others?" [Gieryn, 1995, pp.419-420]

[104] This is somewhat incongruous. I would argue that, like the notion of a boundary, recourse to the notion of a map is virtually inevitable and far more broadly applicable, and applied, than Gieryn suggests. It is perhaps defensible that the way he uses the metaphor – drawing more attention to the 'key' of the map than its features, as it were – is a novel contribution.

achieved. Also, it highlights the contingency and variation of the *maintenance* of such authority. Last, it leads us to ask *why certain strategies* used for this end are more stable, and more socially powerful, than others.

For present purposes, this style of work has value for several reasons. I am examining the relationship between two social fields (*AKA* disciplines) – philosophy and science – during a time period when the notion of science (as an enterprise with distinctive practitioners called scientists) was only just emerging. Furthermore, the process of establishing this science had an effect – if not parasitic, than at least transformative – on the bordering domain of philosophy. Thus, I suggest that the notions just detailed have relevance to the following questions: (1) Given that science was attempting to achieve a certain authority, and philosophy to maintain a certain authority, what distinctive ideological strategies were used to differentiate them and negotiate their relative positions? (2) Viewing the two as distinct but interactive, what manner of boundary-objects – common points of reference and shared subjects and objects – did they have in this situated moment? (3) Viewing the two as a dipolar allied unit, what common boundary-work did they engage in to maintain a common territory against other third-party contestants for social prestige (religion, practical enterprise, *etc.*)?

Furthermore, in the opposite direction, my case provides a relatively hard test of Gieryn's contentions about the distinctive character of science as empty authority-seeking space. While arguing vigorously against essentialism in science studies, Gieryn walks a fine line in suggesting that science *is* an otherwise empty space denoting cognitive authority. Not only is this very close to a demarcation, but it is also a claim that might be applied to alternate social fields. In particular, it would seem that philosophy could be characterized in exactly the same way. This recognition reinforces the significance of examining the science-philosophy relationship, and also provides a convenient occasion to turn my discussion from social studies of science to social studies of philosophy.

2.5.2 Collins and sociology of philosophy in the long view

A recent review by Kurzman and Owens [2002] points out that social studies of intellectual activity have "had a checkered history" – successively surfacing and submerging over the past century as a distinctive field of study. They suggest that this

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domain of sociological inquiry has tended to divide along three distinctive paths: views of intellectuals as a separate class among others in society; depictions of intellectuals as embedded within the elite class; and portrayals of intellectuals as transcending class-distinctions in the pursuit of autonomous ideals. This taxonomy of the problem in terms of class is a useful beginning in tackling the problem of how to view the social position of philosophy. However, for present purposes, it may be more valuable to turn instead to a set of more local questions about the character of the philosophical enterprise.

If we are to understand philosophy in a manner analogous to the picture of science that has been emerging here, it would seem most useful to ask about the practices of inclusion and exclusion (of both persons and subject matters) employed in philosophical circles; their social locales (institutions, resources, *etc.*) in particular periods; their interactive strategies with other fields; and their distinctive identities as projected both outward to the broader society and inward to practitioners. The literature in sociology of philosophy shows a small but growing set of contributions in these areas.[105]

Here, I will concentrate primarily on Randall Collins' *The Sociology of Philosophies* [1998], which provides perhaps the most comprehensive treatment of the subject and also the one most congruent with the set of issues just proposed. Owing to the scope and sheer size of Collins account, I will have occasion to use his 2000 précis of this work – prepared for inclusion with a set of commentaries in the journal, *Philosophy of the Social Sciences* – as a point of reference in what follows.

Collins argues against four possible explanations of intellectual activity, which he characterizes as the positions that (1) "Only ideas beget ideas", (2) "Individuals beget ideas", (3) "Culture begets itself", and (4) "Everything is fluid". The first of these positions, he says, constitutes a reification of ideas, ignoring the nature of thought and ideas as communications. The second amounts to a form of hero-worship, in which the social situation of, and the social ties between, intellectuals are inappropriately minimized. Again, he maintains, the communicative process – situated within social traditions of thought – is the real site of the development of ideas. The appeal to cultural

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[105] These include Coser [1965], Ringer [1969], Fabiani [1988], Heilbron [1995], and Collins [1998]. See Kurzman and Owens [2002] for a considered review of the field of social studies of intellectual activity, which is *not* co-extensive with the sociology of philosophy but which effectively subsumes it.

explanations, third, is also a sort of reification, misinterpreting an *outcome* of social processes as the *explanation* of them. That is, social interactions - in forming ideas - also form culture. Last, while Collins acknowledges the locality and contingency of specific social forms, and thus also specific traditions, he argues that these facts do not necessitate the abandonment of a search for general patterns in social history. The fluidity of society is not total, but rather certain enduring relationships are discernable.[106]

In the domain of philosophy, the patterns that Collins observes revolve around the generation and debate of ideas. First, he suggests a stable role for the philosopher in forming systems of ideas removed from their particular contexts of origination.[107] These idea systems take on, within the intellectual community, a role as a sort of sacred objects – not insofar as they are unquestioned, but rather as they are held as an ultimate value.[108] Within such intellectual communities, 'interaction rituals' develop that bind together practitioners toward a common focus and maintain a high level of 'emotional energy' toward the pursuit of better ideas.[109] For intellectuals (*i.e.*, philosophers – Collins uses the terms interchangeably), the communities they inhabit are maintained by traditions of speaking, writing, and debate.[110] Participation in such rituals, centered on particular systems of ideas, thereby generate what Kuhn would term a disciplinary matrix. However, it is not the particular ideas of successful intellectuals that are of primary value in determining their status, but rather their ability to tap into the emotional energy patterns of former practitioners.[111] Furthermore, the intrinsic character of the intellectual

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[106] Here, he and Gieryn are in perfect accord.

[107] "Intellectuals are people who produce decontextualized ideas and regard them with the same kind of seriousness and respect as Durkheim noted that believers give to the sacred objects of religion." [Collins, 2000, p.159]

[108] That is, it is not particular ideas that are sacred, but the idea of ideas.

[109] We might say, toward the pursuit of truth.

[110] Intellectuals interact in a community of speech making before assembled audiences and of focused discussion and argument, a community that records its judgments in a cross-referenced chain of words and texts that are given the status of "knowledge" or "truth"…This participation generates feelings of disciplinary or status group membership and respect for distinctively intellectual symbols." [Collins, 2000, pp.159-160]

[111] "Successful intellectuals typically emulate not so much the ideas as the EE [emotional energy] of prior leaders of the field." [Collins, 2000, p.160]

domain limits the number of positions that can be viably held – in other words, the number of traditions that can interact in a given time-frame.[112]

In this model, significant reorganizations of ideas are achieved only by exceptional creative figures. However, their efforts are fueled and guided by the emotional energy they can tap from within their existing tradition, making even this apparently individual effort social in basis. Such creativity is, in Collins' view, rare: He arranges his networks of philosophical tradition into chains featuring major, secondary, and minor figures. Even in societies noted for their intellectual productivity - such as classical and postclassical Greece – only an average of one major philosopher, and perhaps 2 secondary and 10 minor ones, per generation is evident in his scheme. Philosophical communities, then, are closely circumscribed ones. Major philosophers tend to occupy 'nodes' in intellectual networks – positions linked to a high number of other figures, particularly successors following their 'school' – and tend to be the innovators of systems.[113] Also, creativity tends to appear in specific circumstances – when a number of schools interact in a particular time and place, during periods of political and economic change. During such periods, strong positions often split and

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[112] "Opportunities are limited because only some half-dozen positions attract much attention in a given intellectual attention space." [Collins, 2000, p.160]
and
"Among intellectuals, the market for conversations reinforces and reproduces the stratification of core and peripheral participants." [Collins, 2000, p.161]
and
"The law of small numbers: the number of schools of thought that reproduce themselves in the following generations is on the order of three to six: the lower limit because the minimum of argument, two positions, usually generates a third as a plague upon both houses; the upper limit because beyond this, additional positions lose viability and cannot recruit followers to carry their memory to the next generations. When the upper limit is violated, the next generation experiences a collapse and amalgamation of schools." [Collins, 2000, p.165]

[113] As Lamont [2001] has pointed out, Collins' use of network models collapses at least two significantly different dimensions. A network, for Collins, can include relations of direct interaction (teacher-pupil, collegial, *etc.*), substantive reference (either deferential or oppositional), or mere symbolic allusion (again, either positive or negative). The role of these various connections in centralizing particular figures is treated in an undifferentiated way. Later in my own development, I will argue for the separation of *iconic* and *genealogical* aspects of such networking, as well as for two other dimensions of textual structure (*conceptual* and *expository*) in the analysis of philosophical networks.

weak ones unite. Further, creativity is oppositional, so that reversals of position (often successive flops back and forth between stable poles) are common and innovators are often conservative figures resisting the dominant trends.

Collins does not ignore the science-philosophy relationship. He characterizes modern science as "rapid-discovery science" and attributes to it two significant distinctions from philosophy – the achievement of consensus, and the acquisition of device-technologies. This depiction, by itself, is something akin to a concatenation of Kuhn's notion of scientific normality with the current vogue in studies of scientific 'materiality' advanced by Hacking, Pickering, and Galison. However, Collins is painting in much broader brushstrokes than any of these figures (except perhaps a superficial version of Kuhn). Since he is less concerned with the distinctive structure of scientific work than he is with the progress of philosophical traditions, he often presents a picture of science that – in its blurriness – is a mirror-image of that provided for philosophy by historians of science. But this is all well and good, since we have the option here of viewing matters from both sides.

Besides, some of Collins' claims about the long term co-evolution of science and philosophy are instructively suggestive. For example, he ascertains – in the seventeenth century European 'scientific revolution' – a splitting of three distinctive institutional trends: mathematical, natural scientific, and philosophical.[114] Closer to my own concerns, he finds – in the nineteenth century – significant influences on philosophy from scientific disciplinary separation, scientific ideologies such as materialism, and scientific (and mathematical) methodologies.[115]

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[114] "Three overlapping restructurings of the intellectual field: (1) Mathematics becomes a rapid-discovery machine. Descartes raises the level of generality of symbolic notation and trumpets the news of an infallible discovery-making method. (2) Natural science builds from several chains: astronomers, physiologists, chemists, and mathematical mechanics; self-conscious unification of these chains (except for chemists) in the early 1600's. (3) Philosophical revolution: Bacon and Descartes formulate ideology of break with scholastic and classical learning." [Collins, 2000, p.186]

[115] "Metaterritories upon the science-philosophy border. Disciplinary differentiation stimulates philosophy during the struggle for separation, promoting accompanying ideologies such as militant materialism of 1850s. Expansion of science stimulates philosophy by methodological disputes that move into the philosophical problem space. Migration of mathematicians and physicists promotes phenomenology and logical

Overall, for all of the possible hubris of Collins' project (he is, after all, attempting to explain some 3000 years of world intellectual activity in a single work), he nonetheless provides a great deal of solid ground from which to address issues about the character of the philosophical enterprise. Finally, with Collins contribution, we see the composition of a theory of philosophical work that can stand alongside Kuhn's theory of science in terms of scope and specificity. He argues convincingly for a set of enduring and distinctive practices in philosophy that are of the same order as Kuhn's image of scientific paradigms but different enough in detail to provide a significant contrast class as we compare science and philosophy in a particular context.

Of course, Collins is not the only game in town. An equally significant, if more conventional, work in contemporary social studies of philosophy is Johan Heilbron's *The Rise of Social Theory* [1995]. Therein, Heilbron – employing a structuralist picture of 'intellectual regimes' inherited from Paul Bourdieu – examines the coalescence of an autonomous field of sociology in nineteenth century France. This story relies on a fairly traditional set of theoretical constructs, including distinctions between pre- and post-disciplinary work and arguments for the national character of the disciplinary enterprise. However, Heilbron does provide two potentially useful new elements for my own story, one analytical and the other factual. First, he emphasizes – in examining the science-philosophy relationship – the 'co-production' of ideas and institutional interests within specific situated autonomous groups (men of letters, philosophers, scientists, *etc.*) with distinctive established loci (the university, the lab, the coffee-house, the court, *etc.*). This image of both actors and ideas as firmly embedded in a web of contexts is, if nothing else, a useful reminder of the poverty of treating intellectual activity as a free-floating endeavor. More generously, Heilbron provides us a set of practical pointers to the complexity of situating such enterprises within an adequate social history. On the second, factual, contribution, however, Heilbron's work does not follow up on its promise. In a brief comparative move across national contexts, he examines the distinct approaches to social theory taken in nineteenth century Scotland and France. He suggests

positivism; further creativity by countermovements, including ordinary language philosophy." [Collins, 2000, p.192]

that the social environment (as just characterized) of Scotland was more conducive to the early emergence of an autonomous science of society than was that of France. However, his account (lasting only ten pages) falls prey to the all-too-common problem of using "The Scottish Enlightenment" as an unexamined signifier. That is, he presents a picture of the Scottish intellectual scene that is 'black-boxed' beyond historical recognition.

Both Heilbron and Collins add significantly to the overall portrait of science and philosophy interacting in the early nineteenth century. Heilbron's contribution is predominantly one of indicating macro-level structural relationships among ideas, institutions, and social and material resources within a given intellectual regime (philosophical or scientific). Collins provides both a macro-model of the enduringly distinct character of philosophy, as well as a micro-level description of the dynamics that drive the enterprise. Both indicate directions for analysis that were not evident in the perspectives previously examined.

2.6 Summary of methods and approaches

What has this trek through various available models added up to? Rather a lot, I think, if we are concerned with the spirit rather than the exact letter of the methodological 'law.' This section will try – from that perspective – to synthesize the various approaches I have discussed into a viable plan for approaching nineteenth century British associationist thought, and then to identify several specific avenues of advance that will be pursued in the following chapters. Along that path, I will indicate again, in brief, where I have obtained useful tools in my survey of previous work.

Kuhn's *SSR* model provides a valuable starting point. Some damage has been done to Kuhn's legacy by those who have adopted the clothing of 'paradigms' and 'normality' without the sophisticated and specific set of criteria that Kuhn intended to be applied to them. However, Kuhn himself is quite precise about the picture of scientific activity he wants to paint. Insofar as we can take Kuhn in a non-essentialist manner, and consider his set of scientific characteristics (see section 2.1.6) as a set of empirical contentions about particular activities, his work is exceedingly useful. I will take this set of contentions under close consideration in my case, paying particular attention to the

notion that associationist thought can be couched as pre-paradigmatic psychology (or social science, more generally).

Kuhn's depiction of science does not, of course, stand alone. Barker has extended Kuhn's model (following Kuhn's post-*SSR* trajectory) to view incremental scientific change in terms of evolving conceptual systems (section 2.3.1). But Kuhn, Barker, and MacIntyre (section 2.2.4) all appear to suggest the equation of scientific change with conceptual change. We might also, as Hacking and Pickering have suggested, need to attend more fully to material aspects of scientific work (sections 2.3.2 and 2.3.3). Hacking, too, proposes that distinctions need to be made between different scientific enterprises – especially between the natural and social – in terms of the extent to which they 'remake' the world (section 2.3.3). From social studies of science, a differently oriented set of concerns arises. As Gieryn proposes, we may need more emphasis on ideological processes of boundary work in stabilizing scientific fields (section 2.5.1). And, above all, we need to pay close attention to the interrelationships of science with neighboring fields. These might include engineering (Kuhn, section 2.1.2), mathematics (Collins, section 2.5.2), religion, natural philosophy, and natural theology (Cunningham, section 2.4.3), and – of course – philosophy proper. When viewing such interrelationships, we need to consider the aspects that distinguish each field from the other, perhaps in terms of game structures (Cunningham, section 2.4.3), differential rhetorical strategies (Gieryn, section 2.5.1), or identifiable institutional or conceptual features.

With regard to philosophy, as an enterprise in and of itself, we must ask questions analogous to those we have posed about science: What characteristics (material, social, conceptual) does it display in particular situated moments? How does it fit into the broader landscape? What factors play a role in creating such situations? We have seen a variety of interpretations of such issues. Kuhn and others (MacIntyre, Rorty, Collins) have asked in what sense we might regard philosophy, like science, as 'progressive.' Also, as with science, we should view the propagation of tradition as requiring mechanisms such as texts (Wood, section 2.2.3) or networks of personal influence (Collins, section 2.5.2). A variety of specific possible features of philosophy as a well-defined human pursuit have been proposed, some in contradistinction to science and

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some independent of such reference. These include: the notion of philosophy as intrinsically comprehensive in scope and orientation (Kuhn, Rorty, MacIntyre, Sellars); philosophy as inherently conflictive, involving communities of debate and multiple competing traditions, perhaps limited in number to a handful (these same authors, and Collins); philosophy as a pursuit open, unlike science, to its past and perhaps having an explicitly originary orientation (Rorty, MacIntyre); philosophy as non-disciplinary, displaying an open community; and philosophy as either potentially normal or non-normal in Kuhnian terms (Rorty, Heelan, Wood). Collins in particular has proposed a multi-faceted description of the enduring character of philosophy, suggesting we view it as involving interaction rituals and transmission processes parallel to those Kuhn described for science. The historically changing character of philosophy has been highlighted as well, with suggestions that we view philosophy – in the sense we now use the term – as being effectively born *with* science in the nineteenth century or when it became a university-based discipline (Rorty, Cunningham, Ross).

Descriptions of the science-philosophy relationship in the literature are equally rich. Their dissimilarities have been characterized as involving material work vs. purely conceptual work (Hacking), presentism vs. conscious foundationalism (Kuhn, MacIntyre), or active vs. contemplative character (Cunningham). We have seen the contention that sciences are born from philosophy, but that philosophy nonetheless requires reference to science (Sellars). Since the two fields clearly interact, the issue arises as to how they mutually carve up the territory that they share. In some cases, this may be essentially a zero-sum game, with contested matter changing hands during episodes of the reorientation of fields (such as that which apparently occurred in the early nineteenth century). On the other hand, there might be instead changes in scope, perhaps parasitic on other fields or perhaps simply opening up virgin territory, that allow for the expansion or contraction of the two together (as with the acquisition of new knowledge or the draining of authority from the theological regime).

Whether we view the two as competing or cooperating, two large and fairly distinct sets of issues have arisen – one regarding conceptual content and the other regarding ideological content. In the former case, several scholars (MacIntyre, Rorty, Cunningham, Barker) have insisted that a proper view of conceptual change must

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incorporate an incremental model of the evolution of concepts embedded within systems of varying scope. Reorientations within such systems (as in Neurath's boat metaphor) are always partial but – whether 'rational' or not – result in important reorderings of the human intellectual world. The latter issue, regarding the competitions for authority and occasions for cooperation that intellectual fields experience, points instead to the science studies 'boundary' approach. Here, we must take into account the moves made by scientific or philosophical practitioners to establish for themselves a stable social space. This involves not just the practice of 'boundary work' in which we can observe the forwarding of specific authority claims, but also the deployment of 'boundary objects' that form functional interfaces between science and philosophy and between this conjoined pair and other fields.

I take all of the problems just detailed to be issues for historical investigation, and I will investigate them as follows. In Chapter 3, I will open up an investigation of intellectual traditions in nineteenth century British associationism. I will differentiate four species of structure that I will call c*onceptual*, *expository*, *iconic*, and *genealogical*. Conceptual structure refers to the systematic organization of concepts evident in the field and propagating over time – something quite close to Barker's depiction. I will utilize a framework similar to Barker's for investigating how such systems, in what I will argue are three relatively distinct traditions, were reorganized over time to create a synthesis of positions. Expository structure, by contrast, refers to the way in which the texts themselves – in which the conceptual structures are laid out – are organized, which follows a distinct pattern. Expository structure indicates traditions of rhetoric that can be independent of conceptual traditions. Iconic structure refers to the allusions, positive or negative, that are made within texts to other authors, past or contemporary. Such allusions indicate one aspect of the web of influences that Collins uses to analyze philosophical networks. Another such aspect is observed in genealogical structure – the real-world connections of tutelage, friendship, or correspondence that obtain between different figures. I will use these four aspects of intellectual structure to analyze the progress of associationism from eighteenth century beginnings in the work of Hume, Hartley, and Bain through the writings of Thomas Brown, James Mill, Sir William Hamilton, and Alexander Bain. It will also emerge from this discussion that the use of

scientific information in the works of these authors demonstrates an interplay between science and philosophy that is congruent with both Sellars' characterization of the two fields' relationship, and with the notion of such information as a boundary object.

Chapter 4 will turn to a different matter. Paying close attention to issues of intentionality, I will examine how the figures in question articulated the distinctions between science, philosophy and other fields. My primary source material will be the first volume of the journal, *Mind*, founded by Alexander Bain in 1876 as an organ for work on what he termed mental science. It will emerge that *Mind*, too, can be viewed as a boundary object of sorts, and that the work of the contributors explicitly addresses – in a sophisticated way – the very issues about how to characterize the scientific and philosophical enterprises as collaborative but distinct. I will also consider here some issues about the relationship of these two fields to other social arenas, including particularly that of religion, to situate them more fully in their cultural milieu.

Following this development, I will return – in my conclusion to some broader analytical issues about the significance of associationism as a pursuit. I will argue that depictions of philosophy – and epistemology in particular - as primarily concerned, in the nineteenth century, with methodology are impoverished and miss a larger connection between epistemology and ontology that relates to the study of the mind. Further, I will contend that these issues regarding the human mind, dating from the eighteenth century form an important set of background assumptions common to philosophy, the sciences, and in particular science studies. I will also consider the 'end game' of associationism – why the tradition(s), which constituted an active and central domain in nineteenth century British intellectual circles, came to a fairly abrupt end (as evidenced by the production of no new texts). I will argue that the cause was neither the subsumption of such work into scientific psychology, nor the philosophical resolution or discrediting of associationist issues, but rather a dispersion of the energy of this tradition into an array of fields.