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**ECONOMISTS' INTERPRETATIONS AND APPLICATIONS OF THOMAS S.  
KUHN'S THEORY OF SCIENTIFIC REVOLUTIONS**

**Volume I**

**By**

**Thomas Walter Patchak-Schuster**

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**ABSTRACT**

**ECONOMISTS' INTERPRETATIONS AND APPLICATIONS OF THOMAS S.  
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**By**

**Thomas Walter Patchak-Schuster**

This dissertation examines economists' understandings as to what comprises a Kuhnian paradigm and the functions paradigms play under Kuhn's conception of science. It surveys paradigms economists have identified in economics' mainstream, heterodoxy and subfields as well as their understandings of the functions of paradigms in economics.

The work explores economists' understandings of Kuhnian normal science, determinations as to whether economics comprises a normal science, and depictions and normative assessments of normal economic science.

The study examines economists' understandings of what comprises a Kuhnian scientific revolution and their applications of Kuhn's scientific revolution concept to economics' history.

Given the heterogeneity of Kuhn's model of science, economists' selective and multiple perceptions of Kuhn's work and economics, and the diverse, multifaceted character of economics, economists have subjected Kuhn's notions, and the field of economics to which they have applied them, to selective and multiple interpretations. They have offered multiple definitions of a Kuhnian paradigm, disagreed whether paradigms serve to hinder scientists (economists) in their work and as to the applicability of the paradigm concept in economics and provided widely varying

specifications of economics' mainstream paradigm. Economists have also differed in their determinations whether a given heterodox school possesses a paradigm, identified different types of economics paradigms and located paradigms at different levels of the discipline.

Economists differ as to the elements of Kuhnian normal science they highlight, their determinations as to whether economics comprises a normal science, and their depictions and normative assessments of normal economic science.

Economists differ as to the extent and nature of the change effected by Kuhnian scientific revolutions, whether economics has ever undergone a scientific revolution, the changes they see effected by the marginal (utility), Keynesian and other putative revolutions in economics history, and in their determinations whether those changes constitute a scientific revolution.

This dissertation therefore raises questions whether there exists an objective Truth about either economics or Kuhn's work, toward which economists have or will ever converge. Indeed, it provides strong indication that any given understanding of economics or Kuhn's work is best understood as part of a larger matrix of interpretations.

**To Pam**

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## CHAPTER ONE: INTRODUCTION

In 1962, Thomas S. Kuhn, a physicist turned philosopher and historian of science, published a work entitled *The Structure of Scientific Revolutions* [Kuhn, 1962]. Although first appearing in *The International Encyclopedia of Unified Science*, a publication associated with positivism, the work raised serious questions about the descriptive accuracy of traditional models of science and proposed a radically different understanding of science and scientific practice. Drawing from his own experiences as a physical scientist, Kuhn found those models wanting. Contrary to positivists' contentions, Kuhn argued that scientists do not assess the truth of their theories from an objective viewpoint. Instead, scientists' assessments of the truth of a theory -- along with the importance lent to the attainment of truth itself -- are relative to "paradigms" within which they worked. Truth in science, Kuhn posited, is a relative, not an absolute, concept. In contrast to the falsificationist conception of science, Kuhn asserted that much of the time scientists engage themselves in the practice of "normal science" in which they accept fundamental theories and conceptions of the world as given. Only rarely, Kuhn argued, do scientists hold these notions up to question. Finally, Kuhn took serious issue with the traditional portrayal of science as progressing gradually via the accumulation of knowledge to truth. Given the relativity of truth in science, science could not be said to "progress" in any meaningful sense. Further, rather than change always being gradual and current science being the cumulation of past science, Kuhn argued that on occasion, a science experienced scientific revolutions which marked sharp breaks with the past. In sum, Kuhn painted a starkly different picture of science and scientific practice from the traditional models of science.

Although Kuhn's revolutionary work examined only the natural sciences, it attracted more than the passing attention of natural scientists and historians and philosophers of the natural sciences. *Social* scientists latched onto Kuhn's notions and sought to apply them to their own disciplines. Among these social scientists were an appreciable number of economists. To economists who had, for decades, drawn parallels between the way in which natural scientists conducted themselves and the way in which economists conduct (should conduct) themselves, Kuhn's model offered an alternative way of looking at economics *qua* science to the ways suggested by the traditional models of science. In applying the traditional models of science to economics, economists were led to ask questions such as: Were economic theories true? How did economists assess the truth of economic theories? Did economists falsify economic theories? If so, how? How has economics progressed to its current state? What is the state of economic progress? Kuhn's model, however, presented economists with a host of vastly different questions to ask about their discipline, such as: Were there paradigms in economics? If so, what were they? Did economists practice normal science? If so, what characterized normal economic science? Had economics undergone scientific revolutions? If so, what were they?

Over the last thirty years, numerous economists have asked these and other related questions such that the economics literature applying and interpreting Kuhn has grown to sizeable proportions. As we shall see in Chapter Two, Kuhn has been cited in the economics journal literature alone over four hundred times -- a figure of which almost any economist would be envious. Undoubtedly, Thomas Kuhn -- though not himself an economist -- has secured himself a prominent position in the history of economic thought. Given this, along with the revolutionary nature of Kuhn's theories and concepts, his impact upon economists' understanding of their discipline warrants examination.

In undertaking this examination, we recognize the complexities involved. Economists' applications of Kuhn and their interpretation of economics in the light of his notions are the product of an intricate web of interpretations which consists, most significantly, of economists' interpretations of Kuhn's notions and economics itself. Kuhn's text, however, does not possess an unequivocal meaning. As we shall see, a single interpreter of Kuhn located twenty-one different ways in which Kuhn himself employed the term "paradigm" in the first edition of *The Structure of Scientific Revolutions* [Masterman, 1970]. Further, Kuhn compounded any complexities found in the first edition by re-publishing that original text [Kuhn, 1970c] without any substantive alterations, along with a "Postscript" in which he, in the eyes of many philosophers, recants and/or revises many of his original positions regarding the nature of science and scientific change [Lakatos and Musgrave, 1970]. Apart from any confusion Kuhn himself may have introduced, we must recognize that the meaning of any given text -- *The Structure of Scientific Revolutions* included -- depends upon not only what an author writes in that text, but also what different readers read from it, i.e., their interpretations of that text. Interpreters of a given work, however, do not approach that work from the same vantage point. Each approaches it with his/her own set of background knowledge, his/her own interests, his/her own preconceptions and his/her own objectives for interpreting that work. Consequently, interpreters arrive at varying understandings of a text's meaning and significance. Recognizing this, we set out to examine economists' interpretations of three of Kuhn's major notions: "paradigm," "normal science," and "scientific revolution" to determine whether these notions have been subject to multiple interpretations, and if so, to identify significant divergences in economists' understandings of them.

Not only does Kuhn's text possess no objectively given meaning and significance; the field of economics to which economists have applied it does not. Resembling a kaleidoscope, economics is multifaceted. Those who purport to practice

"economics" are engaged in a wide range of different projects. They adhere to vastly different value systems. They study a wide range of different "economic" activity and phenomena. They view "economic" activity and phenomena from markedly different worldviews. They have vastly different conceptions of the scope and method of economics. Given this heterogeneity, there is no simple, unequivocal answer to the question, "What is economics?". Given this heterogeneity, one way to define economics is as that which those who purport to be economists do. Many economists, however, do not define economics in such broad and permissive terms. Instead, they are selective in their identification of which projects are the proper concern of economists (and which are not), which value systems are becoming to one labelling himself/herself an economist (and which are not), what "economic" activity and phenomena an investigator *qua* economist studies (and what activity and phenomena an *economist* does not study), what vantage points are legitimate in economics (and which are not), what is the scope of economics (and what lies outside of it) and what methods are permissible in the practice of economics (and which are not). In sum, economists - even while often making these determinations only implicitly -- have selectively interpreted what economics is (and what it is not).

Have economists' selective perceptions of economics translated into disagreements regarding the relevance and importance of Kuhn's model of science to the understanding of economics? Have economists' divergent conceptions as to what comprises "economics" led to differences in economists' specifications of economics' paradigms, normal economic science, or scientific revolutions in economics? If so, what are those differences? Providing answers to these three interpretive questions, and the related question, "Have economists subjected Kuhn's notions to multiple interpretations, and if so, what have those diverse understandings been?" provides the central focus for the present examination regarding economists' interpretations and applications of Kuhn's concepts and theories to economics.

The dissertation is divided into six chapters, including the present introductory chapter. Chapter Two seeks to provide some rough indication as to Kuhn's prevalence in economics via a quantitative analysis of economics journal articles citing Kuhn. Chapter Three explores economists' interpretations and applications of Kuhn's paradigm concept. Working with economists' explicit remarks concerning the paradigm concept, the chapter examines economists' varying understandings of the definition and function of paradigm, along with their assessments as to whether Kuhn employed "paradigm" in an ambiguous manner. The chapter then turns to economists' assessments of the applicability of the paradigm notion to economics, especially with regard to the implications of Kuhn's ambiguous use of "paradigm" and of the differences between the natural and social sciences for the application of Kuhn's paradigm concept to economics. Following this is a lengthy discussion of economists' applications of Kuhn's paradigm notion to economics. In particular, the chapter studies a wide range of different paradigms which economists have located in economics' mainstream, its heterodoxy and various economics subfields. Finally, the chapter looks at economists' understandings of the functions which paradigms have played in economics. In examining economists' treatment of Kuhn's paradigm concept, we find striking evidence that economists have subjected both the paradigm notion in general, as well as economics' paradigms in particular, to multiple interpretations. Economists offer a host of different definitions of "paradigm." They disagree as to the applicability of Kuhn's paradigm concept to economics. Further, they offer a multitude of different specifications of economics' paradigms and even provide divergent characterizations of nominally the same paradigm. They differ as to whether paradigms help or hinder scientists in general or economists in particular in their work. In sum, while economists employ the same terminology (e.g., "economics," "paradigm") and reference the same philosopher (Kuhn) in doing so, we find striking evidence that they are talking about vastly different things.



Chapter Four examines economists' interpretations and applications of Kuhn's notion of normal science. Paralleling our discussion of the treatment of the paradigm concept, the chapter first examines the characteristics which economists have attributed to Kuhnian normal science. Discussion then turns to economists' applications of the normal science concept in both economics' past and present. Examination of economists' applications of normal science identifies striking parallels between economists' interpretation of normal science in general and their descriptions and assessments of normal economic science in particular. However, as with their application of the paradigm concept, we find that economists subject both the normal science concept in general and normal economic science in particular to selective and multiple interpretations. Economists highlight different aspects of a Kuhnian normal science and normal economic science and differ in their descriptions of normal economic science in terms of those characteristics.

Chapter Five focuses upon economists' interpretations and applications of Kuhn's theory of scientific revolutions. It first lays out economists' understandings as to what constitutes scientific revolutions and what causes them under Kuhn's schema and then turns to economists' general objections to the applicability of Kuhn's theory of scientific revolutions to economics. While finding agreements among economists as to all three matters, examination also uncovers significant disagreements among economists as to the definition and causes of a Kuhnian scientific revolution, as well as their assessments of the applicability of Kuhn's theory of scientific revolutions to economics. Following this discussion, the chapter turns to consideration of economists' application of Kuhn's theory of scientific revolutions to the two most often cited revolutions in economics: the marginal (utility) and the Keynesian revolutions. Here, we find that economists' multiple interpretations of both Kuhn's theory of scientific revolutions as well as of the definition of economics (i.e., economics' paradigm) give rise to significant disagreements among economists as to the changes

the marginal (utility) or Keynesian revolutions effected, as well as their determinations as to whether those changes comprised a scientific revolution or not. An examination of economists' interpretations of some lesser cited revolutions in the history of economics, which concludes Chapter Five's discussion, uncovers similar disagreements among economists as to the nature and extent of changes effected by those revolutions, as well as their status as scientific revolutions. As with economists' treatment of "paradigm" and "normal science," we find that economists have subjected both "scientific revolution" as well as the history of economics to which they have applied that concept to selective and multiple interpretations.

Chapter Six reviews the dissertation's major findings and explores the implications of those findings for the application of Kuhn to economics.

## CHAPTER TWO: QUANTITATIVE ANALYSIS OF KUHN CITATIONS IN ECONOMICS JOURNALS

Before entering into an analysis of economists' interpretations and applications of Thomas Kuhn's major notions, we first provide some quantitative measures of the philosopher's prominence in the discipline. In particular, we seek to provide some rough indications as to where in economics Kuhn's prominence has been the greatest and how his prominence in economics has changed over the last twenty to twenty-five years. An ideal exploration into these areas would encompass the examination of the philosopher's prominence in all economics' forums (journal articles, books, conferences, conversations, classroom instruction, etc.). However, such an expansive undertaking is impracticable.

As a practicable (though certainly more limited) alternative, we restricted our attention to the economics journal literature, and still further, to aggregate data on the number of economics articles in which Kuhn has been cited. Utilizing the *Social Sciences Citation Index*, we compiled a list of all economics articles<sup>1</sup> which cited Thomas Kuhn<sup>2</sup> between 1966 and 1992.<sup>3</sup> We then grouped the articles by year to produce aggregate annual citation data, and also made counts of the number of times Kuhn was cited in a particular journal. Finally, we gathered together a subset of the articles citing Kuhn by *Journal of Economic Literature* (JEL) classification category and aggregated the data by subfield.<sup>4</sup>

### A. COMPOSITION OF ARTICLES CITING KUHN

The data indicate that Kuhn's presence in economics has been palpable and that his work has enjoyed significant notoriety in the discipline. Overall, we located 437

different economics articles which cited Kuhn over the twenty-seven year period (an average of 16.2 citations per year). Further, we found Kuhn cited by 351 different economists, and cited in ninety-one different economics journals.<sup>5</sup>

However, aggregate data grouped by journal and a subset of articles, grouped by *JEL* classification codes,<sup>6</sup> suggest that Kuhn's greatest prominence in economics lies at the periphery of mainstream economic practice. Of the eleven journals which cite Kuhn ten or more times, seven subscribe to a heterodox economic position<sup>7</sup> and/or concern themselves with fields holding marginal importance for most academic economists.<sup>8</sup> Articles published in these seven journals account for almost two-fifths of all Kuhn economics journal citations. Two of these journals alone (the institutionalist *Journal of Economic Issues* and the history of economic thought journal *History of Political Economy*) account for over one-fifth of all articles citing Kuhn. Table 1 provides a listing of the eleven journals citing Kuhn ten or more times.

Results from the examination of the subset of articles citing Kuhn, grouped by *JEL* classification codes, are even more striking. Almost two-thirds (64%) of the articles in this subset are classified under the headings "History of Economic Thought," and/or "Economic Methodology."<sup>9</sup> Table 2 provides a breakdown of the articles by *JEL* classification category.

#### B. TRENDS IN CITATIONS OF KUHN

Finally, we turn our attention to trends in economists' citations of Kuhn. Looking only at five-year moving averages of the absolute number of times Kuhn was cited, we find that citations rose steadily from the late 1960s to the late 1970s (See Figure 1). After that point, they levelled off for about ten years, and in recent years, the absolute number of Kuhn citations have begun to decline. This pattern holds both if we look at all citations found in the *SSCI*, or only those citations from journals, listed as fully indexed economics journals in the *SSCI* over their entire lifetime.<sup>10</sup>

However, changes in the absolute number of Kuhn citations are the result of more than simply changes in Kuhn's relative standing in economics. They are also the product of changes in the number of opportunities for citation (i.e., articles). Even if Kuhn's relative standing remained unchanged over time, an increase in the number of economics articles published would increase the number of Kuhn citations. It is thus important to control for changes in the number of articles published each year in order to gain a clearer sense of Kuhn's prevalence in the economics literature.

Here, we focused our attention upon the two economics journals citing Kuhn most frequently (*Journal of Economic Issues* and *History of Political Economy*)<sup>11</sup> and examined the ratio of Kuhn citations in these two journals to the number of articles annually published in them (See Figure 2). Inspecting changes in this ratio over time, we see that it reaches a peak about five years earlier than did our data on the absolute number of Kuhn economics journal citations. Further, we find that the ratio begins declining about the same time as the absolute number of all economics journal citations of Kuhn levels off (around 1978). All this suggests that Kuhn's relative prominence in economics may have peaked and begun declining earlier than the data on absolute citations indicates. The data also imply that Kuhn's relative prominence, as measured by citations, may have rebounded in the 1980s and continued to rise ever since.

Another means by which to assess Kuhn's relative prominence is to compare trends in economists' citations of Kuhn with their citations to other notable philosophers of science. In this respect, we examined the relationship between economists' citations of Kuhn and their citations to a philosopher of science advancing a competing notion of science and scientific change, Imre Lakatos (See Figure 3).<sup>12</sup> We find that, throughout the entire time period 1968 to 1990, economists consistently cited Kuhn more often than Lakatos. We do, however, see that economists' citations to Lakatos rose throughout the mid 1970s and early 1980s, while their references to Kuhn held steady. Thus, over this decade -- even as the absolute number of Kuhn citations

remained above those of Lakatos -- the number of Kuhn citations relative to Lakatos citations fell off. Further, we find that beginning in the mid 1980s Lakatos citations began to decline as Kuhn citations continued to remain relatively steady. Consequently, since the mid 1980s, the number of economists' citations to Kuhn relative to Lakatos citations has risen.

These patterns are consistent with our findings from our examination of the ratio of Kuhn citations to articles in *JEI* and *HOPE*. As with that examination, again we find evidence that in relative terms, economists' citations to Kuhn declined between the mid 1970s and early 1980s, but have increased for the most part since the mid 1980s. What all this suggests about trends in Kuhn's prominence in economics is somewhat unclear. One interpretation is that Kuhn's prominence peaked in the mid 1970s, declined for about a decade, but, since the mid-1980s has enjoyed a resurgence. It may, however, have been the case that the decline in Kuhn citations after the mid 1970s was indication that Kuhn's notions had become so well established and familiar in the economics literature that economists began using his concepts without citing Kuhn (formally or informally).<sup>13</sup> Which of these interpretations -- or whether another interpretation -- provides a better understanding of the movements in the number of Kuhn citations in the economics literature would require further study.<sup>14</sup>

We also sought to gain some insights into how Kuhn's prominence in economics, measured in terms of citations, compared with his overall prominence in the social sciences. Here, we calculated the ratio of the absolute number of economists' citations to Kuhn for five five-year periods: 1966-70, 1971-75, 1976-80, 1981-85 and 1986-90.<sup>15</sup> Graphing the ratios, we find that economists' citations to Kuhn, relative to all social scientists', peaked in the mid 1970s, then fell off considerably, and have remained relatively steady since the mid 1980s (See Figure 4). The data suggest that, since the mid 1970s, Kuhn's notions have become relatively less important to economists than to other social scientists.<sup>16</sup>

**Table 1**

**Economics Journals Citing Kuhn Ten or More Times  
Between 1966 and 1992**

<u>Journal</u>	<u>Kuhn Citations</u>
Journal of Economic Issues	53
History of Political Economy	40
International Journal of Social Economy	23
Review of Social Economics	14
American Economic Review	13
American Journal of Economics and Sociology	13
Journal of Post Keynesian Economics	13
World Development	13
Journal of Economic Literature	11
Rivista Internazionale di Scienze Economiche *	11
Southern Economic Journal	10

Source:  
*Social Sciences Citation Index* (1966-1992)

\**Rivista Internazionale di Scienze Economiche E Commerciali.*

Table 2

**Subset of Economics Journal Articles Citing Kuhn  
During the Years 1970-78 and 1985-92  
Listed by *Journal of Economic Literature*  
Classification\***

<u>JEL Category</u>	<u>Kuhn Citations</u>
000 General Economics; Theory; History; Systems	180
General Economics	36
General Economic Theory	33
History of Economic Thought	74
Methodology	78
History and Systems	9
100 Economic Growth; Development	23
200 Quantitative Economics Methods & Data	4
300 Monetary & Fiscal Theory and Institutions	5
400 International Economics	--
500 Business Finance; Marketing; Accounting	2
600 Industrial Organization	10
700 Agriculture and Natural Resources	7
800 Manpower; Labor and Population	6
900 Welfare Programs; Consumer and Urban	11

**Sources:**

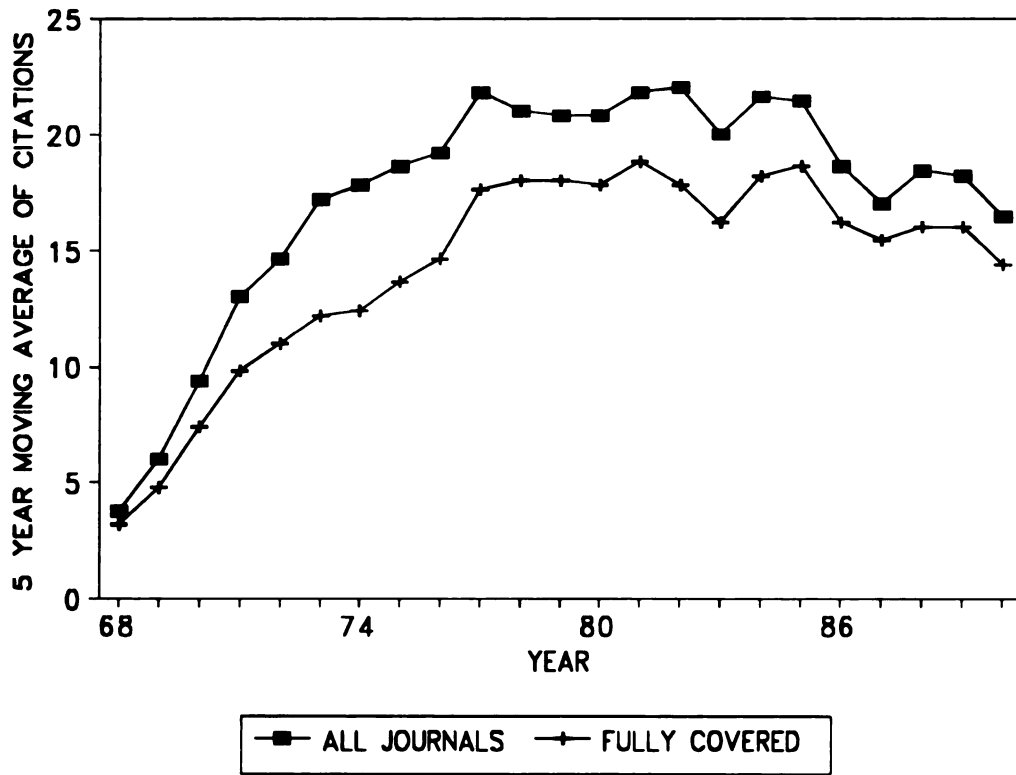
*Social Sciences Citation Index* (1970-80; 1985-92)

*Journal of Economic Literature* (1970-80; 1985-93)

\*The table employs the pre-1990 *JEL* numerical classification system. Any articles classified under the present alphanumeric system were reclassified as necessary under the old categories. Figures on the number of articles listed under the ten major sub-headings add up to more than 211 because some articles were categorized under more than one major sub-heading. Similarly, figures on the number of articles in the five divisions of General Economics add up to more than 180 because some articles were classified under more than one of these divisions.



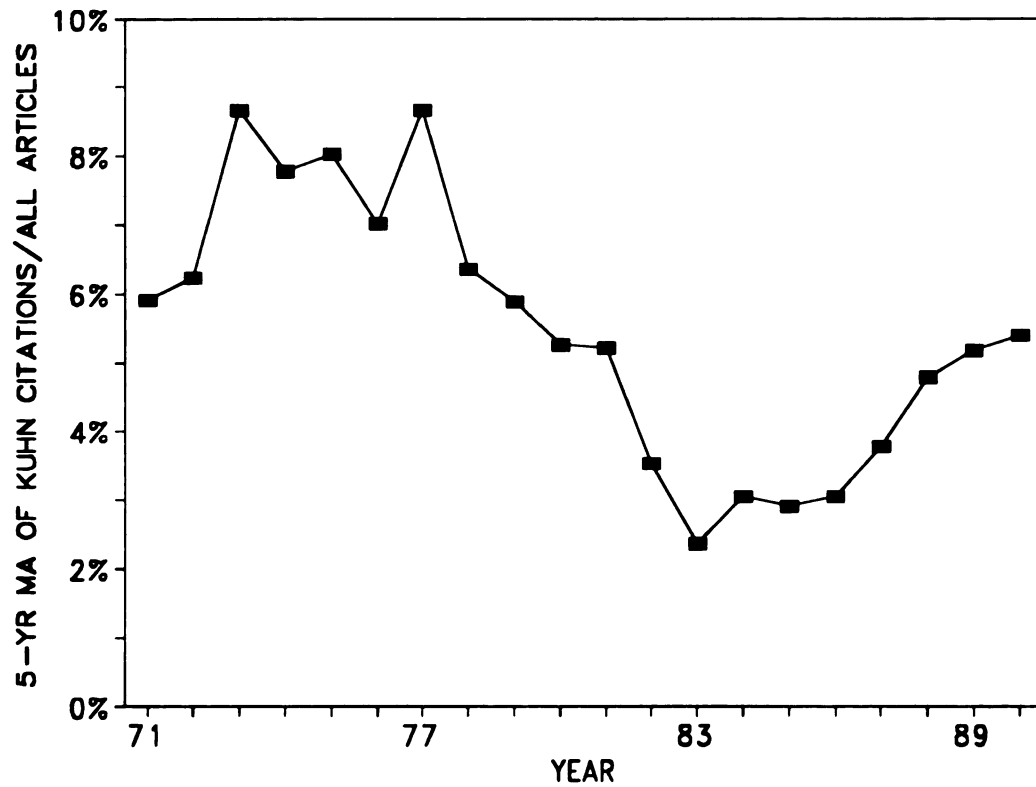
Figure 1

**Kuhn Citations in Economics Journals over Time**

Source: *Social Sciences Citation Index* 1966-92

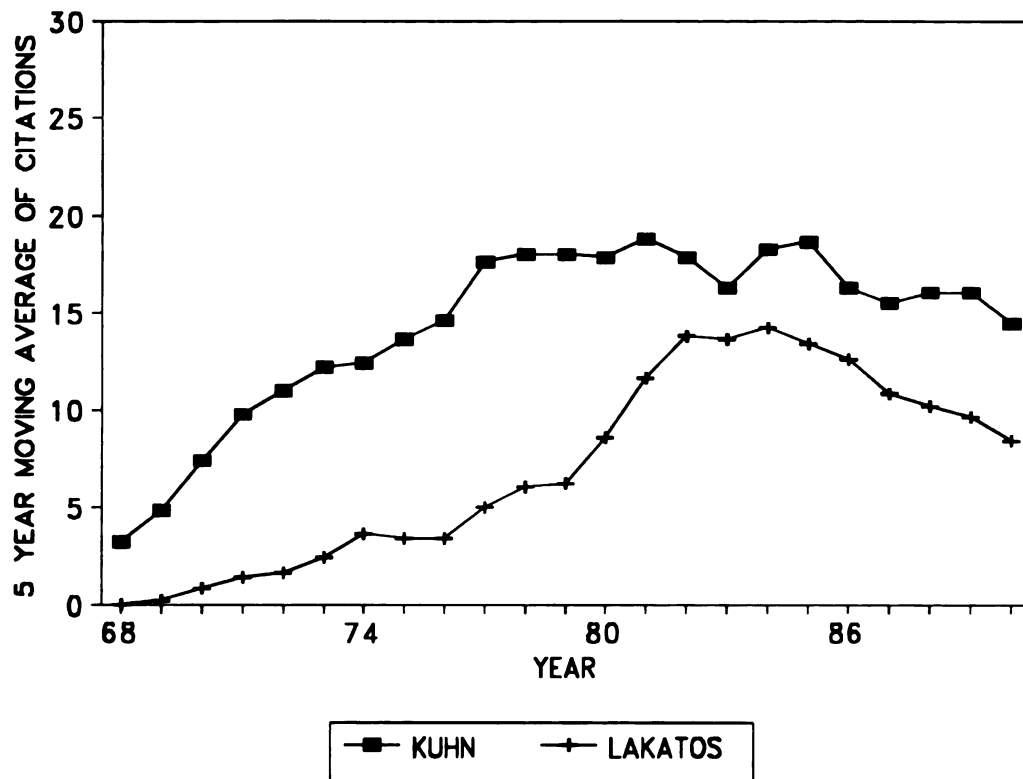
**Figure 2**

**Percentage of *History of Political Economy* and *Journal of Economic Issues* Articles Citing Kuhn**

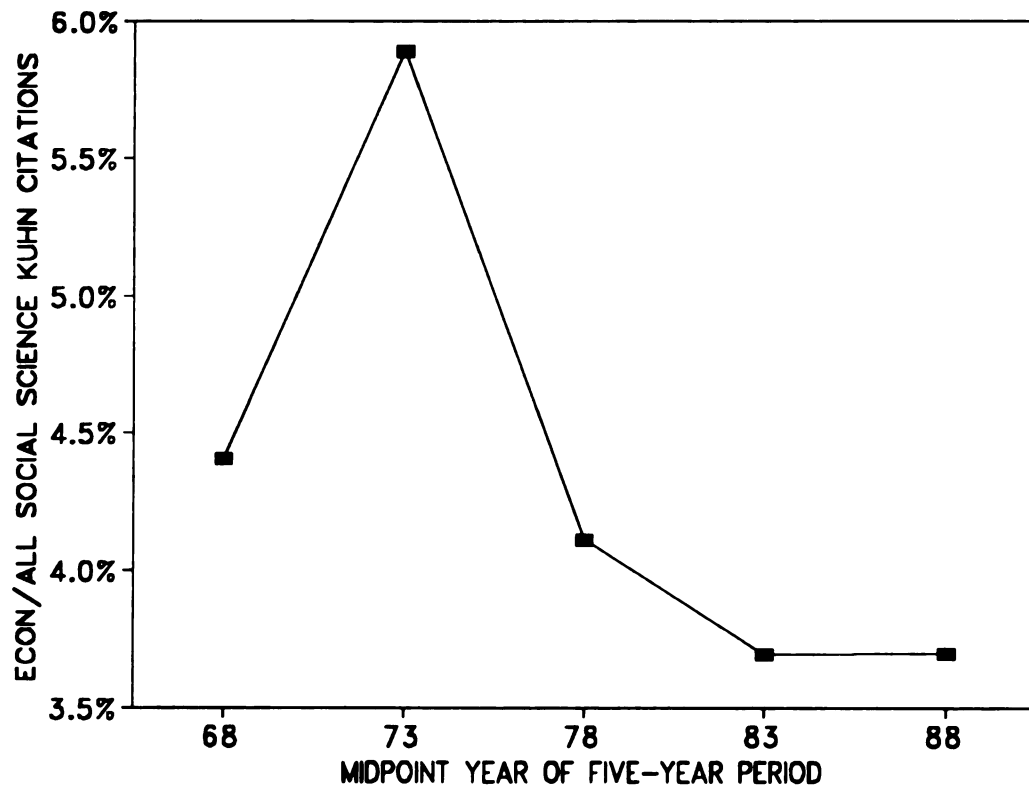


Sources: *Social Sciences Citation Index* 1970-92  
*History of Political Economy* 1970-92  
*Journal of Economic Issues* 1970-92

Figure 3

**Kuhn and Lakatos Citations in Economics Journals over Time**

Source: *Social Sciences Citation Index* 1966-92

**Figure 4****Percentage of Social Science Journal Kuhn Citations from Economics**

Source: *Social Sciences Citation Index* 1966-90

## NOTES

1. The journals which the *SSCI* listed under the subject heading "Economics" changed over time from 1966 to 1992. For our present purposes, we define an "economics journal" as one which, at any time in the *SSCI*'s history, was listed under the heading "Economics," and include *all* articles published by that journal in our sample -- even articles published in years when the journal was not listed under the "Economics" heading. While the *SSCI* does not categorize certain journals commonly recognized as economics journals (e.g., *Journal of Industrial and Labor Relations*) as economics journals, its classification system provides a consistent and well-recognized means by which to categorize journals.
2. In gathering our list of articles citing Kuhn, we looked under the names, "Kuhn," with no first name initial, and all names "Kuhn" with first initial T, with or without a second initial (including those listing a second initial different from S). Only articles which cited an article or book known to have been published by the philosopher of science Thomas S. Kuhn (author of *The Structure of Scientific Revolutions*) were included in the sample.
3. We thus exclude consideration of the book literature -- largely on grounds of practicality. To our knowledge, there exists no *SSCI* analog for the book literature. Given that -- as we will see -- many of the articles citing Kuhn concern themselves with the history of economic thought and methodology (sub fields possessing a significant book literature), the exclusion imposes serious limitations on the informativeness of the present statistical examination. But, if the journal literature provides a fairly accurate reflection of the book literature, this exclusion may not pose a serious problem.  
 In addition, we must concede that, even for the journal literature itself, reliance upon aggregate citation data has significant limitations. If an article employs Kuhn's concepts or notions, but does not formally cite him, the article is not counted. This problem may, however, pose only a minor problem in that articles which extensively employ Kuhn's notions would, in all likelihood, list him in their references and footnotes. However, employing the *SSCI* presents another related problem. Each citation (whether from only a passing mention or an extensive critique or application) is weighted equally.
4. The subset included all articles for which classification codes could be found in the *Journal of Economic Literature* for the years 1970-78 and 1985-92. The *JEL* does not classify foreign language articles unless they are accompanied by an English language summary. In addition, the *Journal* does not index (let alone classify) all journals indexed by the *SSCI*. Thus, these articles were excluded from the analysis.
5. While impressive numbers, care should be taken not to read too much into these data. As noted above, an article's citing Kuhn provides no indication of the importance which the philosopher's work plays in the article.
6. The subset includes roughly half (211) of the articles citing Kuhn in economics. Of the 211, 112 were from articles published between 1970-78, and ninety-nine published after 1985.

7. *Journal of Economics Issues* (institutionalism); *International Journal of Social Economics and Review of Social Economy* (social economics); *Journal of Post-Keynesian Economics* (post-Keynesian economics).
8. *History of Political Economy* (history of economic thought), *American Journal of Economics and Sociology* (sociology), *World Development* (development). In addition, many of the heterodox journals listed above often concern themselves with methodology and the philosophy of science, another field of study lying at the edge of mainstream economic practice.
9. One hundred thirty-five of the 211 articles were classified under either "History of Economic Thought" or "Methodology." Of these, seventeen were listed under both headings.
10. By looking only at journals indexed over their entire lifetimes by the *SSCI* (i.e., "fully covered"), we sought to eliminate any distortions which might have resulted from the *Index*'s adding a journal, previously in existence, to its listings. An example illustrates the problem. If several journals, since their existence in 1970 had cited Kuhn ten times a year, but were not indexed in the *SSCI* until 1975, then the data would suggest a substantial increase in Kuhn citations in 1975, when in fact the jump was an artifact of adding previously unindexed journals to the *Index*. We found no evidence of any such distortion.
11. As we noted above, these two journals alone account for one-fifth of all economics journal articles citing Kuhn. We arrived at the total number of articles published by each journal for each year by counting the number of articles listed in the table of contents for all issues published by that journal in a given year.
12. Here, we limited our analysis to articles published in journals fully indexed by *SSCI* over their entire lifetime.
13. Given this interpretation, however, it is unclear how to read the increase in the relative number of Kuhn citations in economics journals since the mid-1980s.
14. The study would require direct (and extensive) examination of the economics literature in order to determine how the role which Kuhn's notions have played have changed over time. Such an undertaking (even limited to the methodology and history of thought literature, which comprises the bulk of Kuhn citations in the journal literature) would be sizeable.
15. The numerator of this ratio was total number of different economics articles citing Kuhn over a given five-year period. The denominator was the total number of citations listed in the *SSCI* under the headings, "Kuhn, T" and "Kuhn TS" for the same five year period. Given that an article may be listed more than once under the headings, the denominator overstates the total number of articles citing Kuhn. However, it is likely that the proportion of duplicates remained fairly stable between 1966-90. Thus, duplications should not have had an appreciable impact upon the observed trend.
16. Assuming, among other things, the proportion of *SSCI* citations accounted for by economics journals remained relatively constant from 1966 to 1990.

### **CHAPTER THREE: PARADIGMS**

We begin our exploration of economists' interpretations and applications of Kuhn's theories and concepts by looking at his notion of a scientific "paradigm." We first examine interpretations of Kuhn's concept by probing economists' explicit remarks concerning Kuhn's understanding of the definition and function of paradigm. We then turn to consideration of economists' appraisals as to whether Kuhn employed/defined "paradigm" ambiguously. Following this, we look at economists' statements concerning the applicability of Kuhn's paradigm concept to economics. In particular, we focus upon applicability implications stemming from the ambiguity of Kuhn's paradigm notion, and the differences between the natural sciences, on the one hand, and the social sciences and economics on the other. We also examine assessments of the concept's rhetorical worth in economics debates. With respect to each of these issues, we find considerable diversity. Economists differ as to what constitutes a paradigm, what it does, the implications of Kuhn's broad use of the concept/term paradigm, the applicability of the notion to economics and its usefulness for those seeking to advocate for their own position or against another's.

We then turn to paradigms which economists identify in their own discipline. Here, we review both economists' descriptions of the paradigms they locate, as well as justifications -- if any -- they offer as to why the paradigms they identify are paradigms. In our review, we find that economists identify a wide variety of different paradigms in the history of economic thought, as well as in the present day, within the mainstream as well as heterodoxy, and at both the discipline and the sub-discipline levels. In many instances, among economists locating nominally the same paradigm,

we find a diversity of specifications of that paradigm. Finally, we find very few explicit justifications offered by economists as to why the paradigms they identify are paradigms, though we do find several explanations as to why various purported paradigms are not paradigms. We conclude the chapter by laying out the different functions economists find paradigms playing within the province of economics.

#### A. THE DEFINITION OF PARADIGM

Economists have described Kuhn's notion of a paradigm in a wide variety of ways. Here, we identify over ten different interpretations. Overlap certainly exists among them and we are able to detect some common strands running through many of the characterizations. In particular, we find that numerous economists stress the importance of the relationship between paradigms and the scientific community. However, the interpretations located here still indicate a significant lack of consensus among economists as to what constitutes a Kuhnian paradigm.<sup>1</sup>

##### 1. *"Universally Recognized Scientific Achievements"*

Many economists define Kuhn's paradigm concept by quoting the philosopher's first remarks concerning paradigms in the Preface to *The Structure of Scientific Revolutions*: "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" [Kuhn, 1970c, page viii]. However, while many economists quote Kuhn's introductory remarks in their description of the concept, they differ in both their treatment and interpretation of those remarks. Donald Gordon<sup>2</sup> simply quotes Kuhn's description without any further comment [Gordon, 1965, page 122]. Likewise A.W. Coats quotes Kuhn's remark, without explicitly drawing out its implications. Coats, however, cites later remarks by Kuhn, seeking to fill out the definition of paradigm in order to clarify what a paradigm is and is not<sup>3</sup> [Coats, 1969, page 290].



Others like George Argyrous and Keith Tribe employ the remark in order to call attention to what each sees as the central attribute of Kuhn's paradigm. Argyrous italicizes the phrase, "*model problems and solutions*" in order to stress that a paradigm constitutes an exemplar for scientists to follow<sup>4</sup> [Argyrous, 1992, page 233]. Keith Tribe, on the other hand, concludes from Kuhn's quote that "the major point is that it [paradigm] is something that the community *shares*, it is this that makes them a community"<sup>5</sup> [Tribe, 1973, page 469].

Oleg Zinam asserts that he provides an "elaboration" of Kuhn's often quoted definition of paradigm by defining a paradigm as

a mutually consistent system of basic assumptions about the nature, purpose, method, scope and significance of a given science and about the basic components and characteristics of the universe under investigation shared by the inter-subjective consensus of a given scientific school of thought.<sup>6</sup> [Zinam, 1975, page 470]

Another, Henry Spiegel, regards Kuhn's opening remarks as clear and self-explanatory. Spiegel, however, points out that despite the remark's clarity, great ambiguity surrounds Kuhn's definition and use of "paradigm"<sup>7</sup>:

What is a paradigm? Kuhn defines it, apparently unambiguously and in a straightforward fashion, as "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners." In spite of the apparent precision of this definition, interpreters of Kuhn have found a large variety of different shades of meaning of the term, twenty-one or even more, that have been located in Kuhn's work itself. [Spiegel, 1983, page 664]

Quoting Kuhn's opening remarks represents only one of many ways in which economists have sought to define Kuhn's paradigm concept.<sup>8</sup> However, economists have treated and interpreted even this single quote in Kuhn in a variety of ways.

## 2. "*Paradigm*" as a *Worldview*

Many economists see Kuhn's "paradigm" connoting a worldview. Mark Blaug acknowledges that *one* of the ways in which Kuhn defines and uses "paradigm" is as a worldview<sup>9</sup> [Blaug, 1976, page 152]. Lawrence Boland uses the term "paradigm"

interchangeably with "worldview" [Boland, 1977, pages 97-99] and Benjamin Ward employs "worldview" as one substitute for the term paradigm<sup>10</sup> [Ward, 1972, page 248, note 1]. Guy Routh, similarly, understands a paradigm as worldview: "a *paradigm*: the world-view whose acceptance is essential for those who wish to be accepted into the fraternity or 'invisible college'"<sup>11</sup> [Routh, 1989, page 26], and Edythe Miller maintains that "a Kuhnian paradigm consists of a general world view that shapes perception . . ." [Miller, 1991, page 994]. Similarly, William Breit remarks, "The answer is to be found in Kuhn's term, 'world-view.' This term was used by him to characterize a paradigm" [Breit, 1987, page 827].

In addition to those explicitly allying the *term* worldview with Kuhn's "paradigm," a number of economists relate "paradigm" to the *concept* of a worldview.<sup>12</sup>

a. Conception of and Beliefs about "Reality"<sup>13</sup>

Radical economists Paul Sweezy and Michael Zweig describe Kuhn's paradigm as a conception of reality:

Kuhn argues that every scientific theory rests on what he calls a paradigm, which I think is very close to what I have been referring to as a conception of reality (or some aspect of reality). [Sweezy, 1971, page 60]<sup>14</sup>

Kuhn proposes that the development of science is characterized by succeeding "paradigms," basic conceptions of matter, the universe, light, or whatever general object of study. [Zweig, 1971, page 43]

Still again, Oyvind Bøhren remarks:

Finally, the paradigm includes implicit assumptions which may be difficult to deduce from the specific theories generated within the paradigm (for instance, a belief that human behavior is maximizing in an economic welfare sense, that most observable market prices approach or stay close to those of a competitive equilibrium, or that investors normally prefer more information to less). [Bøhren, 1990, pages 10-11]

Likewise, William Breit describes Kuhn's understanding of a paradigm: "To Kuhn a paradigm is a shared set of beliefs about the nature of reality" [Breit, 1987, page 827].

b. Outlook, Perspective and/or Framework from which To View the "World"

Others describe Kuhn's paradigm as a means by which to view the "world" -- or some "relevant" portion of it. L.E. Johnson defines a paradigm as "the 'analytical box' through which the profession views reality" [Johnson, 1983, page 1098]. Maurice Dobb remarks that Kuhn "has used the term 'paradigm' for such a group or cluster of general notions, or 'ways of seeing the world' . . ." [Dobb, 1973, page 18, footnote \*].

J. Ron Stanfield uses "paradigm" interchangeably with the terms "gestalt," "vision," and "perspective" [Stanfield, 1983]. Similarly, Allan Gruchy interchanges "paradigm" with "framework of interpretation" [Gruchy, 1986, pages 806-807] and Anghel Rugina equates Kuhn's conception of a paradigm with a "system of reference" [Rugina, 1986, page 41]. All of these uses connote a paradigm as a vantage point from which to perceive and/or interpret "reality," i.e., a worldview.

Robert Solo refers to a Kuhnian paradigm as a "system of perception" [Solo, 1991, pages 32-33].<sup>15</sup> Finally, D.P. O'Brien provides one of the best descriptions of a paradigm as vantage point from which to view the world: "a pair of spectacles through which we see the world" [O'Brien, 1976, page 142].

Peter Wiles describes Kuhnian paradigms as "the basic models whereby scientists try to understand their subjects at any given time" [Wiles, 1979, page 171].<sup>16</sup> He, however, seeks to distance the notion from the ideological or metaphysical. Kuhn, Wiles notes,

certainly does not state anywhere that paradigms are partly *Weltanschauungen*. The nearest he gets is in the phrase "global, embracing all the shared commitments of a scientific group" -- but these commitments are clearly only analytical or factual. [Wiles, 1981, page 355, quoting Kuhn, 1974, page 460]<sup>17</sup>

Argyrous goes even further, however, and distances Kuhn's intended understanding from any notion of a worldview. According to Argyrous, while Kuhn did use the term "paradigm" *qua* worldview, in his first edition of *The Structure of Scientific*

*Revolutions*, the philosopher sought to move away from the understanding of a paradigm as an overarching worldview toward one seeing a paradigm as an example of "'good' science" [Argyrous, 1992, page 232].<sup>18</sup>

3. *"Paradigm" as Eclectic Collection of Worldview, Method, Values, Theory . . .*

A number of economists understand Kuhn's paradigm concept to connote a collection of disparate elements including perceptual, methodological, metaphysical, theoretical, and valuational elements and actual scientific practice.<sup>19</sup> According to Sheila Dow, Kuhn saw paradigms as having a "dual identity," as a *Weltanschauung*/ideology on the one hand and methodology on the other [Dow, 1981, pages 327-328]. Carol Anderson's description of a paradigm encompasses all of the elements listed above in her own description of a Kuhnian paradigm: "a basic way of perceiving, thinking, valuing and doing associated with a particular view of reality" [C. Anderson, 1982, page 200].<sup>20</sup>

Stephen Worland, defines a paradigm as, "an admixture of basic generalization, law and concept, illustrated with standard models which exemplify the laws and give them empirical content" [Worland, 1972, page 275]. Similarly, Michel DeVroey describes a paradigm as a "system of ideas" and J.C. Glass and W. Johnson describe it as a "conceptual framework":

The notion of paradigm expresses the unity and the coherence of a system of ideas. It encompasses the social vision, methodological principles and categories, theories, techniques and stereotyped examples, all of which together make up a particular system of ideas, the content of which is reflected in textbooks. [DeVroey, 1975, page 419]

a Kuhnian paradigm can be regarded as a conceptual framework which supplies researchers with (a) a perspective for viewing the world, (b) a common view of those features (such as logical consistency, predictive accuracy, broadness of scope, simplicity and fertility) that should characterize a good theory . . . (c) a theoretical framework for analysing problems, and (d) a set of techniques for empirically testing theoretical predictions. [Glass and Johnson, 1989, pages 154-155]

L.E. Johnson identifies four major attributes of a Kuhnian<sup>21</sup> paradigm:

To understand the specific paradigm, one must analyze it in terms of its basic characteristics. Kuhn presents four, though he does so more implicitly than explicitly and does not employ the following terminology. They are: (1) *fundamental theoretical assumptions*; (2) *focal variables and methods of analysis*; (3) *basic issues or problems*; (4) *professional relations and interactions*. [Johnson, 1980, page 56]<sup>22</sup>

4. *"Paradigm" as Eclectic Collection of Worldview, Method, Values, and Theory that Holds Central Importance to the Community to which the "Paradigm" Belongs*

The fourth characteristic of a Kuhnian paradigm which L.E. Johnson cites ("professional relations and interactions") hints at an understanding of paradigm which many economists who regard a paradigm as a constellation of elements share: A paradigm is inextricably linked with a given scientific community for which the paradigm holds a place of central importance. While a paradigm is a complex framework of theory, method, belief, perception, practice and application, not every such framework constitutes a paradigm -- only those allied with a community of practitioners/scientists/professionals.

Solo, for instance, describes a paradigm as "a particular system of perception embodied in a set of theories, hypotheses, techniques of inquiry and analysis, model experiments, overt and covert assumptions, all deeply inculcated in the discipline and perpetuated through its successive generations" [Solo, 1991, pages 32-33].

John Cornwall, Gerald Peabody and Oleg Zinam all regard a Kuhnian<sup>23</sup> paradigm as a set of various inter-related theoretical, methodological, valuational and application elements which a given group of practitioners *share*:

Following Kuhn, the term *paradigm* will be used to denote a set of metaphysical beliefs, assumptions (often, it would seem, irrefutable), and values *accepted by a group* of economists, together with the choice of problems they consider important and the group of techniques deemed worthy for analyzing these problems. [Cornwall, 1979, page 70, emphasis added]

a paradigm . . . contains the collection of theories, techniques, beliefs, values, and so on to which the *group is committed*. [Peabody, 1971, page 1, emphasis added]

paradigm is defined in the spirit of Thomas S. Kuhn as a mutually consistent conceptual system of basic assumptions about the universe studied and about the nature, purpose, scope, methods, and significance of a science *shared by the members of a scientific community* . . . [Zinam, 1981, pages 70-71, emphasis added]

In summary, (1) Many economists regard a paradigm as a grouping of elements ("admixture," "system of ideas," "system of perception," "set," "collection," "conceptual system").

(2) The major elements of a paradigm which economists identify are: (a) Worldview ("social vision," "metaphysical beliefs," "basic assumptions about the universe studied," "fundamental theoretical assumptions"); (b) Theory ("basic generalization, law and concept," "assumptions," "theories"); (c) Values and prescriptions, both methodological and philosophical ("choice of problems they consider important," "methodological principles," "values"); (d) Techniques ("techniques," "techniques of inquiry and analysis," "focal variables and methods of analysis"); (e) Examples of Scientific Practice ("standard models which exemplify the laws," "stereotyped examples," "model experiments").

(3) While most descriptions of a paradigm as a collection of inter-related elements includes worldview, theory, values and prescriptions, and very often techniques, many notably do not include examples of scientific practice. Many economists exclude actual scientific practice from their definition of "paradigm." Indeed, characterizations of a paradigm such as a "system of ideas" and "conceptual system" portray a paradigm as an *ideational* system. Actual practice enters into most definitions -- if at all -- only indirectly in reference to methodological elements and accepted techniques to be used in practice. Few of the economists in this group list actual examples of scientific practice as a paradigm element.<sup>24</sup>

(4) Most economists in the present discussion define a paradigm with respect to its connection and central importance to the community which possesses the paradigm. In particular, many describe a paradigm as some collection of elements which a given

(scientific) community *shares*<sup>25</sup> ("accepted by a group of economists," "to which the group is committed," "shared by the members of a scientific community").

##### 5. *"Paradigm" as Disciplinary Matrix*

Closely tied with those defining a paradigm as a framework of elements which members of a community share are those who ally Kuhn's "paradigm" with an expression the philosopher introduced in the Postscript to the second edition of *The Structure of Scientific Revolutions*: a "disciplinary matrix."<sup>26</sup> Many who ally Kuhn's notion of paradigm with a disciplinary matrix enumerate the four elements of a disciplinary matrix which Kuhn lists in his Postscript. Daniel Hausman's list is typical: "(1) 'symbolic generalizations,' (2) metaphysical and heuristic commitments, (3) values, and (4) 'exemplars'"<sup>27</sup> [Hausman, 1992, page 83]. Larry Reynolds describes a disciplinary matrix as "consisting of symbolic generalizations deployed without question, shared commitments to a set of beliefs, a set of values and 'exemplars'" [Reynolds, 1976, pages 25-26].

Others provide more detailed definitions of a disciplinary matrix by describing each of the matrix elements. Peabody, for example, describes Kuhn's understanding of a paradigm as follows:

In the Postscript Kuhn expands upon this notion [paradigm] by examining four important aspects of the paradigm (or disciplinary matrix). The first are symbolic generalizations that allow the use of logical and mathematical manipulations and function as laws or definitions of symbols. Next are beliefs in particular models which supply the permissible metaphors and analogies and help determine what will be acceptable as a problem solution and what puzzles remain to be solved. Values shared by the community are another important aspect of the paradigm. The most deeply held values in the natural sciences concern the nature of predictions -- they should be accurate, preferably quantitative, and have well-defined error limits. The fourth item he isolates are exemplars. These are the set of problem solutions that demonstrate the empirical content of the theories and provide examples for training students to see puzzles as like problems they have already learned to solve. [Peabody, 1971, pages 1-2]

Deborah Redman defines each term by quoting Kuhn directly:

Three constituents of a disciplinary matrix are symbolic generalizations, models, and exemplars. Symbolic generalizations are "expressions . . . which can readily be cast in some logical form like  $(x)(y)(z)\phi(x,y,z)$ . They are the formal, or the readily formalizable, components of the disciplinary matrix." Models are "analogies," which Kuhn discusses only in passing. Exemplars are "concrete problem solutions, accepted by the group, as, in a quite usual sense, paradigmatic." [Redman, 1991, page 17, quoting Kuhn, 1974, page 463, ellipses in Redman]<sup>28</sup>

Further, she characterizes Kuhn's disciplinary matrix as "composed of general theoretical assumptions, laws, techniques, and metaphysical principles that guide scientists in their work and members of a particular scientific community" [Redman, 1991, page 16].

The reader will note the strong similarity between the four elements identified in Kuhn's disciplinary matrix and the paradigm elements identified by economists characterizing Kuhn's paradigm as an eclectic collection. The expressions, "'beliefs in particular models,'" "metaphysical principles" "metaphysical and heuristic commitments," and "models" correspond to the notion of a worldview. Similarly, we may ally "'symbolic generalizations'" and "general theoretical assumptions, laws" with theory. Finally, Hausman and Peabody both list "values" as a disciplinary matrix element.

The parallels between these two definitions of paradigm are, perhaps, not so surprising. Many, even those not employing the term "disciplinary matrix," may have incorporated it implicitly into their definition. However, given this, it is remarkable that many economists who defined a paradigm as an eclectic collection did not include exemplars (or some similar notion) in their definition, while each economist who specified the elements of a paradigm as disciplinary matrix included exemplars as one component.

Still again, we find another similarity: the sociological characterization of a paradigm. Many allying "paradigm" with "disciplinary matrix" underline not only its *multidimensional*, but its *communal* nature as well: a disciplinary matrix is "the



common possession" of members of a profession. Redman quotes Kuhn directly on this point:

"'Disciplinary' because it is the common possession of the practitioners of a professional discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification." [Redman, 1991, page 16, quoting Kuhn, 1974, page 463]

Joel Jalladeau and Richard Chase similarly remark:

In a revised statement he [Kuhn] therefore proposed the expression '*disciplinary matrix*.' *Disciplinary* implies something in common among the specialists of a specified discipline; a matrix regroups very diverse elements : shared symbolic generalizations, beliefs, values, and examples of solved problems within a scientific circle. [Jalladeau, 1978, page 588]

Kuhn feels that the idea of a structured scientific community is of great importance. And so to more clearly distinguish the use of the paradigm concept at this level of abstraction he suggests the term "disciplinary matrix" -- a term in which the word "disciplinary" suggests the common possession, by a group of practitioners, of a particular set of rules, methods, and the like; and the word "matrix" indicates the idea that such a disciplined way is composed of ordered elements of various sorts, each requiring further specification and or systematization. [Chase, 1983b, page 814]

a. Worldviews, Eclectic Collections and Disciplinary Matrices

It would, of course, be incorrect to sharply divide those characterizing a paradigm either as an eclectic collection or disciplinary matrix on the one hand and those describing it as a worldview on the other. Argyrous, for instance, allies the understanding of "paradigm" as disciplinary matrix with a worldview interpretation [Argyrous, 1992, page 232], and the "system of perception" with which Robert Solo describes a paradigm, certainly constitutes an eclectic collection.

6. *A Paradigm is Not (Strictly) a Collection of Worldview, Theory Methodology and/or Values or Disciplinary Matrix*

As the foregoing indicates, a considerable number of economists describe Kuhn's paradigm as some sort of grouping of varied worldview, theory, methodological and/or valuational elements -- whether that be by defining their own collection of elements or by equating Kuhn's paradigm concept with his notion of a

disciplinary matrix. However, a few economists' characterizations of a Kuhnian paradigm challenge the definition of a paradigm as such a collection. Karel Williams, while regarding a paradigm as a "network" emphasizes that this network constitutes a whole greater than the sum of its parts: "A paradigm is a unity that cannot be fully reduced to atomic components; the identity of components, like terms and concepts, depends on the whole" [Williams, 1975, page 326].

George Argyrous goes further. While acknowledging that worldview, theory, methodology and values may be *implicit* in a Kuhnian paradigm, they do not -- even in part -- constitute the paradigm.<sup>29</sup>

The type of behavior that constitutes a paradigm may take many forms . . . Implicit in this behavior will generally be a particular methodological approach to a given subject, and, on a higher level, will even reflect an underlying world view. . . . But, . . . It is not simply a set of axioms (consumers seek to maximize utility subject to an income constraint) or a basic principle (economics analyzes the allocation of scarce resources among alternative uses), all of which can be written down in fairly explicit terms. [Argyrous, 1992, pages 233-234]<sup>30</sup>

## 7. *"Paradigm" as Exemplar*

As previously remarked, many economists allying Kuhn's paradigm concept with a disciplinary matrix include exemplars as one element of a paradigm. However, many economists understand Kuhn as defining exemplars not simply as a *part* of a paradigm, but as paradigms *in their own right*. Aidan Foster-Carter for example, in outlining Kuhn's paradigm concept, notes that:

At least in the natural sciences, it [a paradigm] is often constituted by an "exemplar": a key piece of research and/or discovery, which on the one hand explains or solves an important problem more satisfactorily than any previous attempt, and which on the other hand can never be so cut-and-dried that it fails to leave "puzzles" that still need solving." [Foster-Carter, 1976, page 169]

Likewise, Mark Blaug observes that "Kuhn frequently employed the term 'paradigm' in a dictionary sense to stand for certain exemplary instances of scientific achievement in the past" [Blaug, 1976, page 152].<sup>31</sup> Bruce Caldwell sees Kuhn

defining paradigms -- at least in one sense -- as exemplars, which Caldwell defines as "concrete, technical problem solutions which the students of a particular discipline encounter in gaining their professional education" [Caldwell, 1982, page 75].<sup>32</sup> Similarly, Chase describes a paradigm as exemplar as "a *shared* example . . . that accepted model which becomes an object for further articulation and specification under standards and conditions that meet some agreed upon criteria for stringency" [Chase, 1983b, page 815].

Argyrous, who provides one of the most detailed explications of Kuhn's paradigm concept,<sup>33</sup> also defines a Kuhnian paradigm as an exemplar:

This is the notion of paradigm-as-exemplar: a concrete piece of research or standard illustration that becomes a classic example of how "good" science is conducted and that suggests further research. [Argyrous, 1992, page 232]<sup>34</sup>

As such, exemplars "manifest" "a shared set of principles and rules covering theory, application, and instrumentation," among "members of a particular scientific community." But, as we have noted previously for Argyrous, it is the example itself and not that which underlies it that constitutes a paradigm *qua* exemplar; an exemplar is a "type of behavior," whose "strategic characteristic . . . is its *concreteness* . . ." <sup>35</sup> [Argyrous, 1992, page 233]. In sum, "A paradigm is a way of 'doing science,' and therefore can be fully grasped only in its performance" [Argyrous, 1992, pages 233-234]. For example, Milton Friedman's publications enunciating the Permanent Income Hypothesis themselves constitute paradigms, but not the theory, method, philosophy and/or values which may be abstracted from the works (e.g, a formal statement of the hypothesis itself or Friedman's (instrumental?) methodology).

## 8. *Summary*

Two common notions run through economists' definitions of a Kuhnian paradigm as an exemplar: (1) An exemplar is an example of *actual* scientific work -- and not the theory, method, etc. which may be inferred directly or indirectly from the

work (e.g., a scientific theory or method). Certainly Argyrous makes this point most emphatically by contrasting paradigms as exemplars ("a type of behavior") with that which implicitly underlies the paradigms (theory, method, worldview, etc.). However, other economists' definitions also make this point, describing an exemplar as a "piece of research and/or discovery," and "concrete technical problem solutions." (2) Exemplars are defined with respect to a given scientific community with which the exemplars are identified. Not all examples of scientific work constitute exemplars, only those which are generally accepted, only those which serve as models for members of a given community to follow.<sup>36,37</sup> In addition to providing examples of "good" science, exemplars also guide scientists, both as students in their training and as researchers in their choosing new avenues of scientific work.<sup>38</sup>

a. Exemplars and "Universally Recognized Achievements"

The reader may recognize a kinship between those who define a paradigm by quoting Kuhn's statement in which he defines paradigms as "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" [Kuhn, 1970c, page viii]<sup>39</sup> and those here who define a paradigm -- or at least one type of paradigm -- as an exemplar. As we have seen, Argyrous employs Kuhn's earliest description to bolster the understanding of a paradigm as an exemplar. However, many who cite Kuhn's page viii quote draw no such parallel. Further, we encounter at least two interpretive difficulties<sup>40</sup> in seeking to equate Kuhn's quote with a definition of an exemplar: (1) Not all scientific achievements may be examples of scientific work; some might, for instance, regard a theory (the outcome of scientific work, but not the work itself) as an achievement. (2) Not everything which provides "model problems and solutions to a community of practitioners" needs to be a model itself. Indeed, as we have seen already, economists have variously interpreted the quote. In addition, as we will see, numerous economists regard Kuhn's overall use and definition of paradigm to be ambiguous.<sup>41</sup>

Finally, we cannot overlook the possibility that many who cited Kuhn's quote did so for matters of expediency, rather than to provide an accurate understanding of a Kuhnian paradigm. Thus, an author citing Kuhn's preface quote may be doing so only because of its convenience, not because the quote best defines a Kuhnian paradigm -- let alone defines it as an exemplar. A.W. Coats, one who employs the quote, makes clear that he provides only a thumbnail sketch of Kuhn's theories and concepts.<sup>42</sup> Another, Donald Gordon, also provides only a brief sketch of Kuhn's philosophy of science.<sup>43</sup>

Thus, it would be imprudent to interpret those quoting Kuhn's early remark as defining a paradigm as an exemplar. We must therefore distinguish between the two definitions as (at least potentially) different interpretations of Kuhn's concept.

b. "Accepted Examples of Actual Scientific Practice" and Exemplars

Some economists define a Kuhnian paradigm by excerpting from the following description found in Kuhn:

. . . some accepted examples of actual scientific practice -- examples which include law, theory, application, and instrumentation together -- [which] provide models from which spring particular coherent traditions of scientific research. [Kuhn, 1962/1970c, page 10]

A.W. Coats, for instance, explains that, "A paradigm . . . incorporates 'accepted examples of scientific practice' which include 'law, theory, application and instrumentation together'" [Coats, 1969, page 290, quoting Kuhn, 1962, page 10]. Excerpting from the same passage in *The Structure of Scientific Revolutions*, Bruce Caldwell similarly defines a Kuhnian paradigm:

a paradigm . . . by which Kuhn means "some accepted examples of actual scientific practice -- examples which include law, theory, application and instrumentation together -- [which] provide models from which spring particular coherent traditions of scientific research." [Caldwell, 1982, page 71, quoting Kuhn, 1970c, page 10]

One might argue that the foregoing descriptions comport with the two major attributes of a paradigm as exemplar we previously identified: (1) "examples of *actual*

scientific practice," which are (2) "accepted" by the scientific community. However, what here counts as actual scientific practice differs from what counts under most descriptions of a paradigm as exemplar. Here, "law, theory, application and instrumentation" all constitute examples of actual scientific practice whereas, under most exemplar interpretations, these elements (with the possible exception of application) are either not explicitly identified as types of exemplars or are specifically excluded. Indeed, rather than fitting well among economists' identifications of a paradigm as exemplar, Coats's and Caldwell's descriptions more appropriately belong with those characterizing a Kuhnian paradigm as an eclectic collection of worldview, theory, technique and application.<sup>44</sup>

#### c. Exemplars and Disciplinary Matrices

Many economists see Kuhn identifying disciplinary matrices and exemplars as the two different major types of paradigms. Redman, for example, in addition to interpreting Kuhn as using the term paradigm to connote a disciplinary matrix, sees "Exemplars" as "the second major sense in which Kuhn uses *paradigms*"<sup>45</sup> [Redman, 1991, page 17]. Richard Chase distinguishes between "the paradigm in its most global sense," which connotes a "disciplinary matrix" and "the exemplary paradigm" [Chase, 1983b, pages 814-815]. Jalladeau makes a similar distinction between a paradigm "in its larger conception" (a disciplinary matrix) and a paradigm "in its strict sense," (an exemplar) [Jalladeau, 1978, page 588]. Along the same lines Bruce Caldwell observes that Kuhn proposed the two terms, disciplinary matrix and exemplar, to encompass "most of the meanings formerly adduced to the single concept paradigm" [Caldwell, 1982, page 75].<sup>46,47</sup>

Some economists point out that Kuhn regarded the understanding of a paradigm as exemplar as richer and more significant than the understanding of the notion as a disciplinary matrix:

The paradigm in this sense of the term [as exemplar] has, in Kuhn's estimation, the deeper of the two meanings (p. 175). Kuhn notes that the exemplary paradigm is the "central element of what I now take to be the most novel and least understood aspect of this book." (p. 187) [Chase, 1983b, page 815, page numbers in parentheses are Chase's and refer Kuhn, 1970c]

This is the notion of paradigm-as-exemplar . . . Kuhn stressed this latter notion of paradigm as the more significant of the two in explaining the basis on which scientific communities resolve questions of theory choice. [Argyrous, 1992, page 232]<sup>48</sup>

Argyrous further describes the understanding of "paradigm" as a disciplinary matrix as the one which Kuhn "regarded as the least innovative and important" [Argyrous, 1992, page 233].

According to some economists, Kuhn introduced the notion of a disciplinary matrix not simply to clarify his prior use, but to re-direct focus away from an understanding of a paradigm as such a matrix, toward one of an exemplar. Peabody, Jalladeau and Argyrous point out that Kuhn sought to distinguish between disciplinary matrices and exemplars in order to emphasize the latter understanding. Peabody only footnotes the point. Having himself defined a paradigm as a disciplinary matrix in the body of the text, Peabody explains Kuhn's reason for introducing the notion of a "disciplinary matrix":

In the postscript to the second edition Kuhn notes that he has used the term paradigm in a variety of ways, but two major usages can be distinguished. In one sense paradigm has been used to denote the entire constellation of theories, values, etc. to which the specific scientific community is committed. . . . However, in the postscript Kuhn suggests the term "disciplinary matrix" instead of paradigm be used for this ensemble of group commitments. He prefers to reserve the use of paradigm for the exemplars or shared examples from which the scientist learns his trade. [Peabody, 1971, page 15, note 3]

Jalladeau understands the purpose of Kuhn's introduction of "disciplinary matrix" much the same way. Kuhn "reserved the term *paradigm* in its strict sense" for exemplars:

What the members of this community commonly possess is what Kuhn, in his original text, calls paradigms. In response to criticisms, he recognized that, used in this sense, the term *paradigm* is not proper. In a revised statement he therefore proposed the expression "*disciplinary matrix*." . . . To those examples common to the group is reserved the term *paradigm* in its strict sense. [Jalladeau, 1978, page 588]

However, while both note Kuhn's later position, each economist continues to apply "paradigm" to both disciplinary matrices as well as exemplars. As we have just noted Jalladeau, while noting Kuhn's position, describes the two notions as different types of paradigms and Peabody, in the main text of his article, defines a paradigm as a disciplinary matrix [Jalladeau, 1978, pages 588-589; Peabody, 1971, page 1].

Argyrous, on the other hand, seeks himself to re-direct attention away from a more "prevalent" worldview conception of paradigm towards what he sees as a more fruitful understanding of a paradigm as an exemplar. To this end, he highlights Kuhn's reason in the body of his text:

Most discussions have employed *paradigm* in the sense of worldview. Yet this is only one of the major ways in which Kuhn himself used the word. Because of this common reading, Kuhn chose to use the term *disciplinary matrix* when discussing paradigms-as-world-views. He did this to refocus attention onto a second notion of paradigm that most writers have neglected (in economics at least). This is the notion of paradigm-as-exemplar: a concrete piece of research or standard illustration that becomes a classic example of how "good" science is conducted and that suggests further research. Kuhn stressed this latter notion of paradigm as the more significant of the two in explaining the basis on which scientific communities resolve questions of theory choice. [Argyrous, 1992, page 232]

Further, Argyrous, unlike Peabody or Jalladeau, defines a Kuhnian paradigm -- in consonance with what he sees as Kuhn's avowed position -- strictly as an exemplar [Argyrous, 1992, page 233].<sup>49</sup>

In sum, according to many economists, Kuhn defined and used "paradigm" both as a disciplinary matrix and an exemplar. A few note that Kuhn regarded the exemplar understanding of a paradigm as richer than a disciplinary matrix interpretation and even sought to reserve the term "paradigm" to the former sense. Still fewer both note that Kuhn took these positions, as well as integrate them into their own interpretation of Kuhn's concept.



## 9. *Paradigms and the (Scientific) Community*

As the prior sections indicate, economists hold a variety of positions regarding the composition of a Kuhnian paradigm. Some equate the concept with a worldview. Others define the concept as consisting of a myriad of elements, encompassing some or all of the following areas: metaphysics/epistemology, theory, methodology, technique, and actual scientific practice and/or work. Still others equate the concept solely (most especially) with actual examples of scientific work. However, within each of the understandings of the composition of a Kuhnian paradigm, we find economists who define a paradigm in terms of its relationship to a given community (of scholars, professionals, scientists).

Among those defining "paradigm" as worldview, Routh equates a paradigm not simply with *any* worldview, but one "whose acceptance is essential for those who wish to be accepted into the fraternity or 'invisible college'"<sup>50</sup> [Routh, 1989, page 26]. As we have already noted, many economists who define a paradigm as that *particular* collection of worldview, theory, method and principles that a given group "shares," to which it is "committed," and/or which it "accepts." L.E. Johnson includes "professional relationships" as an element of a Kuhnian paradigm. Similarly, economists have stressed the *disciplinary* nature of a paradigm defined as a disciplinary matrix. And as we have just seen, a key element in economists' definition of a paradigm as an exemplar is the privileged status which the paradigm holds for members of a scientific community as an example of "good" scientific work for the group's members to emulate and be guided by.<sup>51</sup>

Some economists explicitly underline a paradigm's acceptance by the scientific community and its shared nature in their characterization of a Kuhnian paradigm. Jalladeau, for instance, sees Kuhn defining a given scientific community's paradigm<sup>52</sup> as that which "members of this community commonly possess" [Jalladeau, 1978, page 588]. As we noted earlier, Tribe maintains that "the major point is that it [paradigm] is

something that the community *shares*, it is this that makes them a community" [Tribe, 1973, page 469]. Employing economic terminology, Mark Zupan describes paradigms as "public goods" [Zupan, 1991, page 102].<sup>53</sup>

L.E. Johnson and Robert D. Ley link the acceptance of a given paradigm with a group of scientists' acceptance of "common procedures in their scientific work."<sup>54</sup> Likewise Timur Kuran stresses that, "A paradigm, as Thomas Kuhn emphasizes repeatedly, entails *communal* agreement on the part of a group of scientists as to methodological principles and basic explanations" [Kuran, 1988, page 155].

And, finally, Redman points out that Kuhn first introduced the term paradigm "to show that the scientific community has certain things in common: exemplars, values, teaching methods, metaphysical principles, and so on" [Redman, 1991, page 17].<sup>55</sup>

Breit quotes Kuhn directly to remark upon the reciprocal relationship between a given paradigm and scientific community: "In his [Kuhn's] words, 'a paradigm is what the members of a scientific community share *and*, conversely, a scientific community consists of men who share a paradigm'"<sup>56,57</sup> [Breit, 1987, page 827, quoting Kuhn, 1970c, page 176].

#### 10. *Paradigms as (Relatively) Inviolable*

A number of economists associate Kuhnian paradigms with relative inviolability of one sort or another. By definition, a paradigm is not something which is easily discarded:

##### a. Unfalsifiable

For some, paradigms must, by necessity, be unfalsifiable. According to Lawrence Boland:

The paradigms are the pool of available assumptions used to construct the logical proof of any falsification. They are the basis of our test conventions and are for the present -- the everyday workings of "normal science" -- considered beyond question. [Boland, 1977, pages 97-98]

Likewise, Jon Wisman points out Kuhnian paradigms "are themselves incapable of being falsified through empirical testing" [Wisman, 1979, page 26], and Bøhren remarks that a "paradigm is considered immune to empirical testing" [Bøhren, 1990, page 11].

b. Tenaciously Held to

Without pinning inviolability to logical necessity, other economists describe paradigms as something to which a given scientific community fiercely holds. According to Cornwall, a Kuhnian paradigm connotes something to which practitioners adhere tenaciously and are loathe to abandon [Cornwall, 1979, page 71]. Similarly, S. Parsons allies Kuhn's paradigm concept with "taken for granted scientific truths" [Parsons, 1990, page 317]. James Peach and James Webb, describe a paradigm as "that portion of the body of theory and accompanying ontological beliefs that distinctively characterize a theory and to which scientists accepting the theory hold most tenaciously" [Peach and Webb, 1983, page 713, note 5].<sup>58</sup>

11. *Paradigms as All-Dominating*

Similar to understanding a paradigm as not easily discarded, economists also characterize a Kuhnian paradigm as exercising strong intensive and extensive influence (power) over the scientific community with which the paradigm is associated. For Robert Solo paradigms are "deeply inculcated in the discipline and perpetuated through its successive generations" [Solo, 1991, page 33]. Ken Cole, John Cameron and Chris Edwards similarly understand a Kuhnian paradigm as something which is firmly entrenched within a given discipline. According to them, Kuhn's paradigm concept "draws attention to the tendency for groups of intellectuals to become totally absorbed in the logical puzzles of a particular theory" [Cole, Cameron and Edwards, 1983, page 11].

Jalladeau describes a Kuhnian paradigm as something that controls all aspects of the scientific enterprise:

The paradigm is an articulated system of analytical concepts, methodological principles, techniques, and values constituting the frame of reference governing *every* discourse. [Jalladeau, 1978, page 584, emphasis added]

Similarly, D.P. O'Brien asserts that a paradigm "governs *all* 'normal scientific' activity . . ." [O'Brien, 1976, page 141, emphasis added].

Most forcefully, however, Phyllis Deane observes that Kuhn characterizes a paradigm as exercising complete and total control over members of a scientific community adhering to the paradigm:

The upshot of the debate on Kuhn's theory of scientific revolutions seems to be that there is a strong element of "rhetorical exaggeration" in his concept of a paradigm which fully determines both the world-view of practising scientists and the research agenda of "normal" scientific activity . . . . [Deane, 1978, page xii, quoting Toulmin, 1972, pages 105-106]

## 12. *Level of Generality of Paradigms: Paradigms at the Sub-discipline Level*

As the foregoing discussion indicates, most of the definitions of paradigm offered by economists describe a paradigm as an entity spanning an entire discipline. However, a number of economists point out that Kuhn allowed for paradigms at the sub-discipline level. For example, John Pheby notes that, in writings after the first edition of *The Structure of Scientific Revolutions*, Kuhn sought to clarify that paradigms "are not necessarily monolithic structures that dominate a whole field of scientific activity" and that we may speak of paradigms existing at high degrees of specialization [Pheby, 1988, page 49]. Donald Gordon points out that Kuhn's schema permits "sub-paradigms," which do not span the entirety of a given discipline [Gordon, 1965, page 124]. Hausman remarks that Kuhn's disciplinary matrix refers, *in the main*, to very small communities of only "a few dozen scientists" [Hausman, 1992, page 83].

### 13. *Paradigms and Lakatos's Methodological Research Programmes*

A number of economists detect parallels between Kuhn's paradigm concept and philosopher-of-science Imre Lakatos's notion of scientific research programme.<sup>59</sup>

Many assert an affinity between the two philosophers' concepts:<sup>60</sup>

The methodology of scientific research programs may be considered as an answer to the Kuhnian paradigm approach. The analytical system of Lakatos appears to be created, with the exception of terminology, in the likeness of the Kuhnian model. The research program and its hard core call to mind the concept of paradigm. [Jalladeau, 1978, page 588]

Glass and Johnson see Kuhn's "paradigm" and Lakatos's "scientific research programme" as underlining the need to look at science in terms of theoretical frameworks, not individual theories:

Just as Kuhn's notion of a paradigm emphasizes the need to analyse science in terms of theoretical frameworks or structures (rather than in terms of individual theories....), so also Lakatos's notion of a research programme places the emphasis on theoretical frameworks rather than on individual theories. [Glass and Johnson, 1989, page 166]

Blaug sees a similar parallel, though he finds Lakatos making the point more effectively [Blaug, 1976, pages 149-150]. Many more ally Kuhn's paradigm with Lakatos's notion of a hard core. Vassilios Filios asserts that a Lakatosian hard core "resembles a Kuhnian paradigm which becomes dominant when it is adopted by the scientific community" [Filios, 1984, page 777]. L.E. Johnson, Peach and Webb and Bøhren also link the two notions:<sup>61</sup>

Lakatos's "hardcore" of SRP's can be interpreted as one possible set of paradigm defining characteristics. [Johnson, 1983, page 1108, note 6]

Lakatos's "hard core" is much the same as Kuhn's "paradigm" (in the sense of disciplinary matrix). [Peach and Webb, 1983, page 713, note 5]

The SRP is subdivided into two parts; the hard core and the protective belt. The hard core, which resembles Kuhn's paradigm, contains shared commitments which are not subjected to empirical testing. [Bøhren, 1990, page 11]

While economists do not always make clear the nature of the likeness between Kuhn's and Lakatos's concepts, many of the above comparisons indicate that Lakatos's

hard core and Kuhn's paradigm represent that which a scientific community holds to tenaciously.<sup>62</sup>

Some economists interchange Lakatos's terminology (scientific research programme) with Kuhn's (paradigm). Melvin Reder, for example:<sup>63</sup>

Were it not for fear of becoming involved in side issues, I would have suggested that Chicago economics is a scientific sub-culture in the Kuhnian sense, and spoken of the "Chicago Paradigm" (or family of paradigms), or of the "Chicago Scientific Research Programme" (*pace* Imre Lakatos), rather than the Chicago View. [Reder, 1982, page 319]

In addition, both Ann Mari May and John R. Sellers, and David Colander and Kenneth Koford, whose articles primarily employ Lakatosian terminology, provide instances in which Kuhn's term is interchanged with Lakatos's.<sup>64</sup>

However, the degree to which economists interchange Lakatos's terms "scientific research programme" and "hard core" with "paradigm" should be kept in perspective. The latter two articles each interchange "paradigm" and "scientific research programme" only once. Further, only one paper published from an economics symposium examining Lakatos's philosophy of science and its application to economics [Latsis, 1976] explicitly compares Kuhn's and Lakatos's respective notions: Mark Blaug's "Kuhn versus Lakatos, or Paradigms versus Research Programmes in the History of Economics" [Blaug, 1976]. Here, Blaug describes Lakatos's notion of a scientific research programme as only "faintly reminiscent of Kuhn's 'paradigms.'"<sup>65</sup> In none of the published symposium papers do we find the two philosophers' notions interchanged with one another [Latsis, 1976].

#### 14. *The Relationship between Paradigms and the Dialectic*

A few authors ally Kuhn's paradigm concept with the thesis of a Hegelian or Marxian dialectic. Richard Chase, presenting a dialectical interpretation of Kuhn's theory of science, identifies both the thesis and synthesis in the dialectical process as collections of paradigms (understood as disciplinary matrices) [Chase, 1983b, page

821]. Likewise, Seigfried Karsten likens the thesis of a dialectic with Kuhn's paradigm:

The first step in the Hegelian triad is the thesis or being. It is the thing or the concept which exists at a given point in time. As such it corresponds to Kuhn's concept of a paradigm. [Karsten, 1973, page 407]

Even Martin Bronfenbrenner who sharply contrasts Kuhn's theory of science with his own "crude dialectic" asserts that the dialectic's thesis "includes a set of what Kuhn calls 'standard paradigms'" [Bronfenbrenner, 1971, page 139].<sup>66</sup>

#### 15. *The Relationship between Paradigms and Theories*

A large number of economists have related Kuhn's "paradigm" concept with that of "theory," and economists' understanding of the relationship between the two notions varies widely. However, before examining the differing understandings, an interpretive caveat must be issued: Most economists who relate the two terms do not define one or both of the concepts independent from one another. Earlier discussion demonstrates the wide range of understandings of Kuhn's "paradigm" concept. Further, economists have broadly and variously explicated the "theory" concept [Samuels, forthcoming]. Thus, we must bear in mind that the wide range of understandings of the relationship between "theory" and "paradigm" may be due not only to differing interpretations of Kuhn's "paradigm" concept, but to varying understandings of the "theory" concept as well.

##### a. Exemplar as Part of a Theory

Bart Nooteboom identifies Kuhn's "exemplar" as *one of five* elements comprising a theory.<sup>67</sup>

##### b. Paradigm as a Theory

Zinam defines a paradigm "in its broadest interpretation," as "a scientific theory accepted by a community of scholars in a given science" [Zinam, 1982, page 363].

c. Collection or Framework of Theories

On the other hand, others understand Kuhn's paradigm concept as a *collection* or *framework* of theories. Redman affirms that, "usually when Kuhn uses the term *disciplinary matrix* (or *paradigm*), he means a theory or complex of theories" [Redman, 1991, page 23, note 9]. Likewise, Takashi Negishi equates a paradigm with "an accepted theoretical framework."<sup>68</sup> Henry Woo contrasts Kuhn's notion of paradigms with individual theories and likens it instead to "systems of theories" [Woo, 1990, page 34]. Glass and Johnson describe paradigms as "theoretical frameworks," and differentiate them from "individual theories" [Glass and Johnson, 1989, page 166].<sup>69</sup>

d. Theory as One Among Many Elements of a Paradigm

As we saw,<sup>70</sup> many economists see theory as only one among many of the elements which compose a paradigm as an eclectic collection or disciplinary matrix. Coats makes the point explicit: "A paradigm is not simply a theory," but instead "incorporates 'accepted examples of scientific practice' which include 'law, theory, application and instrumentation together'"<sup>71,72</sup> [Coats, 1969, page 290, quoting Kuhn, 1962, page 10]. Similarly, Wisman describes a theory as "subspecies of a larger 'paradigm'" [Wisman, 1978, page 269].

e. Paradigms as Prior to Theory

Still others regard paradigms as *prior* to theory. Aidan Foster-Carter regards a paradigm as a "*pre-theoretical* entity" [Foster-Carter, 1976, page 168, emphasis added]. Richard Chase explicitly warns of conflating "paradigm" with "theory" and describes a paradigm as prior to and independent from theory:

exemplary paradigms are not to be confused with theory in the sense of theory as generalizing scientific statement and explanation. Indeed, such paradigms are *prior* to theory and thus exist independently of it. A science's theory, along with its laws and concepts, may be abstracted from its exemplary paradigm(s). [Chase, 1983b, page 816]

Bøhren also implies that paradigms (in particular the implicit assumptions included within them) are prior to "the specific theories generated within the paradigm."<sup>73</sup>



Brian Loasby holds a similar position, stressing the distinction between paradigm and hypothesis. A paradigm allows for the creation of, but is not, itself, a hypothesis:

. . . some economic hypotheses turn out not to be hypotheses at all, but paradigms . . .

. . . . .  
 . . . a paradigm defines a set -- often a very large set -- of possible hypotheses, but makes no claims for the validity of any particular members of that set . . . [Loasby, 1971, page 866-867]<sup>74</sup>

Finally, as we noted earlier, Argyrous defines a paradigm in opposition to that which lies implicit within it (including theory).

f. Scientists Hold to Paradigms More Tenaciously Than to Theories

Others distinguish between paradigms and theories on the basis of their *falsifiability* and/or *tenacity*. As we saw earlier, Cornwall specifically distinguishes between paradigms and theories on the basis that the former are less easily abandoned than the latter. Boland draws a similar contrast in terms of the falsifiability of theories versus paradigms:

The danger for the would-be methodologist here is that "theories" might be confused with the "world view;" as to equate the "world view" with our theories (e.g., the theory of the firm would turn all of economics into a tautology.[]) That is, if our basic theories are treated as paradigms -- there would be nothing to test. The fact that we consider alternative theories (of the firm or the consumer) means that the standard theory is *not* a paradigm -- no matter how standard. [Boland, 1977, pages 98-99]

Why the diversity of interpretations regarding the relationship between the "theory" concept and Kuhn's "paradigm" concept? At the outset of this section, we provided two possible explanations: (1) economists' multiple interpretations of a Kuhnian "paradigm" and (2) economists' multiple interpretations of "theory." While, for example, Zinam refers to a paradigm as a "theory," we also have seen that he defines a paradigm as a collection of eclectic elements. Coats, likewise, defines a Kuhnian natural science paradigm as an eclectic collection of elements. But, the latter specifically contrasts a paradigm with a theory, specifically on the basis of a paradigm's being a multi-dimensional collection. Other possible reasons include: (3)

Economists, depending upon their project, are interested in highlighting differing aspects of paradigm and/or theory. Thus, the differences cited may be a function of the purpose at hand -- in addition to different understandings of "paradigm" -- in which an incomplete and necessarily selective definition of paradigm and theory is employed. Cornwall for example employs the term paradigm in order to underline the fact that economists are loathe to reject an existing body of knowledge -- as opposed to "theory" which, to him, suggests a greater likelihood of its being replaced and/or rejected. (4) Kuhn himself may have used and/or defined "paradigm" in a multitude of ways<sup>75</sup> giving rise to a broad range of interpretations not only of the paradigm concept, but of its relationship to theory as a well.

#### 16. *Summary*

In sum, economists' interpretations of a Kuhnian paradigm vary widely. For some, it constitutes a worldview. For many, a paradigm is a collection of various types of elements including some or all of the following: worldview, theory, values and prescriptions, technique, examples of actual scientific work. Some associate the concept with one or the other of the two notions Kuhn introduced after the first edition of *The Structure of Scientific Revolutions*: disciplinary matrix and exemplar. Others draw parallels between Kuhn's paradigm notion and Imre Lakatos's notions of scientific research programme and hard core, and a few ally paradigm with a dialectical thesis (synthesis).

In addition, a considerable divergence of opinion exists regarding the relationship between "theory" and "paradigm." For some, a paradigm constitutes a theory; for others, a theoretical framework or group of theories. Still others see theory as one among many types of elements that compose a paradigm. Others specifically contrast the two notions: for some, a paradigm is prior to and independent of theory; for others, less easily discarded.

However, we have also found a common thread that runs through many economists' definitions of "paradigm": a paradigm is something that a given (scientific) community shares and/or holds in high esteem.

17. *Length and Detail of Economists' Explicit Definitions of a Kuhnian Paradigm*

Before moving on, we must make two points. First, economists provide relatively little explicit discussion regarding the definition, function, and/or ambiguity of Kuhn's paradigm concept. While the economics literature abounds with articles identifying Kuhnian paradigms in economics,<sup>76</sup> most economists provide little or no such discussion. Many employ the term "paradigm" without any characterization of the term at all. Robert Heilbroner, for example, merely remarks in a footnote that he employs "paradigm" as Kuhn defined it, without further comment [Heilbroner, 1971, page 20, note 4]. Even among those who do provide some discussion, most provide extremely brief descriptions (a single sentence to a few brief paragraphs).<sup>77</sup>

Second, for clarity of exposition, we have separated discussion of the definition of paradigm and the function of paradigm into two separate discussions. In the preceding section, we have examined paradigm function only to the extent to which we found it impracticable to present an economist's definition of paradigm apart from its function. In the following discussion, we will provide much more comprehensive treatment of economists' interpretations of the function of Kuhnian paradigms. Our doing so should, however, not be taken to suggest that the definition of a paradigm and understanding of its function can be separated.

## B. THE FUNCTIONS OF PARADIGM

Economists have identified a number of functions which paradigms perform. As with economists' accounts as to what constitutes a paradigm, we locate both a common thread running throughout many of the functions identified (paradigms, in one

way or another, channel scientific inquiry), as well as significant disagreement. In particular, we find that some economists lay emphasis upon how the limits which paradigms impose upon a science impede scientific inquiry, while others point out how those limits facilitate (or are even necessary for) scientific work. We first consider those ways in which economists see paradigms channeling scientific inquiry.

### 1. *Guides Scientific Research and Activity*

Most fundamentally, many economists understand paradigms as guiding scientific activity. Coats notes that "the paradigm's function is regulative (i.e. normative) as well as cognitive since it provides the scientist not only with 'a map, but also with some of the directions essential for map-making'" [Coats, 1969, page 290, quoting Kuhn, 1962, page 108].<sup>78</sup> According to Redman, the elements of a paradigm "guide scientists in their work and members of a given scientific community" [Redman, 1991, page 16], and Worland observes that "It is the function of the paradigm to 'guide research'" [Worland, 1972, page 275]. For Zinam, Kuhnian paradigms serve to "direct and limit production of scientific theory" [Zinam, 1981, pages 70-71].<sup>79</sup>

L.E. Johnson maintains, as well, that a paradigm serves a vital and central regulative function in determining the foci of scientists' work, both in terms of what is studied and how. However, Johnson argues that Kuhn, in his own account of a paradigm, either ignores or leaves implicit the central regulative element of a paradigm (what Johnson terms the "purposive function") [Johnson, 1980, page 56; Johnson, 1983, pages 1101-1104].<sup>80</sup>

#### a. *Restricts the Realm of Legitimate Subject Matter, Methods, and Solutions*

Many economists see Kuhnian paradigms as guiding scientific research by restricting (defining) the realm of legitimate scientific inquiry. In particular, paradigms restrict

(1) The questions that may be asked:<sup>81</sup>

The questions any science asks are fundamentally limited and conditioned by its underlying paradigm . . . [Sweezy, 1971, page 60]

The central value of a paradigm is that *it sets the problem to be solved*. [Stent, 1976, page 3]

. . . one of the important things which a paradigm does is to limit our view of what is the proper area of scientific concern. [O'Brien, 1976, page 142]

The existing exemplar sets the fashion and direction for inquiry not only by raising questions, but also by suppressing problems by defining them to lie outside the paradigmatic pale. . . [Chase, 1983b, page 817]

(2) The means by which those questions may be answered:<sup>82</sup>

A paradigm, in the natural sciences as well as in economics, defines the type of relationships to be investigated . . . [Loasby, 1971, page 866]

It is the function of the paradigm to "guide research." It does so by defining problems and methods, leading the scientist to concentrate on a limited range of problems to "investigate some part of nature in detail and depth. [Worland, 1972, page 275]

the dominant paradigm . . . directs the practitioner as to the key questions and appropriate methods of normal research. . . . The accepted paradigm defines the appropriate problems to pursue and the procedures to be used for this pursuit . . . [Stanfield, 1974, pages 98-99]

(3) The answers that are regarded as legitimate:<sup>83,84</sup>

A Kuhnian paradigm consists of a general world view that . . . indicates . . . the solutions deemed acceptable. [Miller, 1991, page 994]

The paradigm . . . determines the form that answers, to be acceptable, must assume. [Peabody, 1971, page 1]

Many economists describe paradigms as performing all three functions:

The paradigm provides the conceptual framework in which research is conducted; it determines which question will be asked and determines the form that answers, to be acceptable, must assume. [Peabody, 1971, page 1]

A paradigm, in the natural sciences as well as in economics, defines the type of relationships to be investigated and the methods and abstractions to be regarded as legitimate within a particular problem area. [Loasby, 1971, page 866]

A paradigm provides the conceptual framework of scientific research; it delimits the nature of the problems and the types of questions that can be grappled with; it determines the methods which can be utilized and the form in which the answers must be clothed in order that their validity can be ascertained. [Jalladeau, 1975, page 2]<sup>85</sup>

Similarly, Miller maintains:

A Kuhnian paradigm consists of a general world view that shapes perception and constitutes a guide to practice; that is, that indicates the nature of the problems (puzzles) to be addressed, the tools to be used in reaching closure, and the solutions deemed acceptable. [Miller, 1991, page 994]

John Cohen and David Lewis describe a Kuhnian paradigm as playing an even broader role in setting out the realm of scientific work:

In any science at a given point in time, there is generally a fundamental image defining the agenda of topics to be studied, the concepts used in inquiry, the validity of propositions that emerge, the essential elements of the model or framework that integrates them, and appropriateness of policy actions based on the accumulated research. Such an image constitutes a paradigm. [Cohen and Lewis, 1979, page 523]

Some economists point to the obverse of paradigms' restricting the field of the legitimate: their delimiting the realm of the illegitimate. Routh describes a paradigm as setting out not only phenomena "that shall be admitted," but "excluded" as well [Routh, 1973, page 182]. Likewise, Chase characterizes paradigms as both "raising questions" as well as "suppressing problems by defining them to lie outside the paradigmatic pale" [Chase, 1983b, page 817].<sup>86</sup> Stephen Hymer and Frank Roosevelt compare a paradigm with a flashlight to make much the same point. While a paradigm illuminates certain matters; it leaves many others "in the dark":

A paradigm provides a fixed conceptual framework for scientific research, placing limits on the type of *questions* that can be asked, the *methods* that can be used, and the *answers* that are acceptable. Thus a paradigm is like a flashlight in that it allows the scientist to shed light on certain questions, while at the same time leaving large areas in the dark. [Hymer and Roosevelt, 1972, page 645]

One way in which paradigms restrict the problems to be considered is by defining those matters that lie beyond its ken as unscientific:

. . . a science's exemplary paradigm will lead its practitioners to reject certain problems as being either metaphysical or as within the purview of another discipline. [Chase, 1983b, page 817]<sup>87</sup>

b. Governs the Interpretation of "Facts"

Another chief means by which paradigms direct scientific research is by governing the interpretation of the "facts" which scientists encounter. For instance, Bøhren remarks, "The paradigm states proper ways of collecting and interpreting data (for instance, the relevance of laboratory findings for real-world contexts)" [Bøhren, 1990, page 10]. Zweig notes that, "a paradigm . . . serves as the basis for interpretation of raw data and sense perceptions" [Zweig, 1971, page 44].<sup>88</sup>

c. Governs the Selection of Facts to be Studied

Paradigms not only determine how facts will be interpreted, but what facts will be examined in the first place. As Filios strikingly describes, "any particular paradigm, among other things, specifies what are facts and what are non facts" [Filios, 1984, page 777]. Similarly, Bøhren describes paradigms as determining which facts are and are not worthy of study:

The paradigm broadly defines what phenomena are worth studying (for instance, the pricing of shares) and states the rationale for this preference (for instance, changing share prices which influence social welfare in significant and unpredictable ways). [Bøhren, 1990, page 10]

d. Determines What Is (Not) Seen

Bøhren's characterization suggests that paradigms guide scientists to examine certain facts and to put aside consideration of others. However, his characterization implies that scientists have the (ill-advised) option of researching non-worthwhile facts. Many economists describe paradigms as not simply determining what facts should concern the scientists and which ones should not; they go further and assert that paradigms determine what facts scientists will see in the first place and which facts they will be blinded to. Indeed, the characterization of a paradigm by many economists as consisting of or containing a worldview implies that paradigms serve to limit scientists' field of vision and the facts they see, as well as their interpretation of those facts.<sup>89</sup>

Many economists explicitly make the point that paradigms influence (determine) what scientists see and do not see. In the words of Brian Loasby, "A paradigm produces intellectual tunnel vision":

An experiment in which subjects readily identified as normal wrongly coloured playing cards inserted into an otherwise normal pack provides some formal confirmation of the common experience in all manner of contexts that observations are "fitted to one of the conceptual categories prepared by prior experience." A paradigm produces intellectual tunnel vision. [Loasby, 1971, page 868, quoting Kuhn, 1962, page 63]

Likewise Foster-Carter maintains that "For Kuhn a paradigm is indeed a limitation of vision . . ." [Foster-Carter 1976, page 169]. Finally, Worland makes clear a paradigm completely blinds certain "facts" from scientists employing it:

It [a paradigm] serves to forestall random fact gathering by directing the scientist toward those facts which the paradigm "displays as particularly revealing." . . . Phenomena that will not fit into the scheme of the paradigm "are often not seen at all." [Worland, 1972, page 275, quoting Kuhn, 1970c, pages 25 and 24 respectively]<sup>90</sup>

## 2. *Insulates Scientists from Broader, Less Technical Pressing Social Problems*

Paradigms also distance the scientists who work within them from broader, pressing social problems. For instance, Peabody and Miller comment<sup>91</sup>:

Indeed, the paradigm can insulate the community from socially important problems if they cannot be stated within the conceptual framework the paradigm provides. [Peabody, 1971, page 2]

Kuhn notes, moreover, that: "A paradigm may insulate the community from those socially important problems that are not reducible to the puzzle form. . ." [Miller, 1991, page 994, quoting Kuhn, 1962, page 37]

## 3. *Summary*

Thus, in their interpretation of Kuhn, economists see paradigms chiefly as imposing various restrictions upon the scientists working within them: restrictions upon (1) the choice of problems and questions to be addressed, (2) the selection of means by which to pursue solutions and answers, (3) acceptable answers and solutions, and (4) the interpretation, selection and perception of "facts." We next lay out



economists' disagreements as to the implications of these limits for the practice of science.

#### 4. *Impairs Scientific Research*

Some economists see the limits which paradigms impose as hindering scientists in their work. According to Sweezy's interpretation of Kuhn, the limitations which paradigms place upon the questions scientists may ask, in time, impede scientific advance:

The questions any science asks are fundamentally limited and conditioned by its underlying paradigm, which in time thus tends to become a hindrance rather than a stimulus to further advance. [Sweezy, 1971, page 60]

Wilfred David goes further. He finds the limits which a paradigm sets as, by their nature, "hindering scientific progress."<sup>92</sup>

According to Worland, a paradigm exacts a "price" on scientists by blinding them to "phenomena" not amenable to the paradigm.<sup>93</sup> Similarly, for Van Weigel, that paradigms insulate scientists from pressing social problems constitutes an impediment to science:<sup>94</sup>

Unfortunately shared interpretive paradigms do not always facilitate scientific investigation. Kuhn points out that sometimes a paradigm may delay progress by insulating the academic "community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies." [Weigel, 1986, page 1423, quoting Kuhn, 1970c, page 37]

Perhaps most seriously, Michael Storper charges that by acting as a filter, a paradigm obscures its faults from those working within it:<sup>95</sup>

The paradigmatic reasoning that essentialist paradigms use does not even succeed at the instrumental tasks of prediction or technical analysis, because social life consists of open-ended historical processes. The consequence of essentialism and the formalisms associated with it is that the array of possibilities in economic development -- spatial and otherwise -- is obscured, because essentialist paradigms serve as cognitive filters for research agendas.

This point is hardly new, having been made forcefully by Kuhn with respect to the natural sciences and subsequently articulated over the debates . . . [Storper, 1985, page 261]

##### 5. *Facilitates Certain Aspects of Scientific Inquiry*

On the other hand, economists also identify a number of different ways in which the limits a paradigm imposes assist scientists in various aspects of their work.

###### a. Guarantees that Solutions Exist

First, paradigms, chiefly by restricting what questions may be asked and how they may be answered, ensure that answers and solutions will exist to the questions that scientists will ask:<sup>96</sup>

The paradigm defines legitimate research problems and methods, supplies clues to their solution, and guarantees that the clever researcher will find a solution. [Peabody, 1971, page 2]

The accepted paradigm defines the appropriate problems to pursue and the procedures to be used for this pursuit, and it guarantees that solutions exist to the problems using these procedures. [Stanfield, 1974, page 99]

In addition to regulative and cognitive functions, the exemplary paradigm also serves to provide a science and a scientific community with its "promise for success." It does this by establishing the criteria for selecting out problems that can reasonably be assumed to have solutions, for choosing data which are relevant . . . and for selecting and applying appropriate analytic methodology. [Chase, 1983b, page 816]

Albert Breton and Ronald Wintrobe cast this understanding of Kuhn's paradigm function in economic terms. A paradigm serves as a "screening device" by which questions for which no answers can be found are filtered out in order that economists do not waste time on seeking to answer the unanswerable and thus may "economize the costs of testing or experimentation" [Breton and Wintrobe, 1992, page 225].<sup>97</sup>

###### b. Allows Scientists to Ignore Social, Methodological and Epistemological Questions and Focus Upon a Narrowly Defined, Manageable Set of Problems and Questions

Many economists point out that the limitations imposed by paradigms free scientists working within them from having to determine and/or assess the basis, scope

and/or justifications of their work and, thus, allow them to focus their energies on narrower, more tractable questions.

According to Boland, paradigms provide one means by which scientists may avoid an infinite regress in the search for a basis upon which to conduct empirical testing [Boland, 1977, pages 97-98].<sup>98</sup> A paradigm, for those scientists accepting it, provides the point at which the practitioners no longer need to seek legitimation. More broadly, a Kuhnian paradigm, according to Coats's interpretation of Kuhn, allows scientists to put aside questions as to the overall foundation of a science's work. A paradigm

enables the scientists in that field to take the foundation of their knowledge for granted and concentrate their attention on the solution of more concrete problems, or "puzzles." [Coats, 1969, page 290]<sup>99</sup>

Likewise, paradigms enable scientists to avoid questions of scope and, more fundamentally, definition of a science (and the associated psychological discomforts) and focus their energies on work within the confines defined by the paradigm:

Paradigms, far from avoiding the labour of thought, may call for both intense and protracted effort if they are to be expressed in viable hypotheses. Their virtue, in this respect, lies in permitting that effort to be deployed within a well-defined structure, instead of having to be applied to the definition of that structure; they permit a concentration on short-run questions. But in academic work, as in business, long-run questions, even if no more intellectually taxing, are much less comfortable, because they tend to open up an unpalatable range of options. . . . An acceptable paradigm affords protection from such disturbing speculations. [Loasby, 1971, page 869]

c. Provides Focus without Engendering Myopia or Delusion

DeVroey also sees that, "The paradigm enables scholars to concentrate on narrow fields of research." However, the paradigm does so, DeVroey notes, without scientists "losing the feeling that their specialized activities are integrated in a wider context" [DeVroey, 1975, page 419]. Similarly, Boland interprets Kuhn as seeing paradigms providing a basis for empirical testing which "hopefully avoids the two problems of subjective truth -- dishonesty and self-delusion."<sup>100</sup>

d. Imposes Order Upon Facts

Paradigms assist scientific inquiry in another way: they limit where scientists "look" for "facts" and impose some degree of order upon the facts "found." In doing so, paradigms make possible that which would otherwise be impossible. In particular, paradigms, by freeing scientists from "random fact gathering," allow scientists to more closely match fact with theory:

[A paradigm] serves to forestall random fact gathering by directing the scientist toward those facts which the paradigm "displays as particularly revealing." This concentration achieves results, for it leads to a "precision of observation-theory match that could be achieved in no other way." [Worland, 1972, page 275, quoting Kuhn, 1970c, pages 25 and 65 respectively]

The limits which paradigms impose upon scientists' view of the "facts" also render phenomena manageable, and allow scientists to locate the data they need much more easily and efficiently:<sup>101</sup>

Fact-gathering can easily become a tedious hit-and-miss affair that often relies heavily upon the sources of data at hand. However, with a paradigm as a guide, useful and relevant information can more easily be obtained. [Pheby, 1988, page 38]

Some economists see the limits which paradigms impose not simply as making work with data easier, less taxing and/or more effective. Some regard the limits which paradigms impose upon the facts a scientist is able to see as *making scientific research at all possible*. The constraints a paradigm imposes constitute a necessary condition for *any* scientific research:

Kuhn maintains that research problems are defined by and within the paradigm . . . In a sense, then, a paradigm is something which comes between a scientist and "the facts". But such an empiricist formulation runs the risk of missing Kuhn's point, in so far as it might suggest that the paradigm is a limitation or obstacle to scientific work. For Kuhn a paradigm is indeed a limitation of vision, but *ipso facto* necessary if any scientific work is to be done. Research is inconceivable without a paradigm, since the latter imposes a minimal orderliness on what would otherwise be a hopeless and endlessly confusing myriad of sheer data. In fact it is clear that for Kuhn paradigms operate at an extremely fundamental level . . . such that the idea of working without one is not just impracticable but perhaps inconceivable. [Foster-Carter, 1976, page 169]

e. Fosters Creativity

Brian Loasby forwards a novel, yet intriguing, argument regarding the implications of the limitations which paradigms impose: Creativity is not best served by a vacuum, but by a certain measure of order. The strictures which paradigms impose upon scientists provide the necessary order and, thus, foster creativity.<sup>102</sup>

H.A. Simon applies a similar argument more widely. "People (and rats) find the most interest in situations that are neither completely strange nor entirely known -- where there is novelty to be explored, but where similarities and programs remembered from past experience help guide the exploration. Nor does creativity flourish in completely unstructured situations. The almost unanimous testimony of creative artists and scientists is that the first task is to impose limits on the situation if the limits are not already given." It is the role of the paradigm to provide such limitations to the agenda for inquiry. [Loasby, 1971, pages 866-867, quoting Simon, 1965, pages 97-98]

6. *Paradigms' Open-endedness Provides for Scientific Research Opportunities*

Paradigms not only make scientific practice possible by the restrictions they impose, but also by their open-endedness. According to many economists' interpretations of Kuhn, paradigms must, by their nature, be open-ended.<sup>103,104</sup> That is, they leave a number of questions unanswered; a number of problems, unsolved. In doing so, paradigms leave work for scientists to do; they provide vistas for future research and theorizing. George Stigler, Argyrous and Loasby all comment upon how a paradigm's open-endedness provides those working within it a broad range of possibilities for future research and theorizing:

The paradigm is open-ended and thus allows the continuing utilization of its apparatus to deal with an essentially unlimited number of unsolved problems. [Stigler, 1969, page 223]<sup>105</sup>

As Kuhn notes, the paradigm's "achievement was sufficiently . . . open-ended to leave all sorts of problems for the redefined group or practitioners to resolve." [Argyrous, 1992, page 235, quoting Kuhn, 1970c, page 10, ellipses in Argyrous]<sup>106</sup>

A paradigm must therefore be both comprehensive and open-ended; it leaves many problems to be solved . . . [Loasby, 1971, page 866]

Others go still further. A paradigm does not simply afford scientists numerous possibilities for scientific work. A paradigm's open-endedness makes scientific work itself possible. If a paradigm answered all the questions it asked and solved all the problems it posed, scientists working within it would have nothing to do:<sup>107</sup>

Obviously no paradigm is complete; if it were, "normal" scientific activity would cease, for there would be no unsolved puzzles . . . [Coats, 1969, page 291]<sup>108</sup>

The concept of a "paradigm" plays a fundamental role in Kuhn's theory. These paradigms are significant scientific achievements that are sufficiently open-ended to inaugurate coherent research traditions. . . . Normal science exists because paradigms are open-ended. [Mehta, 1978, pages 4-5]

## 7. *The Necessity of Paradigms in Science*

Economists are divided as to whether paradigms are or are not necessary to scientific practice. Many, as we have seen, find them necessary. Their open-endedness provides scientists a needed agenda of inquiry, while, at the same time, the limits which paradigms impose upon the questions asked and scientists' perception and selection of facts render scientific practice tractable. Donald Bear and Daniel Orr describe the "existence of paradigms . . . [as] a *sine qua non* for science [Bear and Orr, 1967, page 193, footnote 15]. Wiles, as well, finds paradigms necessary. He, however, makes clear that they are only "a social necessity." A social configuration of science different from the one currently existing would not require paradigms. And, given that paradigms "stifle basic thought" by requiring the coherence of "basic knowledge," he calls for the restructuring of the scientific community:

In particular, paradigms are not a good idea, but a bad habit. Basic knowledge need not (should not?) be coherent; it need only be noncontradictory. Accepted paradigms stifle basic thought. They are only a social necessity, and the society of scientists should be rearranged so as to do without them. [Wiles, 1981, page 355]

## 8. *Summary*

In sum, we find that economists provide varying accounts as to whether paradigms help or hinder scientific inquiry. On the one hand, we find that economists stress that paradigms hinder scientific progress, that a paradigm's eschewal of social questions is problematical, and that paradigms blind scientists to the paradigm's faults. On the other hand, other economists point out that the restrictions provide scientists with a much needed focus, order facts so as to make inquiry tractable, and, even foster creativity. Finally, as we just noted, economists are divided as to whether paradigms are necessary for scientific inquiry. Some assert that the restrictions paradigms impose and the matters they leave unresolved make the practice of science not only easier, but make it possible. Some others, notably Wiles, however, argue science can and should be restructured so as to get along without paradigms.

## 9. *Paradigms' Role in the Evaluation of Hypotheses*

Almost all economists found commenting upon the role of paradigms in the evaluation of hypotheses agree that paradigms are, at least for the time being, considered beyond question. However, having agreed upon the unquestioned status of paradigms *themselves*, these economists diverge markedly as to the role they ascribe to paradigms in the evaluation of the hypotheses associated with the paradigms.

Some of these economists regard paradigms as providing for the generation, but not the evaluation, of hypotheses:

a paradigm defines a set -- often a very large set -- of possible hypotheses, but makes no claims for the validity of any particular members of that set (some of which, indeed, will be mutually exclusive alternatives) . . . [Loasby, 1971, page 867]

Others go still further. Not only are paradigms inviolable, not only do they generate hypotheses to be tested, but, in addition, paradigms also serve as the basis for the evaluation of those hypotheses:

In Kuhn's model, the adherents to a paradigm do not question the validity of its components under normal circumstances. The paradigm is

considered immune to empirical testing and serves as the general starting point when specific theories and hypotheses are developed and evaluated. [Bøhren, 1990, page 11]<sup>109</sup>

The paradigms are the pool of available assumptions used to construct the logical proof of any falsification. They are the basis of our test conventions and are for the present -- the everyday workings of "normal science" considered beyond question. [Boland, 1977, pages 97-98]

On the other hand, others contend that paradigms, by virtue of their unquestioned status, shelter the hypotheses associated with them from disconfirmation. Paradigms not only do not serve as the basis of the evaluation of hypotheses; they serve to short-circuit the evaluation process altogether:

theories are subspecies of a larger "paradigm" [Kuhn] or "scientific research programme" [Lakatos], and therefore such specific theories are immune to empirical falsification as long the larger body of thought to which they belong remains in tact. [Wisman, 1978, page 269]

#### 10. *Sociological Functions*

The foregoing discussion makes clear that economists see paradigms playing a very active role within a science. Paradigms provide for, facilitate, direct and ultimately limit scientific inquiry and analysis. Paradigms exercise a strong influence upon a science's content and method. This influence implies that paradigms in turn exert considerable control over a scientific community's membership. In addition, the links which economists forge between paradigms and the scientific community point to another unmistakable sociological function for paradigms: paradigms define the bounds of a scientific community and determine who is in and who outside of the community.<sup>110</sup>

Chase makes explicit that paradigms do not simply *regulate* content and method, but actually *govern* scientists themselves:

Paradigms, in both senses of the term, serve both regulative as well as cognitive functions in that they affect both scientific behavior and perception. They do this by governing in the first instance, not a subject matter per se, but rather a group of practitioners. [Chase, 1983b, page 815]



In a word, then, a discipline's paradigms "govern" . . . Paradigms become something akin to a scientific constitution in that they act to delineate disciplinary boundaries, to identify a science's practitioner citizens, and to establish accepted standards to guide professional activity and to evaluate results and competence. [Chase, 1983b, page 818]

Still again, Chase finds that paradigms serve to unify the group of practitioners it defines. In sum, "it is the cement that supports and binds the integrated scientific community":

the exemplary paradigm provides an identifiable locus for professional activity and commitment; metaphorically, it is the cement that supports and binds the integrated scientific community -- a community whose goals, values, beliefs, methods, standards, etc., form the more global (paradigmatic) disciplinary matrix. [Chase, 1983b, page 815]

This unifying influence, Stigler describes as "*necessary* for the existence of a community of scholars" [Stigler, 1969, page 223, emphasis added].

Economists understand paradigms as exercising control over scientists at all stages of their career. Paradigms serve as screens determining who will and who will not be admitted (recognized) as members of a given scientific community. Those who adhere to the paradigm are admitted/accepted as members; those who do not (especially those who explicitly repudiate the community's paradigms) will be excluded.<sup>111</sup> Paradigms shape a newcomer's training and indoctrination. Both Thomas Holland and Argyrous note that paradigms *qua* exemplars provide the means by which students of science learn by doing.<sup>112</sup> Both as well see scientists' repeated exposure to the exemplars as serving to inculcate a given view of the world:

In short, "Scientists solve puzzles by modeling them on previous puzzle solutions, often with only minimal recourse to symbolic generalizations." That is, students and scientists learn "from problems to see situations as like each other, as subjects for the application of the same scientific law or lawsketch." [Holland, 1987, page 197, quoting Kuhn 1970c, pages 189-190 and 190, respectively]<sup>113</sup>

For the practitioner, the paradigm determines which actions will be rewarded and which not. Dewald et. al., for example, notes that paradigms withhold rewards from those seeking to replicate the work of others.<sup>114</sup> Still more, in general, scientists are

rewarded and their work is judged in terms of how well it comports with the regnant paradigm:

One is rewarded when one contributes to the development of the paradigm. Finally, work submitted to journals for publication are frequently judged on the basis of their contribution to the paradigm. [Oromaner, 1981, page 72]

#### 11. *Are Paradigms Rules?*

Surrounding the discussion of the functions which paradigms play within science is the question: Are paradigms, themselves, rules, or do they simply imply them? Ben Seligman explicitly describes paradigms as sets of rules:

It may be recalled that Kuhn spoke of a paradigm as a shared set of rules and standards for the conduct of scientific research. [Seligman, 1971, page 2]

Argyrous, on the other hand, explicitly contrasts paradigms with explicit rules and points out that, for Kuhn, paradigms are not rules, but, instead actual examples of practice which render rules superfluous:

A paradigm is a way of "doing science," and therefore can be fully grasped only in its performance. This is illustrated in Kuhn's discussion of jigsaw puzzles. There are definite rules for piecing together such a puzzle. These include the rules that there should be no leftover pieces, that all the pieces must finish faceup, and that all the pieces must interlock without any obvious extrusions or gaps. Not even a child, however, comes to learn how to piece a jigsaw together by having these rules spelled out: He or she learns to solve a puzzle by seeing how it is done. Once such an illustration is provided and repeated a sufficient number of times, the spelling out of underlying rules seems obvious and indeed trivial. Why worry about rules when we have seen that the paradigm in which they are embodied works? [Argyrous, 1992, page 234]

Akin to Argyrous, Philip Klein notes that Kuhn's most instructive definition of paradigms was as "the concrete puzzle-solutions which, employed as models or examples can replace explicit rules as a basis of the solution of the remaining puzzles of normal science" [Klein, 1990, page 385, quoting Kuhn, 1970c, page 175].<sup>115</sup>

## C. THE AMBIGUITY OF "PARADIGM"

Numerous economists describe Kuhn's use and/or definition of "paradigm" as ambiguous. A large number of them cite the computer scientist Margaret Masterman's finding that Kuhn employed the term in at least 21 different ways [Masterman, 1970, page 61].<sup>116,117</sup> Chase describes Kuhn's initial use of "paradigm" as unclear and indirectly alludes to Masterman to support his contention:

But even though a *feeling* for the paradigm concept definitely emerges from Kuhn's book, it does not do so within a clearly bounded framework or precise and consistent definition. No other critical point is made more frequently than this. And one (sympathetic) critic [Masterman, 1970] has indexed at least twenty-one different usages of the term in *The Structure of Scientific Revolutions*, and observes that this wide variation is in large part due to Kuhn's "quasi-poetic style" -- a style which has made paradigm elucidation genuinely difficult. [Chase, 1983b, page 813]

Still again, Foster-Carter cites Masterman's conclusion: "Exactly what a paradigm is has been one of the most contentious issues in Kuhn's work: his critics have stressed its ambiguities, and even a sympathizer claims to find twenty-one different uses of it" [Foster-Carter, 1976, pages 168-169]! Some economists criticize Kuhn for his ambiguous use of "paradigm" and cite as support Masterman's finding at least 22 different senses in which Kuhn used paradigm in the first edition of *The Structure of Scientific Revolutions*.<sup>118,119</sup>

Economists also criticize Kuhn's use of "paradigm" as ambiguous by citing philosopher Dudley Shapere's assessment that Kuhn applied the term in an overly broad manner.<sup>120,121</sup> Coats, following Shapere, remarks upon the tremendous range of application which Kuhn gives to the concept:

this remark suggests the ambiguity of the paradigm concept, for it may be interpreted as a specific book or style of exposition, a "basic theory," a *Weltanschauung*, or the entire range of scientific activity. [Coats, 1969, page 291]

In addition, Leonard Kunin and F. Stirton Weaver cite Shapere in connection with what they describe as "the looseness of the paradigm concept" [Kunin and Weaver, 1971, page 391].

Other economists point directly to Kuhn's own acknowledgment of the ambiguity of his paradigm concept. Wiles succinctly puts the point: "To say that Kuhn's use of his own term is very unclear is to say little more than he himself admits" [Wiles, 1979, page 171, footnote 12]. Reynolds finds Kuhn admitting that he variously applied the term to a scientific community, on the one hand, but also to a "disciplinary matrix" on the other:

There has been some confusion as to what constitutes a paradigm. In the second edition of his book, Kuhn points out that the term paradigm is used with two meanings. At one level paradigm refers to the "community structure of science." The second meaning is that of a "disciplinary matrix" consisting of symbolic generalizations deployed without question, shared commitments to a set of beliefs, a set of values and "exemplars." [Reynolds, 1976, pages 25-26]<sup>122</sup>

Economists themselves point to a number of different ways in which Kuhn employed the term "paradigm" ambiguously. Some, similar to Shapere, affirm that Kuhn applied "paradigm" to a widely varying array, ranging from examples of good science to sets of techniques and problem foci to far-reaching metaphysical conceptions:

In the first edition of his book, Kuhn frequently employed the term "paradigm" in a dictionary sense to stand for certain exemplary instances of scientific achievement in the past. But he also employed the term in quite a different sense to denote both the choice of problems and the set of techniques for analysing them, in places going so far as to give "paradigm" a still wider meaning as a general metaphysical *Weltanschauung*: the last sense of the term is, in fact, what most readers take away from the book. [Blaug, 1976, page 152]

Similarly, Williams observes that Kuhn moves back and forth between understandings of a paradigm as that which produces science and that which is itself the product of science:

A paradigm is a unity that cannot be fully reduced to atomic components; the identity of components, like terms and concepts, depends on the whole . . . But positively, the nature of this unity is unclear because Kuhn vacillates between two definitions. At times he is dealing with a means of production of knowledge, some kind of instrument or conceptual tool which defines a problem or standard solution. At other times he is dealing with end products or results, and

theoretical conclusions are used to define an organisation of knowledge.  
[Williams, 1975, page 326]

Still others find Kuhn's understanding of paradigm unclear in that he does not clearly specify the level of generality at which paradigms are defined. Do we find paradigms at the rather narrow level of a particular theory or at a much broader level, (e.g., ontology) [Kunin and Weaver, 1971, page 393]? Finally, Stigler faults Kuhn for failing to provide an operational definition of "paradigm" which would allow for the term's meaningful use and testing of Kuhn's various theses [Stigler, 1969, page 225].

#### D. KUHN'S AMBIGUOUS USE OF "PARADIGM" AND THE CONCEPT'S APPLICABILITY TO ECONOMICS

As we have just seen, a large number of economists regard Kuhn's paradigm concept as ambiguous.<sup>123</sup> Where does this leave the applicability of the paradigm concept to economics? Here, we find a variety of answers.

##### 1. *Economists' Varied Assessments as to the Relevance and Importance of the Ambiguity of Kuhn's Paradigm Concept*

Some find the high degree of generality with which Kuhn employed "paradigm" to be an asset, not a liability to its usefulness. According to Dow, those who criticize Kuhn for employing "paradigm" too broadly are mistaken; the term's broad applicability does not impair, but instead strengthens, its utility:

The central concept he [Kuhn] employed was the paradigm, or "disciplinary matrix." This was a concept broad enough to encompass all aspects of a theoretical structure, ranging from practical techniques of analysis to the underlying world-view and mode of thought of the scientist. . . . Its strength lies in its application simultaneously to several levels of theoretical structure. Kuhn explained the paradigm concept by means of historical examples, demonstrating the range of possible applications. He was as a result widely criticized for vagueness . . . But the reaction to Kuhn, extending well beyond questions of pure logic, has amply demonstrated the power and range of the paradigm concept.  
[Dow, 1985, page 27]

Some find that while Kuhn initially used the term ambiguously, he, in subsequent writings, cleared up the meaning of the term. Argyrous finds that Kuhn's Postscript to the second edition of *The Structure of Scientific Revolutions* and other later

writings made clear what, for Kuhn, constituted a paradigm. Here, according to Argyrous, Kuhn stressed that a paradigm should be understood as a concrete example of scientific work. Still further, Argyrous notes that Masterman clarified that a Kuhnian paradigm constituted a concrete example in the same article that so many economists cite as evidence of Kuhn's ambiguous use of the term.<sup>124,125</sup> Similarly, Chase notes the evolution of Kuhn's understanding of paradigm and calls for those examining the paradigm concept to study not simply Kuhn's initial use of the term, but, as well, his notion of "disciplinary matrix" and "exemplar" which he introduced in seeking to clarify the understanding of "paradigm."<sup>126</sup>

Axel Leijonhufvud concedes that if Kuhn is understood as a philosopher, his multiple uses of paradigm pose very real problems. Leijonhufvud, however, implores readers to interpret Kuhn not as a philosopher, but as an historian. Read in this light, Kuhn is the most helpful and his 21 plus uses of paradigm pose no serious difficulty:

*Pace* Kuhn (the philosopher), I still have little sympathy with the criticisms, for example, of Masterman . . . Kuhn's original version comes off best if read as a work of historical induction. Twenty-odd descriptive statements to delineate the "novel" class of empirical phenomena named "paradigms" is then not too much. Read as a piece of philosophical model-building, twenty-odd definitions of a central primitive term for the deductive structure seems a bit much -- and the difficulties will not end there. Thus, my attempt at assessing Kuhn's work has to end on a plaintive note: may one not read the work in the way that gives the best value -- even if the author, *ex post*, won't cooperate? [Leijonhufvud, 1976, page 83, note 32]

Coats, while indicating that Kuhn applied "paradigm" to an overly broad set of notions (ranging from equipment to theory to worldview), finds the ambiguity posing no problems to those seeking to apply the notion in economics. While the term applies to a wide range of scientific activity, given the relative unimportance of theory application for investigators of the social sciences (including economics), the term comes to connote simply and clearly a "basic theory" [Coats, 1969, pages 292-293].<sup>127</sup>

O'Brien, while conceding that Kuhn used the term paradigm in a number of different ways, lays stress upon the usefulness of *one* of those many meanings in the

analysis of economics: as a gestalt, as a "pair of spectacles through which we see the world" [O'Brien, 1976, pages 141-142].<sup>128</sup>

Some, while finding Kuhn's use ambiguous, find that his lack of clarity is not absolute. It is still possible to divine, in general, what a paradigm is (is not). Foster-Carter, for instance, maintains that we are still able to detect from Kuhn "what sort of thing" a paradigm is: "a pre-theoretical entity, a set of domain assumptions which in a very strong sense serve to define the field of study" [Foster-Carter, 1976, page 168].<sup>129</sup> Williams, while finding it difficult to assess what a paradigm is, finds it clear what is a paradigm is not -- a collection of separable entities: "A paradigm is a unity that cannot be fully reduced to atomic components; the identity of components, like terms and concepts, depends on the whole" [Williams, 1975, page 326]. Both Foster-Carter and Williams apply the concept in their analyses: the former, to identify paradigms in development economics and the latter, to point to the error of understanding knowledge in a non-holistic manner [Foster-Carter, 1976; Williams, 1975, pages 326-327].

Reder and Ward, due to the controversy surrounding "paradigm," state their intention to avoid the term's use. However both, while disavowing use of the *term* paradigm, indicate the *concept's* applicability.<sup>130</sup> Conversely, Robert Ekelund and Robert Hébert directly call the applicability of Kuhn's paradigm concept into question on the basis of its ambiguity.<sup>131</sup> However, they use the term in later discussion on heterodox economics, in which they seek to determine the existence of an institutionalist paradigm [Ekelund and Hébert, 1983, page 424]. Still again, Zinam, while citing Kuhn's ambiguous use of "paradigm," provides a clarified understanding of the term in his own analysis of paradigms in economics.<sup>132</sup>

Tribe maintains that a more crucial and fundamental problem lies behind Kuhn's multiple use of "paradigm." Kuhn, according to Tribe, seeks to comprehend the *epistemological* notion of a paradigm as an element of unity within the discontinuity of

a science in *sociological* terms -- defining a paradigm as that which scientists hold in common. However, lacking the proper and firm moorings of a clearly defined epistemological understanding of a paradigm, Kuhn's analysis moves about aimlessly leading to an "apparent proliferation" of uses of the term and the concept paradigm.<sup>133</sup>

Some economists do, however, argue that the ambiguity of Kuhn's paradigm notion seriously impairs its applicability to economics. Roger Backhouse questions the possibility of determining, in any meaningful way, Kuhn's applicability to economics because of the equivocal (even plastic) nature of the paradigm concept. It is possible, employing Kuhn's notion, to divide up economics in innumerable different ways (e.g., as composed of one and only one paradigm, or alternatively, any number of less encompassing ones) such that, with enough ingenuity, one can find a configuration of the field which renders economics amenable to Kuhn's theories:

Were the criteria for testing the applicability of the methodology sufficiently stringent for the results to mean anything? For example, if economics is divided up into chunks, each of which is to be tried out as a Kuhnian paradigm, or as a Lakatosian research programme, there are many ways in which we might divide it up. We might take the whole of economics inquiry since Adam Smith as one unit. We might separate classical economics, marginalist economics and Keynesian economics. Dividing still further we might consider episodes such as the post-Marshallian theory of the firm, or neoclassical growth theory. "Verification" of a methodology ought to be easy, as there are so many possible ways of applying it. [Backhouse, 1985, page 8]

Similarly, Stephen Lofthouse explicitly concurs with Shapere's assessment that Kuhn's definition of paradigm is so overly broad so as to make its application impossible:

Kuhn's view may still appear to have elements of truth. However, the present writer would argue that this is so because, as Shapere puts it, "his view is made to appear convincing only by inflating the definition of 'paradigm' until that term becomes so vague and ambiguous that it cannot easily be withheld so general that it cannot be applied. . . ." [Lofthouse, 1973, page 411, quoting Shapere, 1964, page 393]

Blaug discounts Kuhn's ambiguous use of "paradigm" because he finds the philosopher's theory of scientific revolutions, not his paradigm concept, as Kuhn's major contribution to the understanding of science.<sup>134</sup>



Other economists, however, make little of the implications of Kuhn's ambiguous/excessively broad use of paradigm for the philosopher's application to economics. Stanfield concedes that "*paradigm* is a slippery concept," and agrees, in part, with Stigler's assessment.<sup>135</sup> The former, however, does so only in a footnote, and makes no mention of the matter in the text in which he examines the Keynesian Revolution as a Kuhnian scientific revolution [Stanfield, 1974, page 106, note 3]. Still further, Miller notes Kuhn's ambiguity, but argues that, "there is little to be gained, for our purposes, from an exploration of the various possible meanings of the term [*paradigm*]" [Miller, 1991, page 1003, note 2].<sup>136</sup> Worland, Jalladeau and Peabody all acknowledge the controversy surrounding Kuhn's ambiguity. Jalladeau, however, makes mention of Kuhn's ambiguity only in passing in a brief statement, and both Worland and Peabody bring up the matter only in a footnote [Jalladeau, 1978, page 584; Worland, 1972, page 275, footnote 5; Peabody, 1971, page 15, footnote 3]. Finally, Jörg Baumberger makes only brief mention of Kuhn's lack of clarity at the outset of one of the most scathing criticisms of Kuhn and his applicability to economics. Still further, the mention is wholly incidental to Baumberger's critique.<sup>137</sup>

Many major economics applications of Kuhn make no mention at all of Kuhn's ambiguous use of paradigm. Sweezy and Zweig both seek to identify mainstream and radical paradigms in economics. They, however, make no mention of Kuhn's ambiguity [Sweezy, 1971; Zweig, 1971]. In two articles, both of whose central focus is the applicability of Kuhn's paradigm concept to economics, L.E. Johnson does not bring up Kuhn's broad/ambiguous use of "*paradigm*" [Johnson, 1980; Johnson, 1983].

In sum, economists diverge as to the implications of Kuhn's broad use of paradigm. As we have just seen, many economists make no, or only passing, reference to Kuhn's ambiguous use of the term in their applications and critiques of Kuhn's model of science to economics. One interpretation of this omission is that these economists find Kuhn's ambiguity surrounding the paradigm concept as largely

unproblematical/irrelevant to the application of his notions to economics. Other economists provide more explicit indication that Kuhn's ambiguity poses no significant problem for the applicability of his schema. They cite a variety of reasons: (1) Any initial ambiguity as to Kuhn's meaning and use of paradigm was cleared up by his (and Masterman's) later work. (2) Kuhn's ambiguity was not complete and thus it is possible to get some handle on his understanding of the term. (3) If understood as a historian (rather than a philosopher of science), Kuhn's multiple uses of paradigm presents no difficulty. (4) Differences between the natural and social sciences render his ambiguity irrelevant to Kuhn's application to economics. (5) Among the many ways in which Kuhn interpreted paradigm, certain of these are highly applicable to economics.

In addition to these economists, we find Dow on one end of the spectrum, asserting that Kuhn's extremely broad understanding of paradigm enhances its applicability to economics, and Lofthouse and Backhouse, on the other, affirming that Kuhn's ambiguity poses serious problems for the application of his schema to economics.

Having established the multiplicity of economists' views regarding Kuhn's applicability to economics, we now turn our attention to the most common concern which economists identify regarding Kuhn's ambiguous use of paradigm and his application to economics: the implications of his ambiguity for the identification of paradigm shifts in economics.<sup>138</sup>

## 2. *Implications of the Ambiguity of Kuhn's Paradigm Concept for the Identification of Paradigm Shifts in Economics*

Many economists assert that the ambiguity of Kuhn's paradigm concept makes it difficult to identify scientific revolutions (understood as paradigm shifts) in economics or to determine whether economics has undergone any such shifts. If it is not clear what constitutes a paradigm, then determining whether (or not) a *change* in paradigms

has occurred becomes, at best, highly problematical. Does a given change in economics imply that an old paradigm was replaced by a new one or only that the "old" paradigm was retained but was, in some way, altered?

Kunin and Weaver find that Kuhn's overly broad definition of paradigm, which includes theories as well as ontologies under the rubric, poses problems in the identification of scientific revolutions within economics. Disagreements as to whether a scientific revolution has or has not occurred may, in part, be traced to the different levels at which economists locate paradigms. Such differences of opinion help explain, for instance, why Bronfenbrenner does see the marginal revolution as a scientific revolution, but Stigler does not:<sup>139</sup>

Once we realize that it is really a matter of different levels of knowledge which is at the root of the trouble, we understand why it is that economists differ in the number of scientific revolutions which they perceive as having occurred during the development of economic science.

Combining consciousness about different levels of abstraction in using the paradigm concept with the substitution of dialectical synthesis for complete paradigm overthrow, we are able to follow the alternative logics which lead Bronfenbrenner to see a scientific revolution in the advent of marginal utility theory in the 1870s, while Stigler can maintain that "the essential elements of the classical theory were affected in no respect" by this departure. [Kunin and Weaver, 1971, pages 393-394]

Jalladeau, as well, suggests that ambiguity surrounding the paradigm concept may help to explain why Coats finds no scientific revolutions in economics, while others -- Spengler for example -- do find them.<sup>140</sup> Bøhren notes that, given Kuhn's broad use of "paradigm," it is difficult to determine whether there have been paradigm changes in "stochastic choice theory":

It seems that in this case, the high generality and the correspondingly low precision in Kuhn's model causes ambiguity right at the conceptual level. This is a well-known problem with Kuhn's approach. For instance, Masterman . . . noticed that Kuhn initially used the term paradigm in at least twenty-one different ways. Because of this vagueness in what is probably the model's most important concept, it is for instance hard to determine if the history of stochastic choice theory really involves several simultaneous paradigms (Lakatos's hypothesis) or if there is just a single, dominating one at any point in time (Kuhn's hypothesis). [Bøhren, 1990, page 26]

Stigler faults Kuhn for failing to devise the operationally meaningful definition of paradigm necessary for determining whether or not there have been scientific revolutions in economics. Without a clear understanding of what a paradigm looks like, finding out in practice whether or not one has changed becomes impracticable; the investigator is left with little more than the dictum that a scientific revolution occurs when a scientific revolution occurs. Stigler maintains the ambiguity may lead us to find a revolution in economics when one, in fact, did not occur:

My main quarrel with Kuhn is over his failure to specify the nature of a paradigm in sufficient detail that his central thesis can be tested empirically. If vast changes in the subject and techniques of a science can be accommodated within a paradigm, and hence do not constitute a revolution, Kuhn's assertion that a crisis is necessary to the emergence of a new paradigm is virtually a tautology. If, on the contrary, large changes in the science per se constitute a revolution, Kuhn asserts that there will be an abandonment of the previous paradigm which in actual fact may never have taken place. . . . Until Kuhn gives us criteria of a revolution (or a paradigm) which have direct empirical content, it will not be possible to submit his fascinating hypotheses to test. [Stigler, 1969, page 225]<sup>141</sup>

Some economists, however, while recognizing the problems Kuhn's ambiguity presents for one seeking to identify paradigm shifts, still find it possible to ascertain whether economics has undergone any major paradigm shifts in its history. Gordon finds that the ambiguity introduces an "arbitrary element" in the determination as to whether a given change in economics constitutes a sub-paradigm change or only a modification of the sub-paradigm:

Just where the dividing line lies between a major further extension of the basic model and the revolutionary overthrows of a submodel may involve an arbitrary element, and the arbitrariness may have its source in a certain ambiguity in Kuhn's conception of the paradigm. [Gordon, 1965, page 124]

The ambiguity, however, poses no problem for Gordon in determining whether a major scientific revolution overturning the discipline's major paradigm has occurred: no such revolution has occurred.<sup>142</sup>

As we noted earlier, Reynolds does see Kuhn defining the term paradigm ambiguously. Reynolds further notes the importance of the determination of what constitutes a paradigm for the identification of scientific revolutions within the discipline. He, however, does not seem to find that the ambiguity renders determination of a scientific revolution impossible (or even necessarily problematical). Reynolds has no trouble assessing another economist's identification of revolution in economics history to be based upon an unduly narrow conception of a paradigm,<sup>143</sup> and he clearly concludes, like Gordon, that there has never been a scientific revolution in economics since Adam Smith [Reynolds, 1976, page 30].

Johnson and Ley suggest that the ambiguity Stigler finds may be dispelled if one includes, as Kuhn does not, a purposive function in the specification of economics' paradigms [Johnson and Ley, 1990, page 175, note 35]. Including the function makes clear when a scientific revolution (i.e., paradigm change) has occurred: "a change in the P-F represents the necessary and sufficient condition for interparadigm change hence, scientific revolution"<sup>144</sup> [Johnson and Ley, 1990, page 36].

In sum, many economists argue that the ambiguity of Kuhn's "paradigm" renders the identification of paradigm shifts in economics problematical. Some of these economists, however, while recognizing the difficulties presented by Kuhn's ambiguity, still find it possible to determine whether economics has ever undergone a major paradigm shift in its history.

#### E. THE APPLICABILITY OF KUHN'S PARADIGM CONCEPT TO ECONOMICS

We next turn our attention to various other issues surrounding the applicability of Kuhn's paradigm concept to economics. As we shall see, in many of these areas economists' assessments of the applicability of Kuhn's paradigm concept to economics vary widely.

1. *Necessary Addition of a Central Valuational Element*

Some economists find that Kuhn's specification of a paradigm needs to be broadened in order to make the concept applicable to economics. In particular, they argue that in order to characterize properly an economics paradigm, one must include a central regulating valuational element they see lacking in Kuhn's paradigm concept. That element, they contend, plays a vital role in economic practice and, more than any other component Kuhn includes in his specification of a paradigm, distinguishes one economic paradigm from another. As alluded to earlier, L.E. Johnson maintains that Kuhn omits the centrally important element of an economics paradigm: the purposive function (P-F), "the ultimate purpose or goal pursued by the practitioners of normal science." In "formal orthodox economic analysis, the P-F expresses the quantity that the prevailing paradigm posits as the maximand, social and/or private, that a given body of economic analysis and policy strives to achieve." The P-F plays a major role in regulating and shaping the paradigm's other elements; it "provides focus for the paradigm's analytical methods, underlying and directing both theoretical formulations and empirical research" [Johnson, 1983, pages 1100-1101].<sup>145</sup> Still again, Yngve Ramstad contends that a complete accounting of an economics paradigm must supplement specification of the elements included in a Kuhnian disciplinary matrix with identification of the paradigm's "social value criterion/criteria," (e.g., subjective evaluation by individuals, determination of value by "scientific means.") The addition is necessary because, unlike the physical sciences which Kuhn examines, "[t]he only reason, the only excuse, for the study of economic theory is to make this world a better place in which to live'" [Ramstad, 1989, page 762].<sup>146,147</sup>

For both Ramstad and L.E. Johnson identification of an economic paradigm's guiding valuational element is fundamental. Johnson distinguishes among paradigms in the history of economics chiefly by their different purposive functions.<sup>148</sup> Ramstad contends that economists who do not share the same social value criteria, by immediate

consequence, adhere to fundamentally different and distinct economics paradigms -- even if they adhere to the same methodology and conceptions of the economy:

To sum up. In economics, the term "paradigm" must be understood to encompass both implicit legitimating beliefs *and* the explicit tools, theories and model used to anticipate ("predict") the consequences of alternative institutional adjustments. Consequently, for economists to evince a commitment to the *same* paradigm, they must reveal in the concepts, theories and models they develop and employ (1) the same epistemological and ontological preconceptions, (2) the same image of man, (3) the same conception of the economy, *and* (4) the same social value criterion/criteria. [Ramstad, 1989, pages 764-765]

## 2. *The Sociology of Science and the Applicability of Kuhn's Paradigm Concept*

Economists' assessments of the applicability of Kuhn's paradigm concept in terms of its sociological dimensions span a broad range. On the one hand, we find L.E. Johnson. He includes "professional relationships" as the fourth element of Kuhn's paradigm concept. However, while Johnson concludes that "the first three of these characteristics seem as applicable to economics as Kuhn thought they were in defining paradigms in the physical sciences," he contends that "the fourth characteristic . . . has no analytical content and is probably not crucial to defining an economic paradigm" [Johnson, 1983, page 1099].<sup>149</sup> Further in two articles and a co-authored history of economic thought text, all of which specifically examine economics paradigms, Johnson provides little to no description of relations among economists [Johnson, 1980; Johnson, 1983; Johnson and Ley, 1990]. On the other hand Zinam, seeking to outline a "master paradigm" for economics, adds to Kuhn's paradigm concept (including only matters of scope and method and worldview) understanding of the "organization and power structure within the economics profession" [Zinam, 1975, pages 470-473].<sup>150</sup>

## 3. *The Ability of Kuhn's Paradigm Concept to Elucidate or to Distort Our Understanding of Economics*

Economists also differ as to their assessments of the utility of Kuhn's paradigm concept in the study of the history of economics. Some find Kuhn's concept very

helpful. L.E. Johnson, for one, strongly asserts that Kuhn's paradigm, once supplemented by the purposive function, provides significant direction to the historian of thought. To the misguided objectivist seeking to evaluate past economics on the basis of the current day, the paradigm concept counsels evaluation of past economics on its own terms, i.e., in terms of its paradigm, most especially its purposive function. Doing so "guards against the error . . . of judging past theory by modern standards."<sup>151</sup> To the relativist, seeking to understand past economists' work in terms of the economic policy and institutions prevailing in their day, but hopelessly mired in mounds of information concerning the theory's context, Kuhn's paradigm concept provides the means by which to eliminate irrelevant information and thus make the task manageable. Having specified the operative paradigm, the historian may limit his focus to only that information germane to the paradigm (again most especially the purposive function).<sup>152</sup>

Cornwall, as well, finds Kuhn's paradigm concept applicable to economics, though for a vastly different reason. The post-Keynesian economist specifically points out that he employs Kuhn's paradigm concept because it conveys well the tenacious nature of economic knowledge:

The decision to carry out much of the discussion in this paper in terms of rival paradigms rather than simply rival theories is deliberate. . . . Carrying out the discussion in terms of rival paradigms better conveys the tenacity post-Keynesians feel is so characteristic of economists in real-life situations when they are forced to accept or reject that which appears novel and challenging to currently accepted doctrines. [Cornwall, 1979, page 71]

Deane assents that there are paradigms in economics. Economics does possess certain "theories, concepts and analytical techniques accepted as authoritative . . . by a majority of economists" [Deane, 1978, page xii]. She, however, makes clear that these paradigms do not enjoy the vaunted and inviolable status which Kuhn attributes to paradigms:<sup>153</sup>



No doubt there are dangers in using a term such as paradigm which may imply that the average economist's repertoire of theories and concept is a more fully articulated system than it in practice is . . . . But it is scarcely in dispute that there have been ruling paradigms in economics in that the textbooks describe a related set of theories, concepts and analytical techniques accepted as authoritative (though not necessarily as beyond criticism) by a majority of economists . . . [Deane, 1978, page xii]

Hausman questions the applicability of Kuhn's disciplinary matrix. A disciplinary matrix, he contends, must contain theories which enjoy universal assent (i.e., symbolic generalizations). However, Hausman finds no such widespread agreement surrounding economic theories.<sup>154</sup> To the contrary, he points out that many theories within the main body of economics directly contradict one another. Given this lack of consensus, Hausman concludes that "economics does not fit his [Kuhn's] schema very well" [Hausman, 1992, page 85].<sup>155</sup>

Others, while seeing some degree of utility in the application of Kuhn's paradigm concept, ultimately find Lakatos's notion of a scientific research programme as a superior alternative. Coats cites approvingly that Kuhn's understanding of a paradigm as both regulative as well as cognitive "suggests new relationships among the ingredients in familiar debates" [Coats, 1969, page 292]. He, however, finds Lakatos's model ultimately "a more promising tool for the historian of economics than Kuhn's theory" [Coats, 1977, page 5, footnote 2]. Similarly, Blaug finds merit in Kuhn's "paradigm" notion in that, "suitably qualified" it serves as "a reminder of the fallacy of trying to appraise particular theories without invoking the wider, metaphysical framework in which they are embedded" [Blaug, 1976, pages 149-150]. He, however, notes that Lakatos's notion of MSRP conveys the same moral more effectively than Kuhn's concept. He further finds that paradigms discovered in economics' history amount not to unitary paradigms, but to multi-dimensional methodological research programmes [Blaug, 1976, pages 160-161]. At bottom, Blaug declares "the term 'paradigm' ought to be banished from economic literature, unless surrounded by inverted commas" [Blaug, 1976, page 149].<sup>156</sup>

Baumberger charges that use of Kuhn's paradigm notion is not simply unhelpful, but actually detrimental to the proper understanding of economics. Economics -- indeed all sciences -- are not, as Kuhn would have it, composed, discrete, static, non-interacting paradigms with clearly definable boundaries. The field is instead composed of a complex web of interacting evolving traditions, each which complements certain other traditions and competes with still others. To attempt to carve out of this intertwined web individual paradigm atoms, according to Baumberger, grossly misrepresents the actual state of economics -- as it does any science (including the natural sciences Kuhn seeks to comprehend) [Baumberger, 1977, pages 16-19]:

All the discreteness, constancy, and self-sufficiency that Kuhn ascribes to his entities are abstractions. . . . In actual fact, the transmission constituted by a discipline, or science as a whole, is a *population of far from discrete process fields* or activity areas that are competing and conflicting in many different, but interdependent, complex fashions. Pockets of this whole may well, and are bound to, in an era of large-scale institutionalized science, in an oblique sense, approach the process characteristics of a normal science a la Kuhn. But the whole, even the whole of a subdiscipline, never displays these properties, least so in economics. [Baumberger, 1977, page 16]

#### 4. *Implications for the Applicability of the Paradigm Concept Stemming from Differences between Economics and the Natural Sciences*

Economists diverge as to the consequences they see arising from differences between economics and the natural sciences. Most do not address the issue. Still others simply make note that Kuhn's concepts (including paradigm) evolved out of study of the natural, not the social, sciences, but say nothing more about the matter [e.g., Gruchy, 1986, page 822, note 3].

Some, however, find certain differences between the two realms reducing certain problems in the application of the paradigm concept. As we noted, Coats sees the problems philosophers of the natural sciences find, regarding the ambiguity of paradigm, dissolve in the concept's application to economics because social science

paradigms (unlike their natural science counterparts) boil down simply and less ambiguously to general theories.

Still further, Dow contends that economists face greater difficulty than their natural science counterparts in the interpretation of evidence. This being the case, Dow argues that Kuhn's paradigm concept may hold greater importance to the economist than the natural scientist. The greater interpretive difficulty implies that economists need to gain a more complete understanding of the bases upon which such interpretations are made. Such bases are contained within the field's paradigm:

From the Kuhnian standpoint, the greater problems facing economists in interpreting observations place even more weight on the conceptualization process, and thus on the metaphysical, linguistic and metaphorical content of a paradigm. The paradigm concept thus seems to be even more powerful when applied to economics. [Dow, 1985, pages 35-38]

Others, as we have seen, find that the differences between economics and the natural sciences require modification of Kuhn's concept before applying it to economics. Ramstad, as we noted, sees that differences between the objectives of the physical sciences and economics require that Kuhn's concept be supplemented to include "social value criteria." L.E. Johnson, likewise, sees inclusion of the purposive function as more important in the analysis of economics than in the natural sciences [Johnson, 1983, page 1106].<sup>157</sup> Finally Baumberger, as we have just seen, finds Kuhn's abstraction of a paradigm -- while problematical in the study of any science -- even more troublesome in economics.

##### 5. *The Role of Kuhn's Paradigm Concept in an Advocacy Setting*

Some find appeal to Kuhn's paradigm concept helpful to heterodox economists seeking to overcome being branded as mere criticism, lacking substantive positive content. Raymond Franklin and William Tabb, for example, note that use of Kuhn's concept helps underline that eventually radical economics, though in the *present* branded as "crackpot" will, in the *future*, evolve into substantive body of theory:

The unfamiliarity with radical concepts and general categories on the one hand, and the almost unanimous acceptance of mainstream vocabulary and tools of analysis on the other, put radicals at a competitive disadvantage in many discussions about methodology. . . . However, outside the monastery there is a real and changing reality which will, in the radical view, eventually break up the monastic chanting of mainstream monks. This will happen because, among other reasons, what is now labeled as radical critical sniping sooner or later will blossom into an integrated, formal alternative to mainstream's orientation to the world. This is why radical economists have been so involved in Thomas Kuhn's basic paradigm idea. [Franklin and Tabb, 1974, page 144]

Similarly, Chase cites Kuhn in order to turn what some may view as a liability of Lowe's Political Economics into an asset. Chase acknowledges that the development of Lowe's Political Economics (paradigm) faces "significant technical, political and philosophical difficulties" in its development. He, however, notes that *all* paradigms (not just Lowe's) are not and *must not* be "without unsolved problems;" it is, after all, these problems that define and structure scientists' scope of inquiry:

Indeed the purpose of a paradigmatic model is to define relevant problems requiring solution and to provide a standard for evaluating those solutions. According to Kuhn, a science's paradigm provides a heuristic for practitioners in the "puzzle-solving" endeavors of so-called "normal science" -- *i.e.*, for scientific activity within the (necessarily incomplete) paradigmatic framework. [Chase, 1983a, page 176]

Several of those who advocate for an alternative economics paradigm lay stress upon Kuhn's recognition that paradigms limit the scope of problems that a science pursues and/or its view of "reality." Hymer and Roosevelt, having noted that paradigms act as flashlights -- focusing on certain areas while leaving others in the dark -- contend that Lindbeck, an orthodox economist, given the paradigm in which he operates, has pointed the flashlight in the wrong direction [Hymer and Roosevelt, 1972, pages 644-645].<sup>158</sup> Still again, Paul Joskow notes that paradigms, by their nature, limit the field of questioning and contends that economic reality (in particular economic regulation of industry) is simply too complex for only one paradigm to comprehend. Thus, he concludes, we should widen our embrace to encompass not

only the prevailing understanding of regulation, but alternatives (including his own) to it as well:

By and large the world of the regulated firm is far more complicated than much of the recent theoretical literature might lead one to believe. We have no doubt gained many valuable insights into the implications of rate-of-return regulation in the context of the neoclassical paradigm, in which the regulatory process has been cast. It does appear, however, that our understanding of what is going on in the regulatory process is not so good that we should restrict ourselves to a one-paradigm approach. Particular paradigms not only lead to particular types of solutions, but may also restrict the kinds of questions that are asked. [Joskow, 1973, page 134]

Similarly, Richard Wagner and Warren Weber reference Kuhn regarding the necessity of paradigms in the interpretation of facts and note that our current interpretation of the growth of government is predicated upon only one among many different theories; they point out that under a different theory, our interpretation would change. That is, they seek to remove the current interpretation from unquestioned status; doing so, they suggest an alternative [Wagner and Weber, 1977, pages 66-67].

Some see paradigm status itself as determining the legitimacy of an economic theory or school. David Emanuel, for instance, describes a proposed ad-hoc adjustment of portfolio theory as "suffer[ing] from the disadvantage of not being paradigm based."<sup>159,160</sup>

However, some economists call into question the utility of Kuhn's paradigm concept in seeking to legitimate one's position. Robert Solow, largely on the basis of the current state of radical economics, seriously questions radical economists' use of Kuhn's paradigm concept to argue for their position. The non-radical economist finds little evidence that radical economics constitutes a Kuhnian paradigm and argues "radical economists have corrupted Thomas Kuhn's notion of a scientific paradigm, which they treat as a mere license for loose thinking" [Solow, 1971, page 63].<sup>161</sup>

Further, Solow implies that paradigm status itself does not grant any brand of economics (heterodox or orthodox) a necessarily privileged status. While asserting that

"neoclassical economics is pretty clearly a scientific paradigm," he allows that "it may be a bad one, or a worn-out one" [Solow, 1971, page 64].<sup>162</sup> Still further, Arnold McKee implies that to stamp a school of economics as "one competing paradigm" serves to diminish, not augment, the school's standing.<sup>163</sup>

Others go still further: not only is use of the paradigm concept unhelpful to those seeking to advocate their position, it is actually counterproductive. Couching debate in terms of competing paradigms, according to Elias Khalil, moves debate outside the realm of the rational. Thus, all those employing Kuhn's paradigm concept are denied meaningful appeal to any rational line of reasoning to support their position:

The term "paradigm" has been called upon by mainstream economists in order to show that economics is a science like physics; while the underworld of economics, ranging from radical, Marxian to institutionalist, has sought Kuhn's view of science in order to pose as "legitimate" and equal counterparts of the mainstream. What has been missed by the underdogs of economics is that they cannot appeal to a higher *rational* principle in order to advocate their paradigm versus others if they insist on using Kuhn's scenario. Orthodox economists, likewise, pull the carpet from underneath their feet when they resort to Kuhn in order to argue that economics is a *science*. It is unfortunate that in the rush to buttress one's system, a serious reflection on the nature of economics, in the light of Kuhn's thesis, has been relegated to the background or even denied. [Khalil, 1987, pages 118-119]

## 6. *Summary*

In sum, we find a broad range of opinions regarding the applicability of Kuhn's paradigm concept to economics. Economists differ as to the implications they draw from their recognition of the social nature of science and the differences between the natural and social sciences regarding the applicability of Kuhn's paradigm concept to economics. Further, they diverge in their assessments of the ability of Kuhn's paradigm notion to elucidate (distort) one's understanding of economics, as well as to the usefulness of Kuhn's "paradigm" as a tool of rhetoric. We, however, should note in closing that most economists applying the concept to economics do not even broach

the question of Kuhn's applicability to economics<sup>164</sup> -- as those specifically addressing the issue are wont to point out:

Unfortunately, it has not been generally understood that an important modification, or at least shift in emphasis, must be made to Kuhn's conception of a "disciplinary matrix" or "paradigm" before it can be applied to the "scientific activity" of *economists*. [Ramstad, 1989, page 762]

#### F. PARADIGMS IN THE HISTORY OF ECONOMICS

Economists have located a wide variety of different paradigms in the discipline, both over its history and in the present day. We first examine those paradigms found in economics' past. We then look at the wide diversity of economics paradigms which economists find existing in the present day.

##### 1. *Classical Economics*

Many economists associate a "classical paradigm" with the classical school of economics. For instance, both R.D. Collison Black and DeVroey assert that the Classical School was, at least by the time of the marginal revolution, in possession of a common paradigm. DeVroey, for instance, maintains that within the Classical School at this time:

a consensus existed among these people about the field in which they inquired, the questions they asked, and the main concepts and categories which they used in order to answer these questions. Thus, one already may speak of a "paradigm in dominance." [DeVroey, 1975, page 421]<sup>165</sup>

Those defining the paradigm include class structure and income distribution among the classes as the chief foci of the paradigm and identify the pursuit of economic growth among its chief objectives. We find, however, that they differ as to many of the other major elements they include in their specification of the paradigm.

DeVroey specifies the Classical paradigm across a number of different elements. Its object, according to DeVroey, was to "determine the genesis of wealth and the laws governing its distribution," while it set as the aim of its research

"assist[ing] those policy-makers who aimed to increase the wealth of nations" [DeVroey, 1975, page 421]. It chose as its unit of analysis the social class and sought to examine the institutional framework in terms of the interrelationships between the three classes it identified ("landlords, workers, and the capitalists"). At the core of the Classical paradigm's theoretical structure, DeVroey finds capital, defined broadly to include "machinery as well as raw material and labor" [DeVroey, 1975, pages 422-423]. DeVroey identifies the paradigm's theory of value and conception of profit as two other important paradigmatic elements. The paradigm's theory of value (the labor theory of value) conceived of value in terms of the social division of labor and thus sought to explain value "in terms of production rather than exchange" [DeVroey, 1975, pages 422-423]. Its theory of profits conceived of profits as the product of the unequal distribution of wealth in society and as the impetus to growth [DeVroey, 1975, page 430].

L.E. Johnson and Robert D. Ley also describe the Classical paradigm in terms of a number of different constitutive elements. The assumptions of "natural law, a class conflict view of the economy embodying an opposition of class interests," the "subsistence wage doctrine" and "the doctrine of the wages fund" constituted the paradigm's "fundamental theoretical assumptions" [Johnson, 1983, page 1099]. The paradigm's method was deductive and sought "to provide a long-run dynamic analysis" and focused on the *aggregate* variables: "wages, rents, profits, 'total social surplus,' and the 'wealth of a nation'" [Johnson, 1983, pages 1099-1100]. The "basic issues" with which the paradigm concerned itself were "the questions of economic growth, income distribution, and the interrelationships between them" [Johnson and Ley, 1990, page 97].<sup>166</sup> Finally, according to Johnson and Ley, the defining characteristic of the classical paradigm was its overriding purpose: "examination of the extent to which market directed capitalism leads to the maximization over time of total social welfare, defined in material terms"<sup>167</sup> [Johnson and Ley, 1990, pages 89-90].



a. Smith's Paradigm<sup>168</sup>

Some economists refer not simply to a *Classical* paradigm, but to *Adam Smith's* paradigm in the history of economics. Seligman for instance credits Smith with the introduction of a new paradigm in economics

Adam Smith was so well received -- even eagerly awaited -- because he offered in place of earlier theory a new conception that seemed to exhibit a Copernican power to explain the wealth of nations. It was a paradigm in economics that successfully incorporated new facts into its model and gave economists new rules for research." [Seligman, 1971, page 2]

D.P. O'Brien specifies Smith's paradigm as "that of self-interested pursuit and decentralized decision taking in a growth context viewed as producing a relatively best of affairs and relatively efficient allocation of resources" [O'Brien, 1983b, page 103]. Gordon also links Smith to an economics paradigm. Indeed, Gordon credits Smith with providing the one and only basic paradigm in economics' history up to the present day. For Gordon, the key elements of Smith's paradigm are maximization and the presence of a free market:

Smith's postulate of the maximizing individual in a relatively free market and the successful application of this postulate to a wide variety of specific questions is our basic paradigm. [Gordon, 1965, page 123]

E. Ray Canterbury and Robert J. Burkhardt also find Smith's *Wealth of Nations* providing the basis for the one paradigm which has dominated economics' history. They cite "the self-regulating nature of the market" as the "great truth" of Smith's economics [Canterbury and Burkhardt, 1983, page 22].<sup>169</sup>

b. Ricardo's Economics

O'Brien hedges on the paradigm status of Ricardo's economics, but he does allow that Ricardo's theories did serve a function commonly attributed to a Kuhnian paradigm, namely, insulation of practitioners from pressing social problems. O'Brien notes that the classical economist completely side steps one of the most burning issues of the day: the income tax [O'Brien, 1983b, page 103]. On the other hand, Bronfenbrenner identifies as two "standard" paradigms in Ricardian economics: the

"law of diminishing returns" and the "differential theory of rent" [Bronfenbrenner, 1971, page 139].

c. Classical Economics' Relationship to Neoclassical Economics

Paul Samuelson finds that the Classical economics of Smith, Ricardo and others does not constitute an alternative paradigm to present day neoclassical economics. Instead, he asserts, their economics contained within it the substance, if not fully formed, of neoclassical economics:

To the fascinating question of whether classical political economy does or may be made to, offer an "*alternative paradigm*" -- in the sense of Thomas Kuhn . . . to modern mainstream economics, the present investigation provides an instructive answer. So to speak, *within every classical economist there is to be discerned a modern economist trying to be born*. [Samuelson, 1978, page 1415, first emphasis in original, second emphasis added]

Classical economics did not seek to repudiate supply and demand analysis as much as it "sought to be able to say something significantly limiting about their properties, *quite the same way that we moderns endeavored to do*" [Samuelson, 1978, page 1415, emphasis added]. The modern-day economist also finds, though inchoate, the neoclassical marginal productivity conditions within the Classical paradigm. Once one provides the missing equations (necessary for identification) in the Classical model and commits to three assumptions,<sup>170</sup> Samuelson concludes that, "whether or not the classicist is yet aware of those relations and is able to apprehend them" "ruthless competition will enforce the neoclassical marginal productivity relations in the canonical model." Such relations existed within the classical system in Smith's and Ricardo's day, whether the classical economists detected them or not, just as it was the case that "Before Isaac N[ewtons]'s birth, apples and the moon fell toward the earth in accordance with inverse-square-of-distance gravitational laws" [Samuelson, 1978, page 1423]!

Like Samuelson, Zinam places classical and neo-classical economics under the umbrella of a single paradigm, the "classical/neo-classical paradigm." His description,

however, diverges broadly from Samuelson's account of classical economics in terms of supply and demand analysis, competition and marginal productivity conditions. Zinam sets forth an extensive list of elements of this paradigm. Included as "basic assumptions," we find the assumption that human beings are "rational" and "egoistic," that society is "individualistic," "atomistic" and harmonious," that idea systems are "given" and "determined outside economic system," and that power is "assumed given by economic analysis" and is assumed to "decentralized." The paradigm takes a "predominantly static view of equilibrium of economic forces" and "excludes socio-political-psychological factors" from its scope "by assuming economic man acting within atomistic society." The paradigm's methods are "predominantly static, partial and general equilibria" and include both "analytical and empirical" tools. Zinam, unlike most, incorporates an assessment of its relevance to modern society within his description of the paradigm: due to its narrow scope and "inadequacy of methods," the paradigm is largely "irrelevant" to modern society [Zinam, 1978, page 171].

Contrary to Samuelson and Zinam, DeVroey regards Classical Economics and Neoclassical Economics which followed it as possessing separate and very different paradigms. DeVroey contrasts the two paradigms along a number of different lines. In contrast to the Classical paradigm's *goal* centering upon economic growth, the neoclassical paradigm concerned itself with matters of efficiency. Unlike the Classical paradigm under which the aim of economics was the assistance of policy makers, the neoclassical paradigm sought "for a general theory, universally valid, applicable to all social systems" [DeVroey, 1975, page 426]. While the earlier paradigm took as its unit of analysis the social class, the later neoclassical paradigm conceived of the economy in terms of "a 'general public' making decisions through votes (individually) in the market" [DeVroey, 1975, page 430]. Further, the central concept shifted from capital to prices. The two paradigms also differed in terms of the theories of value and of profit. The Classical theory of value centered upon the production process, while

the neoclassical theory saw value "flow[ing] from the subjective mental evaluation of the individual agents" [DeVroey, 1975, page 430]. Finally, the former regarded profits as the result of distributional inequalities, while the latter saw profits as the "reward for the capitalists' abstinence" [DeVroey, 1975, page 429].<sup>171</sup>

d. Classical Economics is Not a Kuhnian Paradigm

Baumberger regards Classical Economics as a "tradition in progress," which continues to the present day in the form of Marxism. Baumberger affirms, however, that the Classical tradition is far too diverse and fluid to fit within what he sees to be "the Procrustean bed of a Kuhnian normal science paradigm" and warns that one should not seek "to whittle it down to size" to force it into the paradigm mold [Baumberger, 1977, pages 9-10].

2. *Pre-Keynesian Macroeconomics*

A central element of the pre-Keynesian macroeconomics paradigms, economists find, is the notion that forces exist in the economy which ensure full employment and/or the equating of overall supply with overall demand.

Alfred Bornemann identifies classical economics, accompanied by Say's Law, as economics' dominant paradigm prior to the Keynesian revolution. According to Bornemann, the classical paradigm envisions entrepreneurs as setting in motion a normally unbroken circular flow of economic activity that conduces to an equilibrium at which output is maximized:

In classical economics the individual entrepreneur's output based on the firm's least-cost combination of factor inputs initiated the circular flow of economic activity which culminated in a general equilibrium of maximum output for the entire market economy. Any break in the circular flow was caused by misdirected production and only temporary." [Bornemann, 1976, page 125]

Likewise, Ghanshyam Mehta refers to "the main pillar of the Smith-Say-Mill paradigm" as "the proposition that supply creates its own demand" [Mehta, 1978, page 23]. Tied with this notion, Mehta affirms that the "extended quantity theory of

money,"<sup>172</sup> -- which holds that increases in the money supply are almost always met by proportional increases in prices -- "held undisputed sway over the minds of economists for another 200 years or so" from Smith's to Keynes' day [Mehta, 1978, pages 47-48].

Rugina describes the Classical system of reference (a term he equates with Kuhn's paradigm concept) as an "abstract, hypothetical model of stable equilibrium" [Rugina, 1986, page 41].

### 3. *Keynesian Economics*

In comparison to the Classical system, Rugina describes as Keynes' "system of reference" as a "more realistic and relativistic model of disequilibrium or unstable equilibrium conditions" [Rugina, 1986, page 41].

Johnson and Ley identify "the maintenance of full employment" as the Keynesian paradigm's purposive function [Johnson and Ley, 1990, page 135], and "the maximization of employment opportunities" as the paradigm's "primary purpose of economic inquiry" [Johnson and Ley, 1990, page 139].<sup>173</sup>

Bornemann asserts that Keynes' *The General Theory of Employment, Interest and Money* became economics' leading paradigm with its publication in 1936. Keynes' macroeconomic theory put forth in the *The General Theory* "emphasized aggregate income and money demand rather than enterprise and production," as had economics' previous paradigm (Say's Law). Keynes' theory saw there to be deficient aggregate demand due to "failure to operate at the level of full employment." The theory thus, according to Bornemann, underlined the need for government spending in order "to achieve full-employment" [Bornemann, 1976, page 125].

Likewise, Pheby maintains that while many economists prior to 1936 voiced disagreement with the assumption of full employment and the notion that there was no macroeconomic role for government,

it was not until the publication of the *General Theory* that this extraordinary research previously undertaken began to take the shape of

an alternative paradigm with prospects for the development of a normal scientific tradition . . . [Pheby, 1988, page 51]

Keynes countered notions of the ineffectiveness of government spending with his "analysis of multiplier effects." Also distinguishing Keynes' paradigm from its predecessor was the greater stress that Keynes placed upon "aggregate analysis, and the distinction of different demands for money (that is, transactions, precautionary and speculative) [which] provided new insights." It was by virtue of these "significant achievements" that Keynes' *General Theory* "began to attract an enduring group of adherents" [Pheby, 1988, page 51], and thus constituted Keynes' work as a paradigm.

While both Bornemann and Pheby identify Keynes' *General Theory* as the Keynesian paradigm, Mehta maintains that Keynes' earlier tract, his *Treatise on Money* functioned as "the paradigm of Keynesian normal science" [Mehta, 1978, page 118]. The *Treatise* constituted a paradigm in that, here, Keynes "incorporated the idea of his contemporaries in his system" and

did what many others had failed to do. He incorporated quantity adjustments in this model and described the working of the comparative-statics multiplier. Furthermore, he integrated the theory of money with the theory of the process of income change. [Mehta, 1978, pages 149-150]<sup>174</sup>

Another notable attribute of the *Treatise* is its "use of equations in which *the variable P occurs but M does not*," marking a move away from customary practice prior to the *Treatise's* publication in which money played a central role in the determination of the general price level. Finally, Mehta stresses that the *Treatise* proffered a theory of output and employment determination in which "the price-level and output were shown to depend on the offers that the entrepreneurs felt it worthwhile to make to the factors of production," with "the scale of their offers . . . dependent on the relation between investment and saving" [Mehta, 1978, page 149].

#### 4. *Competition*

Some economists identify competition as economics' paradigm around the turn of the twentieth century. Anastasios Petridis describes competition as "the dominant

paradigm in economics" during Marshall's career [Petridis, 1973, page 166], while Thomas Reinwald finds that in the 1930s, "the purely competitive framework came very close to constituting the paradigm of Neoclassical economic thought."<sup>175</sup>

#### 5. *Austrian Economics*

Karl-Heinz Paqué suggests that one must view Hayek's "pattern prediction" as nothing more than "an ordinary prediction," if the pattern predicted is parametric. On the other hand, if the pattern is *non*-parametric, Paqué notes that "the pattern prediction is not falsifiable" and hence is "not a genuine prediction at all but rather a particular way of describing and interpreting reality, e.g., a framework of thinking, or, in the terminology of Thomas Kuhn, a paradigm" [Paqué, 1990, page 292].

#### 6. *Broad Collections of Paradigms Identified in Economics' History*

Some authors identify a host of paradigms in the history of economic thought, of varying character and generality. For example, Karsten, having allied Kuhn's paradigm concept with the dialectical notion of the thesis, identifies both *schools of economic thought* ("mercantilism, Physiocracy, classical, institutional, or Keynesian economics"), as well as various *economic theories* (bullionism, Ricardo's theory of rent, Malthus' population thesis, the labor theory of value, and marginal utility theory) as theses [Karsten, 1973, page 408]. Similarly, Gordon lists various major economic theories as "sub-paradigms" in economics' history: "classical wages fund doctrine", "generalized marginal productivity theory", and "utility theory" [Gordon, 1965, page 124].

### G. CURRENT-DAY MAINSTREAM PARADIGMS

We now turn our attention to paradigms which economists have located in present-day economics. We first examine paradigms located in mainstream economics and then turn our attention to paradigms which economists identify lying outside orthodox economics (i.e., heterodox paradigms).<sup>176</sup> We then conclude our discussion

by looking at paradigms which economists have located in various sub-fields in economics.

As the following discussion demonstrates, economists have identified a large number of different paradigms, within economics' mainstream, outside of it, and within sub-fields of the discipline.

Examining economists' identifications of mainstream paradigms, we are struck by the diversity. Here, we identify at least ten different mainstream paradigms discussed in the literature and further find that, while there is overlap, there are a significant number of different ways in which economists have specified nominally the same paradigm. We consider each paradigm in turn.

#### 1. *Maximization Assumption*

Numerous economists cite the maximization assumption or some variant of it as economics' current paradigm. As noted earlier, Gordon identifies as the one and only *major* paradigm in economics' history, "Smith's postulate of the maximizing individual in a relatively free market and the successful application of this postulate to a wide variety of specific questions is our basic paradigm" [Gordon, 1965, page 123]. Similarly, Christopher Gilbert<sup>177</sup> maintains that "over the post-war period economic theory has been increasingly dominated by the paradigm of atomistic agents maximising subject to a constraint set" [Gilbert, 1989, page 3].<sup>178,179</sup> Worland, building upon Gordon's assessment of economics' paradigm, maintains that the "fundamental behavioral postulate" within the maximization paradigm is "the *utility* maximization assumption" [Worland, 1972, page 280, emphasis added].

Others describe maximization as only *an* (but not necessarily *the*) economics paradigm. Sidney Winter, for example, refers to "the use of the optimization assumptions" as "*an* example in economic science of what Thomas Kuhn calls a 'paradigm'" [Winter, 1981, page 31, emphasis added].<sup>180</sup>



Still others identify the assumption as one among other elements, which constitute modern day economics' paradigm. Guy Ahonen, for example, includes the maximization assumption as one among the three elements in economics' paradigm [Ahonen, 1990, page 96].<sup>181,182,183</sup>

## 2. *Equilibrium Conception/Assumption*

Coats asserts that economics has been dominated by one paradigm throughout its history: "the theory of economic equilibrium via the market mechanism" [Coats, 1969, page 292].<sup>184</sup> Gruchy, likewise, describes economics' orthodox paradigm as the "equilibrium paradigm" which posits that while "an economic system may temporarily vary from the equilibrium model of perfect competition, it sooner or later returns to this basically changeless competitive model" [Gruchy, 1986, page 807]. Similarly, L. William Kapp identifies "stable equilibrium" as a "traditional" "framework"/ "disciplinary matrix"/ "paradigm." He contrasts the paradigm with one which conceives of the economy moving away from, not towards a position of equilibrium [Kapp, 1976, pages 217 and 223].

Charles Fischer does not refer to the "economic equilibrium paradigm" as *the* paradigm of orthodox economics, but does assert that it is "arguably one of the most successful and pervasive reigning paradigms in orthodox economics" [Fischer, 1993, page 52].

Others, as we shall see in the next section, regard the conception/assumption that the economy is in or tending toward equilibrium as one of two major elements composing orthodox economics' paradigm.

Blaug, however, sees the theory of economic equilibrium via the market mechanism not as a paradigm, but instead as "actually a network of interconnected sub-paradigms; in short, it is best regarded as a Lakatosian SRP [scientific research programme]" [Blaug, 1976, pages 160-161].<sup>185,186</sup>

### 3. *Harmony and Equilibrium*

Many economists see an understanding of the economy as tending toward both harmony and equilibrium as constituting orthodox economics' central paradigm.

Zweig, for instance, identifies the bourgeois paradigm as the dominant economics paradigm. The paradigm's "two most central and distinctive elements . . . are harmony and equilibrium." Irrespective of the level of competition,<sup>187</sup> a harmony exists in that each and every individual responds in the same way to the same situation and thus comes to see, in himself, all others:

economic actors . . . are all motivated by formally identical desires (the standard "postulates of rationality"), and in a given situation (including tests and factor endowments) each would do the same. *In this deep and important sense, all men are brothers*, each recognizes himself in all others, all men are "about" the same thing in the same way. This harmony . . . helps "explain" why competitors (are expected to) cooperate with the outcome of the market place in which they compete. [Zweig, 1971, page 45, emphasis added]<sup>188</sup>

As to equilibrium, Zweig notes, "Almost all of economics, whether static or dynamic, micro or macro, long run or short, is organized on the basis of equilibrium or a tendency to such a state," whereas "Explorations of disequilibrium situations are done in terms of tendencies toward equilibrium, and even where these tendencies cannot be found, equilibrium positions constitute the reference point of the analysis" [Zweig, 1971, pages 45-46].

Similar to Zweig's bourgeois paradigm, David asserts that the "salient features" of the orthodox economics paradigm are "harmony, and tendencies toward equilibrium, stability, and balance." He too ties the notion of harmony to the understanding that all individuals are, at bottom, the same. There is a "minimum of conflict between rational economic men and homogeneous economic units" and "the important problems of conflict which arise from the interaction of diverse racial, class, or interest groups are more or less ignored" [David, 1975, pages 74-75].

Sweezy also sees the regnant economics paradigm (orthodox economics) as assuming both harmony and equilibrium. He adds that the paradigm also "takes the

social order for granted" and thus assumes "that the capitalist system is permanent," and that change occurs gradually.<sup>189</sup>

#### 4. *General Equilibrium Theory*

Economists differ as to the paradigmatic status they attribute to general equilibrium. Edgar Dunn describes general equilibrium as the regnant economics paradigm [Dunn, 1970, page 353]. Tong-eng Wang, on the other hand, describes the "general equilibrium approach" as one among many paradigms in economics today. [Wang, 1973, page 151, note 2].<sup>190</sup> Similarly, Roland Aeppli finds that "walrasian equilibrium theory," is only one among three current-day economics paradigms, along with "walrasian *disequilibrium* theory and the New Microeconomics" [Aeppli, 1980, page 708, emphasis added].<sup>191</sup>

Likewise, Dow sees general equilibrium as only the *methodological* expression of the regnant neoclassical paradigm. Accompanying general equilibrium is the paradigm's *ideological* expression: the Invisible Hand.<sup>192</sup>

Still others regard general equilibrium itself as composed of many paradigms. Daniel Fusfeld, for example, describes general equilibrium, which he regards as regnant economic theory, not as a single paradigm, but as a *system* of interconnected paradigms composed of two major elements (the microeconomic and macroeconomic), with the microeconomic in turn divided into three parts: "consumption theory, production theory, and the theory of income distribution," and the macroeconomic into two: "real and monetary equilibria". Still again, the five sub-elements "are each subdivided further into a series of partial equilibrium analyses." In sum, Fusfeld notes "The result is a system of paradigms, or propositions, or models, which in turn rest within a larger general equilibrium model of the economy as a whole" [Fusfeld, 1980, page 11]. Each element is analyzed first in isolation from one another in terms of *partial* equilibrium and are then re-united "into a *general* equilibrium held together

primarily by faith that this abridgment of proper logical analysis will not do an excessive amount of violence to the truth and by the hope that no one will notice" [Fusfeld, 1980, page 13, emphasis added].

Jalladeau identifies general equilibrium theory as the one and only paradigm in economics' history. The paradigm, as he describes it, contains elements discussed in each of the previous sections. He identifies "perfect competition" as one of the theory's "two crucial suppositions" along with "perfect rationality" and directly allies general equilibrium theory with Coats's identification of "the theory of economic equilibrium via the market mechanism" as economics' paradigm. Still more, Jalladeau stresses that general equilibrium theory presumes away conflict and puts in its place "a marvelously harmonious system":

The paradigm rests on a strictly individualistic postulate, namely, that any and all conflicting components in the networks of relations between economic units and groups of economic units have been eliminated. Likewise, the stability of preferences constitutes a comfortable hypothesis. The economic system is conceived of as a self-regulating mechanism, with the role of government being most closely restricted and the interests of individual economic agents being well attuned to each other and to the general interest. This is a marvelously harmonious system. [Jalladeau, 1975, page 3]

## 5. *Neo-Classical Synthesis*

Many economists identify the neoclassical synthesis of Keynes' economics as modern-day economics' regnant paradigm. Wallace Peterson, for instance, notes that "the neoclassical version of J.M. Keynes"<sup>193</sup> is "the accepted paradigm" in economics. Peterson lists as the "essential features of the neoclassical synthesis": "an income-expenditure model," endogenous money, financial variables and labor market relationships -- which help ensure that the economy automatically tends toward full employment -- and the assumption of the neutrality of money.<sup>194,195</sup>

Still again, Stanfield identifies "The current normal science paradigm in orthodox economic thought" as "a combination of Keynesian macro-economics and

neoclassical micro-economics" [Stanfield, 1979, page 3]. The "essense of the synthesis" according to Stanfield is the understanding that *macroeconomic* policies serve to return the economy to full employment in order that we may then turn our attention to the concerns of neoclassical *microeconomics*:

once Keynesian political economy has guided the state in the underwriting of full employment equilibrium, the focus of economics can return to the neoclassical world of relative prices, allocation of given scarce resources to given alternative ends, and the distribution of national income among the factors of production. [Stanfield, 1979, page 4]

While pointing out that the neoclassical synthesis does not imply an unfettered "laissez-faire ideology" -- given that government still has a role in returning the economy to full employment -- Stanfield stresses that the synthesis "is not a complete break with the laissez-faire predisposition . . . ." [Stanfield, 1979, page 5]. The neoclassical synthesis excises from Keynes' economics some of its more radical elements, including "income distribution and the socialisation of investment" [Stanfield, 1979, page 5].

Ralph Anspach describes the neoclassical synthesis not simply as one paradigm, but as the co-existence of two mutually incompatible paradigms: the microeconomics paradigm and the macroeconomics paradigm. On the one hand, the microeconomics paradigm makes "such conscious or unconscious assumptions as man-is-a-factor-of-production, work is painful, rationality, competition, the acceptability of the given income distribution . . . ." [Anspach, 1974, page 2, footnote 3]. While in contrast, the macroeconomics paradigm sees man neither as a commodity, nor as motivated by the rational, but as human and directed by habit. The incompatibility of the two paradigms is underlined by the fact that the macroeconomics paradigm regards unemployment as a serious problem, whereas, according to a strictly microeconomic understanding of man as a commodity, durable or perishable, unemployment represents no problem:

What has been overlooked, however, is that unemployment becomes a major problem only because man cannot be treated with impunity as a commodity, either in practice or in theory. Furthermore, as we will see, responding to unemployment by constructing a separate paradigm to

coexist with the received paradigm, rather than confronting head-on the insupportable man/commodity identity, has not been successful. This is because the two paradigms (micro and macro) are in conflict with each other and the implementation of policies flowing from one seriously distorts the functioning of the economic system described by the other. [Anspach, 1974, page 3]

Similarly, Johnson and Ley, while not referring to the neoclassical synthesis per se as economics' current regnant paradigm, argue that mainstream economics comprises the "unhappy" "union" of neoclassical economics on the micro side, with Keynesian economics on the macro. The two co-exist with one another because they share the rationality assumption and use of the deductive method in common. However, the paradigms fit uncomfortably with one another because they possess different purposive functions [Johnson and Ley, 1990, page 144]. Neoclassical economics' P-F is "the maximization of employment opportunities" [Johnson and Ley, 1990, page 135], while the Keynesian paradigm's P-F is the "the maintenance of full employment" [Johnson and Ley, 1990, page 139].<sup>196</sup>

#### 6. *"Value Free" Economics Paradigm*

Others' descriptions of the mainstream economics paradigm focus upon its pursuit of "value free" analysis. Carol Anderson notes that the paradigm "overlooks meaning, values, goals, and concerns which flow from interaction among individuals in society . . . Today's economic paradigm eliminates the process of creating meaning and value" [C. Anderson, 1982, page 222]. Similarly, Heilbroner identifies as a hegemonic paradigm in economics, "a particular model of that method [the scientific method] that stresses avoidance of explicit value judgments and dependence on relationships capable of rigorous expression, preferably mathematical notation" [Heilbroner, 1971, page 3].

#### 7. *Natural Sciences Paradigm*

Chase, following Adolph Lowe, identifies the "natural sciences paradigm" as the guiding paradigm in modern economics. The paradigm places emphasis upon

prediction and an approach which seeks to identify the means to predetermined ends. Such a paradigm implies two underlying presuppositions: (1) "autonomy of existence,"<sup>197</sup> of economic agents and (2) "inherent orderliness" of the economy [Chase, 1983a, pages 168-169].

#### 8. *Neo-Keynesian Paradigm*

Luther Tweeten asserts that "the neo-Keynesian paradigm (NK) macroeconomic paradigm . . . has guided this nation [United States] for over three decades." The paradigm starts with the Keynesian understanding that the economy operates at less than full employment, and that there exists "a government role in monetary-fiscal policy to stimulate aggregate demand in depressions and severe recessions when planned savings exceed planned investment." However, Tweeten notes the paradigm adds three additional assumptions, namely that "advanced capitalistic nations are chronically prone to (a) high unemployment, (b) economic instability, and (c) increasing concentration of resources and wealth." Underlying this understanding are the assumptions that "spending moods" are highly volatile and unpredictable and that "big business" is wont to exploit both the laborer and the consumer. Such tendencies, according to neo-Keynesian economics, need to be addressed by extensive government intervention, such as:

perennial monetary-fiscal stimulation of aggregate demand, by social legislation to redistribute wealth and protect the worker and consumer, and by formation of politicoeconomic collectives to promote economic democracy through a paternalistic government. [Tweeten, 1980, page 855]

#### 9. *Neoclassical Paradigm*

Many economists acknowledge the paradigm status of neoclassical economics.

Solow affirms:

If you look at Kuhn's examples -- all from natural science, of course -- you will see that they represent well developed models or frameworks for thought. Some of his examples are Newtonian dynamics, Copernican astronomy, Ptolemaic astronomy for that matter, wave

optics, etc. In this sense, neoclassical economics is pretty clearly a scientific paradigm. It may be a bad one, or a worn-out one, or it may have served to advance the interests of the capitalist class, but it is the sort of thing Kuhn means. [Solow, 1971, pages 63-64]<sup>198</sup>

So too Ramstad assents that, "Neoclassical economics is arguably a 'scientific paradigm' with well-known principles and a definite theoretical structure . . . ." [Ramstad, 1989, pages 770-771].

Some directly identify neoclassical economics as the *dominant* economics paradigm. James Swaney and Robert Premus maintain that economics is dominated solely and so completely by the neoclassical paradigm, to such an extent that the paradigm "inhibits inductive development of theories that reflect economic reality" [Swaney and Premus, 1982, page 726].<sup>199</sup> So too do Sheila Dow, John Cornwall, and Alfred Eichner and J.A. Kregel identify the neoclassical paradigm as economics' dominant/prevaling paradigm [Dow, 1980, pages 373-374; Cornwall, 1979, pages 69-71; Eichner and Kregel, 1975, pages 1293-1294].

As we have seen, Johnson and Ley, on the other hand, identify the neoclassical paradigm as one of economics' two dominant paradigms comprising mainstream economics, along with the Keynesian paradigm. [Johnson and Ley, 1990, page 144].

What, however, constitutes the neoclassical paradigm? Here we find a variety of different answers. Kurt Dopfer identifies the paradigm in terms of broad methodological and scope considerations: "a logico-deductive approach," "platonic beauty," and "universalistic doctrine." [Dopfer, 1986, pages 998 and 1006]. Charles Hirschman succinctly speaks of "the neoclassical economic paradigm of supply, demand and general equilibrium" [Hirschman, 1981, page 563]. Ward, on the other hand, identifies liberalism (both political and psychological) as the "framework" of mainstream economics.<sup>200</sup> One aspect of this liberalism is the understanding that the workings of the market eliminates conflict among individuals in the economy and produces a harmony of interests [Ward, 1972, page 26].<sup>201</sup> Also, as we noted, Dow sees general equilibrium analysis as the "methodological expression" of the neoclassical



paradigm, and the "the all-encompassing concept of the Invisible Hand" to be its "ideological expression" [Dow, 1981, page 328].

While acknowledging that we find significant diversity under the heading "neoclassical," (e.g., Walrasian *general* equilibrium vs. Marshallian *partial* equilibrium), Eichner and Kregel assert that "All of these neoclassical models share certain features in common." In particular, they list the presumptions of (1) either no or steady growth, (2) that income distribution is determined by marginal productivity, (3) that agents have "complete foresight" and (4) that in order for "analysis" to be "considered complete," "All markets [must have] cleared with supply equal to demand in each of those markets." (5) Further, "perfect markets with all micro units operating as price takers" constitutes neoclassical economics' "microeconomic base" and (6) the purpose of neoclassical theory is "to demonstrate the social optimality if the real world were to resemble the model" [Eichner and Kregel, 1975, page 1309].

Cornwall's description overlaps Eichner and Kregel's in some respects (points (3) and (5)). Paralleling Eichner and Kregel's inclusion of "perfect markets . . .," Cornwall includes the assumption of the "applicability of the model of a competitive economy."<sup>202</sup> Again, like Eichner and Kregel, Cornwall includes in the specification of that economy the assumption of the existence of "perfect knowledge of past, present, and future events." Cornwall, however, does not include Eichner and Kregel's other elements and includes many components that Eichner and Kregel do not: (1) "constant tastes and technologies (or else changes in these elements that are exogenously determined and predictable);" (2) "consumer and worker sovereignty;" (3) "very simple and well defined functions describing the objectives of consumers, workers, and producers; equally well defined constraints on these objectives;" and (4) "'rational' behavior on the part of all economic actors in the sense that the means they utilize to achieve stated ends or objectives are always the most efficient ones" [Cornwall, 1979, page 71].<sup>203</sup>

As we saw earlier, DeVroey provides yet another specification of the neoclassical paradigm.<sup>204</sup> Similar to Eichner and Kregel's assessment that the neoclassical paradigm assumes no or steady growth and has as its purpose the demonstration of "social optimality . . .," DeVroey specifies the *object* of the neoclassical paradigm to be efficiency, rather than economic growth [DeVroey, 1975, page 426]. DeVroey, however, includes elements that Eichner and Kregel do not, including the school's abstinence theory of profits and subjective theory of value.

#### 10. *Exemplar Paradigms in Neoclassical Economics*

Argyrous and Holland identify paradigms *qua* exemplars within neoclassical economics. In consumption theory, Argyrous identifies Friedman's and Modigliani's work on consumption as exemplars. As exemplars, Friedman's work on the Permanent Income Hypothesis (PIH) and Modigliani's on the Life Cycle Hypothesis (LCH) both (1) provide respected solutions to long-standing puzzles in Keynesian consumption theory and (2) suggest an agenda for economics research. In particular, they both provide a solution to the puzzle presented by the inverse relationship between income and the marginal propensity to consume in cross-section studies on the one hand, and the MPC's constancy over time on the other. Further, they both do so by solving another puzzle: "Could aggregate consumption theory be given a microeconomic explanation?". Modigliani explains the conundrum by positing that people seek to smooth out consumption over their lifetime; Friedman solves the problem by positing that people spend out of permanent but not transitory income.<sup>205</sup> Each hypothesis' predictions not only mesh with the facts; they both comport with the desire to provide a micro-based explanation of consumer behavior [Argyrous, 1992, pages 236-238]. Still more, both hypotheses themselves provide new problems for scientists to explore. The PIH leaves open questions as to whether -- as Friedman contends -- people spend, on average, only out of permanent income. The hypothesis also requires that economists

devise an operational definition of permanent income and explore the policy ramification of Friedman's contention [Argyrous, 1992, pages 238-240].

Holland offers the analysis of labor demand in economics as a paradigm for microeconomics. The analysis, according to him, consists of twenty-three logically sequenced questions which may be boiled down to seven primary questions such as "In total, what do you give up and receive when you purchase and use a resource?" [Holland, 1987, pages 192-193]. These questions, Holland contends, may be used to identify gaps and problems in economics' understanding of labor demand [Holland, 1987, page 194]. Still further, the analysis may be used to formulate parallel questions regarding other economic phenomena (e.g., the supply of labor, the supply and demand for a product). For example, as an analog to the question asked in studying labor demand, in analyzing the supply of a product we ask "In total, what do you receive and give up when you sell and produce a product?" [Holland, 1987, page 193].

#### 11. *Summary and Conclusions*

The above economists all direct their attention to the same field (economics), the same time period (the present day), nominally the same area of economics (the mainstream) and all seek to identify the "same" type of entity (a paradigm). However, as our discussion demonstrates, their specifications of present-day mainstream economics paradigms vary widely. We find at least ten different mainstream paradigms identified in the literature. Further, while in many cases economists' descriptions of the "same" paradigm overlap, we also find significant differences among their specification of that paradigm. Economists' varied descriptions of neoclassical economics and the neoclassical synthesis as mainstream paradigms underline this point best.

Economists also differ as to the paradigmatic status they lend to various notions and theories in economics. What some economists identify as an economics paradigm

in and of itself, others describe as only one among many elements composing a broader mainstream economics' paradigm (e.g., maximization assumption (Ahonen), equilibrium assumption/conception (Zweig and Sweezy), general equilibrium theory (Dow)). What some economists describe as a single economics paradigm, others assert is actually two (Anspach's account of the neoclassical synthesis) or even more paradigms (Fusfeld's depiction of general equilibrium theory). Finally, what some identify as mainstream economics' dominant paradigm, others ascribe less prominence to, describing it as only one among many paradigms populating economics' orthodoxy (e.g., general equilibrium theory (Wang and Aeppli), neoclassical paradigm (Johnson and Ley)).

We must concede that many of the different paradigms identified and the various descriptions of them are not necessarily incompatible with one another. Further the paradigms we laid out are not mutually exclusive from one another. If we find diversity as to economists' descriptions of nominally the same paradigm, we also find commonality across the different paradigms. Jalladeau's specification of general equilibrium as mainstream economics dominant paradigm, as we noted, overlaps with economists' descriptions of many other different paradigms (equilibrium and harmony, in particular). However, the point needs to be stressed: Even with a certain measure of arguable consistency among the different paradigms laid out here, even with overlap both among economists descriptions of nominally the same paradigm and among their depictions of different paradigms, *economists differ as to which elements in economics' current orthodoxy they identify as constitutive of a (the) mainstream economics paradigm*. Further, they differ as to whether a given notion/concept/methodological precept warrants paradigmatic status on its own, or whether it represents only one among many elements which together comprise economics' mainstream paradigm.

In addition, paralleling the multiplicity of understandings of what constitutes a Kuhnian paradigm, we find that the sorts of paradigms economists locate in orthodox

economics vary as well. We find descriptions of paradigms amounting to methodological precepts, worldview, and theories, or some combination thereof. The "value free" economics and the natural sciences paradigms constitute examples of methodological precepts as paradigms. Here, we may also include Winter who identifies the *use* of the maximization assumption as one of economics' leading paradigms. Gruchy's and Kapp's descriptions of the equilibrium paradigm and Sweezy's and Zweig's depiction of the harmony and equilibrium paradigm provide accounts of mainstream economics paradigms as worldviews, i.e., understandings of how the economy operates and/or how economists perceive economic "reality." In addition, we may note that both Cornwall's listing of the assumptions of the neoclassical paradigm and Peterson's description of the neoclassical synthesis highlight worldview aspects of these paradigms. While conceding that the line between theory and worldview is a fuzzy one, we may note Coats's identification of the theory of equilibrium, Gordon's specification of the maximization hypothesis and, in general, economists' identification of general equilibrium theory as examples in which economists have allied economics paradigms with economics theories. Finally, several economists have depicted the neoclassical paradigm as an eclectic collection of theory, method and/or worldview. Here, we may list Dow's depiction of that paradigm as composed of both prevailing ideology as well as methodology, Eichner and Kregel's list of four neoclassical assumptions about the economy, along with specification as to how one determines when analysis is complete, and DeVroey's inclusion of theories (theories of profit and value), a worldview (individualism) and methodological precepts (pursuit of universally applicable general laws) in his accounting of that paradigm.

Along these lines, however, we should stress that we found very few examples of economists identifying mainstream economics paradigms *qua* exemplars in our research. Argyrous' and Holland's are the only two major discussions of exemplars in economics we located. The lack of many significant discussions of exemplars in

economics comports with economists' slighting of this understanding of paradigm in their accounts as to what, in general, constitutes a Kuhnian paradigm.

Having stressed the differences among economists as to their specification of economics' mainstream paradigm, we may conclude our discussion here by noting one significant common strand running throughout almost all of these economists' discussions of mainstream economics paradigms. Rarely do these economists seek to justify that the paradigm they identify as a (the) mainstream economics paradigm is a paradigm. Most appear to presume that there is no need to certify the paradigmatic status of the paradigm they identify. As we see in the following two discussions (on heterodox paradigms and sub-field economics paradigms), presumptions as to paradigmatic status, though still common, are far less frequent.

#### H. CURRENT-DAY HETERODOX PARADIGMS

We next turn our attention toward paradigms we locate outside mainstream economics, i.e., heterodox paradigms. As with our examination of orthodox economics paradigms, we find a variety of different paradigms identified outside economics' mainstream. In part, we may attribute this diversity to the numerous different schools populating economics' heterodoxy. Indeed, for the most part, we have divided the following discussion by heterodox school.<sup>206</sup> However, even within paradigms found within each school, we find significant differences. Further, not only do we find diversity as to the description of a particular school's paradigm(s), but we also identify disagreements as to whether or not a particular school constitutes/possesses a paradigm. We consider each school in turn.

##### 1. *Political Economy*

In assaying the paradigm status of current day Political Economy, Black concludes that the term does not connote, as it did a century before, a "unified subject." Instead, under this heading Black identifies seven different groups:

Marxism, the New Left, Institutionalism, Adolph Lowe's Political Economy, David Winch's Political Economy, Public Choice and Post-Keynesianism [Black, 1983, pages 55-70]. While each of the schools recognizes a "dual dimension -- political and economic" and most<sup>207</sup> conceive of Political Economy as a "moral science," "The *only* characteristic which all the members of this intersection set share is their recognition of a dual dimension . . . ." [Black, 1983, page 65, emphasis added]. Conversely, the groups diverge along a number lines: the breadth of influence they assign to political and economic factors (The New Left envisions a broader scope for the political and economic nexus than does Marxism.);<sup>208</sup> acceptance of mainstream assumptions and techniques (Public Choice embraces and employs the neoclassical rationality assumption and thus might be seen as an extension of neoclassical analysis, while Post-Keynesianism eschews the mainstream's conjunction of short-term Keynesian analysis with long-term neoclassical analysis of the economy.);<sup>209</sup> and, most fundamentally, their goals for society (Winch calls for decentralization of power, while Lowe's aims imply greater centralization; Marxism and the New Left, in contrast to Institutionalism, do not see ties between the political and the economic as ultimately "acceptable.")<sup>210</sup> Thus, Black concludes that Political Economy does not represent "a new paradigm in the sense of Thomas Kuhn," but, instead, "a group of coexisting research programmes in the sense of Imre Lakatos -- coexisting with each other and with the research programs of economics and political science" [Black, 1983, pages 66-67].

A number of economists explore the paradigm status and specifications of many of the individual schools (or subsets of schools) which Black finds included under the rubric, Political Economy. We look at those economists in the following seven sections. We discover that some locate a paradigm within a given school, while others do not. Among those seeing a paradigm, we do find similarities in their characterizations of that paradigm; we also, however, find notable differences in what these economists include and/or lay emphasis upon. By the same token, we also find

similarities between the paradigm which some economists describe in one school with the paradigm(s) identified within another economics' community.

## 2. *Lowe's Political Economy*

Both Chase and Heilbroner identify Lowe's Political Economics as a viable alternative paradigm to the mainstream and define the paradigm as the reversal of the traditional means --> end approach to an end --> means approach, in which ends are established at the outset in order to give direction to an otherwise unordered economy:

This is to say that in Political Economics, economic ends are determined in the first instance through the political process (subject, of course, to technical verification for internal consistency and achievability in light of known material and technical constraints). Then, in the second instance, economic and political (*i.e.* legal, institutional, etc.) *means are instrumentally employed* to achieve a path leading to the predetermined end-state. The determination of goal-adequate paths for the socioeconomic system along with the development of any necessary goal-adequate means and the verification of the latter's suitability and potency, are the primary areas for instrumental theory and analysis; while the iterative application and adjustment of these means so as to maintain the goal-adequate path involves what Lowe calls instrumental inference. [Chase, 1983a, page 173]

The new paradigm, in other words, consists in an abandonment of the view of social analysis as that of determining the immanent destination of a universe of goalless particles, and substituting a view of social science as the search for the means by which social goals can be attained. [Heilbroner, 1971, page 17]

## 3. *Heterodox Radical*

Likewise, W.L. David sees a subdivision of the groups Black discusses (Marxism and the Old and New Left) as constituting the heterodox radical paradigm. All three lay stress upon "the socially conditioned character of general economic relationships" and recognize, following Marx, that, "while economic categories and relationships may possess certain general and timeless characteristics, it is also true that these characteristics were molded by history and socioeconomic conditioning" [David, 1975, page 78].



#### 4. *Marxian Economics*

In opposition to the orthodox paradigm's conceptions of harmony and equilibrium within the economy, Sweezy describes the Marxian paradigm as "stressing conflict, disequilibrium, and discontinuity" [Sweezy, 1971, page 64]. Zinam as well sees that Marxism conceives of society as "based on conflict," and regards ideas as "instruments in class warfare." In addition, according to Zinam, the paradigm sees human beings as imbued with "class consciousness, perfectible and socially determined" and society as "collective" and "organized" and ideas as determined both by class and by the mode of production. Zinam adds to his depiction of Marxism value stances both in terms of *society at large* (concentration of economic and political power is "deplored in capitalism, yet accepted as useful in socialism,") and the objective, scope and method of the *scientific community in particular* (its "nature and purpose," is the "genetic study of development of 'modes of production' within a broad historical setting" and adopts a very broad scope for analysis). The paradigm follows an "historical" and "genetic" approach." Finally, Zinam includes as an element of the paradigm an assessment of its relevance, which he describes as "greatly reduced by erroneous assumptions about the nature of capitalism, man, society and state" [Zinam, 1978, page 171]. Likewise, Heilbroner finds that the "Marxian model of society has not found a *satisfactory* paradigm of its own." The "indeterminacy" of the "dialectical linkages" which Marxism seeks to forge among "social and political events," "has not permitted the [Marxian] model to serve as a basis for reliable social prediction or guidance" [Heilbroner, 1971, page 13, emphasis added]. It is however unclear whether Heilbroner's assertion calls into question only the quality or more fundamentally the existence of the Marxist paradigm.

#### 5. *Radical Economics and the New Left*

Many economists question the paradigm status of radical economics. Solow, for example, does not see radical economics as a Kuhnian paradigm. Unlike the

paradigms which Kuhn identifies in the natural sciences, radical economics neither constitutes a well developed framework, nor provides the research agenda necessary for scientific inquiry:<sup>211</sup> "it is more a matter of posture and rhetoric than of scientific framework at all" [Solow, 1971, page 64]. Similarly, we hear from Worland:

In short, though radical speculation about the role of power in economic relationships is suggestive of future developments; though current controversy over fundamentals suggests that economics needs a new paradigm, one must conclude that radical economists have yet to produce one. [Worland, 1972, page 283]

Zinam, who locates and specifies a number of different economics paradigms, provides no description of a "New Left" paradigm because it remains in a "pre-paradigmatic stage" [Zinam, 1978, page 171].<sup>212,213</sup>

Zweig, on the other hand, does assert the existence of a radical economics paradigm and provides a description. Much as Sweezy did in specifying the attributes of the *Marxian* paradigm, Zweig contrasts the harmony and equilibrium notions within the bourgeois paradigm with the *radical*<sup>214</sup> paradigm's two chief characteristics: "conflict and dialectic":

*Conflict*: "A paradigm of conflict asserts that for each conflict there is a grouping of the members of society into a small number of classes. The class position of an individual is determined by some objectively verifiable relation to the issue of conflict . . ." [Zweig, 1971, page 48].

*Dialectic*: "In place of equilibrium, the radical paradigm of capitalism proposes dialectic processes by which conflicts develop. Economic systems, and associated social relations, change over time in response to the tensions generated by the conflicts, or contradictions, which characterize them. A central tenet of dialectics is that the prime energy for systematic change is internal to the developing system, not exogenously imposed. Furthermore, this internal pressure is always operating within capitalism, first to develop it and then to transform it utterly" [Zweig, 1971, page 48-49].

## 6. *Public Choice*

Charles Blankart affirms that Public Choice constitutes an economics paradigm, which he describes briefly as "the extension of exchange analysis from markets to the public sector and to political processes in general" [Blankart, 1987, page 5]. Blankart

offers both a "subjective" as well as an "objective" reason as to why Public Choice should be regarded as a paradigm. As a subjective justification, Blankart observes, on the basis of his personal experience that

public choice constituted a new paradigm which changed the previous ideologies completely. The nirvana of planning had to be given up. It first became necessary to explain the real world before a change of this world could be envisaged. [Blankart, 1987, page 3]

As more "objective" evidence, Blankart points to the broad based impact which Public Choice has had upon economics:

Public Choice has measurably penetrated nearly all fields of economics since its appearance, such as public finance, monetary theory and policy, foreign trade, antitrust, regulation, etc. Only fields with little practical application remained untouched by public choice. [Blankart, 1987, page 5]

#### 7. *Post Keynesianism and the "Economics of Keynes"*

Peterson sees great similarity between the paradigm emerging out of the "economics of Keynes" and institutionalism. In particular, he notes that "Both seek answers to capitalism's most puzzling question . . . 'Why do we, now and again, have hard times and unemployment in the midst of excellent resources, high efficiency, and plenty of unmet wants?' . . . the 'paradox of poverty in the midst of plenty'" [Peterson, 1977, page 214]. At bottom, Peterson describes the "Economics of Keynes" as an "emerging" paradigm, "more in the nature of an interconnected series of insights and attitudes which pertain to the functioning of the economy, most of which have their inspiration in ideas found in *The General Theory* but largely neglected by conventional analysis" [Peterson, 1977, page 214-215]. Peterson enumerates the "crucial elements" of the economics of Keynes -- all of which Peterson finds closely tied to institutionalism [Peterson, 1977, page 215].

*Pervasive Uncertainty in the Economy.* "Uncertainty pervades economic life because the economy exists in real time, because it cannot be separated from history" [Peterson, 1977, page 215].

*An Inherently Unstable Economy in Persistent Disequilibrium.* "Instead of equilibrium, *disequilibrium* is the normal state of affairs. The economy is always in motion, but most of the time it moves erratically, not tending toward a state of rest or balance of forces. Stability, it has been suggested, may be an unattainable goal, since at any particular time the state in which the economy finds itself -- boom, crisis, depression, or expansion -- carries the seeds of its own destruction" [Peterson, 1977, page 216].

*Irrational and Erratic Human Behavior.* "The economic behavior of human beings is mostly irrational and erratic; therefore, it is not easy to predict. Probably the best course lies in gaining better knowledge and understanding of the institutions which shape and direct behavior" [Peterson, 1977, page 216].

*Money is not Neutral, but Instead Contributes to Economic Instability.* "In Keynes's view, the institutional peculiarities of money are the source of the problem. Money is not like other commodities. It does not obey the 'normal laws' of the market, increasing in supply when the demand for it increases or having other things substitute for it when its price (the rate of interest) goes up. Thus money is not neutral. Its special characteristics and unique role in lulling disquietude about the future are the key to understanding the fundamental flaw in capitalism, the inherent and inescapable tendency of the system toward instability and excessive unemployment" [Peterson, 1977, page 217].

Eichner and Kregel describe post Keynesianism as "a new paradigm" "in Thomas Kuhn's sense."<sup>215</sup> Like Peterson's account of the paradigm of the "economics of Keynes," Eichner and Kregel include uncertainty in their specification of the post Keynesian paradigm: Post Keynesianism assumes an uncertain and unknown future. Echoing -- though perhaps faintly -- Peterson's notion that the paradigm conceives of the economy as unstable, Eichner and Kregel maintain that the theory "assumes pronounced cyclical pattern on top of a clearly discernible secular growth rate," as one of four central elements of the Post Keynesian paradigm. In contrast to Peterson, who asserts that the paradox of poverty which Keynes posited remains to this day unanswered, Eichner and Kregel include as another element of the paradigm the understanding that "institutional factors," along with growth rate changes determine income distribution. Eichner and Kregel's specification of the Post-Keynesian paradigm includes three additional elements: (1) The criteria that for economic analysis to be considered complete, "Discretionary income must be equal to

discretionary expenditures;" (2) The paradigm's "microeconomic base," "Imperfect markets with significant monopolistic elements," and (3) The post-Keynesian conception of the purpose of economics: "to explain the real world as observed empirically" [Eichner and Kregel, 1975, page 1309].

Cornwall's specification of the post Keynesian paradigm resembles Eichner and Kregel's in many ways. He, like Eichner and Kregel, includes the understanding that the economy is both riddled by fundamental uncertainty, which defies reduction to "certainty equivalents," and is dominated by monopolistic factors in his description of the post Keynesian paradigm. Similar to Eichner and Kregel's inclusion of the notion that business cycles, Cornwall maintains that the post Keynesian paradigm does not assume that full employment of all factors of production is continuously maintained. To these notions, Cornwall adds the appreciation that changes in flows and stocks play a central role in directing economic activity.<sup>216</sup>

#### 8. *Institutionalism*

Peterson concedes that the boundaries of institutionalism are fuzzy and that it lacks a common theoretical framework. Nonetheless, he maintains that institutionalism does constitute a paradigm.<sup>217</sup> He cites four fundamental elements of the paradigm. Two methodological dictums: to make the value presumptions that inevitably find their way into economic analysis explicit,<sup>218</sup> and to utilize induction and deduction in order to develop knowledge "needed for solving the practical problems of this world."<sup>219</sup> A conception of the operation of the economy: "the economic process as a dynamic, open-system, a part of a complex network of sociocultural relationships." And, finally, an understanding of human behavior: "collective (or social) rather than an atomistic (or individualistic) view" [Peterson, 1977, page 203].

Zinam specifies the institutionalist paradigm along many of the same lines as Peterson. Like Peterson's inclusion of the conception of humans acting collectively,

rather than individually, Zinam cites as one element of institutionalism, a "broad view of man as a complex sociopolitico-economic creature." Zinam specifies as the scope of the paradigm the understanding that the "economic aspect of behavior [is] inseparable from socio political factors." Further, Zinam, like Peterson includes in his specification of the paradigm a call for economists to seek to solve pressing social problems. The "nature and purpose" of the institutionalist paradigm, according to Zinam, is the "development of institutions devised by mankind to deal with the central economic problem." However, while Peterson specifies the paradigm's method as a blend of the inductive and deductive, Zinam underlines in his description the centrality of inductive methods: "historical and empirical studies." Zinam adds to his description of the institutionalist paradigm its conception of society as "highly organized, subject to both cooperation and conflict," and its treatment of ideas and preferences (both of which it takes as subject to study). In addition, he includes the primary importance the paradigm assigns to the study of institutions and the stress it lays upon "organization and power" as "strategic variables." Finally, as he does with all the paradigms he describes, Zinam lists as a final element of the institutionalist paradigm his assessment of its relevance: "Studies of institutions, organization, power, idea-systems, etc. contribute to broader relevance and significance of the theories" [Zinam 1978, page 171].

Akin to the prior two economist's inclusion of the solution of social problems as the institutionalist paradigm's goal, Miller "at bottom" specifies the "instrumental paradigm" governing institutionalism as seeking to determine "whether or not the operation of the economy works to the benefit of its participants, for the improvement of the human condition, and not whether it fits a pre-determined pattern" [Miller, 1991, page 1001].<sup>220</sup>

Klein justifies institutionalism as a paradigm by appealing to both senses in which, according to him, Kuhn defined the term. As an "entire constellation of beliefs,

values, techniques, and so on shared by the members of a given community," institutionalists may be said to have a paradigm in that they "have as much of this constellation in common as any of the other acknowledged schools of thought." As "concrete puzzle-solutions which, employed as models or examples, [which] can replace explicit rules as a basis of the solution of the remaining puzzles of normal science," Klein notes that we may contrast institutionalism with neoclassical economics not simply in terms of the answers it offers, but problems it posits as well. In this light, the paradigm may be described by noting that "Institutionalists, on the other hand, regard the market as a mechanism along with others that must themselves be judged in light of emergent societal values" [Klein, 1990, page 385].<sup>221,222</sup> Here, Klein resonates with Miller's characterization of the school's paradigm.

Both Kapp and Gruchy describe an institutionalist paradigm as one that regards the economy, not as at or tending toward equilibrium, but instead as an "ongoing process" in a constant state of flux:

In this sense, we believe that it is justified to regard the principle of interlocking circular interdependencies within a process of cumulative causation as the "disciplinary matrix" which provides institutional economists with a new tool for the identification and ordering of the relevant elements in the study of socio-economic processes in their immensely diversified and changing complexity. [Kapp, 1976, page 220]

The central significance of the principle of circular interdependencies and cumulative causation derives from the fact that it abandons and, in fact explicitly rejects the notion of stable equilibrium as a misleading and unwarranted analogy to mechanics. [Kapp, 1976, page 222]

All the fundamentals of institutional economics flow from the paradigmatic concept of the economic system as an ongoing, developing process. . . . What is unique about the economic system when it is viewed as an ongoing cultural process is that it is an open system that has no predetermined ends or goals. [Gruchy, 1986, page 807]

A process is an entity or thing having a development marked by gradual or rapid change, and a structure and functioning that are irreversibly changed over historical time. It stands in marked contrast to an equilibrium having basic structure and functioning that do not change irreversibly over time. [Gruchy, 1986, page 807]

While Kapp does not identify the solution of social problems as an institutionalist paradigm, he does stress that the *significance* of the paradigm lies in its utility in addressing vexing and important social concerns:

More than this, the principle enables institutionalists (and other social scientists) to transform problematical situations and unsolved open problems (as for instance increasing disparities within and between "rich" and "poor" regions) into "puzzles" which can be solved even when a complete theory and the precise knowledge as to the "coefficients of interaction" are not (yet) available. [Kapp, 1976, page 220]

The preceding economists identify a single unifying paradigm within institutionalism. Ramstad, however, argues that institutionalism consists not of a single unified paradigm, but of two conflicting ones: the Instrumental Value Paradigm, born of the economics of Veblen and Ayres, and the Reasonable Value Paradigm, emerging out of Commons' work.<sup>223</sup> The two paradigms butt heads both in terms of the criteria of social value they adopt (i.e., what is valuable and worth promoting) and the analytic tools they employ. Veblen sought to use "instrumental value," derived from "scientific inquiry" as the "valid criteria of social value" and eschewed the role of subjective evaluation as a means to determine what is valuable [Ramstad, 1989, page 766]. Commons, on the other hand, rejected the instrumental measure of value and, instead, "plainly found individual subjective desires -- the purposes of individuals -- to be the proper source of determining what the 'character of output' ought to be" [Ramstad, 1989, page 768]. Similarly, Commons rejected as unhelpful the "instrumental value-ceremonial value distinction," "the cornerstone of the Instrumental Value Paradigm," as a tool of analysis [Ramstad, 1989, page 768].<sup>224</sup>

All but one of the heterodox economists (all within or sympathetic to the institutionalism camp) discussed in this section locate a single centrally prominent paradigm in institutionalism. The other (Ramstad) finds two. The mainstream economists Ekelund and Hébert, however, cast serious doubt upon the existence of an



institutionalist paradigm. Ironically, given the foregoing, the two cite attitudes within the institutionalist school itself as support:

The fate of a *pure* institutionalist paradigm remains somewhat uncertain. In the first place no one, *not even a self-proclaimed institutionalist*, pretends to identify a single, cohesive, and consistent body of thought. Should one identify the "system" of Veblen, or some combination of the writings of Veblen, Commons, Mitchell, and Ayres as the foundation for a school of neoinstitutionalism? [Ekelund and Hébert, 1983, page 424, emphasis added]

Johnson and Ley also describe institutionalism (evolutionary economics) as a "nonparadigmatic" school of economics. They, however, consign institutionalism to this status because it lies outside economics' orthodoxy.<sup>225</sup> They provide no indication that institutionalism lacks a set of definable characteristics. Indeed, they highlight two discernible elements of institutionalism, both of which set it apart from mainstream economics. (1) In opposition to neoclassical economics' faith in market forces, institutionalists express serious doubts as to whether the market alone can fairly and effectively adjudicate among conflicting parties in society, and they affirm the need for government intervention.<sup>226</sup> (2) In contrast to mainstream economics' frequent use of the deductive method, institutionalists champion greater utilization of inductive methods [Johnson and Ley, 1990, pages 145-146].<sup>227</sup>

Numerous economists discuss the existence and characteristics of an institutionalist paradigm. Here, we have discussed nine. Of those, most do locate a paradigm within the school; one finds two, and two, none. Some detail a multi-dimensional entity; others focus upon only one or two notions in their description. We do find some (relatively) common threads across the paradigms found, notably, numerous variants of the dictum that economic practices and institutions need to be evaluated on the basis of their social worth, and, though to a lesser extent, a dynamic historical conception of the economy. However not all of the descriptions include these elements and, we cannot overlook differences among the variants. Indeed, as we saw, Ramstad finds that the two paradigms within institutionalism both presume the need to

evaluate social worth. According to him, however, the two paradigms fundamentally disagree as to how such worth should be determined and thus must be considered separate, even conflicting, paradigms.

## 9. *Summary*

The prior discussion of economists' perceptions of an institutionalist paradigm is illustrative of our findings as to economists' discussions of paradigms in economics' heterodoxy in general. As with economists' discussions of institutionalism, we find disagreement among economists as to whether certain heterodox schools possess/comprise an economics paradigm. While Zweig and Sweezy clearly identify a radical economics' paradigm, Solow and Zinam question its existence. Further, to those calling into question the existence of a particular heterodox paradigm, we may add Black who questions the notion that Political Economy comprises a single paradigm, and Zinam and Heilbroner, who both question the adequacy (if not the existence) of the Marxian paradigm.

The three major reasons which economists forward for the non-existence of a given heterodox paradigm all bear upon the "paradigm's" relationship to the economics community: (1) the lack of consensus among members within a given heterodox school around a common set of notions that would comprise a paradigm (Black, Ekelund and Hébert), (2) the inability of the school's "paradigm" to guide (or even provide) its members a coherent line of scientific inquiry (Solow), (3) its heterodox status (Johnson and Ley).

Further, we find that while economists' depictions of a given heterodox school's paradigm overlap, the overlap is rarely complete. We have already argued this point with respect to institutionalism. We find the same true of economists' portrayals of the post-Keynesian and Marxist paradigms. Most descriptions of the post-Keynesian ("Economics of Keynes") paradigm include the recognition of the important role

uncertainty plays in the economy and the realization that the economy does not automatically gravitate toward full-employment. Cornwall alone, however, includes explicit recognition of the importance of flows and stocks in his specification of the post Keynesian paradigm; only Eichner and Kregel include the methodological dictum that for a theorist's analysis to be considered complete, discretionary income and spending must be equal to one another, and Peterson in his specification of the "Economics of Keynes" paradigm -- unlike Cornwall and Eichner and Kregel's depiction of the post-Keynesian paradigm -- speaks not simply of cyclical, but secular, instability. Similarly, Zinam includes a host of elements in his specification of the Marxist paradigm which Zweig does not.

#### I. CURRENT-DAY SUB-FIELD PARADIGMS

In addition to identifying numerous orthodox and heterodox paradigms' spanning the discipline, economists have also located a number of paradigms within many of economics' subfields. The present research found economists' pointing to paradigms in almost all of the major groupings laid out in the *Journal of Economic Literature*. In particular, we find economists specifying paradigms under all but five of the headings: general economics and teaching; international economics;<sup>228</sup> health, education, and welfare; law and economics; economic history; and economic systems.<sup>229</sup>

##### 1. *Methodology and History of Economic Thought*

Erich Streissler regards Kuhn's own theory of scientific revolutions as the preferred paradigm for the study of the history of economic thought.<sup>230</sup> In addition, the "Natural Sciences Paradigm" and value free paradigms both touch directly upon matters of economic methodology.

## 2. *Mathematical and Quantitative Methods*

Wang identifies the "econometric" paradigm, which he allies with "an econometric approach in economic teaching and research" [Wang, 1973, page 151]. Still further, while acknowledging that "there have been many definitions of econometrics," Wang cites as its "predominant feature" "its emphasis on the empirical verification or falsification of economic theory and, for this very reason, the formulation of theory in mathematical terms" [Wang, 1973, page 156].

## 3. *Microeconomics and Macroeconomics and Monetary Economics*

Most of the discipline-wide paradigms identified earlier fit well under one or both of these headings (e.g., general equilibrium, the neoclassical synthesis, maximization, and equilibrium).

In addition, within the sub-realm of "information and uncertainty," we find that Bøhren suggests that "stochastic choice theory" may be regarded as a single paradigm containing at least two research programmes (expected utility and non-expected utility) [Bøhren, 1990, page 26], which, while different, possess a number of commonalities, namely:

--The relevant aspects of choice can be captured by and decomposed into a set of actions, states, probabilities, outcomes, and preferences;

--Given the preferences, the decision maker is doing her or his best in terms of maximizing the level of satisfaction;

--The decision maker is consistent, has a complete ordering, and appreciates high probabilities of attractive outcomes;

--The decision maker has unlimited intellectual capacity in the sense that every relevant aspect of the decision problem is clearly perceived. [Bøhren, 1990, page 20]

## 4. *Financial Economics*

Michael Jensen cites the efficient market hypothesis as an economics paradigm<sup>231</sup> in that "EFM is accepted as a fact of life" and "scholars that model in violation to EFM face a very difficult task."<sup>232</sup>

### 5. *Public Economics*

Wagner and Weber see as a regnant paradigm of government behavior, the "perfectly competitive theory of government," which conceives of government as responding to constituents in much the same manner as a competitive market responds to consumers. As an alternative paradigm, Wagner and Weber proffer one which "holds that governments are able to some extent, to act in only partial subservience to the wishes of citizens because of various institutional peculiarities of government" [Wagner and Weber, 1977, page 66].<sup>233</sup>

### 6. *Labor and Demographic Economics*

Michael Piore, while not describing the labor segmentation thesis as a paradigm in its own right, does seek to find a paradigm which "recognizes and encompasses social, as opposed to individual, phenomena" as a home for the thesis. Rejecting the Marxist paradigm as the appropriate setting for the thesis because it "fails to provide a bridge between the individual and the social," Piore concludes that labor market segmentation belongs within the structuralist paradigm, whose "core" lays stress upon the genesis and development of the "interpretative framework," without which "human thought, indeed, human existence is impossible" [Piore, 1983, pages 252-253].<sup>234,235</sup>

### 7. *Industrial Organization*

Storper identifies the "product cycle" and the "oligopoly" paradigms [Storper, 1985, page 260]. Yakov Amihud and Haim Mendelson, though not directly linking Kuhn with the paradigm concept, describe the "administered prices" paradigm as contending, "following Means, . . . that industrial prices are hardly responsive to business conditions" [Amihud and Mendelson, 1983, page 87].<sup>236</sup> On the other hand, Boland explicitly rejects the notion that economics' standard theory of the firm (or any other standard theory) constitutes an economics *paradigm* because "we consider alternatives to the theory."<sup>237</sup>

8. *Business Administration and Business Economics; Marketing; Accounting*

Gordon Foxall identifies the cognitive information processing paradigm as the dominant paradigm in consumer research.<sup>238</sup> He cites four features of the paradigm:

The CIPP has the following features by which it conforms generally to the requirements of a scientific paradigm . . . (1) a recognisable philosophical foundation in which behaviour is explained as the outcome of intrapersonal factors under varying degrees of autonomous control; (2) a defined subject matter consisting of experience and consciousness as well as behaviour; and (3) a feasible methodology that rests particularly upon the statistical comparison of the means and proportions of groups of sample subjects. [Foxall, 1986, page 395]

9. *Economic Development, Technological Change, and Growth*

In development economics, we find the most extensive discussion of sub-discipline paradigms. While acknowledging that "It would seem then that mainstream Western development theory has been pre-paradigm in characteristic ways," Foster-Carter asserts, "Yet there is another sense in which all the various and apparently competing schools . . . might be said to share something like a paradigm" [Foster-Carter, 1976, page 172]. He then enumerates the common assumptions of the dominant "paradigm" in development economics:

that development was a non-contentious process, not involving irreconcilable conflicts of interest between developed and underdeveloped countries or between different social groups within the latter; that there was no structural connection between underdevelopment and development; that what was "modern" was good and what was "traditional" was bad, and that the two were unconnected (dualism); that development meant becoming more like the West. [Foster-Carter, 1976, page 172]

Foster-Carter further identifies Gunnar Frank's theory of underdevelopment as a real and viable alternative paradigm to the regnant development economics paradigm [Foster-Carter, 1976, page 175].<sup>239</sup> Similarly, while acknowledging that "No unified theory of dependency commands universal assent," John Browett concedes, "it does tend to be acknowledged that the works of Frank, Wallerstein and Amin are so sufficiently similar that they constitute and embrace something, termed a dependency

paradigm or perspective . . . ." [Browett, 1985, page 790]. Browett describes the central characteristic of the paradigm as

the notion that, as a result of dependency and unequal exchange, capitalist development in some places (the core or the metropolises) continuously and necessarily creates underdevelopment at other places (the periphery or the satellites) in the world capitalist system. [Browett, 1985, page 790]

Gabriel Palma does identify a "central nucleus around which the analysis of . . . [certain] dependency writers is organized:" "capitalism, in the context of dependency, loses its historical progressive character, and can only generate underdevelopment." He however maintains that due to the "purely and simplistically ideological position" "now or never" approach to "revolutionary struggle," dependency economists have "fail[ed] in their attempt to establish a new paradigm." Dependency economics remains, according to Palma, woefully underdeveloped [Palma, 1978, page 904].

Nathaniel Leff sees social benefit cost analysis as the paradigm dominating economists' understanding of how investment decisions should be made in lesser developed countries. The paradigm instructs that one should make investment decisions on a project by project basis and should choose investment projects "on the basis of their rates of social return" [Leff, 1985, page 336].

In the realm of growth theory, Cornwall defines the "neoclassical (growth) paradigm" as consisting of the assumptions of the overarching neoclassical paradigm, as well as the assumptions of "constant returns to scale in production, diminishing marginal products to all factors, and fixed savings propensities," and "the assumptions of exogenously determined rates of growth of the labor force and of technical progress. . . . ." [Cornwall, 1979, page 72].

#### 10. *Agricultural and Natural Resource Economics*

Stent characterizes the "dominant paradigm" in *Australian* agricultural economics as "best described as 'positive' and emanating from the 'Chicago School' . .

. which places little emphasis on the inductive but greatly stresses the deductive aspects of economics" [Stent, 1976, page 7]. Cohen and Lewis, on the other hand, point to the dominant paradigm in *international* agricultural economics: The work of Johnston, Kilby, Mellor and Lele [Cohen and Lewis, 1979, page 523]. While not spelling out the contents of the paradigm, Cohen and Lewis note that the four economists' work assumes that "rural families have low incomes and that rural poverty is a major problem" [Cohen and Lewis, 1979, page 526].

Within resource economics, Terry Anderson identifies the neoclassical paradigm as the guiding paradigm. He cites as its "central elements:" "marginal analysis," "information and uncertainty," and "interest theory" [T. Anderson, 1982, page 928]. Though not directly referring to Kuhn's paradigm notion, Alan Randall finds four paradigms coexisting within the sub-field: "institutionalist/land economics," "neoclassical/rational planning," "public choice/utilitarian," and "public choice/individualist" paradigms, each with different methodological orientations and distinct positions regarding how benefits and costs may be determined [Randall, 1985, pages 1022ff.].<sup>240</sup>

James Bird asserts that "central place theory" (CPT) has fulfilled the role of a dominating paradigm" in economic geography. The theory, according to Bird, assumes "that the system is closed; outside [are] influences not only ignored but actually wrongly included as part of the closed system" and "assumes that [the] service function of settlement can be correctly measured by available data (e.g. number of employees, sales, floor space, etc.)." Bird, as well, offers an alternative to CPT [Bird, 1983, page 201].

Jordan Louviere, without direct mention of Kuhn, speaks of the wide variety of paradigms available to the economic geographer, each of which provides a different understanding regarding the relationship between data and certain algebraic relationships: "social judgment theory," "conjoint measurement," "functional



measurement," and "the general area of axiomatic utility theory" [Louviere, 1981, page 308].

#### 11. *Urban, Rural, and Regional Economics*

Footnoting his use of "paradigm" to Kuhn, A.R. Banai-Kashani finds a number of paradigms in the economic study of urban household location. In particular, he proffers "Analytical Hierarchy Process" (AHP). AHP, unlike more the mechanistic paradigms he finds in the subfield, explicitly incorporates choice into its framework. The author gives the alternative paradigm rigorous mathematical specification [Banai-Kashani, 1984].<sup>241</sup>

Cedric Pugh, as well, seeks to outline the paradigms in urban sociology (Chicago School and various versions of Marxism) governing housing theory and policy [Pugh, 1986, pages 20-24].

#### 12. *Summary*

The foregoing provides strong indication that economists have found paradigms in virtually every corner of the discipline. Undoubtedly, a more exhaustive study than the present one would even locate instances of economists identifying paradigms in the five sub-fields for which we found no examples. In addition, our discussion here, in conjunction with our examination of discipline-wide economics paradigms, illustrates that economists have found paradigms at a variety of different levels in economics. Not only do economists identify an abundance of economics paradigms spanning the entire discipline, they also find many paradigms within various subsectors of the field of economics.

### J. THE FUNCTIONS OF ECONOMICS PARADIGMS

Economists identify a number of ways in which paradigms have influenced economics and economists. For the most part, economists find that economics paradigms limit, in one way or another, scope and/or perception. They differ,

however, as to whether they regard the limits which economics paradigms impose as beneficial (or detrimental) to the practice of economics.

1. *Insulate Economists from Important Social Problems*

First, many find that orthodox economics paradigms downplay the importance of social problems. Having quoted Kuhn directly regarding that ability of paradigms to insulate scientists from pressing social problems, Janet Seiz notes that the "primary theoretical method of neoclassical economics -- individual optimization and market equilibrium modeling limits our ability to understand gender relations." In particular, she cites neoclassical economics' conception of human nature (rationality), the stress it lays upon "individual choice" and "its inability to deal with power relations" as most problematical [Seiz, 1992, page 294]. She then explains how each of these three problems conduce to gender-biasedness and distort reality [Seiz, 1992, pages 294ff.]. Similarly, Worland explains the orthodoxy's neglect of important social problems (war, racism, the environment) as the product of the limiting influence of the regnant paradigm:

Thus, Kuhn's thesis would explain the "irrelevance" of orthodox economics if those critical problems which the standard analysis is accused over overlooking are specified as the phenomena "not seen at all," or "suppressed" because of restrictions implicit in the paradigm employed. Trying to fit social reality into the "preformed box" of the neoclassical paradigm, conventional economics cannot work a concern for war, racism, destruction of the environment and similar factors into its pattern of analysis. Thus it may be true, as the radicals say that orthodox economics "cannot deal with the important problems of modern society." The reason, Kuhn's analysis suggests, derives not from the ideological bias of economists, but from the restriction of the economist's vision caused by the paradigm employed. [Worland, 1972, page 275]

Heilbroner cites the restrictive powers of the natural sciences paradigm as one of the major reasons for mainstream's silence on political matters:

The difficulty, however, is that this paradigm, applied to the field of social problems, tends to rule out of bounds those kinds of issues that resist accurate measurement, or that lend themselves only awkwardly or

not at all to mathematical representation . . . In a word, it tends to rule out most "political" matters. [Heilbroner, 1971, page 3]<sup>242</sup>

Finally, as we noted earlier, O'Brien sees Ricardo's "paradigm" as insulating Classical economists from the troublesome questions surrounding the income tax.<sup>243</sup>

## 2. *Limit the Availability of Data*

Mainstream economics paradigms, others maintain, limit what data is and is not available to economists. Dale Poirier, for example, holds the "bourgeois paradigm" partly accountable for the lack of certain corporate and labor force participation data:

However, even more important than the quality of data is its existence, and as a second possible explanation for radicals' apparent adverseness toward econometrics, whether the bourgeois paradigm has had an effect on the availability of data. Clearly, the national income accounts are direct outgrowths of the Keynes revolution, and the privacy of much corporate data is, in part, the result of the "competitively vulnerable" image of the firm in neoclassical theory. More subtly, however, Duncan Foley . . . has contended that in the ideological sphere pre-existing ideologies like racism and sexism are adopted, shaped and reproduced by bourgeois society. Thus, for example, sexism may ultimately affect the data which are available. In particular, Morley Gunderson has noted that the researcher working on labor force participation may find that data on the number of children has been collected for a sample of women, but not for men, presumably out of a preconceived notion of sexual roles. [Poirier, 1977, pages 395-396]

## 3. *Determine/Restrict What Questions are Asked*

Argyrous explains how Friedman's work on the Permanent Income Hypothesis as exemplar paradigm laid out a broad research agenda for economists. It provided a number of questions to be answered, a number of problems to be solved. For example: Was the marginal propensity of consumption of transitory income, as the hypothesis maintained, equal to or close to zero? What consumption should be included in permanent consumption? What were the policy implications of the PIH [Argyrous, 1992, pages 236-241]?

As we noted Joskow points out the neoclassical paradigm restricts -- as does any paradigm -- the questions that can be raised.<sup>244</sup> We noted that Coats saw Keynes' theory as approximating a Kuhnian paradigm in that it "provided his professional

colleagues not only with a new 'map, but also with some of the directions essential for map-making'" [Coats, 1969, pages 293-294, quoting Kuhn, 1962, page 108].<sup>245</sup>

#### 4. *Influence How Economists Model/Understand the Economy*

Several economists' descriptions of paradigms in their field constitute and/or presume a worldview. Many, see that the mainstream paradigm conceives of the economy as harmonious and in or tending to equilibrium, while many of the heterodox paradigms view the economy as riddled with conflict and dynamic.

As we also noted, Wagner and Weber note how the regnant competitive market theory of government channels our interpretation of the data concerning government growth [Wagner and Weber, 1977, pages 66-67]. Likewise, Joskow notes how it is that the neoclassical paradigm limits -- as, indeed, all paradigms must -- our understanding of the regulatory process [Joskow, 1973, page 134]. And, as we will see, Argyrous find that economic paradigms shape an economics student's view of the world.<sup>246</sup>

Routh finds that the mainstream paradigm conditions the way in which economists econometrically model the economy. He finds that the regnant paradigm excludes human beings (in any meaningful sense) from its models -- including humans only to take account of differences between actual and predicted outcomes:

In orthodox econometrics, the paradigm postulates a set of relationships between economic variables from which man may be excluded, because it is assumed that he himself reacts in a determinate way to economic stimuli of a defined type and magnitude. When he is admitted, it is as a disturbing influence used to explain deviations between performance and expectation -- "trade union pushfulness" or "incomes policy on." [Routh, 1973, pages 182-183]

Likewise, Poirier points out that economic paradigms determine what variables will be taken as given and which modelled within an econometric system:

The important distinction between the statistician and the econometrician is that the latter employs her or his statistical tools to the analysis of *economic* models. These economic models are the products of economic paradigms, and these paradigms serve as the bases

for which endogenous versus exogenous classification and identifying restrictions are made. [Poirier, 1977, page 393]

##### 5. *Distort/Block Economists' Perception of Reality*

More than simply conditioning economists' understanding of the economic reality, many economists contend that regnant economics paradigms *distort* economists' perceptions of economic reality. According to Leff, the ruling economic paradigm in LDC investment (social benefit cost analysis) has kept researchers from seeing the realities as to how investment decisions are actually made in third-world countries. The approach which prescribes that investment choices be made on a project by project basis has kept economists from seeing that most investment decisions in these nations are made across sectors, not individual projects. The "paradigm block" has not been without consequences. In particular, economists' failure to recognize the reality and thus failure to formulate any theoretical framework for the intersectoral approach has left practitioners without a much needed analytical basis [Leff, 1985, page 354-355].

Douglas Vickers finds that economics' general equilibrium paradigm seeks to "abolish" the "realities of ignorance," i.e., that individuals must and do make decisions and take action in the light of a future that is "unknowable" [Vickers, 1983, page 254].<sup>247</sup>

Swaney goes still further. The regnant neoclassical paradigm so dominates the economics profession that it actively blocks *any* empirical investigation. The result is that modern day economics has a grossly inaccurate understanding of the economy and the role of people within it:

the dominant neoclassical paradigm is such a complete and dominating world view that it inhibits inductive development of theories that reflect economic reality. In short, economists all too frequently leap from their oversimplified view of the nature of man and their naive view of the institutional framework as a God-given constant to elegant and sophisticated theories void of constructive policy content. [Swaney and Premus, 1982, page 726]

Similarly, Storper describes the "essentialist" product cycle and oligopoly paradigms as excluding from an economist's ken "more complex forms of explanation of economic geography." By acting as screening devices, the paradigms leave economists with only a narrow focus of economic reality (that which can be modelled mechanically). Storper directly links the distortive limitations imposed by these paradigms directly to Kuhn:

This point is hardly new, having been made forcefully by Kuhn with respect to the natural sciences and subsequently articulated over the debates in the history of science and the structure of ideology in social science. [Storper, 1985, page 261]

#### 6. *Foster Progress in Economics*

According to Stanfield, paradigm discipline (i.e., adherence to a paradigm's precepts) is essential to providing practitioners within a school of economics the focus necessary to foster the school's progress.<sup>248</sup> He finds such discipline extant within the neoclassical and Marxist economics and bemoans its absence in the institutionalist and Austrian schools [Stanfield, 1989, pages 177-78].

#### 7. *Inhibit Progress and Forestall Innovation*

At the same time, Stanfield concedes that a paradigm's hold may become so extreme so as to forestall (not facilitate) progress:

I should add that arrested development within a paradigm can occur for reasons that are the very opposite of indiscipline: dogmatic discipline. Where a paradigm is well-insulated from anomalous happenstance and competing perspectives, ossification can generate arrested or distorted development. [Stanfield, 1989, page 178, note 2]

Solo, in many ways, mirrors Stanfield's position regarding the role of paradigms in fostering progress in economics. While Stanfield lays emphasis upon how "paradigm discipline" fosters progress, Solo stresses that the "rule of paradigm"<sup>249</sup> is so absolute in economics that it has served to frustrate efforts seeking to innovate the field:

The problem of the rule is that it blocks the entry of all significant novelty, excluding the possibility of scientific revolution, throwing up a barrier against any substantive transformation and development of a science from within. Given the effective application of the rule, the discipline cannot but go on being what it has been. [Solo, 1991, page 80]

Similarly, while Stanfield places his concession that paradigm discipline may devolve into unhealthy dogmatism in a footnote, Solo downplays his own acknowledgment that the "rule of paradigm" may play a beneficial role in economics:

On the other hand it needs to be recognized that the rule has a positive value. It assures the coherence and continuity of a discourse spread out in space and time and drawing into itself participants from a variety of cultures. It fosters the transgenerational learning from a shared body of thought with a stable system of signs essential for effective communication, so that participants speak together in a common tongue and from the same reference base. Thereby it protects the integrity of the discipline and the coherence of the enterprise from the possibly shattering effect of a random introduction of novelty. [Solo, 1991, page 80]

Economists see the dominance of economics paradigms as retarding the subject's progress by closing out the publication of dissenting articles and by foreclosing an economist's imagination:

But I have been forced to acknowledge that in certain fields, like nutrition policy, the paradigm and its establishment can seriously retard the progress of scientific investigations.

A concrete example is provided by the remainder of this paper. Over the past two years, the *Bulletin of the Nutritional Foundation of India (BNFI)* has published a series of criticisms of Sukhatme, Margen, and me . . . Of course this is good and desirable, as science develops through critical exchanges. However, much to my astonishment, the *BNFI* has refused to publish my, or any other, reply to these criticisms. This policy reduces the *BNFI* to nothing more than a house organ for a particular paradigm and establishment in nutrition that has no claim to serious consideration in scientific circles. [Seckler, 1984, page 1886]

Dewald, Thursby and Anderson, who stress the importance of replication in the advancement of knowledge, point out that social science paradigms fail to reward and, actually discourage, the replication of other's work [Dewald, Thursby and Anderson, 1986, page 587]. The fact that paradigms exercise such an influence takes on perhaps a more serious tone given that Dewald et. al., seeking themselves to replicate a number

of different economic studies, are unable to reproduce those studies' results in almost every case [Dewald, Thursby and Anderson, 1986].

Economists also identify instances in the history of economics in which an economics paradigm has served to forestall (at least for a time) advances in the field. The strong hold of the "purely competitive" paradigm, according to Reinwald, explains Chamberlin's initial reluctance to abandon the supply curve (which played a central role in the reigning paradigm) from his analysis of monopoly. Given that he and his advisor were both trained in the paradigm, it is not surprising that Chamberlin did not immediately dispense with the supply curve (which he identified with the average cost curve) [Reinwald, 1977, pages 526-527]. In short, "The omnipresence of received doctrine [the orthodox paradigm] was evidently too much to be overcome in one fell swoop, particularly when it concerned such a fundamental concept as supply" [Reinwald, 1977, page 527]. The paradigm's hold on Chamberlin, however, was not so absolute. By 1933, he had abandoned completely the supply curve from monopoly theory. Still further, Reinwald contends that had Chamberlin had the marginal revenue concept available to him six years earlier, he would have jettisoned the notion of a supply curve at that time [Reinwald, 1977, page 528].<sup>250</sup>

Likewise, Petridis links the strong influence which the paradigm of perfect competition exercised over Marshall to the neoclassical economist's resistance to incorporate trade unions into his labor market analysis.<sup>251</sup> In sum, Marshall, seeing basic incompatibilities between trade union (monopolies) and competition, hesitated to introduce unions into his theoretical framework:

At the time Marshall was writing, competition was the dominant paradigm in economics, and a pervasive influence over all of Marshall's writing. It will be seen that Marshall's uncertain, ambivalent, sometimes contradictory attitude as well as his analytical treatment of trade unions may have been, at least in part, a reflection of his awareness of and attempt to reconcile the anomalies created by the presence of trade unions in an (assumed) competitive system. [Petridis, 1973, page 166]



Instead of revising theory to incorporate unions, Marshall retained the dominant competitive framework while at the same time moving almost seamlessly in his discussion from the "competitive world into one in which there were monopoly elements, such as trade unions or employers' organizations" in order to give "his writing the air of realism he desired" [Petridis, 1973, page 195].

#### 8. *Train and Indoctrinate Economists*

Paradigms *qua* exemplars, Argyrous notes, play a central role in training economists. They do so by teaching by example. Students are introduced to key economics notions not by being presented with an explicit list of assumptions and economic theories; they instead learn by repeatedly being exposed to concrete examples (exemplars) of those principles and theories in action. They do not, for instance, learn the Keynesian multiplier by being taught the economic theory or algebra behind the multiplier. They instead learn by being presented and working through for themselves examples in which, for example, a first person spends 100 dollars on a second person's goods, who, in turn spends 80 of the 100 dollars he received on a third person's services, who in turn . . . [Argyrous, 1992, page 235]. The paradigm makes sense of the abstract theory. Students come to learn the formal theory and method associated with the exemplars only after long exposure to the profession's exemplars -- both by being presented the examples as well as being charged to work out problems based upon those examples. The long-term and frequent exposure to the exemplars leaves the student with a sizeable investment whose returns the economist is loathe to sacrifice by abandoning the precepts. Still more, the work with the paradigms shapes the economist's view of the world:

It is only at the advanced graduate school level that students come to grapple with assumptions and conditions needed to support the paradigms, which they have already acquired. By that stage, however, they have already invested time and money into the acquisition of this body of knowledge; coming to terms with these assumptions and conditions, therefore, rarely involves questioning the paradigms with

which they are associated. . . . More importantly, the student gradually comes to "see" what previously may have seemed totally unrelated situations as examples of essentially the same (maximization) problem. Thus, the world view itself forms around these simple paradigms. [Argyrous, 1992, page 235]

## 9. *Summary*

Our findings regarding economists' understandings of the function of economics' paradigms very closely parallel our findings as to their understanding of the functions which Kuhnian paradigms play in general. Economists see economics paradigms channeling economic inquiry: by insulating economists from important social problems, limiting the data available to them, determining/restricting what questions economists should answer, by influencing how economists model/understand the economy. Further, just as we saw that economists' attitudes toward the limits which paradigms imposed varied, we find that economists disagree as to whether the restrictions economic paradigms place upon economists' activity/perception help or hinder them in their work. While some economists see economic paradigms as fostering progress in economics, others find them inhibiting progress in economics and forestalling innovation.

## K. SUMMARY AND CONCLUSIONS

The present chapter provides compelling testimony that Kuhn's paradigm concept has been subject to multiple interpretations in the economics literature. If Masterman located at least twenty-one different ways in which Kuhn employed the term in the *The Structure of Scientific Revolutions*, we here find over ten different ways in which economists have understood a Kuhnian paradigm: "universally recognized achievements;" worldview (understood as a conception of "reality"); worldview (understood as a means by which to view the "world;" not a worldview; an eclectic collection of worldview, method, values, theory, etc.; a disciplinary matrix; an exemplar; examples of actual scientific practice; a methodological research programme

(or the programme's hard core); a dialectical thesis; a theory; a set of theories; a pre-theoretical entity; a set of rules; or not a set of rules. That economists have understood the paradigm concept in a variety of different ways is further underlined by the different types of paradigms they locate in economics. For example, some economists identify economics paradigms which constitute worldviews, while others identify economics paradigms which comprise eclectic collections of worldview, theory, etc. Even though referring to the same term -- and for the most part, to one philosopher's understanding of it -- economists have not meant the same thing when employing the term paradigm.

Nor, as we have seen, have economists meant the same thing when referring to economics' mainstream paradigm. Kenneth Boulding has observed that, "Neoclassical economics is a very fuzzy set. Almost everyone draws his own boundaries around it, and the boundaries are determined by what we like or do not like" [Boulding, 1975, page 223]. The present research suggests that Boulding's observation applies as well to economists' specifications of economics mainstream paradigms. Owing to the ill-defined nature of "mainstream" economics and economists' selective perception of that "mainstream," we find that economists have identified a host of different mainstream paradigms: the maximization assumption; equilibrium conception/assumption; harmony and equilibrium; neo-classical synthesis; "value free" economics paradigm; neo-Keynesian paradigm; neoclassical paradigm; and certain exemplars. Still further, we find indication of economists' selective perception of economics' "mainstream" in their varied specifications of nominally the same paradigm. Finally, we find that economists widely vary as to the paradigmatic status they lend to a particular notion, theory or methodology in economics. What some identify as deserving to be called a paradigm in its own right (e.g., maximization), others lend less importance to it -- either not including that notion in their specification of paradigm at all, or including the notion as only one among several components of mainstream economics' paradigm. Finally,

what some conceive of as a single economics paradigm, others regard as two or more. Thus, economists, in applying the paradigm concept to their examination of economics' mainstream, have variously interpreted economics mainstream.

We also find indication of economists' selective perception of heterodox economics paradigms. While some argue that a particular heterodox school possesses/constitutes a paradigm, others argue it does not. Further, we also find varied specifications of a given school's paradigm. Although there is overlap in these specifications, the overlap is not complete.

Finally, our discussions of discipline-wide mainstream and heterodox paradigms, along with that of paradigms found in economics' subfields, demonstrates that economists have found paradigms throughout the discipline and at varying levels of generality.

As to the function of paradigms, economists have identified a number of different ways in which both paradigms, in general, and paradigms in economics, in particular, have channeled scientific activity. In particular, we may note that economists see (economics) paradigms as guiding scientists (economists) in their work by placing restrictions upon (1) the choice of problems and questions to be addressed, (2) the selection of means by which to pursue solutions and answers, (3) acceptable answers and solutions, and (4) the interpretation, selection and perception of "facts." Economists have disagreed as to whether in general or in economics in particular, these strictures advance the cause of science (economics).

As to whether Kuhn employed "paradigm" ambiguously economists are in relative agreement: he did. They, however, depart from one another as to whether that ambiguity presents a problem to the application of the notion to economics. Also with respect to Kuhn's applicability, economists have disagreed as to whether various other considerations (the differences between the natural and social sciences, the role of social factors in economics) impair, enhance, or make little difference to the application

of Kuhn's schema to economics. Economists also disagree as to whether Kuhn's paradigm concept clarifies or distorts one's understanding of economics and whether or not the paradigm concept provides economists a useful rhetorical device.

The economists examined here all interpret and apply the same term, "paradigm." Further, they evaluate the applicability and applying the term to the "same" discipline, "economics." However, as the foregoing demonstrates, they forward (1) a multitude of different interpretations of the term, (2) divergent assessments as to Kuhn's applicability to economics, (3) identify a variety of different mainstream (and heterodox) paradigms, and (4) specify nominally the same paradigm in wide variety of different ways. In short, economists have forwarded multiple and selective interpretations of both Kuhn's notion of paradigm as well as the field of economics itself. While employing the same terminology ("paradigm," "economics," "mainstream," "institutionalism," etc.) they have been talking about very different things.

## NOTES

1. The present analysis, for the most part, limits selection of economists' descriptions of "paradigm" to those in which Kuhn is explicitly linked with the author's characterization and/or use of the term paradigm. Paradigm descriptions in articles which did not mention Kuhn were, in the main, not considered. In addition, paradigm descriptions in articles in which Kuhn was discussed/referred to, but in which the author did not refer to Kuhn in characterizing a paradigm were also largely excluded. There were at least two reasons for adopting such a selection method:

First, manageability. Given the broad currency of "paradigm" in the economics literature, the scope of articles had to be narrowed in order to make the project tractable.

Second, interpretive reasons. Even though use of the term paradigm exploded after Kuhn's publication of *The Structure of Scientific Revolutions*, the term pre-dates Kuhn. Eliminating those articles in which Kuhn is not directly mentioned with regard to the paradigm concept ensures that the author is not simply referring to the term in its traditional dictionary sense. Further, the selection criterion helps ensure that an economist's description of "paradigm" is, at least to some degree, based upon a primary interpretation of Kuhn's use and description of "paradigm" (i.e., an interpretation predicated upon one's own reading of Kuhn's text) -- and not a secondary interpretation (an interpretation of Kuhn predicated upon one's interpretation of others' interpretations of Kuhn's text) or even a tertiary interpretation of Kuhn's understanding of the concept (an interpretation predicated upon one's interpretation of secondary interpretations of Kuhn).

For much the same reason, discussion later in this chapter identifying the different paradigms identified in economics will lay emphasis upon those paradigms identified by economists who explicitly ally Kuhn with the paradigm concept. We do consider some interpretations in which there is no direct explicit link made between paradigms and Kuhn. In those cases, we make note that no such link is made. We still do, however, almost exclusively limit our discussion to those articles which cite Kuhn.

Taking such a tact, is, of course, still fraught with interpretive burrs. First, simply because an economist associates the paradigm concept explicitly with Kuhn (even to the extent of asserting that, "Kuhn defined 'paradigm' as . . .") should not be mistakenly taken as an exclusively primary interpretation. The definition allied with Kuhn is most likely also the product of an author's reading of others' interpretations of Kuhn's paradigm concept as well. Thus, what comes across as a primary interpretation of Kuhn's paradigm concept, may, in actuality, be the product of the intermingling of primary and secondary interpretations. We may also find that the interpretation is colored by an interpretation of another interpreting the concept. Thus, the interpretation becomes the product of a primary, secondary and tertiary interpretation.

Still further, an author who asserts Kuhn's definition of paradigm may not at all be the product of primary interpretation. The author may have not read any of *The Structure of Scientific Revolutions* except for the passage which his reading of the secondary literature led him to. Given, as we shall see, that many of the interpretations are held by a large number of different economists, attribution of a particular definition to a particular economist may be difficult and unnecessary.

Finally, we confront a more fundamental interpretive difficulty. An interpreter works within an intertwined matrix of interpretations, such that it is impossible to extract the object of interpretation from the interpretation itself. No absolute point of reference exists from which one may work. One cannot separate the object being interpreted from the interpretations themselves. In the present case, we thus confront a highly entangled matrix of interpretations of Kuhn's theories and concepts.

In sum, the lines separating primary, secondary and tertiary interpretation are not well defined. Even in the restricted sample examined here, a number of economists' understandings of Kuhn's paradigm concept are a mix of primary and secondary interpretation. Seeking to limit consideration, as much as practicable, to primary interpretations sharpens the interpretive focus and provides the most powerful test of the hypothesis that economists broadly interpret the paradigm concept.

2. Gordon's 1965 *American Economic Review* article is the first economics journal article found which discusses Kuhn and his application to economics.
3. For further discussion of Coats's interpretation of a Kuhnian paradigm, see *Paradigm" as Eclectic Collection of Worldview, Method, Values, Theory . . .* and *The Relationship between Paradigms and Theories* below.  
Like Coats, Peterson asserts that Kuhn defines a paradigm as "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners," but also notes that, "The achievements must be open-ended . . ." [Peterson, 1977, page 202].
4. Chase, likewise, allies Kuhn's quote with the notion of paradigm as model example: "In this meaning of the term, an exemplary paradigm devolves from concrete past achievements, and the achievements become for a time universally recognized scientific accomplishments able to provide model problems and solutions to a community of practitioners" [Chase, 1983b, page 815]. See *"Paradigm" as Exemplar* below for further discussion.
5. See *Paradigms and the (Scientific) Community* below.
6. We should note that Zinam is not alone in understanding a paradigm as an eclectic collection of worldview, method, theory . . . . See *"Paradigm" as Eclectic Collection of Worldview, Method, Values, Theory . . .* below. Zinam does, however, stand out for representing this understanding as an elaboration of Kuhn's prefatory quote.
7. The contrast which Spiegel draws between Kuhn's earliest description of a paradigm and the philosopher's later remarks also suggests numerous interpretive difficulties in defining Kuhn's understanding of paradigm. For example: If Kuhn did employ and define "paradigm" in a variety of ways, what were those ways and what relative weights should be applied to each? Should his earliest and most "straightforward" characterization of paradigm be given more weight than later less clear remarks? Should later remarks be viewed as an articulation of Kuhn's paradigm concept and be given more weight than an explicit introductory characterization? Should any remarks by Kuhn which purport to clarify any avowed ambiguity be taken as his definitive understanding of paradigm -- or at least be given considerable weight? And so on.  
For discussion regarding economists' positions regarding the ambiguity of Kuhn's use and definition of paradigm as well as their understanding of the

relationship between Kuhn's discussions about paradigms in the first edition of *The Structure of Scientific Revolutions* versus his later writings, including his Postscript to the second, enlarged edition of *The Structure of Scientific Revolutions*, see THE AMBIGUITY OF "PARADIGM" below.

8. Along these same lines, we should note that Barrie Pettman defines Kuhnian paradigms as "model problems and solutions which serve as standards to a group of scholars" [Pettman, 1977, page 105].
9. Blaug, among other economists, holds that Kuhn applied the term "paradigm" in wide variety of ways, *one* of which was as a worldview. See THE AMBIGUITY OF "PARADIGM" below.  
George Argyrous sees worldview as the most common sense in which economists employ the term [Argyrous, 1992, page 232]. The present discussion concerning the *explicit* definition of Kuhn's paradigm indicates the widespread use and understanding of Kuhn's "paradigm" as a worldview. The analysis also points to a variety of other common understandings.
10. Ward avoids the use of the term paradigm, given the great degree of controversy surrounding it. In its place, Ward uses a host of substitute terms, one of which is worldview. See KUHNS AMBIGUOUS USE OF "PARADIGM" AND THE CONCEPT'S APPLICABILITY TO ECONOMICS below.
11. The reader will note that Routh does not define a paradigm as simply *any* worldview, but, more specifically as the one whose acceptance is necessary for admission into the profession. See both *Paradigms and the (Scientific) Community* and THE FUNCTIONS OF PARADIGM for further discussion. Still further, Routh limits the term's application only to a *dominant* paradigm.
12. We employ "worldview" as Peter Angeles defines the term in his *Dictionary of Philosophy* as "The collection of beliefs (ideas, images, attitudes, values) that an individual or a group holds about things such as the universe, humankind, God, the future, etc." and/or "A comprehensive outlook about life and the universe from which one explains and/or structures relationships and activities" [Angeles, 1981, page 319].
13. The term "reality" here and "world" in the following heading are used in quotes because it is not clear that all economists are speaking -- or in the author's opinion could be speaking -- about the same reality/world or even type of reality/world (metaphysical reality, heuristic fiction and/or device, etc.). Considerable ambiguity surrounds the determination as to whether a given author uses the terms "reality," "world," "worldview" or allied terms as

(1) a *realist*: The world being viewed actually exists and exists independent of man and man's worldview. The worldview serves as a vantage point from which to perceive and understand the actually and independently existing world.

(2) an *instrumentalist*: The world being "viewed" and the worldview itself serve as instruments with which to make predictions, provide explanations for observed phenomena and/or formulate policy. A worldview constitutes a heuristic device and/or policy tool.

(3) a *constructivist*: The world being viewed, along with the worldview, are constructed by, depend upon and are given



effect by man. Independent reality, if it exists, is not directly accessible to man. Determination of the degree to which the world "seen," employing a given worldview, resembles any independently existing world is impossible and thus the question moot. Here, a worldview may be considered closer to an orientation rather than a perspective.

Thus considerable ambiguity exists regarding such questions as (1) Does the world exist independent of man?; (2) Can the world and man's perceptions, pre-conceptions, values and beliefs regarding the world be separated from one another? Detailed discussion of broad questions of ontology and epistemology such as these are, however, beyond the scope of the current discussion.

14. Likewise, Wilfred David describes a paradigm as "our conception of reality or some aspects of reality" [David, 1975, page 73].
15. Similarly, Edgar Dunn describes a paradigm as "a metaphor, conceptual framework or unifying theory . . . ." [Dunn, 1970, page 353]; Paqué allies Kuhn's paradigm concept with "a particular way of describing and interpreting reality, e.g., a framework of thinking" [Paqué, 1990, page 292];, and Miller asserts, "A Kuhnian paradigm consists of a general worldview that shapes perception and constitutes a guide to practice" [Miller, 1991, page 994]. Mark Oromaner describes a paradigm as that "framework" "that scientists are trained to view their world within" [Oromaner, 1981, page 72].
16. Wiles cites two examples of paradigms from Kuhn: "the Ptolemaic paradigm [which] put the earth at the center, with the sun, moon, and planets revolving around it," and "The Copernican paradigm [which] put the sun at the center" [Wiles, 1979, page 171].
17. Deborah Redman, while noting that many economists often "use *paradigm* interchangeably with *Weltanschauung*," asserts that this is "a sense in which Kuhn does not want it to be used" [Redman, 1991, page 144].
18. Argyrous allies a worldview understanding of paradigm with Kuhn's disciplinary matrix. Thus, he also calls into question the interpretation of a paradigm as a disciplinary matrix. See below, *A Paradigm is Not (Strictly) a Collection of Worldview, Theory Methodology and/or Values or Disciplinary Matrix* and the subheading, Exemplars and Disciplinary Matrices, under the upcoming *Summary*.
19. These authors who define a paradigm as a *set* of such elements must be contrasted with those who understand Kuhn defining "paradigm" ambiguously by applying the term to a wide variety of methodological, metaphysical, epistemological, etc. entities, each *by itself* constituting a paradigm. See THE AMBIGUITY OF "PARADIGM" below.
20. Carol Anderson allies Kuhn's paradigm concept with his own notion of a "reality base" [C. Anderson, 1982, page 200].
21. L.E. Johnson highlights a fifth paradigm characteristic which he asserts Kuhn either ignores or leaves implicit: the purposive function. See below THE APPLICABILITY OF KUHN'S PARADIGM CONCEPT TO ECONOMICS.

22. With the exception of "professional relationships," L.E. Johnson provides examples of the basic characteristics of a Kuhnian paradigm: (1) "*fundamental theoretical assumptions*": "For example, it can be argued that the analysis of the classical political economists was based on the assumptions of natural law, a class conflict view of the economy embodying an opposition of class interests, the subsistence wage doctrine, and the doctrine of the wages fund." (2a) "*methods of analysis*": "distinctions between inductive and deductive reasoning, comparative statistics [sic?] and dynamics . . ." (2b) "*focal variables*": "distinctions. . . between microeconomic and macroeconomic variables, stocks and flows . . ." (3) "*basic issues*": "questions of allocative efficiency, price and employment stability, equity, economic growth, and development" [Johnson, 1983, page 1099-1100].
23. Zinam defines a more elaborate and inclusive notion of a paradigm in his description of paradigms in economics. According to Zinam, Kuhn's paradigm consists of the elements contained within only the first two of the four quadrants which comprise Zinam's own "master" paradigm. See THE APPLICABILITY OF KUHN'S PARADIGM CONCEPT TO ECONOMICS below.

DeVroey hints at the social nature of paradigms by noting that a paradigm's "content . . . is reflected in textbooks" [DeVroey, 1975, page 419].
24. But see below under "*Paradigm*" as *Disciplinary Matrix* and "*Paradigm*" as *Exemplar*.
25. For further discussion of the sociological character of paradigms see *Paradigms and the (Scientific) Community* and THE FUNCTIONS OF PARADIGM below.
26. In addition to the economists listed below, Sheila Dow also allies Kuhn's paradigm concept with his notion of disciplinary matrix [Dow, 1985, page 27].
27. Worland, most likely referring to Kuhn's notion of a disciplinary matrix, remarks: "In his [Kuhn's] 1970 postscript, a paradigm is characterized as a 'matrix' of shifting elements including basic definitions and laws, models, and 'shared exemplars'" [Worland, 1972, page 275, footnote 5].

R.X. Chase similarly remarks: "A non-exhaustive list that is suggested by Kuhn of the main sorts of components of a disciplinary matrix would include such things as 'symbolic generalizations' (p. 183), commitments to shared beliefs and values (pp. 184-86); and 'exemplars,' the latter being the concrete problem solutions used to demonstrate, to teach and to extend the shared paradigm. (p. 187)" [Chase, 1983b, page 814, page references in parentheses are Chase's and refer to Kuhn, 1970c].

Jalladeau also lists as a disciplinary matrix's components: "shared symbolic generalizations, beliefs, values, and examples of solved problems within a scientific circle" [Jalladeau, 1978, page 588].

Reynolds sees one of the two senses in which Kuhn defines a paradigm as "that of a 'disciplinary matrix' consisting of symbolic generalizations deployed without question, shared commitments to a set of beliefs, a set of values and 'exemplars'" [Reynolds, 1976, pages 25-26].
28. Stent, likewise asserts that Kuhn "suggests" that a paradigm is a disciplinary matrix and describes the matrix's four components ("a shared symbolic generalization," "metaphysical parts of a paradigm," "shared" "values," and "[m]ost important of all are the shared *exemplars*") [Stent, 1976, page 3].

29. Argyrous does acknowledge that Kuhn uses the term, "paradigm" to denote both a disciplinary matrix and a type of behavior. However, in describing how the philosopher *defined* the term, Argyrous limits his attention strictly to the understanding of a paradigm as an exemplar. See below under the sub-heading Exemplars and Disciplinary Matrices, under the upcoming *Summary*.
30. Similarly, Aidan Foster-Carter points out that *particular* paradigms are rarely defined explicitly -- except in times of professional crisis: "It may be only in crisis that a paradigm is even so articulated that those who have been operating within it actually become aware of it" [Foster-Carter, 1976, page 169].
31. "But," Blaug continues, "he also employed the term in quite a different sense to denote both the choice of problems and the set of techniques for analysing them, in places going so far as to give 'paradigm' a still wider meaning as a general metaphysical *Weltanschauung* . . ." [Blaug, 1976, page 152]. See below under THE AMBIGUITY OF "PARADIGM."
32. Likewise, Thomas Holland asserts, "one of Kuhn's conceptions of a paradigm may be interpreted to mean that an exemplar that is articulated is a paradigm" [Holland, 1987, page 191].
33. While most economists present only a few sentences or paragraphs describing Kuhn's conception of a paradigm, Argyrous provides an incisive five page analysis of paradigms. Along with L.E. Johnson, Argyrous focuses the most attention upon and provides the most detailed description of a Kuhnian "paradigm." More will be said about Johnson's interpretation of Kuhn's paradigm concept below, under THE APPLICABILITY OF KUHN'S PARADIGM CONCEPT TO ECONOMICS.
34. Argyrous cites a few examples of the forms a paradigm may take: "It may involve the application of a particular mathematical technique such as differential calculus, to certain types of problems, or statistical operations to certain other types of problems" [Argyrous, 1992, page 233].
35. Argyrous seeks to base his discussion upon Margaret Masterman's interpretation of a Kuhnian paradigm as a concrete example [Argyrous, 1992, page 233, footnote 2]. Her paper, "The Nature of a Paradigm," [Masterman, 1970] along with others critiquing Kuhn's work (along with Kuhn's replies [Kuhn, 1970a and 1970b]), were published in Lakatos and Musgrave, 1970.  
 As we shall see below Argyrous is not alone in citing Masterman's discussion of Kuhn. However, most other economists who cite Masterman (a computer scientist) do so to point out Kuhn's ambiguous use and/or definition of the concept. Richard Chase was the only other author found to cite Masterman for a reason other than to indicate Kuhn's ambiguous use of "paradigm." Chase, however, only quotes a brief quote from Masterman and does not purport -- as Argyrous does -- to base his analysis upon the computer scientist's understanding of Kuhn [Chase, 1983b; Argyrous, 1992].
36. For example in the above definitions, a paradigm as exemplar is described as "accepted by the group," an "accepted model," "a classic example of how 'good' science is conducted" and as "a key piece of research and/or discovery, which on the one hand explains or solves an important problem more satisfactorily than any previous attempt. . . "

In addition, Peabody, while defining and using "paradigm" in the text as a disciplinary matrix, acknowledges that Kuhn sought to restrict "paradigm" to denote an exemplar and defines exemplars as "the set of problem solutions that demonstrate the empirical content of the theories and *provide examples for training students* to see puzzles as like problems they have already learned to solve" [Peabody, 1971, page 2, emphasis added].

37. Such an interpretation is highly akin to the standard dictionary definition of an exemplar.
38. For example, the above authors describe a paradigm as that which "suggests further research," "can never be so cut-and-dried that it fails to leave 'puzzles' that still need solving," constitutes "an object for further articulation and specification," and which "students of a particular discipline encounter in gaining their professional education."  
We are here confining our discussion to those authors who include this function of a paradigm in their definition of the concept. See below, THE FUNCTIONS OF PARADIGM, for further discussion of this and other functions economists understand a Kuhnian paradigm to serve.
39. Here, we may also include those such as Karsten who describe a paradigm by quoting Kuhn's description of a paradigm as an "accepted model or pattern" [Karsten, 1973, page 402].
40. The author, of course, identifies these interpretive difficulties via his own interpretation of Kuhn's remark and the terms contained within it (especially, "achievements," "models," and "provides") .
41. See below, THE AMBIGUITY OF "PARADIGM".
42. "This bald summary does scant justice to Kuhn's cogent and subtle argument, which is buttressed by a wealth of historical illustrations . . . " [Coats, 1969, page 291].
43. Gordon's *entire* discussion of Kuhn, including his application of Kuhn's concepts to economists consumes only half of his nine page article [Gordon, 1965].
44. Placing Kuhn's quote in context, we should note in the paragraph directly prior to this quote, Kuhn enumerates classic scientific tracts, akin to economists' interpretations of exemplars as paradigms: Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Franklin's *Electricity*, Lavoisier's *Chemistry* and Lyell's *Geology*. Directly following the quote, however, the philosopher cites examples implying a much broader understanding of the term (more akin to an eclectic collection): "'Ptolemaic astronomy' (or 'Copernican'), 'Aristotelian dynamics' (or 'Newtonian'), 'corpuscular optics' (or 'wave optics'), and so on" [Kuhn, 1970c, page 10].
45. Like Redman, Weigel asserts that "Kuhn understands paradigm in two senses of the term: (1) as 'the entire constellation of beliefs, values, techniques, and so on shared by members of a given (scientific) community'; and (2) more restrictively, as denoting 'one sort of element in that constellation, the concrete puzzle-solutions which . . . [are] employed as models or examples . . . "

[Weigel, 1986, page 1423, quoting Kuhn, 1970c, page 175]. Klein also points out that Kuhn uses paradigm in two different senses:

Several comments are in order. First in the second edition of his book, Kuhn suggests that he used the word "paradigm" in two senses in the first edition. . . . The first sense is indeed the "entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. . . ."

The second sense in which Kuhn uses the term paradigm, one that he describes as the "deeper of the two" is less well-known . . . Kuhn argues that paradigm "denotes one sort of element in that constellation [of beliefs, etc.] the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis of the solution of the remaining puzzles of normal science." [Klein, 1990, page 385, quoting Kuhn 1970c, page 175, brackets in Klein]

46. We need to draw a distinction between Redman, Weigel and Klein, all of whom assert *for themselves* that Kuhn employs "paradigm" in these two senses and Caldwell who merely reports that the philosopher *proposed* the two terms to encompass the ways in which he employed paradigm. Caldwell's remark begs the question: Kuhn's proposal aside, did the philosopher actually employ and/or define "paradigm" in these two senses. Caldwell does not, however, raise any explicit objection to Kuhn's contention. Below is Caldwell's remark in greater context:

Kuhn pleads guilty to the charge of vagueness on this point [his use and definition of paradigm], and attempts reconciliation by defining two new concepts, exemplars (concrete, technical problem solutions which the students of a particular discipline encounter in gaining their professional education) and disciplinary matrices (the symbolic generalizations, models, values, commitments, and exemplars shared by and which unite given scientific communities) which he feels captures most of the meanings formerly adduced to the single paradigm concept. [Caldwell, 1982, page 75]

47. For further discussion of the relation between Kuhn's treatment of "paradigm" in the first edition of *The Structure of Scientific Revolutions* and his treatment of the concept in his later writings (especially in the Postscript to the second edition of *The Structure of Scientific Revolutions*), see THE AMBIGUITY OF "PARADIGM".
48. Klein as well points to a paradigm understood as "'one sort of element in that constellation [of beliefs, etc.] the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis of the solution of the remaining puzzles of normal science'" as "'the deeper of the two'" of Kuhn's definitions of paradigm [Klein, 1990, 385, quoting Kuhn, 1970c, page 175, brackets in Klein].
49. Deborah Redman sees Kuhn as introducing both "exemplar" and "disciplinary matrix" to replace the term paradigm entirely [Redman, 1991, page 17].

50. Likewise, Dunn describes a paradigm as "a metaphor, conceptual framework or unifying theory that has emerged out of earlier scientific practice" [Dunn, 1970, page 353].
51. Among those defining a paradigm as a theory, Zinam asserts "In its broadest interpretation, paradigm means a scientific theory *accepted by a community* of scholars in a given science" [Zinam, 1982, page 363, emphasis added].  
In addition to economists listed in the previous sections, we may add Stigler, O'Brien and Wang to those who include scientific community acceptance in their definitions of paradigm. Stigler describes a Kuhnian paradigm as "the corpus of theoretical knowledge and analytical and empirical techniques which is *accepted by the dominant group of members a science*. . . " [Stigler, 1969, page 223, emphasis added]. O'Brien characterizes a paradigm as "the view of what science (or sometimes a branch of it) is about and what and how it does its work that is *accepted by the 'scientific community'*" [O'Brien, 1983b, page 102, emphasis added]. Finally, Wang asserts that, "According to Kuhn, the paradigm is a *professionally accepted* framework of inquiry and research" [Wang, 1973, page 151, footnote 2, emphasis added].
52. At least as Kuhn used and defined the term in the first edition of *The Structure of Scientific Revolutions*.
53. Similarly, W. David notes that, "The paradigm is normally *shared by a community* of scholars and refers to the 'entire constellation of beliefs, values, techniques' to which the *scientific community is committed*" [David, 1975, page 73, quoting Kuhn 1970c, emphasis added].
54. Johnson and Ley: "Paradigms are accepted by a discipline informally when its members come to adopt common procedures in their scientific work" [Johnson and Ley, 1990, page 26].  
Along the same lines, Seligman asserts that, "Kuhn spoke of a paradigm as a *shared* set of rules and standards for the conduct of scientific research" [Seligman, 1971, page 2, emphasis added].
55. Likewise, Reynolds remarks, "In the second edition of his book, Kuhn points out that the term paradigm is used with two meanings. At one level paradigm refers to the 'community structure of science'" [Reynolds, 1976, page 25].
56. William Dugger's interchangeable use of "paradigm" with "school of thought" (a term traditionally associated not simply with a given set of concepts, theories and principles, but the community of practitioners adhering to them as well) further demonstrates economists' appreciation of the link between paradigm and community [Dugger, 1976].
57. Similarly, the two "particularly notable" features of paradigms" according to Pheby, bear upon its popularity among practitioners and ability to guide them:

There are two features of paradigms that are particularly notable: firstly, it signifies an achievement that is considered so important that it attracts an "enduring group" of adherents away from competing modes of scientific activity; secondly, such a development leaves unresolved a sufficiently large number of problems for these new adherents to research into. [Pheby, 1988, page 37-38]

58. Along the same lines, Jack Connor describes Kuhnian exemplars as "unchallenged theses like Newton's Second Law or Ohm's Law" [Connor, 1991, page 59]. In addition, we may note that Connor allies Kuhn's concept of exemplar (a notion which many economists relate to Kuhn's paradigm concept) with his own notion of science(g), which Connor defines as that portion of science which is held by a scientific community as "entirely credible" [Connor, 1991, page 58].

Similarly, Reynolds describes the symbolic generalizations included within a disciplinary matrix as "deployed without question" [Reynolds, 1976, pages 25-26]. See also Hausman, 1992, page 84.

59. See Lakatos, 1970. Lakatos proposes his Methodology of Scientific Research Programmes as a response to the difficulties posed by the Duhem-Quine thesis (and the difficulties he sees stemming from the role lent to irrationality in Kuhn's schema). According to the thesis: It is impossible to evaluate a theory in isolation. Instead, the theory must be tested in conjunction with a host of auxiliary conditions. Thus, the interpretation of results which (appear to) run counter to a theory is ambiguous. No conclusive way of knowing whether the counterinstance incriminates the theory, the auxiliary assumptions, or both exists.

Scientists, according to Lakatos, do/should not engage in the testing of individual theories, but instead, in the evaluation of larger, more complex scientific research programmes. A given programme includes a hard core, negative heuristic, protective belt and a positive heuristic. The negative heuristic forbids incrimination of the theories, hypotheses, etc. inhabiting the hard core. ("The negative heuristic of the programme forbids us to direct the *modus tollens* at this 'hard core'" [Lakatos, 1970, page 133].) Instead, the programme's positive heuristic directs scientists' attention toward the construction and testing of hypotheses found within the programme's protective belt.

According to Lakatos, scientists do/should evaluate not individual theories for their truth or falsity, but instead, do/should evaluate the progressiveness of entire scientific research programmes. Further, scientists should not abandon a research programme even if working within it, they are unable to take account of certain counterinstances (anomalies). Instead, scientific programmes should be evaluated over the long run. Lakatos defines progressiveness in terms of a programme's ability to generate and obtain empirical support for novel facts (empirical and theoretical progressiveness).

As the prior chapter indicated a number of economists have cited Lakatos in the journal literature. Many economists have sought to provide substantial interpretations and applications of Lakatos's Methodology of Scientific Research Programmes. In particular, see Blaug, 1976; Diamond, 1988; Remenyi, 1979, and Stephens, 1981.

60. Deborah Redman and Charles Fischer make similar assertions. Redman affirms that "Paradigms and research programs are essentially the same thing: as Lakatos concedes normal science with its accepted paradigm 'is nothing but a research programme that has achieved monopoly'" [Redman, 1991, page 145, quoting Lakatos, 1970, page 155]. Likewise, Fischer remarks that "Lakatos chose the label 'research programme' because of its historical connotation. This suggests that it evolves from scientific activity undertaken over time by a group of scientists with similar intellectual background, training and goals. As such, it bears a close affinity with Kuhn's concept of paradigm" [Fischer, 1993, page 57].

61. Miller, as well, likens Lakatos's hard core concept and Kuhn's paradigm concept [Miller, 1991, page 1003, note 1].
62. The context of Peach and Webb's remark: "Lakatos's 'hard core' is much the same as Kuhn's 'paradigm' (in the sense of disciplinary matrix). This is that portion of the body of theory and accompanying ontological beliefs that distinctively characterize a theory and to which scientists accepting the theory hold most tenaciously" [Peach and Webb, 1983, page 713, note 5].  
Similarly, from Bøhren: "The *Scientific Research Programme* (SRP) is the basic concept in a related model of theory development suggested by Lakatos. The SRP is subdivided into two parts; the hard core and the protective belt. The hard core, which resembles Kuhn's paradigm, contains shared commitments which are not the subjected to empirical testing" [Bøhren, 1990, page 11].
63. Similarly, Rugina, referring to his new system of economic theory, remarks: "The final product was a new, more comprehensive methodological apparatus, a new research programme, if we adopt the terminology of Lakatos, or a new paradigm, if we follow the language of Kuhn" [Rugina, 1986, page 16].
64. May and Sellers: "The neo-classical paradigm that now dominates the profession is purportedly based upon a methodological foundation of logical empiricism, which the neoclassical economists themselves cannot conform to, but which nonetheless has proven to be quite useful in undercutting the legitimacy of those alternative research programs that do not support the ideological conclusions of neoclassicism" [May and Sellers, 1988, page 397].  
Colander and Koford: "The 'optimal' investment rule in research is to undertake all projects with a positive present value, given some social discount rate, or, since many research programs are interdependent (complementing or competing), the bundle of research programs with the highest present value."  
"Much of the disagreement as to the value of alternative paradigms is due to differences in internal rate of return" [Colander and Koford, 1979, page 712].  
We should note that May and Sellers apply the term "paradigm" only to the economics mainstream and refer to the status quo's alternatives as "research programmes." Though the author has not been able to detect a pattern of such usages, a more focused analysis examining economists' understanding of the relationship between Kuhn's and Lakatos's philosophy of science might reveal one.
65. Blaug further maintains that Lakatos' overall philosophy of science is far superior to Kuhn's: "If the concept of SRP is faintly reminiscent of Kuhn's 'paradigms,' the fact is that Lakatos's picture of science is much richer than that of Kuhn's. Furthermore, it begins to provide insight as to why 'paradigms' are ever replaced, which is one of the central weaknesses of Kuhn's work" [Blaug, 1976, page 157].
66. Economists draw links between Kuhn's paradigm concept and other economists' notions. Andrew Skinner, for example, allies Kuhn's paradigm concept with Adam Smith's notion of a "system: "Kuhn cites Ptolemaic Astronomy, Aristotelian Dynamics, and Newtonian Optics as examples; usages which suggest that he used the term 'paradigm' in much the same sense as Smith used that of 'system'" [Skinner, 1979, page 118]. Skinner defines Smith's notion of "system" by quoting the Classical economist: "A system is an imaginary



machine invented to connect together in the fancy [imagination] those different movements and effects which are already in reality performed" [Skinner, 1979, page 111, quoting Smith, 1980, section 4, paragraph 19].

67. Nooteboom, however, does not explicitly relate an exemplar with the notion of a paradigm. Nooteboom allies Kuhn's notion of "exemplar" with his own concept, "standard applications of the theory" [Nooteboom, 1986, page 201-3]. In Nooteboom's definition of a theory, these applications -- one of five elements of a theory -- "serve as aids for teaching, understanding, and absorbing the theory, and as guidelines for its application and further development" [Nooteboom, 1986, page 203].
68. Negishi's definition implies that all paradigms are theoretical complexes. It does not, however, imply that all theoretical complexes are theories -- only those "accepted" by the professional community. Similarly, Charles Wilber and Jon Wisman ally Kuhn's paradigm concept with that of a *general* theory that "guides the choice of problems, provides the analytic tools, and supplies a general vision of how reality is structured" [Wilber and Wisman, 1975, page 671].
69. Glass and Johnson: "Just as Kuhn's notion of a paradigm emphasizes the need to analyse science in terms of theoretical frameworks or structures (rather than in terms of individual theories....), so also Lakatos's notion of a research programme places the emphasis on theoretical frameworks rather than on individual theories" [Glass and Johnson, 1989, page 166].
70. See *"Paradigm" as Eclectic Collection of Worldview, Method, Values, Theory* . . . above.
71. Interestingly, Coats, while stressing that a Kuhnian paradigm in the *natural* sciences is more than simply theory, maintains that *social* science paradigms amount to little more than theory. See KUHN'S AMBIGUOUS USE OF "PARADIGM" AND THE CONCEPT'S APPLICABILITY TO ECONOMICS.
72. Likewise, Stent notes that "A paradigm entails theory but is much wider than that" [Stent, 1976, page 3].
73. Bøhren: "Finally, the paradigm includes implicit assumptions which may be difficult to deduce from the specific theories generated within the paradigm (for instance, a belief that human behavior is maximizing in an economic welfare sense, that most observable market prices approach or stay close to those of a competitive equilibrium, or that investors normally prefer more information to less)" [Bøhren, 1990, pages 10-11].
74. Although Jalladeau does not explicitly differentiate between paradigm and theory, he does implicitly describe a paradigm as prior to theory in explaining a paradigm's function: He sees paradigms as establishing the "scheme of operation," which, in turn, "determines the theoretical structure" [Jalladeau, 1978, page 584].
75. See below, THE AMBIGUITY OF "PARADIGM".

76. See below, PARADIGMS IN THE HISTORY OF ECONOMICS, CURRENT-DAY MAINSTREAM PARADIGMS, CURRENT-DAY HETERODOX PARADIGMS and CURRENT-DAY SUB-FIELD PARADIGMS.
77. Here, we are referring to the length/prominence of discussions concerning only Kuhn's "paradigm" -- apart from applications of "paradigm" and analysis of the role and character of paradigms in economics.

Some economists *do* provide significant treatments of the Kuhnian paradigm concept. In the book literature, Redman [Redman, 1991], Caldwell [Caldwell, 1982], and Solo [Solo, 1991] all provide significant discussions regarding Kuhn's paradigm concept. These authors' descriptions are, however, within the context of potted -- albeit lengthy -- descriptions of Kuhn's philosophy of science. More notable are Argyrous' five page analysis of the nature of a Kuhnian paradigm [Argyrous, 1992, pages 232-236] and L.E. Johnson's lengthy discussions regarding the constituents of a Kuhnian paradigm (both in a journal article and a co-authored textbook which applies Kuhn's philosophy of science to the study of the history of thought) [Johnson, 1983; Johnson and Ley, 1990].
78. Jalladeau concurs with Coats's assessment [Jalladeau, 1975, page 2].
79. "The function of a scientific paradigm," according to Solow, "is to provide a framework for 'normal science'" [Solow, 1971, page 64].

Similarly, Jalladeau holds that, "The paradigm is an articulated system . . . constituting the frame of reference governing every discourse" [Jalladeau, 1978, page 584], and Zweig maintains that "a paradigm, or basic conception, is important in that it regulates and systematizes particular investigations . . ." [Zweig, 1971, page 44].
80. L.E. Johnson: "A fifth characteristic, which is crucial to the construction of a paradigm (at least in economics) but is treated so casually by Kuhn that it cannot be clearly identified, is the central focus of inquiry, or guiding question, that underlies and directs both theoretical formulations and empirical research" [Johnson, 1980, page 56].
81. As we saw earlier, Deane understands Kuhn as maintaining that a paradigm "fully determines both the world-view of practising scientists and the research agenda of "normal" scientific activity" [Deane, 1978, page xii].

In addition, we find a number of economists concurring that paradigms serve to limit the questions scientists ask and the problems they address. Routh: "The paradigm demarcates the field of activity" [Routh, 1973, page 182]; Weigel: "Kuhn argues that these paradigms provide a critical foundation for systematic scientific investigation, in that they map the issues significant to a discipline . . ." [Weigel, 1986, page 1423]; Bøhren: "According to Kuhn's model of the history of ideas . . . [t]he paradigm broadly defines what phenomena are worth studying (for instance, the pricing of shares) and states the rationale for this preference (for instance, changing share prices which influence social welfare in significant and unpredictable ways)" [Bøhren, 1990, page 10]; Oromaner: "Issues that cannot be answered through this framework [the paradigm] are put to the side for another time or are ignored" [Oromaner, 1981, page 72]; Storper: "essentialist paradigms serve as cognitive filters for research agendas . . ." [Storper, 1985, page 261]; Poirier: "As Thomas Kuhn noted, one important characteristic of any paradigm is that the paradigm itself suggests the

problems and questions its practitioners should investigate" [Poirier, 1977, page 394].

82. Similarly, Zweig understands a paradigm to provide, "a framework by which to choose 'relevant' or 'interesting' experiments . . ." [Zweig, 1971, pages 43-44].
83. We find still others. Williams allies Kuhn's paradigm concept with his own notion of "framework" which, according to Williams, "directs attention to problems and defines what would constitute a solution" [Williams, 1972, page 467]. Miller asserts that: "A Kuhnian paradigm consists of a general world view that shapes perception and constitutes a guide to practice; that is, that indicates the nature of the problems (puzzles) to be addressed, the tools to be used in reaching closure, and the solutions deemed acceptable" [Miller, 1991, page 994]. Along related lines, Larry Dwyer, though not employing the term "paradigm" explicitly footnotes Kuhn (along with Lakatos, Feyerabend and Suppe) with the notion of "conceptual perspective" that, among other things, "shapes the interests of the scientist and determines the questions he asks, the problems he attempts to solve, the answers he deems acceptable" [Dwyer, 1982, page 76].
84. Lawrence Tribe notes that one of the ways in which Kuhn employs the term, "paradigm" is to denote an "index of the constriction" [Tribe, 1973, page 476].
85. Also from Jalladeau: "The paradigm is an articulated system of analytical concepts, methodological principles, techniques, and values constituting the frame of reference governing every discourse. The established scheme of operation then determines the theoretical structure and dictates to the investigator the meaningful questions, the legitimate methods, and the nature of the admissible answers" [Jalladeau, 1978, page 584].
86. Quoting Chase more fully: "The existing exemplar sets the fashion and direction for inquiry not only by raising questions, but also by suppressing problems by defining them to lie outside the paradigmatic pale . . ." [Chase, 1983b, page 817].
87. Thus, in some sense a paradigm does not simply restrict what scientists study, but also defines what is taken to be scientific and what not.
88. Similarly, Foxall comments: "It follows, for instance, that even the most casual investigator is influenced by some framework of assumption through which the 'facts' are construed. (This article uses the term 'paradigm' to refer to such a framework) . . ." [Foxall, 1986, page 394]. Similarly, Wagner and Weber direct readers to Kuhn for discussion concerning "the necessity of paradigms for the interpretation of evidence" [Wagner and Weber, 1977, page 67, note 12], and Chase points out that ". . . paradigms affect cognition as well as perception" [Chase, 1983b, page 820].

Without directly allying the notion with Kuhn or his paradigm concept, Dwyer associates Kuhn (along with Suppe, Lakatos and Feyerabend) with the notion of a "conceptual perspective," that "shapes . . . perception of 'the facts,' [and] . . . the categories in terms of which his experiences are organized" [Dwyer, 1982, page 76].

89. See discussion above, *"Paradigm" as a Worldview* under, THE DEFINITION OF PARADIGM. Miller's characterization, in particular, makes explicit that a paradigm "shapes perception."
90. Numerous economists remark upon the blinkering and filtering function which economics paradigms play. See below, THE FUNCTIONS OF PARADIGM.
91. Likewise, Seiz quotes from Kuhn, "A paradigm can . . . even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies" [Seiz, 1992, page 294, quoting Kuhn, 1970c, page 37].  
O'Brien identifies "one of Kuhn's paradigm functions" as "insulating scientists from important [social] problems" [O'Brien, 1976, page 142].  
Similarly, Fischer remarks upon the role which a paradigm plays in insulating practitioners from social problems: "A paradigm also may insulate the profession from socially important problems (e.g., income distribution). These problems may be considered external to normal science puzzle-solving activity because of their lack of 'relevance' concerning paradigm articulation" [Fischer, 1993, page 56].
92. David: "The conditions underlying the paradigm set a limit to the questions which the scientist asks, thereby hindering rather than stimulating progress" [David, 1975, page 73].  
Parsons implies that according to Kuhn, paradigms, by restricting a scientists' imagination, render him unable to produce scientific breakthroughs: "For Kuhn, scientific 'breakthroughs' are invariably produced by people who have not spent any considerable time operating within a given 'scientific paradigm' -- that is, their imaginations are *not* restricted by the previously 'taken-for-granted' truths" [Parsons, 1990, page 317, footnote 100].
93. Worland: "But this process of abstraction and concentration also has its price. Some factors are, thanks to the paradigm, excluded from observation; while some linkages between variables are illuminated, others are obscured. Phenomena that will not fit into the scheme of the paradigm 'are often not seen at all.' 'Fundamental novelties' that might upset the basic paradigm are often 'suppressed.' . . . " [Worland, 1972, page 275, quoting Kuhn, 1970c, pages 24 and 5, respectively].
94. Of course such assessments are more the product of a given economist's understanding of the mission of science (and economics) than their interpretation of the philosopher. Those who see finding solutions to burning social problems as (one of) the chief purposes of science will regard exclusion of social matters as an impediment to science. Thus, they will understand a paradigm's insulating scientists from broader concerns as hindering the cause of science.
95. This line of argument, of course, presumes that some sort of extra-paradigmatic basis for paradigm evaluations exists and that the faults which are "obscured" do exist. More generally, we might have made the point: By serving as a filter, a paradigm "obscures" from scientists working within them, that which others working outside of the paradigm may see as faults.

96. Along the same lines, Loasby maintains that a paradigm "holds out the prospect of successful solutions to those who formulate and test with skill and care particular hypotheses consistent with the paradigm" [Loasby, 1971, page 866].  
We should note, however, that economists understand from Kuhn that on rare occasions paradigms fail to provide answers to certain vexing questions. See the section on CAUSES OF SCIENTIFIC REVOLUTIONS in Chapter Five.
97. Breton and Wintrobe: "Kuhn's view of how markets for scientific ideas operate is based on the history of science and not on a logical theory of scientific method. That view can, however, be given a logical or efficiency rationale using the model proposed in this paper. In that model, a paradigm is simply a screening device which sorts hypotheses into two categories: those which are consistent with (may be derived from) the paradigm and those which are not. The function of paradigms, like that of any screen, is to economize on the costs of testing or experimentation . . . ." [Breton and Wintrobe, 1992, page 225].  
However, Breton and Wintrobe assert that a paradigm enjoys a monopoly within a science. As such it may be necessary to institute regulations to assure that a paradigm does not use its power to outlive its usefulness to the profession. Carrying the economic analogy still further, Breton and Wintrobe caution against "inefficient entry" in those cases in which a natural monopoly (by virtue of decreasing marginal costs) exists in the "market" which the paradigm dominates [Breton and Wintrobe, 1992, pages 228-229].
98. Scientists encounter the infinite regress when they seek to legitimate a given conclusion at which they arrive. To do so they must legitimate the means by which they arrived at that conclusion. In turn, they then need to legitimate the legitimation by which they legitimated the conclusion, and again, that legitimation by which they legitimated the means by which they arrived at a given conclusion, and so on [Boland, 1977, pages 97-98].
99. Along the same lines, Bornemann quotes Kuhn: "'By focusing attention upon a small range of relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable'" [Bornemann, 1976, page 130, quoting Kuhn, 1970c, page 24].  
Similarly, Pheby understands from Kuhn that, "The paradigm also has the advantage of releasing scientists from the need to engage in debates over fundamental assumptions -- such functions can be achieved by textbooks. Scientists can then concentrate on the 'subtlest' and more 'esoteric' aspects of the phenomena that interests the group . . ." [Pheby, 1988, page 38].
100. Boland explains, "The avoidance is possible because the criteria are slowly developed within the community and only with ample support of the entire community of scientists are they still applied" [Boland, 1977, page 98].
101. Similarly Seligman describes paradigms as serving to make "sense of the great mass of facts that an investigator had to confront" [Seligman, 1971, page 2].
102. Numerous economists associate allegiance to a paradigm as antithetical to creativity. Paradigms stifle, not foster, creativity. For discussion of these economists, see Chapter Four.
103. Chase notes: ". . . as Thomas Kuhn argues in his *Structure of Scientific Revolutions*, a paradigm is never complete and never without unsolved problems" [Chase, 1983a, page 176].

104. There is, however, some question as to whether open-endedness constitutes a necessary or only a highly desirable paradigm attribute.
105. Stanfield quotes Stigler's observation in his own description of a paradigm [Stanfield, 1974, page 106, note 3].
106. Pheby, along the same lines, remarks: "There are two features of paradigms that are particularly notable: firstly, it signifies an achievement that is considered so important that it attracts an 'enduring group' of adherents away from competing modes of scientific activity; secondly, *such a development leaves unresolved a sufficiently large number of problems for these new adherents to research into*" [Pheby, 1988, pages 37-38, emphasis added].
107. The above line of reasoning, of course, implies that the paradigm alone poses the questions and problems which direct inquiry. Otherwise, a paradigm's answering all the questions it posed and solving all the problems it posited need not imply that scientists are left with no work to do. Nor would a paradigm's open-endedness alone provide the questions to be answered and the problems to be solved.  
 This line of reasoning may also directly relate to the definition of what constitutes (normal) science. Many economists, as we shall see, understand Kuhn's definition of normal science as science conducted within a given paradigm. Given this interpretation, a paradigm's open-endedness *would* be necessary for work within normal science. See Chapter Four.
108. Jalladeau echoes Coats's observation [Jalladeau, 1978, page 589].
109. O'Brien similarly affirms that a Kuhnian paradigm "governs all 'normal scientific' activity *including the actual process of testing*" [O'Brien, 1976, page 141].
110. See *Paradigms and the (Scientific) Community* above.
111. Chase: "The central importance of the paradigm concept in its sociological totality is threefold. First, such a paradigmatic matrix establishes at some point in time the boundaries for a particular science; secondly, it identifies those individuals who are members in good standing of that scientific community; and third, it provides an instrumentality -- that is, an exemplary model -- that organizes and directs professional activity and standards. Thus, within the Kuhnian framework, scientists and scientific activity exist only in terms of a paradigmatic structure that defines and identifies and which provides direction and evaluative standards" [Chase, 1983b, pages 815-816].
112. Connor: "The student who is learning science learns it by studying 'exemplars' . . . unchallenged theses like Newton's Second Law or Ohm's Law . . ." [Connor, 1991, page 59].
113. Argyrous makes a similar observation about exemplars within economics. See below.
114. Dewald, Thursby and Anderson: "Replications in the physical and social sciences are attempted infrequently, however. Thomas Kuhn (1970) emphasized that replication -- however valuable in the search for knowledge -- does not fit within the 'puzzle-solving' paradigm which defines the reward

structure in scientific research. Scientific and professional laurels are not awarded for replicating another scientist's findings. Further, a researcher undertaking a replication may be viewed as lacking imagination and creativity, or of being unable to allocate his time wisely among competing research projects. In addition, replications may be interpreted as reflecting a lack of trust in another scientist's integrity and ability, as a critique of the scientist's findings, or as a personal dispute between researchers. . . ." [Dewald, Thursby and Anderson, 1986, page 587].

115. Similarly, Holland, quoting Kuhn directly notes: "Writing in reference to the physical sciences, Kuhn concludes that: 'To borrow once more Michael Polanyi's useful phrase, what results from this process is "tacit knowledge" which is learned by doing science rather than by acquiring rules for doing it.'" [Holland, 1987, page 197, quoting Kuhn, 1970c, page 191].  
 Backhouse: "This is the *disciplinary matrix* within which normal science is carried on. In undertaking normal science, scientists are not following a series of explicit rules, but they are following an example. Provided the initial scientific achievement, and the results obtained, are accepted without question, rules are not needed. Even if they were desired, suitable rules to govern the conduct of research might prove hard, if not impossible, to articulate" [Backhouse, 1985, page 4].
116. Masterman constructed the list, D.P O'Brien notes, while laid up in the hospital [O'Brien, 1976, pages 141-142].
117. Jalladeau footnotes Masterman with reference to the assertion that, "This concept of paradigm, used in 21 different senses in *The Structure of Scientific Revolutions*, is largely the source of the debates between Kuhnians and Popperians" [Jalladeau, 1978, pages 584 and 605, note 1].
118. Pheby, 1988, page 43; Caldwell, 1982, page 75; Veen, 1976, page 371.
119. As we noted earlier, Spiegel makes indirect reference to Masterman and notes that while Kuhn defined paradigm in an apparently unambiguous manner, "interpreters of Kuhn have found a large variety of different shades of meaning of the term, twenty-one or even more, that they have located in Kuhn's work itself" [Spiegel, 1983, page 664].  
 In addition Redman, citing Masterman remarks, "Because Kuhn used *paradigm* in no fewer than twenty-one different senses . . . , it has become the source of considerable misunderstanding" [Redman, 1991, page 16].  
 Still again, Williams also describes Kuhn's use of paradigm as ambiguous and references Masterman for corroboration [Williams, 1972, page 478, footnote 22].  
 Bøhren: ". . . the high generality and the correspondingly low precision in Kuhn's model causes ambiguity right at the conceptual level. This is a well-known problem with Kuhn's approach. For instance, Masterman . . . noticed that Kuhn initially used the term paradigm in at least twenty-one different ways" [Bøhren, 1990, page 26].
120. Shapere: "The term 'paradigm' thus covers a range of factors in scientific development including or somehow involving laws and theories, models, standards and methods (both theoretical and instrumental), vague intuitions, explicit or implicit metaphysical beliefs (or prejudices). In short, anything that

allows science to accomplish anything can be part of (or somehow involved in) a paradigm" [Shapere, 1964, page 385].

121. Both Pheby and Chase cite Shapere's article on this point [Chase 1983, page 813, note 14; Pheby, 1988, page 43].
122. Caldwell, notes that Kuhn, speaking with regard to his use of paradigm, "pleads guilty to the charge of vagueness on this point" [Caldwell, 1982, page 75]. Chase also notes Kuhn's admission that he employed "paradigm" ambiguously in his first edition of *The Structure of Scientific Revolutions* [Chase, 1983b, page 813]. As does Blaug: "The second edition of *The Structure of Scientific Revolutions* admitted to terminological imprecision in the earlier version . . ." [Blaug, 1976, page 152].
123. All of the above at least acknowledge that wide controversy exists surrounding Kuhn's varied use of "paradigm."
124. Argyrous: "The treatment that Masterman's paper has received is rather perplexing. In the negative aspect of her article, she details the considerable ambiguity in Kuhn's notion of paradigm in the first edition of *Revolutions*. However, Masterman's objective in doing this was to eliminate the ambiguity and focus on the particular notion of paradigm-as-exemplar. Critics have ignored this positive part of her paper and employed the negative aspect to criticize Kuhn's work" [Argyrous, 1992, page 233, footnote 2].  
Argyrous' understanding of Masterman's interpretation of "paradigm:" "Masterman describes a paradigm as 'a concrete "picture" of something, A, which is used analogically to describe a concrete something else, B . . . a known construct, an artifact'" [Argyrous, 1992, page 233, quoting Masterman, 1970, page 78].
125. Without assessing Kuhn's success, Caldwell as well notes that Kuhn sought to clarify his understanding of paradigm by introducing the twin notions, "disciplinary matrix," and "exemplar" [Caldwell, 1982, page 75]. Redman does likewise [Redman, 1991, page 23, note 9].
126. Chase: "It is because of this evolution in Kuhn's thought on such a critical issue as the concept of the paradigm that it is useful and even necessary to re-examine this idea along with its essential relationship to the overall line of argument developed by Kuhn" [Chase, 1983b, page 814].
127. Coats: ". . . the paradigm concept . . . may be interpreted as a specific book or style of exposition, a 'basic theory,' a *Weltanschauung*, or the entire range of scientific activity. Fortunately, this definitional difficulty creates no fundamental problems for the social scientist for he is much less concerned with instruments, apparatus, and applications of theory than the natural scientist, and in the social science context a paradigm may be defined as a 'basic theory'" [Coats, 1969, pages 291-292].
128. O'Brien: "Firstly, although the concept of paradigm has come under considerable attack from philosophers -- one of Kuhn's supporters (Masterman, 1970) handed a great weapon to his opponents by painstakingly passing a period spent in hospital recording twenty-one different uses of the term by Kuhn in his book -- it is actually a very helpful one. It can be seen to be useful when we consider a paradigm as a pair of spectacles through which we see the world.



This is the interpretation suggested by Kuhn's discussion of the concept in connection with Gestalt psychology" [O'Brien, 1976, pages 141-142].

129. Foster-Carter: "Exactly what a paradigm is has been one of the most contentious issues in Kuhn's work: his critics have stressed its ambiguities, and even a sympathizer claims to find twenty-one different uses of it! Nonetheless it is perhaps fairly clear what *sort* of thing it is: a pre-theoretical entity, a set of domain assumptions which in a very strong sense serve to define the field of study. At least in the natural sciences, it is often constituted by an 'exemplar': a key piece of research and/or discovery, which on the one hand explains or solves an important problem more satisfactorily than any previous attempt, and which on the other hand can never be so cut-and-dried that it fails to leave 'puzzles' that still need solving" [Foster-Carter, 1976, pages 168-169].
  
130. Reder: "Were it not for fear of becoming involved in side issues, I would have suggested that Chicago economics is a scientific sub-culture in the Kuhnian sense, and spoken of the 'Chicago Paradigm' (or family of paradigms), or of the 'Chicago Scientific Research Program (*pace* Imre Lakatos), rather than the Chicago View.'  
 "Let me elaborate: initiation to the Chicago sub-culture is through a rigorous training program in which failure is for many a distinct possibility, and placement in a well defined pecking order a concern of all. Success is achieved by mastery and application of certain tools and concepts to obtain correct answers to analytical problems (Kuhnian puzzles). . . ." [Reder, 1982, page 19].  
 Ward: "One of Kuhn's favorite terms, 'paradigm,' came in for a good deal of criticism as being used in many different senses in the work, a criticism which Kuhn has accepted. For this reason the term is avoided, for the most part, in this work, the principal substitute concepts being world-view, and Kuhn's own 'network of commitments' and 'puzzle'" [Ward, 1972, page 248, note 1].
  
131. Ekelund and Hébert: "The explanation is satisfactory (and even appealing) on the surface, but some gnawing problems remain to be worked out. For example, how is a paradigm in economics to be properly identified? What exactly is a 'body of interrelated principles?' . . . the question of *identifying* a paradigm is still at loose ends. . . . Are there paradigms within paradigms? How is an interrelated system to be interpreted? Much remains, in other words, to be worked out in applying Kuhn's interesting thesis to economic thought . . ." [Ekelund and Hébert, 1983, pages 9-10].
  
132. Zinam: "economic paradigm . . . is defined as a system of mutually consistent basic assumptions about the economic universe under study and about the nature, purpose, scope, methods and significance of economic science shared by a community of economists at any historical period. This definition is based essentially on Kuhn's term, *though it is much more specific than his rather vague definition*" [Zinam, 1978, page 157, emphasis added].
  
133. Tribe: "Masterman (1970) identifies twenty-one variants of the term 'paradigm,' and then reduces these to three basic types. However, this apparent proliferation of the term is a result of confusing *concepts* with *words*: many of the alternatives identified by Masterman are not connected with Kuhn's concept, but are symptoms of an attempt to think an epistemological concept through the

confines of a sociological one. She thus takes what is in fact a symptomatic proliferation of variants as an elaboration and deepening of the term.

"Kuhn can be said to use the term to designative '. . . universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners'. That is, the major point is that it is something that the community *shares*, it is this that makes them a community. These are the unities that exist within a discontinuous (simple) chronology of the history of a science. However, while this unity-discontinuity is an advance in terms of the history of sciences, Kuhn is unable to develop the concepts to correctly specify them; the couple remains at the level of an *image*, and is not theorised. . . ." [Tribe, 1973, pages 468-469, quoting Kuhn, 1970c, page viii].

134. Commenting upon Kuhn's concession that he employed "paradigm" ambiguously and the adjustments he offers to the understanding of the term, Blaug comments, "These are not fatal concessions for the simple reason that the distinctive feature of Kuhn's methodology is not the concept of paradigms that everyone seized on, but rather that of 'scientific revolutions' . . ." [Blaug, 1976, page 153].
135. Stanfield: "It should be noted that Stigler's 'main quarrel with Kuhn' is the lack of sufficient specification of the term *paradigm* to allow empirical testing. . . . However, *paradigm* is a slippery concept, and there is substance to Stigler's point, even though one does not quite know how to interpret 'empirical testing' after reading Kuhn" [Stanfield, 1974, page 106, note 3].
136. Miller continues, "The definition here adopted unquestionably would be included prominently among those proffered in all of the various interpretations" [Miller, 1991, page 1003, note 2].
137. Here, Baumberger hypothesizes that Kuhn's ambiguity helps to explain, in part, the numerous misinterpretations of the philosopher by economists [Baumberger, 1977, page 1].
138. We here consider only one understanding of scientific revolution identified by many economists: paradigm change. We consider other characterizations in Chapter Five.  
Mark Blaug, however, eschews the understanding of a scientific revolution as simply a paradigm change. What distinguishes a Kuhnian scientific revolution is not a paradigm change (or shift), but the high degree of difficulty in scientists' understanding one another and the irrational basis upon which a change is made. Thus, Blaug's understanding of a scientific revolution helps explain why he, unlike those like Kunin and Weaver and Reynolds discussed below, find Kuhn's ambiguous use of paradigm as unproblematical [Blaug, 1976, page 153].
139. Kunin and Weaver do not explicitly state Bronfenbrenner's or Stigler's position vis-a-vis the level of generality of a paradigm. Further, it is unclear whether those who regard a paradigm as less general or abstract would see more (or fewer) scientific revolutions than those who view a paradigm more generally. Basic tools and theories may stay the same, while the overarching worldview in which they operate may alter dramatically. On the other hand, a science's set of tools (such as experimental and/or statistical methods) may change a great deal while the ontology/worldview remains constant. Thus we are left not only with the notion that those viewing the history of economics as Bronfenbrenner

does would identify more (or fewer) scientific revolutions. We open the door to the possibility that each group may identify a scientific revolution that the other does not, and vice versa. Unfortunately, Kunin and Weaver elaborate on neither the more general question, nor on Bronfenbrenner's or Stigler's position in particular.

140. Jalladeau: "According to Coats, political economy has been dominated historically by only one paradigm: 'the theory economic equilibrium via the market mechanism.' For Joseph Spengler, only the Keynesian revolution can pretend to paradigmatic investiture. The difference of opinion is probably explained by the ambiguity of the Kuhnian notion of paradigm" [Jalladeau, 1978, page 592, quoting Coats, 1969, page 292].
141. Bronfenbrenner concurs with Stigler: "I accept Stigler's claim that, in the absence of operational definitions of 'paradigm' and 'catastrophe,' 'Kuhn's assertion that a crisis is necessary for the emergence of a new paradigm is virtually a tautology'" [Bronfenbrenner, 1971, pages 140-141, quoting Stigler, 1969, page 223].
142. Immediately after having noted the problem involved at the *sub*-paradigm level, Gordon declares, "But economics has never had a major revolution; its basic maximizing model has never been replaced" [Gordon, 1965, page 124].
143. Reynolds: "The way in which we define a paradigm for economics determines the changes we categorize as 'revolutions.' It is quite possible that Bronfenbrenner was defining the paradigms for economics narrowly. Hence, he saw revolutions which were actually only reformulations of the basic paradigm" [Reynolds, 1976, page 26].
144. Such an assertion, however, still begs the questions: What constitutes a change in the purposive functions? and What is/was economics P-F?
145. Purposive functions are, according to L.E. Johnson, also present in the natural sciences. He, however, conjectures that Kuhn either ignored their importance or assumed that a single purposive function ("to discover the 'laws of nature'") has existed throughout the history of the natural sciences. Either explanation presents difficulties for Kuhn's theories of scientific revolutions and progress. [Johnson, 1983, pages 1100-1101].
146. Here, Ramstad quotes Wesley Clair Mitchell. Ramstad cites as his source for the quote a former Mitchell student Charles A. Gulick [Ramstad, 1989, page 772, note 3].
147. Ramstad: "Returning again to the topic of economic paradigm, I propose in light of the above that in economics 'paradigms' should be understood to connote the constellation of *shared* beliefs and practices evident in the joint endeavor of a 'community of inquiry' ('invisible college') to identify and promote 'good' institutional adjustments ('wise' policy)" [Ramstad, 1989, page 764].
148. Johnson and Ley see the economics paradigms they identify (Classical, Neoclassical, and Keynesian) as sharing many common elements in terms of both method and conceptions of the world (e.g., the deductive method, the assumption of rationality) [Johnson and Ley, 1990, pages 139-144]. They,

however, differentiate each paradigm from the other in terms of the different P-Fs they possess [Johnson and Ley, 1990, pages 121 and 132].

149. L.E. Johnson: "Kuhn suggests four basic paradigm characteristics. In our terminology, these are: (1) fundamental theoretical assumptions; (2) methods of analysis and focal variables; (3) basic issues; and (4) professional relationships. . . . The first three of these characteristics seem as applicable to economics as Kuhn thought they were in defining paradigms in the physical sciences. The fourth characteristic, which received considerable attention from Benjamin Ward and others in their attempts to demonstrate the scientific nature of economics, has no analytical content and is probably not crucial to defining an economic paradigm. However, an understanding of shifts in professional relationships might shed light on the process of paradigmatic change" [Johnson, 1983, page 1099].
  
150. Describing his model in terms of four quadrants, Zinam includes only the first two quadrants of his master paradigm under the rubric, "paradigm proper," which contains "the concept of economics as seen by economists and the basic assumptions of economists about economic systems." He specifies two additional quadrants; one of these two bears upon professional relations (power relations in particular). Zinam's paradigm's third dimension pertains to "real, objective conditions of a socio-economic system," referring here to the broader society within which the profession finds itself [Zinam, 1975, pages 470-473].
  
151. L.E. Johnson: "the present version of the Kuhnian framework provides a useful resolution of the controversy concerning evaluative criteria. Once the basic characteristics of a particular paradigm are properly defined, its body of economic analysis can be evaluated with reference to the standards and practices of normal economic science representative of that paradigm. . . . the modified paradigm approach guards against the A-O error of judging past theory by modern standards" [Johnson, 1980, pages 58-59].  
 As we see from this quote, Johnson actually refers not simply to objectivists, but to "economic-analysis objectivist[s]" (A-O). The A-O not only conceives of economics progressing from error to truth; he also focuses his examination upon economic theories, as opposed to policy or institutions [Johnson, 1980, page 53].
  
152. L.E. Johnson: ". . . only those aspects of the general context that are necessary to define a particular paradigm need be analyzed. The general context need not be studied in depth for its own sake" [Johnson, 1980, page 58].  
 Here again, we should note that Johnson speaks not simply of relativists, but of the "economic doctrine relativist," who not only seeks to examine economics on its own terms, but stresses the influence of economic practice and institutions upon economics.
  
153. Deane: "The upshot of the debate on Kuhn's theory of scientific revolutions seems to be that there is a strong element of "rhetorical exaggeration" in his concept of a paradigm which fully determines both the world-view of practising scientists and the research agenda of 'normal' scientific activity . . . " [Deane, 1978, page xii, quoting Toulmin, 1972, pages 105-106].
  
154. Hausman does find that Kuhn's disciplinary matrix "provides a useful checklist of what to look for in examining the large-scale structures of economic theorizing" and further enumerates, at least in part, three of the four elements

in economics. As "*metaphysical or heuristic commitments*," he finds economics instructing its practitioners to make "phenomena" amenable to mathematics and to exclude from examination certain "aspects of human social life, such as emotion, irrationality, or mistakes" when searching for causes. He assents that *exemplars* play a key role in economists' education and identifies as some of economics' *values* its stress upon "mathematical elegance" and slighting of "experimentation, data gathering, and testing," as well as "policy relevance" [Hausman, 1992, page 83-85, emphasis added].

155. Hausman: "The role of the assumptions of the basic equilibrium model or of the fundamental laws of equilibrium theory is not well described in Kuhn's categories" [Hausman, 1992, page 85].
156. Here, we may also note that Joseph Remenyi bases his own model of economic science upon Lakatos' superiority to Kuhn: "The theory of core demi-core interaction is predicated on the belief that Lakatos is a 'better' theory than Kuhn. The theory of SRPs incorporates Kuhn by also focusing on the tenacity of given theory in the face of anomaly and the crucial place of artifact building and refinement" [Remenyi, 1979, page 33].  
 Remenyi then goes on to modify and expand upon Lakatos's theory by introducing subdiscipline analogs of Lakatos's SRP and modelling the interaction among subdisciplines themselves and the discipline's overarching SRP [Remenyi, 1979, pages 33ff.].
157. L.E. Johnson contends that "to ignore the P-F, or to treat it implicitly, is probably an error in the natural sciences." He, however, finds its omission in the study of economics "totally unacceptable" [Johnson, 1983, page 1106].
158. Hymer and Roosevelt: "Thus a paradigm is like a flashlight in that it allows the scientist to shed light on certain questions, while at the same time leaving large areas in the dark. It is our contention that Lindbeck, while pointing with one hand at the right questions, holds in his other hand a flashlight (the economics paradigm) that is shining in the wrong direction.  
 "But the New Left will not tolerate changing the questions. For they have arrived at their questions not, as Lindbeck suggests most economists choose theirs, 'by considerations of available analytical techniques', but through their experience" [Hymer and Roosevelt, 1972, pages 644-645, quoting Lindbeck, 1971, page 22].
159. Emanuel: "[P.D.] Praetz (1976) suggests an *ad hoc* adjustment procedure based on the frequency of being short . . . which suffers from the disadvantage of not being paradigm based; see Kuhn (1970)" [Emanuel, 1980, page 379, footnote 5].
160. On the other hand, we should note that another economist, Donald Green, employs another of Kuhn's concepts (scientific revolution) to vindicate *ad hoc* (understanding the term in the non-pejorative sense) development of models of the Soviet economy [Green, 1977, pages 267-268].
161. Solow does qualify his remarks: "Classical Marxian economics is a different matter, of course, though there is a problem about its relation to the classical paradigm. Anyhow that is another story. 'It is no accident,' as they say, that modern radicals are not much interested in the old man who wrote *Capital*."

They are much more interested in the Hegelian Marx, and the author of the Paris Manuscripts of 1844" [Solow, 1971, page 64].

162. Along similar lines, the social economist Roger McCain replies to those who proffer sociobiology as a new paradigm simply, "That is as it may be" [McCain, 1980, page 125]. He then goes on to lodge fundamental criticisms against the "new paradigm" [McCain, 1980, pages 125ff.].
163. McKee: *"My conclusion is that economics must be headed no more and no less in the direction of a certain re-orientation of conventional economics.* You can, if you like, speak of it as one competing 'paradigm', and the case for taking such a position is surely strong. At present it commands the interest and support of only a small part of the economics profession, and the vast bulk of writing and teaching continues ostensibly, to be cast along positive lines. However, given the problem of the virtual breakdown of positive economics at the present time and its construction of false values, there is for me little doubt that social economics cannot aim at less than some eventual reshaping of conventional economics" [McKee, 1982, page 14].
164. Here we may list Heilbroner, 1971; Eichner and Kregel, 1975; Sweezy, 1971; Bornemann, 1976, and Kapp, 1976.
165. Black similarly asserts that by the end of the nineteenth century, "most of us would feel inclined to accept that classical political economy had established a paradigm" [Black, 1972, page 367].
166. L. E. Johnson: "In examining the work of the classical political economists, several writers have persuasively argued that they were concerned with a group of related issues including the definition and measure of the 'wealth of a nation'; what components of the former represent the 'total social surplus'; the determination and direction of the changing class income distribution pattern; and the implications of that pattern for market-directed capitalist growth and development" [Johnson, 1983, page 1100].
167. Johnson and Ley point out that Classical economics set both a necessary and a sufficient criteria for the maximization of social welfare. Maximization of a nation's wealth served as the necessary condition, while "an ethically acceptable distribution of income . . . among economic classes" constituted the sufficient condition:

First, with some qualification, they argued that social welfare would be directly proportionate to the wealth of the nation, defined as the value of the annual flow of final goods produced by society's productive resources. This definition of the nation's wealth is similar to the modern concept of national product or income minus the value of services. Second, the classical economists argued that total social welfare was maximized only if an ethically acceptable distribution of income existed among the economic classes in society. Thus, maximizing social welfare not only required the maximization of the nation's wealth -- the necessary condition -- but also demanded an ethically acceptable distribution of income among economic classes -- the sufficient condition. [Johnson and Ley, 1990, page 90]

168. Another author, M.B. Harvey-Phillips, speaks of Malthus as employing the "Cartesian circuit as his methodological paradigm" [Harvey-Phillips, 1983, pages 190-191].
169. Canterbury and Burkhardt: "The paradigm of orthodox economics takes its lead from Adam Smith's *Wealth of Nations*. Orthodox economists recognize that, although Smith's work is 'wrong' in spots, it is nonetheless based on a great truth, the self-regulating nature of the market, which marks the book as the initial achievement in economics" [Canterbury and Burkhardt, 1983, page 22].
170. Those three assumptions, according to Samuelson, are " $(\alpha)$  free-entry and widely-shared knowledge,  $(\beta)$  constant-returns-to-scale technology, and  $(\Gamma)$  smooth variability of the  $(L_t, K_t)$  components of the  $V_t$  dose," where  $L$  denotes labor,  $K$ , capital and  $V$  a composite dose of labor and capital, all at time  $t$ " [Samuelson, 1978, page 1423]. Samuelson defends this last assumption as consistent with the Classical system against those who maintain that Classical economics assumes a fixed proportions technology:

Ricardo and Marx were not so naive observers as to believe literally in fixed proportions between capital goods and labor. Their knowledgeable commentaries on current events presuppose recognition that, at certain price and profit rates, substitutions will be made that would not be competitively viable at other price and profit rates. So it is a caricature to insist on fixed-proportion doses,  $V = \text{Min}[L, K]$ . [Samuelson, 1978, page 1423]
171. For alternative specifications of the neoclassical paradigm see discussion below regarding current orthodox economics paradigms.
172. Mehta differentiates the "*extended* quantity theory" of money from its simpler version and notes that the former, more sophisticated form allows for the possibility that velocity may vary and thus that increases in money would lead to less than proportional general price increases [Mehta, 1978, pages 47-48].
173. Likewise, Stanfield asserts that under the view which Keynes introduced into economics "not saving or thrift, but aggregate demand determines the prosperity of a nation and government is given a consistent theoretical rationale for fiscal intervention, and the focus of economists working within the Keynesian framework was upon "the determinants of the level of employment and policies to achieve full employment" [Stanfield, 1974, page 103]. Stanfield speaks of the introduction of Keynesian economics as a scientific revolution in which economics abandoned the Classical vision of the economy and replaced it with the Keynesian vision. However, he never refers to a Keynesian *paradigm*, per se.
174. *Quantity Adjustment*: "The *Treatise* describes a process of contraction or expansion of money income and real income. The saving-investment tool is used to describe this process. If the amount of saving is equal to the investment that is being undertaken, income is in equilibrium. Suppose now that saving exceeds investment. The first effect of this is that the price of investment goods and consumption goods falls. Initially the system responds to a disturbance by a price adjustment. When prices fall, entrepreneurs make losses. As a result of their losses, they will reduce the offers they make to the factors of production, and output and employment fall. In the second stage the system responds by

quantity adjustments. When output falls income also falls. If at this reduced level of income saving still exceeds the cost and value of investment, entrepreneurs will continue to make losses and will reduce their offers to the factors of production. . . ." [Mehta, 1978, page 139].

*The Multiplier:* "Nowhere in the *Treatise* does Keynes say that the fundamental equations must be interpreted in term of *actual* profits and not *anticipated* profits. If we interpret the fundamental equations in terms of anticipated profits a startling result follows. On this interpretation it turns out that the *fundamental equations describe the logical theory of the multiplier*" [Mehta, 1978, page 148].

175. Reinwald, speaking of the high regard with which the paradigm was held, continues: "The independently acting forces of supply and demand, when coupled with a universal profit motive, provided a framework wielding a predictive power similar to that which the Newtonian framework was at one time thought to provide for the physical sciences" [Reinwald, 1977, page 526].
176. We concede at the outset that those paradigms which we include in economics' orthodoxy versus heterodoxy, as well those which we categorize as discipline-wide versus sub-field paradigms are -- as they inevitably must be -- a function of our own outlook on economics. Others examining the literature would, most likely, select different sets of paradigms and categorize them differently.  
In addition, we do not pretend to provide "objective" descriptions of the economists we examine. As are all interpretations, ours is necessarily selective and conditioned by the interpretive framework through which we view the texts we analyze.
177. Gilbert, however, does not directly link the paradigm concept with Kuhn.
178. Gilbert continues: "It is true that game theory has provided a new set of models, although these game theoretic models typically imply fewer restrictions than the atomistic optimisation models which remain the core of the dominant neo-classical or neo-Walrasian 'research programme'" [Gilbert, 1989, page 3].
179. Boland notes that most "followers of Kuhn or Lakatos" "usually" "identify only the maximization hypothesis" in their specification of "the 'paradigm' of neoclassical economic theory" [Boland, 1982, page 7]. The analysis here, however, suggests that maximization is only one among many paradigms which economists locate within their discipline.
180. Further, Winter refers to the "qualitatively correct predictions of substitution effects" as one of the "prominent" "'exemplars' upon which the paradigm is founded" [Winter, 1981, page 31].
181. Ahonen identifies two additional elements of the current economics paradigm which have survived from the classical structure: "1. The atomistic and individualistic reduction of social welfare" and "2. the demarcation of the object of knowledge to those elements of society which have monetary expressions (restriction to price theory with given tastes and technology)" [Ahonen, 1990, page 91].
182. In addition, Brian Loasby argues that *profit* maximization is "not a hypothesis but a paradigm" [Loasby, 1971, page 867, emphasis added].



183. In this regard, we may also mention Reynolds. Within the one and only conceptual framework he finds in economics' history, he includes the conception of "maximizing individuals in a relatively free market" as only one element of the fundamental structure of economics. Reynolds never speaks of the framework as a paradigm; he nonetheless does construct it with reference to the determination as to whether economics has ever undergone a Kuhnian scientific revolution. Reynolds includes four other propositions in his specification of economics' framework: (1) maintaining the existence of a social institution (private property), (2) positing a human motivation (acquisitiveness), (3) championing a normative philosophy (the work ethic), and (4) a broad methodological construct (the mechanical analogy) [Reynolds, 1976, pages 28-29].
184. In this context, we may also list Canterbury and Burkhardt who find orthodox economics to be founded upon Adam Smith's *Wealth of Nations*, which in turn is "based on a great truth, the self-regulating nature of the market" [Canterbury and Burkhardt, 1983, page 22].
185. Blaug asserts that the 'hard core' of economics consists of "weak versions of . . . the 'assumptions' of competitive theory . . ." He lists five elements of the positive heuristic: "(i) divide markets into buyers and sellers. . . (ii) specify the market structure; (iii) create 'ideal type' definitions of the behavioural assumptions so as to get sharp results; (iv) set out the relevant *ceteris paribus* conditions; (v) translate the situation into an extreme problem and examine first- and second-order conditions" [Blaug, 1976, page 161].
186. While not enthusiastically advocating the understanding of equilibrium as an economics paradigm, Ekelund and Hébert note that "If method identifies a paradigm, one could conceivably lump classical, neoclassical, Keynesian, and imperfect competition together as *one* paradigm, that of equilibrium economics" [Ekelund and Hébert, 1983, page 10].
187. Zweig explicitly excludes competition as a characteristic of economics' mainstream paradigm [Zweig, 1971, page 44].
188. Zweig also sees the passive acceptance of wealth and budget constraints by economic actors under the bourgeois paradigm as another example of how the paradigm describes the economy as a harmonious place. Each actor accepts, without significant rancor, his lot:

This harmonious framework extends to the core of microeconomic activity, the process of maximization subject to constraint. For the bourgeois analyst of capitalism, economic man takes constraints as given and finds the best action consistent with those constraints. This suggests a passivity, or at least a resigned peace, with respect to the typical constraints, budget and factor endowment, and with respect to the larger institutional and legal or customary constraints on production and distribution within a capitalist society. Once again, competitive behavior of actors is built on a more fundamental harmony, each person with respect to his own situation. [Zweig, 1971, page 44-45]

189. Sweezy: "Let us now turn to a consideration of the case of orthodox economics. Here it seems to me that the underlying paradigm, along with the

normal science to which it gives rise, can and should be subjected to critical attack on several grounds. As I have already suggested, this paradigm takes the existing social order for granted, which means that it assumes, implicitly if not explicitly, that the capitalist system is permanent. Further, it assumes that within this system (a) the interests of individuals, groups, and classes are harmonious, or, if not harmonious, at least reconcilable; (b) tendencies to equilibrium exist and assert themselves in the long run; and (c) change is and will continue to be gradual and adaptive" [Sweezy, 1971, page 62].

190. Wang cites as two other examples of paradigms in modern day economics, the "dynamic approach" and the econometrics paradigm. See below in discussion of paradigms in economic subfields for further discussion of the econometrics paradigm.
191. Aeppli: "An exact comparison of these specific components (*i.e.*, the symbolic generalizations, the metaphysical parts, the values and the exemplars) shows that they differ so fundamentally that we must speak of three different paradigms. . . ." [Aeppli, 1980, page 708].  
Aeppli explores the paradigm status of these three paradigms and describes each in terms of the four elements of a Kuhnian disciplinary matrix. The article, however, published in *Kyklos* is written in German. We here only cite his conclusions found in the English language abstract to the article.
192. Dow: "Neoclassical economics *does* have a general ideology or paradigm. In its *Weltanschauung* form, it embraces the all-encompassing concept of the Invisible Hand. Its methodological expression consists of deterministic, general equilibrium analysis" [Dow, 1981, page 328].
193. According to Peterson, the neoclassical synthesis "has become the standard postwar textbook interpretation of Keynes" [Peterson, 1977, page 212].
194. Peterson: "The essential features of the neoclassical synthesis are well known. Its starting point is an income-expenditure model, the roots of which are in Keynes's principle of aggregate demand as developed in *The General Theory*. The most widely accepted version of the model was worked out by J.R. Hicks in his famous article, 'Mr. Keynes and the Classics,' in which he sought to show that the economics of *The General Theory* were not much different in their essentials from Marshallian, that is, neoclassical, economics. Money and financial variables are introduced in the Hicks-Hansen model as endogenous parameters affecting the key functional relationships, a development which pushes the analysis back toward the neoclassical notion that the endogenous processes of the system automatically lead to full employment. The neoclassical synthesis is completed when the classical demand and supply curves for labor are added to the model. This, plus the assumption that money is 'neutral' (the equilibrium value of all variables other than the price level is assumed to be independent of the money supply), completes 'capture' of *The General Theory* by the general equilibrium spirit of neoclassical economics [Peterson, 1977, page 213].  
Peterson: "The combination of the real balance effect and the equations of the labor market means that, in principle, the market mechanism will bring about a full-employment equilibrium" [Peterson, 1977, page 213].
195. In addition to the attributes listed above, Peterson also notes, "the formal neoclassical paradigm depicts a world in which the participants are wholly

without power. Firms and consumers, in other words, are quite unable to exert any influence on the outcome of events" [Peterson, 1977, page 208].

196. Here too, we might also mention Bronfenbrenner. He however identifies the neoclassical synthesis as economics' normal science, not its regnant paradigm. Unlike those such as Stanfield, Anspach, and Johnson and Ley, Bronfenbrenner provides no indication that the synthesis is problematical. For Bronfenbrenner, the synthesis constitutes the dual recognition of the importance of imperfect competition in the micro-economy and Keynesian macroeconomics, which repudiates Say's Law and recognizes a need for government intervention into the macroeconomy [Bronfenbrenner, 1971, pages 146-147].
197. Chase defines "autonomy of existence" as the notion "that the object for scientific inquiry can be parametrically defined so that it is at least logically capable of independent function within the given parametric constraints, and, further, that so-called exogenous factors will have known or knowable effects on the system as defined" [Chase, 1983a, page 168].
198. In addition, Baumberger implies that neoclassical economics, given its greater "homogeneity and discipline" resembles a Kuhnian paradigm [Baumberger, 1977, page 10].
199. Swaney and Premus: "the dominant neoclassical paradigm is such a complete and dominating world view that it inhibits inductive development of theories that reflect economic reality. In short, economists all too frequently leap from their oversimplified view of the nature of man and their naive view of the institutional framework as a God-given constant to elegant and sophisticated theories void of constructive policy content" [Swaney and Premus, 1982, page 726].
200. As we noted earlier, Ward avoids the use of the term "paradigm." One of the substitutes he employs for "paradigm" is "framework." He also uses the term "puzzle" as another one. We will discuss the puzzles Ward identifies in mainstream economics in the next chapter.
201. Ward's identification of the "harmony of interest thesis" in neoclassical economics' framework, of course, resonates with those who argue that the assumption of harmony among economic actors is an essential element of mainstream economics' paradigm.
202. Cornwall notes however that "the competitive assumptions are not usually taken to hold literally. Rather, it is generally implicit that conditions will deviate from this simplified situation to varying degrees" [Cornwall, 1979, page 71].
203. Cornwall: "When neoclassical economics is viewed as a whole, the neoclassical paradigm is seen to include the following assumptions: applicability of the model of a competitive economy (including perfect knowledge of past, present, and future events and complete mobility of factors); constant tastes and technologies (or else changes in these elements that are exogenously determined and predictable); consumer and worker sovereignty; very simple and well defined functions describing the objectives of consumers, workers, and producers; equally well defined constraints on these objectives; and 'rational' behavior on the part of all economic actors in the sense that the means they

utilize to achieve stated ends or objectives are always the most efficient ones" [Cornwall, 1979, pages 71-72].

204. See above under discussion concerning the relationship between the Classical and Neoclassical paradigm. DeVroey never explicitly asserts that the neoclassical paradigm remains economics' regnant paradigm. However, a line towards the end of his discussion implies that neoclassicism remains economics' prevailing paradigm: "This [political] inoffensiveness was and *is*, from the viewpoint of the capitalist ruling class, the main quality of the neoclassical paradigm" [DeVroey, 1975, page 435, emphasis added].
205. According to Argyrous, the Life Cycle Hypothesis provided the micro based explanation for the inverse relation by pointing out that people's incomes are lower earlier and later in life and higher in their prime. People, seeking to smooth consumption over their lifetime, will borrow when young, save in their prime and dissave when old. Thus, the inverse relation. Friedman explained the inverse relation by positing that people consumed out of permanent and not transitory income and that the proportion of transitory income increased with income. Thus, those with lower incomes and higher proportions of their income being permanent would spend a greater portion of each income dollar than those with high incomes, of whose income a smaller portion was permanent [Argyrous, 1992, page 236-238].
206. As we acknowledged at the outset of our discussion regarding current-day economics paradigms, we must here again point out that the schools we identified and the paradigm discussions we included under each of them is a function of our own interpretive framework. In particular, many might question our inclusion of public choice in the present discussion -- as opposed to either discussion of mainstream paradigms or possibly subfield paradigms in economics.
207. For Black, Public Choice, which seeks to examine economics in a non-normative fashion, represents the one exception [Black, 1983, page 65].
208. Black remarks that while the New Left, like Marxism, seeks to stress that income distribution is the artifact of sociopolitical forces, the "New Left economists have often stressed the interaction of political and economic factors in a broader sense than this. They have pointed to relations of domination and servitude both within and between economies and underlined the conflicts and disruptive changes to which they lead" [Black, 1983, page 58].
209. Black: "The use of rational choice analysis might be said to be one of the hallmarks of neoclassical economics, and in one sense the main achievement of the public choice school can thus be said to be the extension of neoclassical theory. By contrast, the post-Keynesians have little use of the neoclassical approach and in particular have urged that it is inappropriate to combine the short-period theory of Keynes with neoclassical long-period analysis" [Black, 1983, pages 60-61].
210. Black: "The main difference between Lowe's political economics and Winch's political economy seems to lie in the relation between the state and other major economic agents, such as corporations and trade unions. For while Winch would wish political economy to consider means of restructuring these 'so as to decentralise responsibility as well as power,' in Lowes's political economics the

purpose would be simply to plan the appropriate conditions for achieving the politically chosen goals, which might seem to imply at least the possibility of even greater centralization and control of economic power" [Black, 1983, page 60-61].

Still further from Black: "there is a vital difference between recognizing and observing values, including the recognition that the observers own values enter into the analysis, and promoting an ideology which involves a specific teleological view of the politicoeconomic process. In this respect Marxists, whether of the old or new left, cannot appropriately be placed in the same category as, say, institutionalists. To them the view of political economy as an intersection of two sets of ideas, economic and political, might be acceptable as a statement of its *present* position but not of its prospects" [Black, 1983, page 65, emphasis added].

211. Solow: "Here are some examples of what I mean. Professor [John] Gurley says: 'As radical economists see it, the shares of national income going to workers and to property owners are largely determined by the relative power of the two groups, although relative factor supplies set limits within which the power exerts itself.' Am I to presume from this that there are studies of time series that show that short-run fluctuations in distributive shares reflect short-run fluctuations in the distribution of power in society? This would mean that workers are more powerful when there is a lot of unemployment than they are when there is very little, because the share of wages is highest when the economy is most depressed. Or has it been found in many countries that the direction of long-run change in distributive shares corresponds to the long-run trend in the independently measured distribution of power in society? Or are there perhaps cross-section studies among industries showing that the share of wages in value added is highest in industries where the power of the workers is highest, and lowest where the social power of capitalists is highest? Or is it demonstrable that the international differences in functional income distribution correspond more or less closely to international differences in the relative power of workers and property owners?" [Solow, 1971, page 64, quoting Gurley, 1971, page 59].
212. Along the same lines Howard Wachtel describes the radical paradigm as "emerging" [Wachtel, 1972, page 187].
213. Speaking with respect to another school of economics, Social Economics, George Rohrlich maintains that the school "*may* become a new economics paradigm" if it "turns out to possess greater explanatory value" and provides superior solutions to vexing social problems than the established paradigm [Rohrlich, 1977, page 333].
214. Zweig notes that the "best known elaboration of a paradigm of conflict is Marxist thought." However, Zweig locates as the source of conflict not market competition -- as he asserts Marx did -- "but in relations between classes of people; for instance, owners and non-owners of means of production. Since competition in the market is a feature of capitalism with which both neo-classical economists and Marxists deal, it is not a distinctive feature of either paradigm. The distinction arises in the different social and inter-personal context in which that competition proceeds" [Zweig, 1971, page 49].
215. Eichner and Kregel, like many economists championing heterodox paradigms, affirm that "post-Keynesian theory has the *potential* for becoming a

comprehensive, positive alternative to the prevailing neo-classical paradigm" [Eichner and Kregel, 1975, page 1294, emphasis added].

216. Cornwall: "The post Keynesian paradigm is described by a vastly different set of metaphysical beliefs and assumptions, values, and choice of techniques and problems. For example, uncertainty of the type that cannot be reduced to certainty equivalents is assumed, along with certain kinds of market imperfections that give rise to a lack of perfect mobility. Further, while it might be recognized that there are certain parts of the economy where some of the competitive assumptions hold -- for example, where some firms act as price takers -- the post Keynesian paradigm shifts emphasis toward the recognition of monopolistic and oligopolistic elements and other forms of market imperfections. A large part of the economy is composed of firms that are price makers, which price on a markup basis and not in terms of the strength of demand pressures . . . Moreover, in many cases, prices are not the information most sought by the economic actors involved, nor do changes in relative prices necessarily act as signals leading to a reallocation of resources and production in such a way that markets are (rapidly) cleared. This paradigm places much greater emphasis on changes in flows and stocks (e.g., job vacancies and inventories) as the most important information signalling the need for adjustments in the coordination of economic activities. In a related way, the post Keynesian paradigm does not assume that full employment of all factors of production is continuously maintained" [Cornwall, 1979, pages 73-74].
217. Peterson: "It can be argued that both Keynesian economics and institutionalism are 'paradigms' in the sense in which Kuhn uses this term, although in candor one must admit that it is easier to press Keynesianism into this mold than to do the same for institutionalism.  
"This is so because, as we all know, there is no precise unified body of theoretical ideas which we can readily and clearly identify as institutionalism. Perhaps this is its most fundamental characteristic. . . . The vagueness of its boundaries and the diversity of its content mirror accurately the wide-ranging interests, the insatiable curiosity and probing, and the free-wheeling spirit of those who have made enduring contributions to this branch of economics" [Peterson, 1977, pages 202-203].
218. Peterson: "First, conventional economic analysis contains powerful normative elements, all too frequently hidden from view, and institutionalists believe that these should be brought into the open and made explicit. In institutionalism there is a healthy skepticism as to whether economics can be value free" [Peterson, 1977, page 203].
219. Peterson: "Finally, the methodology of institutionalism is one which blends both induction and deduction and which is strongly oriented toward developing the pragmatic economic knowledge needed for solving the practical problems of this world" [Peterson, 1977, page 203].
220. Strictly speaking, Miller speaks not of the instrumental *paradigm*, but of the instrumental *economist*. However, she draws this conclusion in her article's final section, "The Instrumental Paradigm" [Miller, 1991, pages 997-1001].  
Further, Miller asserts at the outset, "My reasoning is related to my perception of the prevailing paradigm in the economics discipline, using the term 'paradigm' in its Kuhnian sense" [Miller, 1991, page 993].

221. Klein: "One way of differentiating mainstream economics from institutionalism is to note that if resource allocation is regarded as the central 'puzzle' of economics, the mainstream 'solution' is reliance on the market. Institutionalists, on the other hand, regard the market as a mechanism along with others that must themselves be judged in light of emergent societal values. It may be, therefore, that mainstream economists and institutionalists can be differentiated not just in their solutions to the puzzle, *but by their very formulation of what it is that constitutes the puzzle*. If this is correct, it seems clear that Commons, Veblen, and Mitchell, did indeed have a consistency in their view of what it is that is the task of economic theory" [Klein, 1990, page 385, emphasis added].

222. Having asserted that institutionalism constitutes a single paradigm and describing it, Klein concludes that institutionalists' time would be better served by getting on with the work at hand than it would be by debating the contents of its paradigm:

In my judgment, it is probably time to pay less attention to endless efforts to restate the principles of institutionalism, or to define its 'paradigm' (although some attention to 'basic theory' is always appropriate). We should spend more time applying our approach to contemporary economic analysis in the effort to make economic policy work more effectively. If we do this successfully, what we are and what we believe, as well as who we are collectively, will take care of itself. [Klein, 1990, page 387]

223. Ramstad: "In other words, referring to Commons's paradigm as the Reasonable Value Paradigm, it is my contention that the Instrumental Value Paradigm and the Reasonable Value Paradigm are in actuality *competing* paradigms, that is, substitutes" [Ramstad, 1989, page 765].

224. Ramstad attributes the mistaken notion that institutionalism consists of a single paradigm to the tendency to define institutionalism in contrast to the economic orthodoxy:

Institutional economics has always been discussed, by adherents and opponents alike, in the context of whether or not and how it constitutes an alternative to 'orthodox' or 'neoclassical' economics. As a result, the attempt to characterize institutional economics is almost without exception structured in more or less the following manner: Neoclassical economics is arguably a 'scientific paradigm' with well-known principles and a definite theoretical structure; what, then, are the central principles of *the* alternative and what is the structure of *its* theory?

In fact, institutional economics writ large has never been reducible to a single paradigm, or a consistent body of theory. . .

. . . To be in agreement as to which side of the divide to stand on regarding these issues is *not* to share a paradigm or a common theory. [Ramstad, 1989, pages 770-771]

225. Johnson and Ley thus appear to link paradigmatic status only to economics' orthodoxy.

226. Johnson and Ley: "Institutionalists are skeptical of the market's ability to insure social harmony. Instead, they view contemporary society as composed of competing interest groups. In this setting, any initial advantage gained by one group makes it easier for that group to gain further at the expense of others. . . .  
"Abstracting from the richness and diversity of institutionalist thought, all these writers had a common belief in the need for the state to supplement markets in order to assure a tolerably just and efficient society" [Johnson and Ley, 1990, page 145].
227. Johnson and Ley, who describe institutionalists as heirs to the historical school, assert that "the second major criticism coming out of the historical school tradition focuses on the methodology of mainstream economics. While deductive models have been used in mainstream economics for several centuries, historicists believed that such deductive, abstract theories are of limited use. . . . As an alternative to the orthodox position, they advocated primary reliance on inductive analysis" [Johnson and Ley, 1990, page 146].
228. However, Thomas Willett does allude to an incipient scientific revolution in international economics -- which he equates with a paradigm shift [Willett, 1970, page 449].
229. Although we have not found paradigms located in these subfields, we do find use of some Kuhn concept or another in each of these sub-disciplines, with the exception of law and economics.
230. Streissler: "I myself am, however, a firm adherent of Thomas Kuhn's 'The Structure of Scientific Revolutions' as the appropriate 'paradigm' (with certain variations) for understanding particularly the history of economic thought. The various epoches of a science differ, according to Kuhn, above all as to the questions addressed in each period, the vision of the world, the fundamental concepts most likely to be appealed to, the decision what is to be core and what fringe in a theoretical framework" [Streissler, 1991, pages 391-392].
231. Noting that the hypothesis has been variously described, Jensen cites as "the simplest and most general way to express it": "A market is efficient with respect to information set  $\Theta_t$  if it is impossible to make economic profits by trading on the basis of information set  $\Theta_t$ " [Jensen, 1978, page 96].
232. Michael Keenan, though not linking the paradigm concept directly with Kuhn, identifies another paradigm within financial economics: the paradigm of equity valuation which "suggests that security prices can be functionally related to expectations about firm financial variables" [Keenan, 1970, page 244].
233. Wagner and Weber continue, "Furthermore, this paradigm argues that governmental services sometimes, and within limits, create a demand for further services. Within this perspective, differences in spending patterns would reflect, among others things, differences in political institutions" [Wagner and Weber, 1977, page 66].  
Wagner and Weber never explicitly refer to theories of government as paradigms. They do however reference their discussion about such theories to Kuhn's understanding of the necessity of paradigms. Furthermore, the article's abstract explicitly refers to the need for "alternative paradigms" -- not simply theories [Wagner and Weber, 1977, pages 59 and 66-67].



234. Interestingly, Piore describes Kuhn's theory of scientific revolutions itself as belonging to the structuralist paradigm [Piore, 1983, page 253].
235. Bennett Harrison and Andrew Sum do not link Kuhn directly with the paradigm concept, but do associate the philosopher with paradigm debates. Further, they, while only intimating a paradigm status for both the human capital and segmented labor market theories, do seek to delineate the "paradigmatic contrasts" between the two theories. The former focuses upon an individual having free choice, while the latter conceives of "'groups or classes who face objectively different labor market situations which systematically condition their 'tastes,' and restrict their range of effective choices.'" Further, while "'orthodox models [take] institutional parameters [as given] . . . The segmentation theories attempt to explain the development of the institutions themselves as the result of interactions of groups or classes of individuals with objectively different interests'" [Harrison and Sum, 1979, page 695, quoting Carnoy and Rumberger, 1977].
236. Here, we may also note that while making no reference to Kuhn with relation to the paradigm concept, Forte identifies and describes the "Alchian paradigm," which, at bottom, maintains that firms best adapted to their environments survive and prosper [Forte, 1982, page 227-228].
237. Boland: "The fact that we consider alternative theories (of the firm or of the consumer) means that the standard theory is *not* a paradigm -- no matter how standard" [Boland, 1977, pages 98-99].
238. Foxall: "The delineation of an extant paradigm is a hazardous task: historians of science have not always agreed upon the scope and content of completed research programmes, let alone ongoing ones. Nevertheless, the predominance of cognitively-based explanations in consumer research is undeniable. . . . It is proposed to refer to this perspective as the cognitive information processing paradigm (CIPP)" [Foxall, 1986, page 395].
239. Foster-Carter cites as the salient features of Frank's paradigm: "the new paradigm stresses the interconnectedness of development and underdevelopment, of 'traditional' and 'modern,' and indeed of everything in general (the concepts of 'totality' and 'dependence'); it sees many conflicts and clashes of interest in the development process, both between nations and between social classes within underdeveloped countries; it stresses historical factors, specifically the active creation or 'development of underdevelopment'; it speaks of 'imperialism' and 'capitalism,' of 'social formation,' 'mode of production' and 'class,' in the language of Marx; it sees development as a revolutionary break rather than a continuing evolution from the present; and it advocates socialism" [Foster-Carter, 1976, pages 174-175].
240. Randall does not provide clearly delimited definitions of each of these paradigms; nonetheless, we may cite some the major notions which he associates with each of the paradigms. The neoclassical/rational planning paradigm adheres to the "naivete" of "market failure/government fix" and "attempted an intellectual justification for welfare state and regulated economy policies" [Randall, 1985, page 1022]. The public choice/utilitarian paradigm has its roots in the works of Coase, Buchanan and Tullock and Posner and "applies utilitarian (e.g. benefit cost) criteria to identify the waste-minimizing configuration of imperfect markets and imperfect government institutions"

- [Randall, 1985, page 1023]. The institutionalist/land economics paradigm adheres to a methodology which champions "a methodology of pattern modeling, storytelling, and holism" and regards society as more than simply the sum of its parts [Randall, 1985, page 1023]. Finally, the public choice/individualist paradigm degrades the worth of empirical work, "denies utilitarian (e.g., benefit cost) public choice criteria because they are collectivist" and "insists that benefits and costs can be known only subjectively and cannot be read from market-generated data" [Randall, 1985, page 1025].
241. We may as well include Hubert Law-yone and Rachel Wilkansky who examine paradigm change in economic development planning in Israel [Law-yone and Wilkansky, 1984, pages 367-372]. As with Willett in international economics, these authors imply by the use of the notion of paradigm the existence of paradigms within Israeli economics development planning without clearly specifying the paradigm. Further, they leave open the question whether the history of planning in Israel has seen a single paradigm or four different paradigms [Law-yone and Wilkansky, 1984, page 371-372].
  242. Heilbroner cites as another reason, economists' political stance: "let me broach a second reason for the lack of interest of professional economists in 'political' political issues. It is quite simply that economists are mainly of conservative orientations, and therefore do not *wish* to investigate these questions" [Heilbroner, 1971, page 4]. In this same vein, we should note that Worland explicitly notes that Kuhn's understanding of the role of paradigms provides a non-ideological explanation for economists' ignoring social problems [Worland, 1972, page 275].
  243. O'Brien: "Now it certainly fulfilled one of Kuhn's paradigm functions -- that of insulating scientists from important problems; and we can see this exhibited very clearly in Ricardo's discussion of tax matters in which he managed to avoid altogether dealing with the burning tax question of his day, the income tax" [O'Brien, 1976, page 142].
  244. See THE APPLICABILITY OF KUHN'S PARADIGM CONCEPT TO ECONOMICS above.
  245. Similarly, we note that Bird maintains that central place theory in economic geography "has fulfilled the role of a dominating paradigm, with the usual blinkering . . ." [Bird, 1983, page 196].
  246. See below under, *Train and Indoctrinate Economists*.
  247. Vickers: "The fruits of the actions and decisions that individuals take depend on what happens in the future spread out before them, a future determinable to a significant degree by their own actions. But at the decision points that future is unknowable and decisions must be made -- are necessarily made -- in conditions of ignorance. It is this, in turn, the realities of ignorance, which cannot be brought within the general equilibrium-theoretic scheme of things and which, as a result, that paradigm must abolish" [Vickers, 1983, page 254].
  248. Without paradigm discipline, Stanfield contends "Scholarship tends to lack focus and published works do not interact and build upon one another in the time-honored fashion of the scientific, artistic enterprise. In short, there is no Kuhnian normal science with its advantages of discipline, focus and direction" [Stanfield, 1989, page 178].

249. Solo defines the "rule of paradigm" as the principle that holds as legitimate "only statements that derive from, are commensurable with or incremental to the established assumptions, theories, models and analytic techniques that constitute the established paradigm can be admitted into the discourse" [Solo, 1991, pages 79-80].
250. Reinwald: "While marginal revenue is not essential in determining equilibrium solutions per se, it is proposed that its use would have served to elucidate other aspects of Chamberlin's work in 1927, and, in fact, did do so in 1933. In particular, it is proposed that Chamberlin would have abandoned his nonsensical supply curve in 1927 had marginal revenue been at his disposal. And it is very likely that this 'bit of technical equipment' was responsible for the abandonment of the supply curve in 1933" [Reinwald, 1977, page 528].
251. Petridis' interpretation suggests that Marshall retained the competitive framework also because he came to believe that competition ultimately conduced to the social good and grew more and more dubious about the ability of trade unions to ameliorate society (and more concerned about their potential for harming it) [Petridis, 1973, pages 187ff.].

## **CHAPTER FOUR: NORMAL SCIENCE**

We next turn our attention to an examination of economists' interpretations and applications of Kuhn's normal science concept. Here, our discussion is in three parts. First, we examine economists' characterizations of normal science in which we identify the chief attributes which economists associate with normal science. We then provide a brief discussion of examples of normal science that economists have located in economics' past. Finally, we turn to economists' examinations of normal science in present-day economics. Here, we survey approximately thirty individual economists' depictions of normal science in economics. We find that economists' descriptions of past and present normal science, as well as their determinations as to whether economists were/are engaged in the practice of normal science, parallel economists' characterizations of normal science in general. However, we find disagreements among economists as to whether economics (or some part of it) comprises "normal science." Finally, we find that while some economists laud the practice of normal science in economics, most writers on the subject are highly critical of normal science (or normal science-like) activity in economics.

### **A. DEFINITIONS OF NORMAL SCIENCE**

Turning first to consideration of economists' characterizations of a Kuhnian normal science, we find that economists have highlighted a number of largely interrelated attributes of normal science. However, as we shall see, economists differ as to the relative emphases they place upon some of these aspects.

## 1. *The Practical Application Of Theory*

Some economists ally Kuhnian normal science with the practical application of theory. Colander implicitly allies normal science with his own notion of the "realistic method." Practitioners following this method seek to be consistent with theory, but are very willing to sacrifice desirable characteristics of a theory in order to advance its practical application.<sup>1</sup> Similarly, Thomas DeGregori draws parallels between normal science and work designed to draw out the technological implications of a theory, such as in the development of navigational methods arising from Copernican astronomy.<sup>2,3</sup>

We do find, however, economists who take explicit exception to this characterization of normal science. According to Connor, "Kuhn's normal science . . . excludes what engineers and technicians do; scientists(i) [normal scientists] think of that as 'hackwork'" [Connor, 1991, page 59].

## 2. *Puzzle Solving*

Many economists describe normal science as "puzzle solving." Some make the point rather succinctly. Tribe, for instance, asserts that "In SSR [*The Structure Of Scientific Revolutions*], 'real science' is 'normal science,' that is, it is constituted by the activity of 'puzzle-solving'" [Tribe, 1973, page 472]. Similarly, O'Brien asserts that "Normal science is puzzle solving activity" [O'Brien, 1983b, page 102].<sup>4</sup>

Many, however, go still further. Normal science is not simply any puzzle solving activity, but instead, puzzle solving activity within the confines of a given paradigm.<sup>5</sup> James Wible, for instance, explains that "Normal science is characterized by highly specialized puzzle-solving within a given paradigm" [Wible, 1984, page 94]. Others, still more narrowly, speak of normal science as puzzle-solving within a discipline's *dominant* paradigm:

Kuhn described normal science as a cumulative progression of puzzle solving within the context of a generally shared theoretical framework or approach. [Willett, 1970, page 449]

Typically, scientists go about solving the somewhat challenging but solvable "puzzles" suggested by the main body of theory of the discipline (which is included in Kuhn's notion of "paradigm"). [Webb, 1987, page 405]

Once the basic ideas of a science are settled, its adherents apply themselves to the solution of a multitude of puzzles within the framework of those basic ideas. Kuhn calls this framework a *paradigm*: the world-view whose acceptance is essential for those who wish to be accepted into the fraternity or "invisible college." Normal science then settles down to a routine solution of the puzzles shown by the paradigm to exist and to have solutions. [Routh, 1989, page 26]

What does it mean to say, however, that normal science amounts to puzzle solving? Here, economists offer a few related understandings of the activity. First, puzzle solving implies that normal scientists are virtually guaranteed from the outset that solutions exist to the puzzles they work on [Blaug, 1976, page 169]. According to others, not only do normal scientists enter into work on a puzzle with the assurance that it is soluble, but, also, with the expectation of what that solution will/should be -- if accepted practice is followed:

Normal science . . . is a "puzzle-solving" activity -- the scientist proceeds according to a well-specified set of rules; solutions are usually anticipated in advance . . . [Caldwell, 1982, page 71]

The activity of normal scientists can be compared to puzzle-solving. In solving a puzzle the puzzle-solver knows that a solution exists. He knows what to expect. His ingenuity consists in actually finding the solution according to the "rules of the game." [Mehta, 1978, page 4]<sup>6</sup>

One of the most distinguishing characteristics of normal science as puzzle solving, according to many economists, is that puzzle solving does *not* entail the evaluation of the paradigm within which scientists are working, but instead, the assessment of the competence of scientists working within the paradigm. It is the individual scientist's, not the paradigm's, reputation and standing that are on the line. If a scientist finds a puzzle insoluble, or arrives at a "solution" other than the one expected by the paradigm, he and his abilities as a scientist are impugned, not the paradigm:

Thus, the Kuhnian description of normal scientist as puzzle-solver is meant to indicate that the difficulties he confronts are "like crossword puzzles or chess puzzles, challenges only to his ingenuity. [Controversion means that] *he* is in difficulty, not current theory." [Miller, 1991, page 996, quoting Kuhn, 1970a, page 5, note 1, emphasis in Kuhn, bracketed expression in Miller]

The accepted paradigm defines the appropriate problems to pursue and the procedures to be used for this pursuit, and it guarantees that solutions exist to the problems using these procedures. Normal science is puzzle-solving; when an experiment fails to produce the anticipated result, the puzzle solver, not the puzzle (paradigm) is considered inadequate. [Stanfield, 1974, page 99]

Such normal science has implications for the nature of the scientific community. . . . professional competence is judged in terms of ability at solving the research puzzles produced by the paradigm, for failure to solve a puzzle does not discredit the paradigm so much as the scientist who fails. [Backhouse, 1985, page 4]<sup>7</sup>

Contrary to positivistic notions of science that envision scientists testing theories/paradigms, it is the scientist, not the paradigm, that is being tested during puzzle solving. Puzzle solving tests a scientist's aptitude, not a paradigm's degree of corroboration with the facts:

Puzzle-solving, as an activity, is not designed to test the paradigm, for this is usually taken for granted. Indeed, when a puzzle is unsolved the blame is initially directed at the aspiring puzzle-solver. That person's competence to deal with the puzzle is what is immediately questioned. Normal science imposes standards, and expectations, that effectively test the skills of the scientist. [Pheby, 1988, page 40]

If a researcher attempts to solve some puzzle by a further articulation of existing theories, and if this conjectured puzzle solution fails when empirically tested, this failure is not taken as an indication of the inadequacy of the prevailing paradigm but rather as an indication of the researcher's inability to solve this puzzle. In other words, while empirical tests are carried out during normal science, these tests are not viewed as tests of current theories but rather as tests of the puzzle-solving ability of researchers. When an empirical test indicates that a proposed puzzle solution has failed it is the researcher who is discredited and not the current theoretical framework. [Glass and Johnson, 1989, page 156]<sup>8</sup>

What sorts of puzzles do normal scientists try to solve? Many economists note that Kuhn divided normal science activity into three different types of puzzles: (1) the identification and determination of important facts (i.e., facts which hold special significance to the extant paradigm, such as Avogadro's number or Newton's

gravitational constant), (2) extending the application of the current paradigm to areas to which it was not originally applied, and (3) increasing the match between fact and paradigm:

Kuhn divides such puzzles into three main areas: establishing facts (these being required either because they are interesting in their own right, or in order to help confirm the superiority of the paradigm involved over another); applying the paradigm to the new areas; and reformulating the ideas involved in the paradigm, the first articulation of which may well have been clumsy, or difficult to apply to certain problems. [Backhouse, 1985, page 4]<sup>9</sup>

Philip Mirowski, describing normal science *qua* puzzle solving, provides the most scathing characterization of Kuhnian normal science by an economist. He equates the activity of normal scientists with that of rats:

The widespread academic disdain for any semblance of mere yeoman service in the cause of furthering knowledge is exemplified by Thomas Kuhn's description of "normal science" as "puzzle-solving": a mundane activity, similar to the solution of a crossword puzzle, or the successful negotiation of a maze by a rat. After all, who wants to grow up to be a normal scientist? [Mirowski, 1986, page 2]

### 3. *Excludes Consideration of Social Questions*

Some economists describe puzzle-solving as normal science's only concern. Normal science puts aside consideration of broader social problems because they cannot be made to fit the puzzle mold:

In effect, normal science, and the educational institutions which house and propound it, "insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies." [Rousseas, 1973, page 75, quoting Kuhn, 1962, page 37]

Kuhn's normal science . . . insulates the community from "those socially important problems that are not reducible to the puzzle form." [Miller, 1991, page 1001, quoting Kuhn, 1962, page 37]

### 4. *Marginalizes the History and Philosophy of Science*

Some economists note that in addition to laying aside social concerns, normal scientists have little interest in the history of their discipline or any philosophical considerations that might be undergirding their analysis:



a normal science . . . finds no necessity for including its history as a part of professional training. [Gordon, 1965, page 126]

during a normal science period researchers implicitly accept the fundamental assumptions underlying the prevailing paradigm . . . [Glass and Johnson, 1989, page 156]

[A normal scientist] will definitely *not* be seriously engaged in answering the question, "What's wrong with science X?". [Ward, 1972, page 6]

##### 5. *Narrow, Technical Focus*

All of the foregoing accounts of normal science convey a sense that normal science is a narrowly focused, highly specialized activity. Some economists make this point explicit:

"The areas investigated by normal science are . . . miniscule" and ". . . the enterprise. . . has drastically restricted vision." [Bornemann, 1976, page 130, quoting Kuhn, 1970c, page 24, ellipses in Bornemann]

The problems on which they [normal scientists] typically work are problems of detail. An individual researcher is working at any one point in time on some relatively minor aspects of "the science." [Ward, 1972, page 6]

Thus, normal science involves a highly restrictive sort of scientific activity. But there are advantages which accompany this narrowness of focus, for without it, the subtlety and depth of scientific investigation that also characterizes normal science would not be possible. [Caldwell, 1982, page 71]

Normal science foci, Nancy Folbre notes, "are often limited to questions that can be answered simply by means of technical ingenuity" [Folbre, 1986, page 246].

##### 6. *The Scientific Community is the only Relevant Authority*

A normal science's eschewal of social issues, its own history, and philosophy implies that such a science most likely does not turn to the social reformer, historian of science, or philosopher for advice. More generally, some economists describe a normal science as one whose members look exclusively to each other (and more generally, to the community which they form) for advice, direction and ultimate approval. For normal scientists, their scientific community is the only relevant authority:

[In a normal science,] [t]he members of the profession are the sole judges of the value of their scientific contributions. [Jalladeau, 1975, pages 2-3]

Only the judgment of colleagues is accepted as relevant in defining problems and solutions. If religious or political authorities who are not trained for membership in the invisible college interfere in its operation or are accepted in practice as authorities higher than the college itself, normal science does not take place. [Ward, 1972, pages 6-7]

Along similar lines, Miller characterizes normal science as a "closed society" [Miller, 1991, page 995].

#### 7. *Scientific Work/Research within the Regnant Paradigm*

As we noted, many economists speak of normal science not simply as puzzle solving, but as puzzle solving within a paradigm -- in particular, within a discipline's regnant paradigm. Many others, without making reference to puzzle solving, describe normal science as scientific activity which takes place within the confines of a paradigm.<sup>10</sup> Negishi, for example, defines normal science as "problem-solving activity in the context of an accepted theoretical framework, a paradigm . . ." [Negishi, 1985, page 4].<sup>11</sup> Along similar lines, Almarin Phillips, describes normal science as "the 'mopping-up' operations within the discipline, confined to methods and problems consistent with 'the preformed and relatively inflexible box that the paradigm supplies'" [Phillips, 1966, page 302, quoting Kuhn, 1962, page 24]. Sweezy and L.E. Johnson, respectively, identify normal science as:

the posing and answering of the questions which are explicitly or implicitly allowed by the new paradigm or conception of reality. [Sweezy, 1971, page 61]

research activity is carried on within the boundaries of a paradigm: the "analytical box" through which the profession views reality. [Johnson, 1983, page 1098]

Each of the authors discussed in this section, when speaking about normal science, refers not simply to a school within a given discipline, but instead, to the discipline as a whole. Given this, they may fairly be interpreted as implying that normal science is science conducted not simply within any paradigm, but within the

discipline's dominant paradigm.<sup>12</sup> Others make explicit that normal science takes place within the field's dominant paradigm:

In a Kuhnian normal science period, research work is viewed as taking place within the context of a generally-accepted, dominant *paradigm*. [Glass and Johnson, 1989, page 154]

Miller characterizes normal science's confinement by the regnant paradigm as severe: "Normal science, according to Kuhn, is science in the grip of a prevailing paradigm" [Miller, 1991, page 994].

#### 8. *Unquestioned Acceptance of the Prevailing Paradigm*

Another attribute of normal science which economists identify -- implicit in the foregoing -- is the unquestioned acceptance of the prevailing paradigm. As we noted, one characteristic of normal science *qua* puzzle solving is that the regnant paradigm is not held up to empirical testing in the positivistic sense. It is the scientist, not the paradigm, that is being tested. Similarly, Glass and Johnson observe that, while normal science may seek to improve the fit between fact and theory, normal science tests evaluate a particular solution's ability to improve the fit, "*while still maintaining implicit trust in the prevailing paradigm*."<sup>13</sup>

Argyrous and Johannes Klant point out that normal scientists conduct testing for purposes other than (dis)proving the paradigm:

This is usually the process of testing the paradigm, wherein the word *testing* is employed in a very general sense. It does not mean an attempt to prove or disprove the exemplar involved, but simply to assess whether the "degree of fit" between appearance and theory requires some further modifications. [Argyrous, 1992, page 239]<sup>14</sup>

According to Kuhn, the practice of normal science does not consist of severe testing with refutation as the objective but on the contrary in exploration for the purpose of consolidation. [Klant, 1984, page 43]<sup>15</sup>

Likewise, Williams finds that the Popperian checks of "experimental test and critical discussion" are subverted during normal science.<sup>16</sup> And Cornwall who, as we shall see, equates normal science with the accepted paradigm notes that the paradigm "seems

to be particularly impervious to tests whose predictions tend to refute one or more of the interconnected theories associated with it" [Cornwall, 1979, page 75]. More generally, economists remark that normal science marks a time during which the prevailing paradigm is largely exempt from any sort of criticism or evaluation. Along these same lines, D. Wade Hands allies Lakatos's notion of the "immunity of the hard core" with "Kuhnian 'normal science' where the critical spirit was temporarily arrested" [Hands, 1992, page 33].<sup>17</sup> Similarly, though appreciably more derisively, Stephen Rousseas observes about normal science that "These mopping-up operations are non-critical of the paradigm itself and can therefore be described as paradigm polishing" [Rousseas, 1973, page 75].

#### 9. *Articulation of the Regnant Paradigm*

We hinted at another understanding of normal science in the last chapter when we discussed the notion proffered by many economists that the open-endedness of the regnant paradigms makes scientific work possible. Again, briefly, if the paradigm provided comprehensive answers to all the questions it asked and complete solutions to all the problems it posed, there would be no work for left scientists to do. This line of reasoning, however, presumes a particular understanding of normal science: (1) the articulation of the paradigm within which scientists work, which (2) lays to the side extraparadigmatic concerns.<sup>18</sup> That is, normal science concerns itself chiefly (if not exclusively) with filling out the specification of the paradigm which, at its outset, is incomplete. Normal science is, according to Bornemann, the realization of the promise implicit in a paradigm's open-endedness:

Normal science, which is the customary or day-to-day practice of science, is not concerned with discovering new theories or displacing the paradigm but is pursued with the paradigm as the foundation. It involves research and practice on specialized and technical problems, the solutions to which promise to illuminate the paradigm. Kuhn wrote that "the success of a paradigm . . . is at the start largely a promise of success discoverable in selected and still incomplete examples. Normal science consists in the actualization of that promise, an actualization

achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing . . .". [Bornemann, 1976, page 130, quoting Kuhn, 1970c, pages 23-24, ellipses in Bornemann]<sup>19</sup>

Caldwell as well speaks of normal science as work seeking to "extend and articulate the paradigmatic structure assumed" [Caldwell, 1982, page 71].<sup>20</sup> Swaney and Premus also assert that in normal science "inquiry is directed toward finding the missing pieces of the paradigm-defined puzzle." For Swaney and Premus, those pieces are explanations and/or predictions of "relevant empirical phenomena," and the means of articulation, positivistic canons in inductive logic [Swaney and Premus, 1982, page 715].<sup>21</sup>

Argyrous also understands normal science as paradigm articulation. He, however, stresses that the activity is very different from mindless imitation; it is creative extension and refinement:

Kuhn, however, uses the word *articulation* to describe normal science in order to signal that he is using the notion of paradigm in a manner different from the dictionary definition, in which paradigm is referred to as a pattern that can be "replicated." Replication implies an uncreative and slavish activity; articulation implies that a paradigm is sharpened and applied in novel ways and under more stringent conditions. Although Kuhn agrees that hack science occurs, it is not this activity that he sought to identify as the hallmark of mature normal scientific activity. For him, normal science is creative, even though (and indeed because) it occurs within the constraint of a given world view. [Argyrous, 1992, page 242]

#### 10. *Does Not Aim at Novelty*

Argyrous, however, is an exception. Most economists who comment upon the originality of normal science focus upon the novelty which normal science is unwilling to tolerate, rather than upon any novelty that its strictures might engender. Miller, for example, describes normal science as the rather unoriginal enterprise of the "reiteration" -- albeit "in novel ways" "of phenomena already specified by that paradigm."<sup>22</sup> She further notes that "normal science is not concerned with the breaking of new ground" [Miller, 1991, page 994].

Peabody notes that normal science distinguishes itself from the "normal image of scientific work" in that "it does not aim at novelty in fact or theory" [Peabody, 1971, page 2].<sup>23</sup> Robert Tollison allies normal science with conservatism and an unwillingness "to destroy the human capital stock of normal scientists" [Tollison, 1986, pages 921].<sup>24</sup> Finally, as we noted, Bornemann describes normal science as "not concerned with discovering new theories or displacing the paradigm but is pursued with the paradigm as the foundation" [Bornemann, 1976, page 130].

Connor, however, takes a wholly opposite position from most economists on this point. Normal science as he characterizes it does not avoid novelty; it does not resist change; it actually actively seeks to effect change, even fundamental change:

Kuhn is interested in investigative science, science(i), which he calls "normal science." Science(i) proposes new hypotheses and suggests tests in order to establish new science(g). It cannot be reliable; mostly the claims of scientists(i) will come to be seen as mistaken or unimportant. Only a small part of science(i) ever becomes science(g) [exemplars]. [Connor, 1991, page 59]

#### 11. *Normal Science's Treatment of Contrary Evidence*

Consistent with the understanding that normal science seeks to remain within the confines of the extant paradigm and is loathe to abandon the paradigm, many economists note that normal scientists, when greeted with evidence that runs counter to paradigmatic expectations either ignore, do not see or actively suppress the evidence. According to Pheby's interpretation, not only do normal scientists not seek to make "new discoveries," but are unlikely even to see "phenomena that do not fit into the paradigm" in the first place [Pheby, 1988, page 39].<sup>25</sup> James Webb as well notes scientists' ability to overlook conflicting evidence during periods of normal science:

Quite the contrary, Kuhn contends, scientists (at least in the practice of "normal science") show a remarkable ability to ignore numerous pieces of evidence that are contrary to the theory. [Webb, 1987, page 405]

Rousseas notes that normal science "is predicated on the assumption that 'the scientific community knows what the world is like'," and thus, "behaves in such a way

as to suppress 'fundamental novelties as necessarily subversive to its basic commitments'" [Rousseas, 1973, page 75, quoting Kuhn, 1962, page 5].

However, these sentiments are not universal and two exceptions stand out. Avi Cohen, while conceding that normal science retains "accepted theory," characterizes its response to "contrary evidence" in a different light. Rather than speaking of any resistance the evidence might engender, Cohen stresses that the findings spur scientists to improve the theory they retain:

During periods of normal science, an accepted theory is not discarded in the face of contrary evidence. Instead, the contrary evidence serves as impetus for modifying and improving the theory by adding ancillary hypotheses . . . [Cohen, 1984, page 616-617]

Similarly, as Argyrous describes normal science activity, scientists do not ignore or suppress discrepancies between theory and fact; they instead seek "to come to terms with any differences" between the facts adjudged relevant by the paradigm and "their theoretical counterparts,"

often [by] . . . the articulation of the paradigm theory itself, which may involve the redefinition of key terms, or the appearance of new entities in the world, or the introduction of new assumptions about their behavior. [Argyrous, 1992, pages 238-239]

Similarly, according to Swaney and Premus, unexpected results do not lead normal scientists<sup>26</sup> to dig in their heels, but instead lead them back to the drawing board. There, scientists seek to determine where mistakes have been made and what data had been incorrectly excluded from their original theoretical formulations:

However, if a mistake has been made -- if significant elements have been cast aside -- attempts to apply the theory to a broader range of phenomena will fail, and the scientist will retreat to a lower theoretical level to reexamine the set of common and crucial elements. If this reexamination is successful, theoretical development can once again progress to a higher level and encompass previously peripheral phenomena. [Swaney and Premus, 1982, page 717]

Like those discussed previously, neither Cohen, nor Swaney and Premus, nor Argyrous portray scientists *qua* normal scientists as stepping outside the bounds permitted by the paradigm within which they work. For Cohen, improvements are

made within the paradigm. And, in the case of Swaney and Premus, the data pool to which scientists return when unexpected results arise is determined by the paradigm within which normal scientists work. Under their understanding of normal science, when scientists go back to the drawing board, they limit their focus to evidence which the paradigm allows, but which they had excluded in their initial formulations.<sup>27</sup> And, as we have noted, Argyrous stresses that testing is conducted to determine necessary modifications or additions to a paradigm's theory, not to prove or disprove it [Argyrous, 1992, page 239]. Thus, these economists share much in common with those discussed previously in this section. According to them, normal scientists do not respond to unexpected results by jettisoning the paradigm. They seek to work within it. However, those like Cohen and Swaney and Premus, and Argyrous do differ from the earlier authors in that they portray scientists as doing more than looking the other way when such results arise, and instead highlight the positive action which normal scientists take to deal with the findings.<sup>28</sup>

## 12. *Resistant to New Ideas*

Along the same lines, others describe normal science as, at the very least, resistant to those with views substantially different from the prevailing wisdom:

practitioners of a science governed by a particular paradigm, having enough to do to work out the implications of their own theorems, tend to resist new points of view. [Burt, 1972, page 282]

Beyond simply resisting alternative viewpoints, normal scientists greet with hostility those espousing views which substantially depart from the prevailing wisdom:

When suggestions for such projects arise, either from students or competing members of the community, they are as often treated by sarcasm and ridicule as the subject of seasoned discourse and debate. [Piore, 1983, page 249]<sup>29</sup>

Or even excommunication:

Such normal science has implications for the nature of the scientific community. Acceptance of a particular form of normal science leads to a more rigid definition of a field of research, those who do not



accept its basic assumptions being excluding from the relevant scientific community. [Backhouse, 1985, page 4]<sup>30</sup>

Dudley Dillard and J. Brian Hardaker bemoan normal science's shunning those with novel ideas. Doing so, they charge, impedes significant progress:

The tragedy of control of a discipline by the practitioners of normal science is that potentially creative scientists are shut out or discouraged from entering by the closed nature of the society. [Dillard, 1986, pages 360]

Such behavior, sadly, will inhibit work on radical ideas or approaches that could produce major advances in our discipline, tending instead to perpetuate what Kuhn (1970) called "normal science." [Hardaker, 1985, page 98]

### 13. *Scientific Progress and Normal Science*

Most economists agree that a discipline undergoes positive, incremental progress during normal science. James Thompson notes that during normal science, "scientific progress occurs in an orderly, evolutionary fashion" [Thompson, 1975, page 175]. As we noted, Argyrous lauds the fact that during normal science "a paradigm is sharpened and applied in novel ways and under more stringent conditions" [Argyrous, 1992, page 242].<sup>31</sup> Johnson and Ley also offer a very upbeat assessment of the scientific advance that takes place during normal science:

In other words, normal science leads to intra-paradigm development during which the discipline is united in purpose and method. In the context of normal science, it is reasonable to speak of the paradigm improving in an evolutionary fashion similar to that described by the objectivist. Here, paradigm characteristics are honed by experience to enhance their descriptive, predictive, or prescriptive powers, or to allow them to be applied to a wider range of problems. In short, normal science enhances the paradigm's puzzle-solving ability. [Johnson and Ley, 1990, pages 27-28]<sup>32</sup>

While both remarks shine a positive light on the progressivity of normal science, they also both imply that the progress is incremental and paradigm-specific (that is, to use Johnson and Ley's words, "intraparadigmatic"). In both cases normal science is described as improving the regnant paradigm, not replacing it with a better one. Chase

explicitly underlines the intra-paradigmatic (as opposed to inter-paradigmatic) nature of the progress that a science undergoes during normal science [Chase, 1983b, page 821]:

Thus normal scientific activity tends to yield great strides in *intra-paradigmatic* advance. Scientists are given direction and method within a framework that allows them to "concentrate on problems that only their lack of ingenuity should keep them from solving." Thus normal science is productive of *a kind* of scientific progress. [Chase, 1983b, page 817, quoting Kuhn, 1970c, page 37]

Some economists directly contrast progress occurring under normal science with more major advances:

One should distinguish, as Thomas Kuhn has shown, between areas in the history of science where the major work to be done involves the working out of a given framework or paradigm, and others where significant advance involves the development of a new framework. [Hirsch, 1976, page 206]<sup>33</sup>

The counterpart of Kuhn's normal science is normal social system problem solving restricted to the reorganization of subsystem boundaries. This process is essentially incremental and piecemeal. The reorganization of the behavior of system components is carried out without any major change in the total system objectives or controls. [Dunn, 1970, page 359]

Similarly, as we just saw both Dillard and Hardaker find that normal science's aversion toward novel viewpoints stultifies major advance.

#### 14. *Normal Science's Acceptance of the Regnant Paradigm and Scientific Progress*

As many economists read Kuhn, the progress that occurs under normal science is dependent upon accepting the paradigm as a given. Doing so frees scientists from broad philosophical and methodological questions and thus allows them to refine the paradigm's puzzle-solving ability:

Far from such an uncritical attitude being a problem, as might be inferred from Popper's theory, it is only such an uncritical attitude which, according to Kuhn, permits the application of the theory to a large number of problems, enabling a large number of detailed aspects of the world to be investigated. If scientists spent all their time arguing over fundamentals, they would never manage to investigate many "small" phenomena. Within normal science, therefore, most research takes the form of "puzzle-solving." [Backhouse, 1985, page 4]

Since during a normal science period researchers implicitly accept the fundamental assumptions underlying the prevailing paradigm, this means that they can concentrate their research efforts on extending the existing theoretical framework rather than challenging or re-examining its basic assumptions. Hence, instead of engaging in the difficult and costly business of starting afresh, researchers are free to undertake more precise, esoteric work within the existing theoretical framework. According to Kuhn, this concentration of research effort will, in turn, result in both more efficient and more rapid progress in puzzle-solving. [Glass and Johnson, 1989, page 156]<sup>34</sup>

Richard Brinkman also sings the praises of normal science's restricted focus in conducting to scientific progress. He however adds a cautionary note. Constraining science too tightly may forestall rather than foster progress:

All this is not to deprecate a given disciplinary matrix as normal science, nor normal science as a social institution. The function of normal science is to focus the full force of the scientific community on particular puzzles as delineated by the disciplinary matrix in dominance. If not for this the practitioner of science would see and look at everything and see and know nothing. Specialization was long ago noted as the fountainhead of economic advance and productivity and, consequently, in this sense, normal science provides a positive function. However, overspecialization can lead to rigidities of structure resisting change and transformation and in the process promote stagnation. [Brinkman, 1981, page 38]

15. *Normal Science's Role in Effecting Fundamental Scientific Change (i.e., Paradigm Change)*

Running throughout all the foregoing characterizations of normal science is the sense that normal science is science conducted within, and defined by, a prevailing paradigm. While change does occur during normal science, the paradigm within which normal scientists work is left undisturbed. The paradigm may be sharpened or refined, but it is not replaced. It is not even brought into question.

Even putting aside certain exceptions, (e.g., Connor), it would be incorrect to say that economists view normal science strictly as a deterrent to scientific change. Many economists note that normal scientists (despite wishes to the contrary) end up uncovering vexing difficulties (anomalies) within the current paradigm. The anomalies may, in turn, lead a discipline out of a period of normal science during which the regnant paradigm is taken for granted into a period of revolutionary science during

which the paradigm loses its inviolable status and out of which the old paradigm may be replaced by a fundamentally new one. Thus, even while it might be true that normal science, by definition, is a period of time during which no fundamental (i.e., inter-paradigmatic) change takes place, it is also true that work conducted under normal science's umbrella may uncover anomalies which the paradigm can neither incorporate nor push to the side. The anomalies constitute the seeds of a normal science's paradigm's destruction<sup>35</sup> and usher in paradigmatic change. We will discuss this matter at greater length in the following chapter. Very few who note the link between normal science and the production of anomalies directly characterize normal science as an engine of change. They do so only implicitly. Stanfield is a notable exception:

Normal science is a period of relative quiescence, but this should not be given an equilibrium or static interpretation. Normal science is a period of calm and orderly cumulative change and not an equilibrium situation to be interrupted only by exogenous happenstance. As an articulation or day-to-day working out of the directions given by a paradigmatic axial structure, normal science itself sows the seeds for a period of storm, crisis, and redirection. [Stanfield, 1978, page 112]

Glass and Johnson maintain that Kuhn advocated the practice of normal science because it inevitably "leads to scientific revolutions and hence to scientific progress" [Glass and Johnson, 1989, page 170]. Caldwell also notes that Kuhn understood normal science leading "to revolutions, and thus ultimately to scientific development" [Caldwell, 1982, page 77]. However, Caldwell expresses his own reservations about normal science's ability to effect positive change:

It is clear that monism permits the intensive investigation of a particular subject which can lead to fruitful and sophisticated analysis. Monism can also lead to revolutions, if the scientists involved pay proper attention to the anomalies which may (or must a la Kuhn) eventually surface as a result of their intense efforts. But if anomalies are generally ignored or "patched up" with ad hoc, theory-saving devices, normal science can also become stagnant and dogmatic. [Caldwell, 1982, pages 91-2]

16. *Consensus*

Corresponding to the normal scientist's firm commitment to the prevailing paradigm, normal science is also portrayed as a period of time during which a high level of consensus exists among members of a given discipline.<sup>36</sup> Members within a discipline, in large part, stand in agreement with one another -- especially with respect to the discipline's guiding fundamentals.<sup>37</sup>

The characteristic feature of Kuhnian normal science is that it portrays research work as being carried out by a scientific community that is in general agreement, not only with regard to the fundamental assumptions that underlie theoretical analysis, but also with regard to both the research problems that need to be solved and the theories and techniques that are to be utilized in attempts to solve these problems. [Glass and Johnson, 1989, page 155]

As noted, it is Kuhn's contention that it is characteristic of normal science that there be agreement on paradigmatic fundamentals among its practitioners. "[I]t is precisely the abandonment of critical discourse" in his view "that marks the transition to a science." [Miller, 1991, page 994, quoting Kuhn, 1970a, page 6]

In other words, normal science leads to intraparadigm development during which the discipline is united in purpose and method. [Johnson and Ley, 1990, page 27]

Normal scientific activity takes place only when the relevant members of the science or one of its sub-specialities have agreed on the fundamentals of the subject. [Mehta, 1978, pages 4-5]

Members of the invisible college<sup>38</sup> are in general agreement as to what the main problems are that are suitable for research, and they are also agreed as to the general form that a solution should take. These agreements are a product of the similar training the scientists have received, the common body of theory, established fact, and laboratory procedure that they know. [Ward, 1972, page 6]

Disagreements that do arise are rare according to economists' interpretations, usually concern only minor points, and/or are settled quickly:

"Normal" science is performed by "men whose research is based on shared paradigms." They are committed to the "same rules and standards for scientific practice." Above all they seldom disagree about fundamental assumptions, values and methods of inquiry. [Lekachman, 1976, page 51, quoting Kuhn, 1970c, page 11]

According to Kuhn's version of this thesis, scholarly thought in any scientific field tends at most times to be under the domination of a single master theory or paradigm. Scientists working under the intellectual

influence of this paradigm will ordinarily have the same basic framework of ideas, a common technical vocabulary, and the same set of procedures for resolving differences. Consequently, during long periods of "normal science" minor disagreements among scientists are settled without serious difficulty and scientific progress occurs in an orderly, evolutionary fashion. [Thompson, 1975, page 175]

Ward argues that normal scientists agree as to the means by which to settle any disputes that do arise and identifies the resolution of disagreements within an agreed-upon framework as a vital element of normal science. Within a normal science, he explains,

There may be several proposed solutions to a particular problem and disagreement as to which one is correct. But this is true only in a relatively modest number of places in the science, and even in such cases there will be pretty good agreement as to what sort of research in principle would resolve the disagreement. Indeed, this process of resolving disagreements within a broader framework of general agreement is the normal process of development of a science. [Ward, 1972, page 6]

Prior discussion has suggested a flip-side to the high level of consensus existing under normal science: extreme pressure for conformity. Those advocating novel positions are met with hostility or are shut out altogether. Redman, however, implies that while consensus is a necessary requirement for a normal science, that consensus must be "natural," not "imposed."<sup>39</sup>

17. *Period of Time During which One and Only One Paradigm Exists within a Discipline*

Some economists forward a more extreme understanding of normal science as a period of consensus. Normal science, they maintain, implies that all members of a discipline operate under one and only one paradigm (the regnant paradigm). Along these lines, Glass and Johnson refer to normal science as a period of time during which "the prevailing paradigm has a monopoly position."<sup>40</sup> "'Normal'," Redman notes, "assumes [among other things] the existence of . . . one reigning theory" [Redman, 1991, page 150].

Likewise, Baumberger stresses that Kuhnian normal science implies that the discipline as a whole -- not simply a part of it -- professes allegiance to single

paradigm. Normal science, as he notes, refers to a "period" in a discipline's history, not just a "pocket" of it. Thus, even if a discipline's mainstream adheres to one and only one paradigm, the presence of a viable heterodoxy adhering to an alternative framework precludes the existence of normal science:

His [Kuhn's] repeated reference to "periods," not just "functions," strongly suggests that there is a *period* of normal science (not just a pocket) during which the revolutionary mode does not obtain for the discipline in question. In this period the victorious paradigm reigns sovereign and ensures a peaceful and piecemeal development within an agreed upon framework. The two modes seem to be discipline-wide behavioral and sociological phenomena. [Baumberger, 1977, page 6]

. . . Kuhnian normal science, in the only nontrivial . . . sense, is not just any puzzle-solving science, but a whole *period* of it, that is, a period when the scientific community *as a whole* is operating within a common paradigm with respect to a wide range of questions. [Baumberger, 1977, page 10, first emphasis in original, second emphasis added]

#### 18. *The Dominant Paradigm*

As we have seen from the foregoing discussion, normal science, for most economists, refers to the scientific activity taking place within the confines of a field's dominant paradigm. Some, however, ally normal science directly with the dominant paradigm itself (or its allied theories):

*Normal Science* is the term used by Kuhn to describe a universally accepted paradigm. [Rousseas, 1973, page 75]

If this paradigm is the one most widely adopted by the profession, we can designate the group of interconnected theories emanating from the paradigm as "normal science." [Cornwall, 1979, pages 70-71]

Thomas S. Kuhn in *The Structure of Scientific Revolutions* uses the term "normal science" to indicate a prevailing interpretation of past scientific theories that a "particular scientific community acknowledges for a time as supplying the foundation for its further practice." Such theories, . . . he calls "paradigms." [Burt, 1972, page 281, quoting Kuhn, 1962, page 10]

The Hegelian dialectic, as is well known, involves a basic thesis or orthodoxy. This corresponds to Kuhn's term "normal science" and includes a set of what Kuhn calls "standard paradigms." [Bronfenbrenner, 1971, page 139]

### 19. *What Scientists Normally Do*

Perhaps the most straightforward interpretation which economists offer of normal science is that normal science is what scientists normally do. Brinkman does just that, defining normal science as "what scientists, as members of a given scientific community, normally do" [Brinkman, 1981, pages 35-36].<sup>41</sup> Similarly, Peabody describes normal sciences as the "usual activity of the community of scholars engaged in research of a specialty in the natural sciences" [Peabody, 1971, page 1], and Bornemann defines normal science as "the customary or day-to-day practice of science . . . ." [Bornemann, 1976, page 130]. As does Piore: "'normal science' is in fact largely a set of practices in which members of a given scientific community *customarily* engage" [Piore, 1983, page 249].<sup>42</sup>

### 20. *Normal Science, Journals and Textbooks*

Normal science also distinguishes itself, according to some economists, by the nature of its publications. Textbooks play a central role in the education of a normal scientist,<sup>43</sup> and normal science practitioners, rather than producing lengthy tomes, concentrate their efforts on the publication of shorter, more narrowly focused journal articles. Wible, for example, asserts that

Normal science is characterized by highly specialized puzzle-solving within a given paradigm, by pedagogical communications dominated by textbooks, and by professional communications oriented toward short journal articles rather than monographs. [Wible, 1984, page 94]

### 21. *Summary*

Most of the foregoing descriptions of normal science forge some sort of identity between normal science and paradigm. Economists, however, relate the two notions in a number of different ways in their depictions of Kuhnian normal science: science practiced under the confines of a paradigm (dominant paradigm); the articulation of a paradigm (dominant paradigm); puzzle solving under the confines of a paradigm (dominant paradigm), (dominant paradigm whose validity is not held up to question); a



period of time during which a single paradigm dominates a given discipline; the dominant paradigm *itself*. Further, while most definitions make mention of paradigm, not all do. Most notably, we find a number of brief descriptions of normal science as puzzle solving, without explicit mention of the relationship between paradigm and normal science.

A second notable feature of economists' characterizations of normal science is its narrow scope of concern and concomitant eschewal of social questions and the history, philosophy and methodology of science. Third, many of the characteristics of normal science that economists identify point to a reluctance/aversion on the part of normal science to fundamental change and/or novelty. In particular, economists point to an unwillingness on the part of normal scientists to forsake a regnant paradigm. Fourth, progress under normal science is characterized as incremental (as opposed to radical) change/progress. In this respect, we find that economists differ regarding the relative emphasis they place upon the incremental progress occurring under normal science, as opposed to normal science's resistance to more fundamental change/progress. Fifth, economists point out that a high degree of consensus exists among members of a normal science (at least as to fundamentals). As we will see in the discussions which follow, economists variously employ these characterizations in their identifications of normal science in economics.

## B. PAST NORMAL SCIENCE

We now turn our attention to economists' applications of Kuhn's normal science concept to economics. We first consider their descriptions and assessments of normal science in economics' past. Here, we find that economists employ varying understandings of normal science in their identification of normal science arising out of classical and Keynesian economics. We also find that they reach divergent conclusions as to whether either school of economics engaged in the practice of normal science.

# 1. *Classical Economics*

Economists highlight different aspects of Kuhn's normal science concept in assessing the existence and nature of normal science in economics during classical economics' reign. O'Brien doubts that Classical economists engaged in very much normal science puzzle solving.<sup>44</sup> In particular, despite Ricardo's "inclination as a puzzle solver," O'Brien argues that it would grossly misrepresent Ricardo's place in economics' history to portray him merely as a puzzle solver working within the confines of the Smithian paradigm:<sup>45</sup> "such a view is sustainable only by removing the corn model and the invariable measure from a central role in his work" [O'Brien, 1983b, page 104]. Similarly, Baumberger asserts that Marx, a descendant of classical economics, "fails to provide a convincing example of puzzle solving normal science" [Baumberger, 1977, page 9].

Likewise, Deane questions whether British Classical economists could be said to constitute a Kuhnian science. She grants that they shared a great deal in common, in terms of the training they received and the notions they adhered to. She, however, expresses serious reservations as to whether economics at that time comprised a science in Kuhn's sense of the term.<sup>46</sup>

Dillard, on the other hand, identifies classical economics (in particular Ricardo's *Principles*) as the field's normal science prior to the rise of neoclassical economics [Dillard, 1986, pages 357 and 361]. He further identifies a parallel between classical economics and Kuhn's understanding of normal science: It was a "closed society."<sup>47</sup>

DeVroey, unlike Dillard, maintains that economics under the domination of Classical economics lacked clearly defined boundaries. Nonetheless, given that economics during this time was in possession of a common paradigm, economics did constitute a normal science -- albeit one in its infancy.<sup>48</sup>

Likewise, Johnson and Ley identify classical economists as engaging in the practice of normal science<sup>49</sup> under a common paradigm, whose purposive function was "an examination of the extent to which market directed capitalism leads to the maximization over time of total social welfare, defined in material terms" [Johnson and Ley, 1990, page 90].

Significantly, however, Johnson and Ley point out, pursuit of the common goal set out by the Classical P-F led classical economists to divergent conclusions regarding capitalism's ability to maximize long-term social welfare.<sup>50</sup> Smith differed from the Physiocrats in positing the market, not the state, as the force by which capitalism would attain overall social welfare [Johnson and Ley, 1990, pages 102-103]. Malthus "denied the direct connection made by the Physiocrats and Smith between the nation's wealth and society's welfare" [Johnson and Ley, 1990, page 106]. Still again, Malthus' and Marx's pessimism as to capitalism's ability to maximize social well-being contrasts sharply with the Physiocrats', Smith's and even Ricardo's hopefulness [Johnson and Ley, 1990, pages 106-108 and 113ff.].

Like all normal scientists, according to Brinkman, economists prior to the 1930s agreed as to which questions were permissible to ask, and which not. In particular, given their adherence to Say's Law, pre-Keynesian economists did not regard the explanation of unemployment as a meaningful question, but instead sought to solve the puzzle as to how it was that supply created its own demand.<sup>51</sup>

## 2. *Keynesian Economics*

Pheby finds that Keynes' theory resulted in the practice of normal science. The evidence: consensus existed among economists as to which models to employ in their work and economists engaged in extensive puzzle solving (both theoretical and empirical).<sup>52</sup>

According to Stanfield, Keynes' theory, given its open-endedness, spawned a great amount of normal science activity aimed at filling in the gaps (both theoretical and empirical) in Keynes' work:

As for Keynesian normal science, despite its fundamental simplicity, Keynes's theory was sufficiently open-ended to allow substantial articulation. For example, note the surge of econometrics and national income accounting, the consumption function debates, the stagnation theories, and the portfolio balance approaches to liquidity preference. [Stanfield, 1974, page 105]

Deane as well assents that Keynes' economics gave rise to a sizeable body of theoretical and empirical work. However, for her, that work does not comprise/indicate the practice of Kuhnian normal science given "wide divergences between the basic assumptions and analytical techniques adopted by economists who would regard themselves as working within a Keynesian tradition" [Deane, 1978, page 188].<sup>53,54</sup> Stanfield, we should note, makes no mention of such differences. Rather, his portrayal of the discipline's acceptance of Keynes's theory implies that economists did come to share a common set of techniques and view of the world.<sup>55</sup>

According to Bornemann, Keynesian normal science, following Kuhn's schema, became increasingly narrow in its focus. It came to limit itself to "only such eventually customary general expressions and phrases as stagnation and the shortage of aggregate demand" in its explanations of economic phenomena and, regardless of economic circumstances, advocated increased government spending. As a result, economists lost sight of the deleterious consequences involved in overheating the economy.<sup>56</sup>

### 3. *Summary*

Many of the reasons which economists give as to why Classical or Keynesian economics was (was not) a normal science parallel economists' depictions of a normal science: (1) Economists did (did not) share a common paradigm (DeVroey, Johnson and Ley, Stanfield and Deane). (2) Economists did (did not) engage in puzzle solving activity (O'Brien, Pheby and Stanfield). (3) Economists' work was narrowly focused

(Bornemann). However, economists lay varying levels of emphases upon each of these criteria in their assessments/descriptions of normal science. Further, we find that economists are not in agreement as to whether the schools practiced/composed a normal science. Whereas O'Brien and Deane conclude that Classical economists did not practice normal science ala Kuhn, DeVroey and Johnson and Ley charge they did. Pheby, Stanfield and Bornemann all find evidence that Keynesian economics constituted a Kuhnian normal science, but Deane asserts it did not. In some cases, these disagreements correspond with the employment of different criteria as to what constitutes a normal science. Both DeVroey and Johnson and Ley identify the possession of a common paradigm as a major reason why Classical economics constituted a normal science, whereas O'Brien and Deane focus upon other aspects of a normal science in casting doubt upon whether Classical economics constituted a normal science. However, differences in criteria employed do not account for Deane's disagreement with Pheby and Stanfield as to why Keynesian economics was not a normal science. Whereas Deane argues that Keynesianism was (is) not a normal science because Keynesians did/do not operate under a single, shared paradigm, Pheby and Stanfield argue that Keynesianism was/is a normal science because under it economists *did/do* share a common paradigm.

#### 4. *Selected Past Economists' Methodological Positions*

We end the present discussion by noting a couple of economists who ally Kuhn's notion of normal science with the methodological stances of some past economists. Barbara MacLennan relates Stanley Jevons' methodological positions to Kuhn's normal science concept. In particular, she finds that Jevons' conception of science as a "piecemeal process" and his concomitant proposals (1) to base social science inquiry upon "trivially true axioms" which can safely claim universal status, and (2) to eschew attempts to found any broad sweeping claims about society, amount

to a call for economists' to practice normal science. MacLennan, however, criticizes Jevons' proposal and, by implication, normal science, for failing to recognize the important "role of the bold hypothesis" in scientific inquiry.<sup>57</sup>

Abraham Hirsch allies one of J.S. Mill's better known dictums -- "that the economists develop theory by integrating 'disturbing causes' into an accepted framework" -- with Kuhn's normal science concept [Hirsch, 1978, page 206]. Hirsch, however, points out that the prescription is not well-suited to those times in economics' history when a new paradigm is called for, such as was, he asserts, the case in macroeconomics around the turn of the century:

It might appear so on the face of it, and yet is apparent today that Mill oversimplified the kind of issue involved. One should distinguish, as Thomas Kuhn has shown, between areas in the history of science where the major work to be done involves the working out of a given framework or paradigm, and others where significant advance involves the development of a new framework. Mill's suggestion that the economist develop theory by integrating "disturbing causes" into an accepted framework would still seem valid for "normal" research, that is, work which fits within the bounds of a given paradigm. It could not create new paradigms.

There was surely a need in macroeconomics and economic dynamics for a new paradigm when Mitchell came upon the scene, and Mitchell was well aware of it. [Hirsch, 1976, page 206]

Speaking with reference to Stigler's advocacy of "intellectual autonomy," David DeShon makes a similar point. Stigler's recommendation applies to periods of normal science, but not during "revolutionary science."<sup>58</sup>

### C. CURRENT-DAY NORMAL SCIENCE

Finally, we examine economists' varied descriptions and assessments of current-day normal economic science. As the following discussion of individual economists' depictions of normal science in economics demonstrates, we find many parallels between economists' descriptions of normal economic science in particular and their characterizations of normal science in general. In particular, we find that many economists' discussions of normal economic science (1) forge a strong identity between

economics' dominant paradigm and normal economic science, (2) highlight normal economic science's resistance to novel ideas or facts contradicting the regnant paradigm, (3) point out that normal economic science marginalizes the importance of social issues and the history of economics, (4) contend that the normal economic science community is insulated from outside influences and cares only about its own opinions as to what constitutes good or worthwhile work, (5) involve consideration of the degree and nature of the consensus among economists and/or (6) focus upon the economics' journal literature. We, find, however, a wide range of answers to the question as to whether economics (or some part of it) constitutes a normal science. Finally -- as we noted at the outset of this chapter -- we see that most economists who provide a normative appraisal of normal economic science are highly critical of it.

#### 1. *George Argyrous*

Arranging our discussion alphabetically, we begin with an examination of George Argyrous' discussion of normal economic science in the consumption literature. Argyrous examines thirty years' worth of articles published in the *American Economic Review* which concerned themselves with Friedman's permanent income and/or Modigliani's life cycle hypotheses of consumption.<sup>59</sup> In so doing, he finds considerable evidence of normal science activity. In some instances, Argyrous marks out the evidence explicitly. Houthakker's construction of data from BLS statistics to test Friedman's assumption that all consumption is independent of transitory income is cited as an example of "an important aspect of normal science:" "the extract[ion] [of] the meaningful facts from the information sources that are available."<sup>60</sup> And, Eisner's reply to Houthakker's test in which the former reworked the latter's test into a more powerful one is characterized as

a good case of the type of detailed, thoughtful, and skillful research that Kuhn identified as normal science. As Houthakker (1958b, page 991) described it, it was "a brilliant filling-in of some of the gaps in Friedman's argument." But this "filling-in" was no easy matter, to

which the intricate and careful nature of much of Eisner's analysis was testimony. [Argyrous, 1992, page 240]

More generally, Argyrous sees normal science at work throughout the *AER* articles he surveys which, he contends, can easily be understood in terms of the trichotomy of normal science activity that Kuhn laid out<sup>61</sup>:

In each article it is relatively easy to determine in quite narrow terms the specific issues being addressed and to classify the normal scientific activity arising from them into the threefold division between fact gathering, relating fact to theory, and theoretical development. [Argyrous, 1992, page 241]<sup>62</sup>

Further, by implication, Argyrous points to a parallel between the nature of the testing of various assumptions and corollaries of the hypotheses and the sort of testing Kuhn described going on under normal science. As we noted last section, Argyrous pointed out that testing under Kuhnian normal science<sup>63</sup> does not seek so much to prove or disprove the paradigm as it does to assess the fit between fact and paradigm and determine ways in which the fit might be improved. Consistent with this understanding, Argyrous notes that economists did not construe initial tests which yielded results unfavorable to Friedman's hypothesis as "conclusive test[s] of the PIH." Economists, instead, proceeded on with more testing with "a perception . . . that appropriate adjustments to the paradigm could be made in order to generate a sufficiently satisfactory solution," (i.e., a solution which comported with the hypothesis' predictions and/or assumptions) [Argyrous, 1992, page 240].<sup>64</sup> And, as Argyrous describes them, economists undertook to make certain "appropriate adjustments" with some favorable results.<sup>65</sup>

We also noted last section that Argyrous portrays Kuhnian normal science in a highly positive light. Given that he sees economists engaged in such practice, it is not surprising that he looks very favorably upon the normal science work economists have undertaken in examining the consumption hypotheses. Indeed, he cites the normal science he identifies in economics as evidence for his more general position that normal science is a creative undertaking rather than "a slavish and unimaginative activity that



engages only the hack scientist."<sup>66</sup> We have already noted the praise he heaps on Eisner's work for being an excellent example of normal science. Still further, Argyrous cites the high caliber of those engaged in normal science activity centered around the consumption function and the great respect which the economics profession has accorded their work as a reason for and an indication of the quality of the normal science:

The reason why normal science must be something more than hack science is clear when we consider its role in the sociology of scientific communities. Success at puzzle-solving is the criterion by which a professional community judges who are its leading exponents, and skill in this activity is necessary for initiation into the scientific community and progress within it. Because of this, it naturally draws the attention of "the best." It can hardly be disputed that much of the work on the development of the consumption function was not conducted by hacks. It absorbed the interest and resources of the major academic institutions and research centers, the results of these researches were the subject of major journal publications, and two Nobel Prizes emerged from it. If the whole line of work conducted in relation to the consumption function is to be dismissed as unimaginative and uncreative, one may then wonder what standards are to be met for something to be considered "non-hack science." [Argyrous, 1992, pages 242-243]

## 2. *Jörg Baumberger*

Baumberger concedes that economists have engaged in "a great deal of puzzle-solving, limited scope science," especially since the rise of neoclassical economics [Baumberger, 1977, page 10]. This, however, Baumberger is quick to point out, is not the same as saying that economists have practiced normal science. As we recall from last section, Baumberger defines normal science as a period of time during which an *entire* discipline (not just some part of it) works under a common paradigm. Given this interpretation of normal science, Baumberger expresses serious doubt whether economics has ever experienced a period of normal science.<sup>67</sup> He provides specific indication as to how it is that following the emergence of neoclassical economics, the field has not undergone a period of normal science. Ever since neoclassicism's rise,

economics, far from being united around a shared paradigm, has been embroiled in conflict:

there has been less than complete peace in economics ever since the neoclassical "revolution." By any standards, the classical paradigm (if we may call the content of the tradition by that name) has been around all the time, and the battle is far from finished. If there was something like a revolution a hundred years ago, the intervening century clearly was not sufficient to consolidate it. [Baumberger, 1977, page 10]

What looks to be normal science activity is rather "the expression of the sheer scale of institutionalized science" [Baumberger, 1977, page 10].

Baumberger does not directly address how it was that economics, prior to neoclassical economics' rise, never underwent a time of normal science. We may, however, surmise at least one possible explanation. We will recall from last chapter that Baumberger voices serious reservations about classifying the classical tradition as a paradigm. The Classical tradition that guided much of economics before the "revolution" was too fluid and heterogeneous to count as a rigid Kuhnian paradigm [Baumberger, 1977, pages 9-10]. But if the classical tradition is not a paradigm, then it follows that economics, at least prior to 1870, did not operate under the narrowly defined entity which Kuhn referred to as a paradigm. Given that normal science implies practice under a common *paradigm*, economists prior to 1870 -- lacking a paradigm -- could not be said to have practiced normal science.

Still further, Baumberger questions whether any science (not just economics) possesses the unanimity and insularity which Kuhn's normal science concept presupposes:

All the discreteness, constancy, and self-sufficiency that Kuhn ascribes to his entities are abstractions. . . . In actual fact, the transmission constituted by a discipline, or science as a whole, is a *population of far from discrete process fields* or activity areas that are competing and conflicting in many different, but interdependent, complex fashions. Pockets of this whole may well, and are bound to, in an era of large-scale institutionalized science, in an oblique sense, approach the process characteristics of a normal science a la Kuhn. But the whole, even the whole of a subdiscipline, never displays these properties, least so in economics. [Baumberger, 1977, page 16]

### 3. *Charles Blankart*

Public choice economists, according to Blankart, engage in widespread normal science practice. *Qua* normal scientists, public choice economists are chiefly concerned with working out the problems that arise from the significant and substantial disagreement among the field's recognized classics:

These five works<sup>68</sup> may be regarded as the classics in public choice literature. But since the views expressed therein are so widely dissenting with respect to the appropriateness of optimism and pessimism in the assessment of the collective decision-making process,<sup>69</sup> they raised more questions than they answered. The disentanglement of these problems was and still is the task of what Kuhn calls normal science. [Blankart, 1987, page 6]

Thus, in contrast to the understanding of normal science as a enterprise built upon consensus, the normal science Blankart describes, exists within the context of and is motivated by, fundamental disagreement. Rather than public choice economists following a single line of coherent research, work is conducted along five, largely separate, paths,<sup>70</sup> each of which conducts empirical and theoretical research designed to extend its respective classic's conclusions. Further, Blankart provides little to no indication of work which crosses between the lines of research, let alone any efforts to bridge the differences among them.<sup>71</sup>

### 4. *Christopher Bliss*

Bliss concedes that economists are not in complete agreement with one another and that, in some areas, significant differences of opinion exist. Nonetheless, he implies, economics may still be considered a normal science, since none of the disagreements extend beyond the bounds of a normal science. Significantly, Bliss includes the economics mainstream and certain radical economists within the same normal science fold.<sup>72</sup>

### 5. *Oyvind Bøhren*

Bøhren identifies the 1960s and early 1970s as a period of normal science in stochastic choice theory. During that time, Bøhren suggests, economists in this sub-

field belonged to the same paradigm (stochastic choice paradigm) [Bøhren, 1990, page 26]. However, the paradigm's leading research programme, though never in danger of being overturned, was subjected to probing criticism from adherents of another, emergent programme (non-expected utility, (NEU)). Thus, the period of normal science marked a time when the sub-discipline's practitioners worked within the same paradigm, but lacked consensus and engaged in critical discourse:

The activity of NEU research has been varying over time. Despite its rather flying start in Paris in 1952, the program was no real threat to the EUT during the sixties and early seventies. In this period, the major achievement of the NEU program was to uncover defects in the EU model as a positive behavioral hypothesis. In Kuhnian terms, this was a period of puzzle-solving and normal science. [Bøhren, 1990, page 25]

#### 6. *Lawrence Boland*

Boland asserts that the bulk of economic theorizing amounts to Kuhnian puzzle solving -- which he equates to normal science in another context [Boland, 1982, page 161]. For Boland, this means that economic theoreticians have limited themselves to proving only that which they can utilizing logic and a set of permissible givens (the "universe of discourse").<sup>73</sup> Boland finds the results of such puzzle solving highly unsatisfactory. The puzzle solving has, in the main, produced only "trivial results," "usually . . . nothing but some familiar theorem from standard neoclassical theory" [Boland, 1982, page 132]. More seriously, such a procedure yields only "pseudo tautologies," whose "truth" depends upon the definition of the world set out by the "universe of discourse." These tautologies are, however, true in a very restricted sense. What is true given one "hypothetical world" need not be true assuming a different one; nor need a hypothetical world bear any resemblance to "the real world."<sup>74</sup> Finally, *qua* tautologies, the truth of pseudo tautologies -- given the "universe of discourse" -- is never in doubt. Thus, their "truth is, in a sense, too easy"

and their derivation not very enlightening.<sup>75</sup> Economists, Boland indicates, should strive for a broader sense truth:

What is important at this stage is the recognition that when we want to provide a true explanation or theory for something, we do not want our explanation or theory to be true merely because it is a tautology. [Boland, 1982, page 135]

7. *Richard Brinkman*

Brinkman does not insinuate (nor does he, however, deny) that all normal science must, by its nature, marginalize the treatment of broad social factors. He does, however, assert that current-day normal economic science instructs an economist, from the outset of his career, "to exclude the institutional and the cultural as integral parts of his theoretical explanation." Brinkman implies that such instruction leads economists to prematurely dismiss consideration of institutional elements from their analyses.<sup>76</sup>

8. *E. Ray Canterbury and Robert Burkhardt*

Canterbery and Burkhardt assent that economics constitutes a Kuhnian science in that most economists adhere to and work under the field's prevailing paradigm. Economics' status as a Kuhnian science, however, leaves them singularly unimpressed:

Is economics a science? . . . economics has a paradigm and devout practitioners, so that economics is a science in a Kuhnian sense of having a single overarching paradigm to which most practitioners in the field subscribe. *But so what?* [Canterbery and Burkhardt, 1983, page 35, emphasis added]

To say that economics is a Kuhnian science is to say very little and to say nothing that invests the field with any special honorific standing. To say further that the only way in which economics may be construed as a science is as a Kuhnian one is to recognize its failure to live up to the positivistic precepts it espouses and to suggest that economics is little more than a social science analog to Ptolemaic astronomy.<sup>77</sup>

Economic science, as Canterbury and Burkhardt see it, has insulated itself from broader social changes and has shunned (even shut out) radical viewpoints.<sup>78</sup> Above all else, what matters most to the economics profession is its own opinion of itself<sup>79</sup>

and what matters most to its members are the opinions of their colleagues [Canterbery and Burkhardt, 1983, pages 23-24].

9. *A.W. Coats*

Coats leaves the question of the existence of normal science in economics an open one. He does evaluate economics' performance in terms of the three activities of normal science Kuhn lays out. In doing so, however, he finds that while economics has done well in two of the three areas ("actualizing the promises inherent in their paradigm" and "improving the 'articulation' of the paradigm itself"), it has not had much success in the third area ("efforts to improve the match between the facts and the paradigm's predictions"). However, economics' poor track record in this last area leads Coats to raise questions about whether economics can, as yet, be considered a normal science.<sup>80</sup>

10. *Avi Cohen*

Cohen calls upon the members of one of economics' heterodox schools of economics (post-Keynesianism) to practice normal science. That call, for Cohen, amounts to a plea that post-Keynesians engage in empirical testing in order to find their own theory's faults along with ways to improve it -- rather than seeking to demonstrate their theory's preeminence.

In this encouraging atmosphere, post Keynesians face two challenges. . . . The second, and far more important, challenge is to use that methodology to provide good historical and theoretical explanations of economic phenomena. Above all, those explanations must contain causal mechanisms, but must also include empirically testable (ex post) hypotheses. The lesson to remember from the history and philosophy of science is that the function of those empirical tests is not to prove or disprove post Keynesian theories or to prove superiority over competing theories. They function, instead, as an activity of "normal science" -- to discover shortcomings in a theory and to determine how to develop fruitfully and improve the theory. For it is on its fruitfulness in furthering our understanding and control (through policy) of the functioning of real market economies that post Keynesian theory will ultimately be judged. [Cohen, 1984, page 627]

Cohen cites some examples of such normal science practice on the part of mainstream economists: "the invocation of transactions costs and imperfect information to augment the hypothesis of maximizing behavior" [Cohen, 1984, page 617].

11. *John Cornwall*

Cornwall allies economics' current normal science with the field's regnant paradigm.<sup>81</sup> For Cornwall, this is neoclassical economics. Cornwall is highly critical of the normal science.<sup>82</sup> According to him, neoclassical economics falters because it marginalizes that which should be of utmost importance to economics: "to develop theories that attempt to explain and predict real-world events" [Cornwall, 1979, page 76]. As a result, economic normal science premises its theories upon assumptions at wide variance from what actually goes on "in the real world," and thus offers either no<sup>83</sup> or only highly implausible<sup>84</sup> explanations for observed phenomena and completely overlooks many vitally important economic problems altogether.<sup>85</sup>

The above discussion . . . should . . . have made clear . . . how neoclassical growth theory has misunderstood the chief aims of science to explain and predict, and how this misunderstanding can be traced back to the paradigm of neoclassical economics. One does not really "explain" growth -- much less, its changing patterns -- by asserting that the crucial driving forces behind growth are exogenous and that growth is always a balanced process occurring in a competitive, frictionless world devoid of structural change. [Cornwall, 1979, page 86]

As a further consequence, economic normal science finds itself unable to craft the policies necessary to effectively address the difficulties which beset modern capitalism.<sup>86</sup>

The current normal science, Cornwall notes, has been besieged by a broad number of anomalies (i.e., "important assumptions and predictions that are clearly at variance with real-world events").<sup>87</sup> Despite this, the current economic normal science persists, largely for two reasons. First, economics paradigms, including the neoclassical paradigm, "are less precise and less liable to refutation than those in the natural sciences" [Cornwall, 1979, pages 86-87]. Second, the lack of realism of the

neoclassical paradigm buys economists the best chance they have to emulate (at least ostensibly) the work conducted in the "hard" sciences -- of which economists are so envious:

As was suggested previously, an additional factor may be at work in the strong desire among many economists to be identified with the "hard" sciences and to keep as great a distance as possible from the other "soft" social sciences. *No other paradigm offers the preciseness, generality and, especially, rigor of the neoclassical one.* [Cornwall, 1979, page 87, emphasis added]

Economists do practice outside the neoclassical paradigm (e.g., post-Keynesians), Cornwall notes, but they, by definition, practice revolutionary (as opposed to normal) science [Cornwall, 1979, pages 69-70].

## 12. *Dudley Dillard*

Like Cornwall, Dillard identifies economics' current normal science with what he sees to be the field's regnant theory: Walrasian general equilibrium theory, which "portrays the mutual interdetermination of the prices and quantities of factor services and consumables by a system of simultaneous equations that result in equilibrium" [Dillard, 1986, page 359]. He criticizes the normal science for failing to do that which theory should: provide a means by which to order economic reality. The theory fails to do so because its conceptions are at wide variance with actual economic conditions and it does not provide viable theories for certain economic phenomena which feature prominently in the current economy.<sup>88</sup>

Walrasian theory maintains its status as the field's normal science because comprehension of and adherence to the theory has become a pre-requisite to attaining and maintaining one's status as "a professional economist, and especially an academic one"<sup>89</sup> [Dillard, 1986, page 360].

Judgments concerning professional competence are controlled by members of the dominant paradigm of normal science. The methodology is the ideology. [Dillard, 1986, page 362]



He finds the strong hold very troubling given that normal science exacts a very high toll upon society by blocking innovation, and that economics lacks the checks against the lasting imposition of invalid theory found in the natural sciences:

The tragedy of control of a discipline by the practitioners of normal science is that potentially creative scientists are shut out or discouraged from entering by the closed nature of the society. The danger of this happening, with consequent loss to society at large, is probably greater in a social science such as economics because the tests of validity are more difficult than in natural sciences. . . . Tests of the validity of a general economic theory seldom go beyond the logical consistency of the major model. [Dillard, 1986, page 360]

13. *William Dugger*

Dugger provides a rare example of an author who speaks of normal science being practiced not only in economics' mainstream, but by its heterodoxy as well. In particular, he contrasts the sort of normal science which neoclassical economists practice with the sort institutionalists engage in. On the one hand, neoclassical economists, as normal scientists, test logically deduced predictions via quantitative methods.<sup>90</sup> On the other hand, institutionalists, during periods of normal science, employ a case study approach in order to extend pattern models they have developed.<sup>91</sup> Thus, Dugger allows for the possibility of the simultaneous practice of two very different varieties of normal science in economics.

14. *H.I. Dutton and J.E. King*

Dutton and King observe that in general, normal economic science more often leaves "economic heretics" alone, rather than hurl criticism at them:

Economic heretics are generally ignored rather than burned at the stake. The practitioners of "normal economic science" rarely engage dissidents on their own or any other terrain. Heretics ask embarrassing questions, investigate problems which are not generally accepted as legitimate, and provide answers which rely upon unusual concepts, unfamiliar reasoning, and inadmissible evidence. [Dutton and King, 1986, page 259]

15. *Aidan Foster-Carter*

Foster-Carter finds no examples of normal science in development economics. No theory of development, he contends, commands a following of sufficient magnitude so as to comprise a normal science. Even Rostow's theory of stages, which enjoyed the widest currency, "did not carry the day" and "there has apparently been nothing so much as a school, let alone a full-scale Rostovian 'normal science' . . . " [Foster-Carter, 1976, page 172]. Still again, unlike Kuhn's vision of science in which a single paradigm dominates a field, development economics houses at least two co-existing "paradigms" (Rostow's theory of development and Frank's theory of underdevelopment) [Foster-Carter, 1976, pages 170-171].

Development economics also fails to exhibit another characteristic which Kuhn associated with a normal science: the widespread use of textbooks [Foster-Carter, 1976, page 171].

16. *Donald Gordon*

Similar to Coats, Gordon identifies examples of normal science in the history of economic analysis using Kuhn's threefold division of normal science activity.<sup>92</sup> As examples of "'further articulation,'" of the basic paradigm, Gordon cites "the addition of the principle of variable factor proportions, or the notion of the consumer with relatively stable transitive preferences." Instances of the "'specification under new . . . conditions'" of economics' basic paradigm<sup>93</sup> abound in the history of economics and include "analyses of monopoly and competition, tariffs and free trade, money and government deficits, excise and income taxes, unions and minimum-wage legislation" [Gordon, 1965, pages 123-124]. Significantly, Gordon provides no examples of normal economic science's improving the fit between fact and paradigm. Nor, however, does he make any comment as to normal economic science's success or failure in this area.

Writing in 1965, Gordon affirms that "economic theory is very much like a normal science."<sup>94</sup> He explicitly cites one likeness: "like a normal science" economic theory "finds no necessity for including its history as a part of professional training"<sup>95</sup> [Gordon, 1965, page 126]. However, in doing so, he hints at a second, more fundamental way in which economics in the mid-1960s resembled normal science: the high degree of consensus among economists at that time.<sup>96</sup>

#### 17. *Joel Jalladeau*

Economics is, according to Jalladeau, a normal science. Given this, the field's dominant paradigm (neoclassical economics) determines what questions can legitimately be asked and what explanations, legitimately offered. Economics' current paradigm restricts economists to matters tractable to the "calculus of economic efficiency," and, in turn, rules out of bounds that which the calculus cannot handle [Jalladeau, 1975, page 4]. Most significantly, the paradigm excludes consideration of broad social forces such as socioeconomic "power, class structure, conflict and economic change" [Jalladeau, 1975, page 10].<sup>97</sup>

Jalladeau finds such limitations highly problematical. The socioeconomic questions that the current paradigm disallows even asking are precisely those questions which economists need to answer in order to effectively confront the pressing problems facing the world's economies.<sup>98</sup> He concedes that the neoclassical paradigm can address certain questions very well and stresses that he is not raising objection to the paradigm, in and of itself. However, he takes issue with economics' exclusive reliance upon the one paradigm,<sup>99</sup> and indicates his preference for an economics which complements the neoclassical approach with one which addresses the broad social concerns, which neoclassicism does not.<sup>100</sup>

But, while Jalladeau certainly raises objections to the current normal science in economics, he never directly addresses whether the practice of normal science *per se* in

economics is (un)desirable. He, for example, neither answers nor even asks such questions as, "Would the practice of normal science be acceptable/preferable/possible<sup>101</sup> if the field's dominant paradigm were broader in its focus?".

#### 18. *Hans Jensen*

As Jensen sees it: Stanley Jevons and Alfred Marshall (recognized founders of modern-day microeconomics) and John Maynard Keynes (credited with providing the basis for modern-day macroeconomic theory and policy) espoused and based their work unmistakably upon social economics' principles and conceptions of the world.<sup>102</sup> However, normal economic scientists, in their "articulation" of these works, quickly and effectively excised from them any hint of these broader concerns. The articulators instead focused their energies on the works' more technical (and more easily formalizable) elements. The result: a mainstream economics with a very narrow conception of the economy, which concerns itself little with problems such as poverty and income inequality -- despite the discipline's founders' expansive view of the economy and emphasis upon social problems. We find indication of this in Jensen's descriptions of normal economic science's treatment of all three economists' work. First, Jevons:

The period from the turn of the century to the middle of the 1930's was "The Years of High Theory" in economics [Shackle]. Those were the years in which the theoretical apparatus of the neoclassical paradigm was subjected to its first "articulation" by practitioners of "normal [economic] science"

The primary goal of the articulators was not to intensify "the light . . . [which] pure economics sheds . . . on social economics," however. Rather, they concentrated their efforts on the task of sharpening those purely analytical tools which the fathers of neoclassicism had devised for the purpose of determining "the optimal allocation of resources" in their model -- "under conditions of perfect knowledge and a purely static economy." In the interwar years, neoclassical microeconomics was therefore a far cry from the social economics of Jevons and his coauthors. Unlike the latter, the former was not designed "to have . . . direct influence on political events, or economic policy." [Jensen, 1977, pages 251-252, quoting Kuhn, 1970c, page 24 in the first paragraph, and Walras, 1954, page 392;

Farrell, 1973, page ix; and Little, 1960, page 5, respectively, in the second paragraph]

Next, Marshall:

Why, then, is Marshall not considered one of the premier contributors to social economics? Because his followers in the neoclassical tradition concentrated their attention on his formal analyses. Thus Marshall became the exclusive property of those who practiced "normal research" in mainstream economics. And once the first generation of Marshallians had completed the initial phase of their "articulation" . . . of his formal, static analysis, no traces were left of Marshall the social economist. [Jensen, 1987, pages 34-35, quoting Kuhn, 1970c, pages 36, 23, respectively]

And, finally, Keynes:

The general theory which Keynes constructed in 1936 was "in essence a translation into [theoretical] terms . . . of [his] perception of historical discontinuity" This theory was intensively articulated by the so-called "Keynesians." By virtue of their concentration on analyses of the interrelationships among the variables of the model built by Keynes, most of the works of the Keynesians are ahistoric, however. Hence they largely disregard the subject of reform which Keynes emphasized because of his conviction that it was historically evolved institutional perversities that were responsible for the onset of the great depression. Thus instead of carrying the mantle of reformers, the Keynesians cast themselves in the role of managers who "loosen" or "tighten . . . monetary and fiscal tourniquets" in order to "provide the essential stability" which an otherwise relatively hale economy may occasionally fail to achieve. [Jensen, 1977, page 255, quoting Skidelsky, 1975, page 93; and Heller, 1966, page 9, respectively]

Thus, if one consequence of the practice of normal science in economics has been the eschewal of social economics,<sup>103</sup> another has been the distortion of the field's founders' theories and policy directives.

#### 19. *Hans Lind*

Lind conducts an analysis of normal research in mainstream theoretical economics.<sup>104</sup> His study is highly circumscribed: He looks only at theoretical work conducted in a single economics subfield (international economics); and, still further, only at the published journal articles of a single theorist (Lars E.O. Svensson). Lind, however, brings his study's conclusions to bear upon the whole of mainstream theoretical research [Lind, 1992, pages 94ff.]. While acknowledging the problems

inherent in generalizing from a single case study, Lind contends that given the respect accorded Svensson's work by other economists, his articles "should therefore tell us about much more than a single economist" [Lind, 1992, page 85]. The obverse of this justification, however, is that Lind's study does *not* look at how a less well-known and less well-respected economist conducts his/her analyses.<sup>105</sup>

Lind arrives at a number of conclusions from his study. As to the structure of the normal research he examines: In almost all cases, research begins by laying out an assumed model of the economy, and then proceeds to set forth a number of proofs concerning that model's properties [Lind, 1992, pages 86 and 98]. Most strikingly, however, Lind finds "no trace . . . of the instrumentalistic view that the legitimacy of making false assumptions shall be judged by the effect on the predictive ability" in the normal research [Lind, 1992, page 99]. Realistic assumptions are seen as desirable because they may heighten the applicability of the theory and its findings to the real world, and, in fact, "the dominating type of contribution is to analyze a model-economy that is more realistic than earlier ones in some dimension that is judged to be of importance" [Lind, 1992, page 98].<sup>106</sup> Further, unrealistic assumptions are justified/excused on grounds other than a theory's (expected) predictive ability [Lind, 1992, page 95].<sup>107</sup> Finally, in contrast with the many economists that describe normal economic scientists proceeding from the deductive derivation of theories and hypotheses to their inductive evaluation, Lind finds "no trace . . . of the hypothetico-deductive method" [Lind, 1992, page 99].

Lind states at the outset that he is interested in what is, rather than what ought to be.<sup>108</sup> Consistent with this stance, he adopts an agnostic stance toward the normative status of the normal research he uncovers:

Just as there are no grounds for looking on the analysis of model-economics as *The Rigorous Way* of doing theoretical science, there seem to be no grounds for looking on it as in general more questionable than other types of scientific activity. [Lind, 1992, page 99]

20. *Edythe Miller*

According to Miller, normal economic scientists<sup>109</sup> begin with "first principles" provided from sources outside the field. From these principles, they deduce a "model of the world;" and from this model, "truths about reality." Finally from these truths, they deduce hypotheses. They then subject these hypotheses to empirical test. Economists subject only these hypotheses (not the "first principles," the "truths," or their "model of the world") to empirical evaluation.<sup>110</sup> Miller neither forges any comparisons, nor draws any contrasts between her description of normal economic science and Kuhn's conception of normal science.

She does, however, draw comparisons in other respects. Miller finds that normal economic science resembles Kuhn's depiction of a normal science in that normal economic science "is characterized by that 'abandonment of critical discourse' that Kuhn apprehends as a hallmark of normal science" [Miller, 1991, page 995]. The parallel, however, troubles Miller for the "abandonment of critical discourse" is complete: Normal economic science has become "'a closed society of closed minds'," with "a model of the world that admits of one conclusion only, irrespective of the problem (puzzle) posed." This presents a very serious problem to economics if that model "is irrelevant or otherwise incorrect:"<sup>111</sup>

even very sophisticated manipulation will be for naught when it comes to research and policy prescription; that is, in the solution of problems that are more than or other than Kuhnian puzzles. [Miller, 1991, page 995]

Still further, if Donald McCloskey is correct in that the scientific community determines what counts as economic truth, then any resemblance by economics to a Kuhnian normal science makes "the prospect of the establishment of truth and policy . . . a chilling one" [Miller, 1991, page 997]. What precisely makes the prospect chilling, Miller does not directly specify. However, the lack of critical discussion which Miller finds in a Kuhnian normal science is very likely the source of her concern.<sup>112</sup> A second, though less probable cause, is the unquestioned status which

normal science, according to Kuhn, lends to theory -- even in the face of contradictory empirical evidence.<sup>113</sup>

In the end, however, normal economic science distinguishes itself from Kuhnian normal science. Within Kuhn's conception of normal science, findings that contradict a theory do not reflect badly upon the theory, but do impugn the abilities of the scientist arriving at the findings. In normal economic science, however, contradictory results discredit *neither* the scientist, nor the theory.<sup>114</sup> "The facts are simply assumed away."<sup>115</sup>

Normal economics today sees competition as both ideal form and controlling reality, irrespective of the facts of current existence. That is, by procrustean means, reality is trimmed to fit the theory. In that sense, neither Kuhn nor Popper is proven accurate: Irrespective of the poor fit between the facts and the theory, neither the theorist nor the theory is "in difficulty." [Miller, 1991, pages 1001-1002]

Thus, while Miller is critical of economic normal science's similarities to a Kuhnian normal science, she censures it even more harshly for its differences.

Finally, we may note that Miller directly contrasts institutionalists' practice of economics with Kuhnian normal science in that the former seeks to solve social problems, while the latter looks to avoid consideration of such matters altogether:

At bottom, the instrumental economist is asking whether or not the operation of the economy works to the benefit of its participants, for the improvement of the human condition, and not whether it fits a predetermined pattern. That is the distinction between the institutionalist science, a science that contributes to "active control over the changing course of events" and Kuhn's normal science that insulates the community from "those socially important problems that are not reducible to the puzzle form." [Miller, 1991, page 1001]

## 21. *Lars Osberg*

Osberg identifies human capital theory as the normal science of the economics of earnings behavior. He justifies the theory's normal science status by noting (1) its widespread acceptance and application and (2) the fact that economists find (force, if



necessary) explanations for any anomalies that might arise to fit with human capital theory.<sup>116,117</sup>

## 22. *Michael Piore*

Piore notes that normal economic science greeted the labor market segmentation thesis in much the same hostile manner as Kuhn sees normal scientists greeting radically new ideas.

But as an exponent of labor market segmentation in the community of "normal" economics, I can assure you that labor market segmentation does not fit the paradigm. The sentiments and reactions which Kuhn tells us greet the abnormal and ~~a~~paradigmatic in a discipline, that is, fury, disdain, resentment, sarcasm, and condescension have definitely greeted labor market segmentation. This is a matter of fact; and observation about praxis. [Piore, 1983, page 249]<sup>118</sup>

Dutton and King, on the other hand, observe that, in general, normal economic science more often leaves "economic heretics" alone, rather than hurl criticism at them.<sup>119</sup>

## 23. *Melvin Reder*

Reder portrays the Chicago School as a Kuhnian normal science.<sup>120</sup> Members of the school, he notes, are evaluated in terms of their ability to correctly solve Kuhnian puzzles (i.e., analytical problems) with "correct" solutions needing to comport with the school's fundamental theory/paradigm (tight prior equilibrium (TP)<sup>121</sup>).<sup>122</sup> Like any other normal science, Reder observes, the school resists change. The more fundamental the proposed change, the greater the impediments placed in its way.<sup>123</sup> Resistance runs along both empirical and theoretical fronts. The school is loathe to accept empirical results which run counter to TP<sup>124</sup> and has a decided preference for "'paradigm preserving' or 'paradigm extending'" theories, over "'paradigm shattering'" ones. "Paradigm extending" theories broaden the application of the Chicago View to new areas, while "paradigm preserving" ones provide explanations consistent with the View for phenomena previously thought to run counter to it.<sup>125</sup>

Reder, however, does not read the Chicago school's conservatism as necessarily problematical. It may even be beneficial. After all, not all innovations are worthwhile; most, in fact are not. Given this, the school's resistance to novel approaches serves the vitally important role of separating the wheat from the chaff:

On this view, scientific progress is a process of creative destruction. What is destroyed is the intellectual capital of other scientists whose resistance to accepting new contributions is not only understandable, but desirable; it is only by overcoming this resistance that the few genuine contributions can be separated from the more numerous invalid proposals. [Reder, 1982, page 20]

Similarly, Reder concedes that the school's bias toward accepting data reports meshing with TP may devolve into "dogmaticism," but he sees it just as likely that open-mindedness may cross over into excessive credulity.<sup>126</sup> Reder notes at the outset of his discussion regarding the treatment of evidence: "A new finding is, and should be, screened to see how it bears upon the findings of research programs in a number of related fields" [Reder, 1982, page 21].

24. *Deborah Redman*

Redman definitively denies the existence of normal science in economics: "How 'normal' are economists? To the point: they are not 'normal' at all" [Redman, 1991, page 150]. She cites the lack of a commonly shared paradigm and significant disagreement among economists (even among just mainstream economists) as her reasons. Economists, she claims, do not even agree as to the field's proper focus.<sup>127</sup>

25. *Stephen Rousseas*

Writing in the 1970s -- twenty years earlier than Redman -- Stephen Rousseas saw economics on the verge of becoming a normal science. He cites as evidence the then high degree of and growing consensus among economists. Disagreement, he assents, still existed and economists at that time did not cluster around a single paradigm. However, the distance between adherents of conflicting paradigms was

narrowing and, more and more, economists were gathering around a common paradigm (Keynesian economics):

Of all the social sciences, economics, whether of the type practiced in the capitalist countries of the West or communist countries of the East, comes closest to the Kuhnian idea of normal science. Admittedly, there are divergent "schools" in the West -- the two major ones being the Keynesian and Friedmanian paradigms -- but no less a person than Milton Friedman himself, much to the consternation of Allan Meltzer and Karl Brunner, has now placed himself squarely in the Keynesian camp. We seem to be moving towards a universally received paradigm in Western economics -- witness Walter Heller's joy a few years ago when he announced the conversion of the Republican Party to Keynesian economics . . .

Our graduate schools and the remarkable sameness of our elementary textbooks are a testament to this. . . . [Rousseas, 1973, pages 75-76]

Further, following his description of normal science, Rousseas affirms that economics had come to concern itself with narrow technical problems and had pushed to the side broader social concerns:<sup>128</sup>

the controversies that remain being little more than technical disputes among paradigm polishers. [Rousseas, 1973, page 76]

Our existence is untidy and modern social science seeks to *impose* order and clarity to our existence while ignoring the "totality" for the sake of what can be reduced to manageable, quantitative proportions. As a result, our graduate schools grind out technically trained, puzzle-solving, unimaginative social engineers locked into what is; in a word, plumbers rather than innovative social architects. [Rousseas, 1973, page 77]

Thus, Rousseas finds economics' development into a normal science highly problematical. The growing concern for narrow technical problems to the detriment of the study of broader social questions had stultified economists' critical spirit, imagination and creativity.

Like Dillard, Rousseas concludes that once a central paradigm has been established in the social sciences, displacing it will be considerably more difficult than in the natural sciences:

If I am correct in my contention that we are moving towards a universally accepted (I'm tempted to say "imposed") paradigm within the social sciences, the probability of a paradigm shift would, in my view, be smaller than that for the natural sciences. [Rousseas, 1973, page 76]

26. *Robert Solow*

Solow doubts whether radical economists can, want, and by implication do, practice normal science. We may infer from his remarks that the reason lies in radical economists' inability and unwillingness to engage in empirical work aimed at testing their school's predictions:

But there is little evidence that radical political economics is capable of generating a line of normal science, or even that it wants to.

Here are some examples of what I mean. Professor [John] Gurley says: "As radical economists see it, the shares of national income going to workers and to property owners are largely determined by the relative power of the two groups, although relative factor supplies set limits within which the power exerts itself." Am I to presume from this that there are studies of time series that show that short-run fluctuations in distributive shares reflect short-run fluctuations in the distribution of power in society? This would mean that workers are more powerful when there is a lot of unemployment than they are when there is very little, because the share of wages is highest when the economy is most depressed. Or has it been found in many countries that the direction of long-run change in distributive shares corresponds to the long-run trend in the independently measured distribution of power in society? [Solow, 1971, page 64, quoting Gurley, 1971, page 59]

27. *James Swaney and Robert Premus*

Swaney and Premus do identify and describe normal science in economics. In doing so, however, they sharply contrast the way in which theories are formulated in normal economic science from the way in which "theory construction usually proceeds" "in normal science" in the natural sciences. In the natural sciences, theories are "usually" constructed upon an empirical basis. Scientists begin by evaluating, examining, selecting and ordering facts. Then, on the basis of those facts, they develop theories (which are subsequently tested against other data and revised, if necessary) [Swaney and Premus, 1982, pages 716-717]. In normal economic science, however, economists take a "quantum leap" past the first stage directly to the formulation of theory; and, instead of deriving theory on the basis of fact, deduce theories out of their (inadequate) view of the world.<sup>129</sup> Then, much as in normal natural science, economists test their theories against the facts.<sup>130</sup> However, given

economists' failure to take a careful look at the facts in the first place and to build their theories upon those facts, empirical work in economics is much more hit or miss than it is in the natural sciences. *In principle*, "quantum leap theorizing" might lead economists to the same point as their natural science counterparts: consensus around well-established theories. However, the route to this end, if accomplished, will be significantly more circuitous and contentious than if economics had followed natural science's example and constructed theories on the basis of well examined, chosen and ordered facts in the first instance:

The result is that model verification in economics is frequently a highly subjective trial-and-error process involving numerous applications of econometric methods. It is trial-and-error because the econometric models are not based on reality, but rather contain the nonscientific biases of the theoreticians. A voluminous and often controversial literature on the subject consequently emerges. This process may eventually result in a sorting out of the common and crucial elements and a forging of a qualified consensus of opinion, but it is a far cry from logical positivism as practiced within normal natural science.

. . . . a more straight-forward procedure -- consistent with the application of scientific methods in the natural sciences -- would be to place more emphasis at the initial stage of theory development on the use of inductive methods to analyze the institutional, cultural, social, and technological interactions of the phenomena. [Swaney and Premus, 1982, pages 720-721]

And, *in practice*, far from producing such a consensus, normal economic science's quantum leap theorizing has produced widespread controversy within the profession:

disagreements over relevant variables, the functional relations of these variables, and estimation procedures and interpretations of empirical works are combined with a general vagueness of concepts in both the migration and monetary literature. We interpret this generally poor state of empirical knowledge as reflecting a peculiar and inefficient application of the scientific process; namely, the repetitive econometric applications of models in an attempt to "discover" facts that are relevant (irrelevant) to the common and crucial elements of theories insufficiently grounded in empirical reality. [Swaney and Premus, 1982, pages 724-725]<sup>131</sup>

Ironically, Swaney and Premus cite the complete and utter domination of the neoclassical paradigm in economics as the root cause of economists' disagreements. The neoclassical paradigm's strong hold upon the discipline has marginalized the

importance of empirical investigation. As a result, economics -- though dominated by a single paradigm -- now finds itself populated with a multitude of divergent theories:

Thus, the continued proliferation of theories, rather than indicating economics is preparadigmatic science, suggests that the dominant neoclassical paradigm is such a complete and dominating world view that it inhibits inductive development of theories that reflect economic reality. [Swaney and Premus, 1982, page 726]

In addition, the quantum leap theorizing spawned by the neoclassical paradigm's domination "has not been successful in establishing theories with significant empirical import" [Swaney and Premus, 1982, page 726] and has yielded inferior and irrelevant policy prescriptions [Swaney and Premus, 1982, pages 721 and 726]:

It is through this quantum leap process that overgeneralization occurs and "realism" in the sense of policy relevance is lost. [Swaney and Premus, 1982, page 726]

Here again "sub-quantum leap" theorizing as practiced in normal natural science would be of help to economists.<sup>132</sup>

While Swaney and Premus can most accurately be seen as contrasting normal economic science with another type of normal science (normal natural science), they to some degree can be interpreted as contrasting normal economic science with Kuhnian normal science. First, they ally normal natural science with Kuhnian normal science. Their explanation of "progress in the natural sciences" consists of little more than their interpretation of Kuhnian normal (and abnormal) science [Swaney and Premus, 1982, pages 715-716]. Second, while consistently referring to "normal economic science," they often refer simply to "normal science" when speaking about normal science in the natural sciences [Swaney and Premus, 1982, pages 716ff.]. Given the connections they draw between Kuhnian normal science and normal science in the natural sciences, we might interpret their contrasts between economic and natural normal science as contrasts between economic and Kuhnian normal science. Still further, as we noted in the previous section, Swaney and Premus ally Kuhnian normal science with scientific

practice consonant with positivists' understanding of science. The authors, however, contrast normal economic science practice directly with the positivistic description:

The first stage of theory development in normal economic science, then, is typically a quantum leap from reality to theory. Economists typically engage in nonscientific behavior to develop their theories, and only then do they begin to employ the modern empiricist scientific process described by Carl Hempel and other logical positivists. [Swaney and Premus, 1982, page 720]

We are thus left with at least two understandings of what Swaney and Premus are doing: (1) They contrast two types of normal science (economic and natural normal science) with one another and find the latter type preferable. (2) They find that economics does not practice Kuhnian normal science, but would fare better if it did.

28. *George Vredeveld*

Vredeveld employs Kuhn's trichotomy of puzzle-solving activity to assess the success of modern econometrics and arrives at a similar conclusion regarding econometrics as Coats did about economics in general. Econometrics has performed poorly in seeking to heighten the match between fact and theory, but has functioned well in the generation of facts "to explain the nature of things," such as the estimation of elasticities.<sup>133</sup> However, Vredeveld applies the tri-fold division without mention of the term "normal science," and thus reaches no conclusions about whether economics (or econometrics) constitutes a normal science.

29. *Benjamin Ward*

Ward finds that neoclassical economics passes all the tests necessary to qualify it as a Kuhnian normal science: (1) Neoclassical economists form an "invisible college" whose members are united by virtue of the training they receive, their frequent interaction and the research projects they conduct [Ward, 1972, pages 8-9]. (2) Economists primarily restrict their attention to matters of detail.<sup>134</sup> (3) While there are frequent disagreements among economists, "the important point is that these disagreements occur within a framework of general agreement" and presume "that a

way has already been found to prescribe the procedures for resolving the conflict among members of the scientific community that are generally acceptable to that community." Thus, rather than finding economists' disagreements antithetical to normal science, Ward concludes: "Such disagreement within agreement lies at the heart of the process of normal development of a science" [Ward, 1972, pages 11-12]. (4) Paralleling the requirement that in a normal science, only the opinions of the discipline's members count, for neoclassical economists "there can be no appeal other than to their peers in the field" [Ward, 1972, page 12]. (5) Finally, with the exception of the history of economic thought, "all the fields of economics have been quite astonishingly self-sustaining," evidenced by the ten-fold increase in the number of economists from 1932 to 1972 and the large number of regular contributors to economics journals [Ward, 1972, page 13]. Thus, Ward concludes, economics qualifies as a Kuhnian science.<sup>135</sup>

Ward, however, upon examining the practice of normal economic science, finds reasons for concern. First, he finds it problematical that an ideology (liberal philosophy and the pretension of value neutrality [Ward, 1972, pages 24-28]) has such an overwhelming influence over economic practice.<sup>136</sup> Second, the nature of puzzle solving in neoclassical economics troubles Ward. True, economists engage in a considerable amount of puzzle solving ala Kuhn.<sup>137</sup> But, while economists agree as to what puzzles should be worked on, they rarely agree on how to solve them or, still further, what constitutes a correct/acceptable solution. Consequently, economists, even though continually positing new puzzles, rarely solve any old ones [Ward, 1972, pages 19 and 32]. What thus comes to matter is not finding a solution to a puzzle, but rather "mak[ing] an ingenious attempt at solving it within the conventional framework of puzzles" [Ward, 1972, page 19].<sup>138</sup> Economists, however, in valuing form over solution, run the risk of losing touch with the real world.<sup>139</sup> Finally, Ward finds it



worrisome that neoclassical economics' system of social control so tightly restricts economists' practice in order to sustain the field's status quo:

And still more fears will come from recognition of the possibilities afforded by stylized facts and the power of insiders to control the trajectory of the science by focusing practitioners' attention on areas that are consistent with the survival of the existing structure, social and substantive, of the science. [Ward, 1972, page 32]

It is important to note, however, that Ward's objections are directed at what he finds in economics' practice of normal science -- not, necessarily, normal science in general. Ward, it would seem -- all else equal -- would be less critical of economics' normal science practice if it weren't ruled by ideology. Still further, if economists actually found solutions for most of the puzzles they posed, Ward would likely be less concerned about economists' practice of normal science. Here, though, we should note that Ward implies that it is highly unlikely that economists could ever solve a significant number (let alone a majority) of puzzles they pose owing to the lack of clearly defined concepts and the impracticability of obtaining the information necessary to conclusively solve an economics puzzle.<sup>140</sup> All that we can say definitively is that Ward has serious reservations about the practice of normal science in economics.

According to Ward, Marxism has the potential for being a normal science. It certainly lays out numerous puzzles for practitioners to work on solving (e.g., "How can there be exploitation when goods exchange at their labor-values?") [Ward, 1972, pages 58ff.].<sup>141</sup> As well, Marxists share a common framework within which to work (a dialectical interpretation of the socio-economy, the centrality of class conflict and the notion of surplus value) [Ward, 1972, pages 63-66]. They even share a common outlook on world issues [Ward, 1972, pages 66-67]. Marxism in practice, however, fails to amount to a normal science. The primary reason: rather than busying themselves with detailed work aimed at filling in the lacunae left by the school's founders, Marxists have spent the bulk of their time engaged in ideological debate:

Far from exhibiting the analysis of problems of detail that one expects from a normal science, far from exploring the ramifications of the theory left in so unfinished a state by Marx and Engels, these [Marxist] journals are mostly devoted to journalistic accounts of contemporary events, nitpicking discussions of essentially definitional matters, and reviews and polemics on the works of Marxists and neoclassicals. With only occasional exceptions, Marxism comes across in the journal literature as an ideology, not a science. [Ward, 1972, page 68]

There were, according to Ward (writing in 1972), no more than twenty Marxists engaged in puzzle solving activity. Why so little puzzle solving, why so few puzzle solvers?

Ward cites several reasons. (1) In some instances, governments and/or political parties -- rather than Marxist economists themselves (as would be the case under a Marxist normal science) -- have ultimate authority in determining the direction of the school's work [Ward, 1972, page 68]. (2) In hostile environments, Marxist economists feel the need to restrain their work in order "to avoid the misunderstandings that may result from the inevitable controversy that accompanies the normal development of a science" [Ward, 1972, page 69]. (3) Often, Marxist economists channel their energies into converting "the masses" rather than the practice of science [Ward, 1972, page 69].

Further, despite having noted that Marxists share common puzzles, similar perspectives on world issues, and a shared conceptual framework, Ward finds the potential of an invisible college of Marxists unrealized. Marxists remain significantly divided from one another, largely by virtue of their "inability to transcend nationalism:"

One of the greatest failings of twentieth-century Marxism was its inability to transcend nationalism. We need not list the reasons for this, but a consequence was the reinforcement of the divisive tendencies inherent in the breakup of the potential invisible college into a set of schools whose conflict is not basically over the scientific interpretation of Marxist economics, but over power in the wider world. [Ward, 1972, pages 69-70]<sup>142</sup>

30. *Richard Whitley*

Whitley maintains that, like initiates to a normal science, economic students are indoctrinated into the field in such a way that they come to adhere to a common set of core beliefs, values and accepted methods. Chief among these is a high priority for theoretical work and a concomitant neglect for empirical investigations.<sup>143</sup>

31. *The Journal Literature*

Many economists cite journal articles or papers (as opposed to longer works) as examples of normal science research in economics. Almost all the works Blankart cites in his description of normal science in public choice economics are either journal articles or papers presented at professional meetings [Blankart, 1987, pages 6-8]. Still further, the second half of Blankart's discussion of normal science concerns itself with a quantitative analysis of the papers presented at the meetings of the European Public Choice Society [Blankart, 1987, pages 8-11]. Likewise, Argyrous examines the journal literature in analyzing the normal science research spurred by Friedman's and Modigliani's work in the area of consumption [Argyrous, 1992, pages 239-243]. As does Lind, when investigating normal research in theoretical economics, analyze the content of the journal articles published by a prominent theorist [Lind, 1992].<sup>144</sup>

Thus, a number of economists find normal science in the economics journal literature. However, this is not to say that they, in doing so, implicitly equate normal science with the journal literature. Foster-Carter does contrast the relative lack of importance of the journal literature in development economics as one chief contrast between the practice of economics in that subfield and the practice of normal science. However, reviewing his arguments, the lack of a consensus around a common paradigm figures more significantly in his determination that development economics does not comprise a Kuhnian normal science than does the relative unimportance of journals in that subfield.<sup>145</sup> Still more, Argyrous, while focusing his attention on the journal literature, makes clear that the existence of a journal literature (even a sizeable

one) is not, in and of itself, sufficient proof that economists are engaged in normal science: "the number of articles devoted to a subject does not of itself prove the existence of a normal science tradition" [Argyrous, 1992, page 238].

Others draw a more explicit link between journals and normal science. Connor, who allies investigative economics (economics(i)) with Kuhn's notion of normal science, describes "economics(i)" as "what economists are publishing" [Connor, 1991, page 59]. He remarks further, "In the main economists(i) are academics. Their success depends on articles published . . . " [Connor, 1991, page 60]. And, Charles Rowley describes "scholarly journals" in general as "conduits facilitating that preoccupation with 'puzzle-solving' that normal science epitomizes" and, thus, "vehicles of normal science, constrained by the vision of the past" [Rowley, 1991, page 164].

Finally, some economists, while not making direct reference to Kuhn's normal science concept, cite the philosopher's recognition of the important role played by scientific journals as a prelude to a quantitative examination of the economics journal literature. Coats, for example, introduces his statistical analysis of the *A.E.A. Index of Economic Journals* by citing Kuhn (among others) as examples of the "Several recent studies in the history, philosophy, and sociology of science [that] have demonstrated the central, if subtle and complex, role of the publication process in the communication, scrutiny, and acceptance of new ideas" [Coats, 1971, page 29].

### 32. *Summary and Conclusions*

We find that economists characterize normal economic science in much the same way as they do normal science in general. Paralleling economists' recognition of the importance of the journal literature in normal science in general, we have seen that discussion of the economics journal literature figures prominently into many economists' discussions of normal *economic* science. Further, consistent with the

connections which many economists draw between normal science and a science's dominant paradigm, we find that a number of economists link normal economic science with economics' mainstream paradigm. Dillard and Cornwall in large part equate normal economic science directly with economics' regnant paradigm -- though, we should note, they do not identify the same paradigm. In addition, several economists (Dillard; Rousseas; Swaney and Premus, and Ward) point out that economics' mainstream paradigm exercises an extremely strong (perhaps even insuperable) hold over normal economic science. Similarly, Miller parallels mainstream economists' unquestioning acceptance of regnant theory with Kuhn's understanding of normal science as an uncritical enterprise.

Conversely, we find only one example of an economist speaking of the practice of normal science in economics' heterodoxy (Dugger (institutionalism)),<sup>146</sup> and only one advocating the practice of normal science outside the mainstream (Cohen (post-Keynesianism)). Along these lines we should note that Dugger, even while identifying a normal science in institutionalism, contrasts institutionalist normal science with mainstream normal science. Still more, we find a number of economists (Miller (institutionalism), Cornwall (post-Keynesianism)) who expressly distance the practice of normal science from work conducted by a heterodox school of economics. In addition, though not in principle ruling out that heterodox economists may practice normal science, both Solow and Ward contend that radical economics and Marxism, respectively, do not comprise a normal science. Finally, Baumberger's argument that economics does not comprise a normal science hinges largely upon his finding a vital heterodoxy which exerts a palpable influence upon the mainstream.

As we also saw, economists allied normal science with a resistance to ideas or facts at wide variance with the regnant paradigm. Here again, we find descriptions of normal economic science which parallel this understanding. Piore characterizes mainstream economics' reception to novel ideas as hostile; Dutton and King describe it

as cool. Canterbury and Burkhardt maintain that normal economic science has shunned radical viewpoints, and Reder likewise sees Chicago School normal scientists' resistance to new ideas as directly proportional to those ideas' innovativeness.

As to normal economic science's reluctance to accept evidence running counter to the paradigm's expectations, we may list several economists, including Argyrous, who highlights normal economics' hesitation to accept evidence running counter to Friedman's consumption hypothesis as conclusive evidence; and Reder, who describes Chicago School normal science as slow to accept evidence running counter to the tight-prior equilibrium assumption. Similarly, Osberg contends that normal economic scientists in labor economics continue to adhere to human capital theory in the face of anomalies. Finally, as we saw, some economists (Cornwall and Miller) contend that normal economic scientists are undisturbed by the irrelevance of their theories to economic reality.<sup>147</sup> Related to a perceived lack of concern on the part of normal economic science to contradictory evidence, some economists (Swaney and Premus, and Whitley) contend that normal economic scientists lay considerably greater importance upon *deduction*, as opposed to *induction*.

Similarly, just as economists ally normal science with the marginalization of social concerns, we find that many economists describe normal economic science as casting aside such matters. As we saw, Jensen explains how normal scientists, in their articulation of various prominent economists' writings, excised these economists' deep-seated concerns for social matters. Miller, as well, contrasts institutionalism's concern for social matters with an insulated Kuhnian normal science. Here, we may also include Brinkman who sees economics' current normal science as training its students to disregard broader social factors in their analysis, as well as Jalladeau, who contends that current normal economic science excludes consideration of social factors. Similarly, Gordon argues that economics, in resembling a normal science, places little importance upon the study of its history.

Along similar lines, we find that many economists (Ward; Canterbury and Eichner, and Dillard) describe normal economic science -- as economists had described normal science in general -- as insulated from outside influences and setting itself up as the sole arbiter as to what constitutes good work or work worth doing.

As we also noted earlier, many economists cite the existence of consensus (at least as regards fundamentals) among a discipline's scientists as another attribute of a normal science. Consistent with this, we find that many economists' (Baumberger's; Bliss's; Canterbury and Burkhardt's; Foster-Carter's; Gordon's (by implication); Osberg's; Redman's; Rousseas', and Ward's) determinations as to whether economics (or some portion of it) comprises a normal science hinge, at least in part, upon an assessment of the degree of consensus among economists . We, however, find that these economists vary as to whether economics (or some part of it) constitutes a normal science. These assessments vary along a couple lines. First, sub-field. Osberg, examining the economics of earnings, argues that the high degree of consensus around human capital theory indicates the existence of a normal science, whereas Foster-Carter, looking in development economics, finds no indication of the consensus required for a normal science. Second, time. Gordon writing in the mid-1960s and Ward, in the early 1970s, argued that economics possessed the high degree of consensus among its members necessary to constitute economics as a normal science. Redman, however, writing in the early 1990s, contends that economics is not a normal science -- in part, because of the significant lack of consensus among economists.

At the same time, however, we find disagreements among economists writing around the same time as to whether economics possesses the sort (and level) of consensus that a normal science does. Here we find minor as well as major divergences in economists' assessments. Rousseas, writing only one year after Ward, does not contend -- as Ward does -- that economics *already* was a normal science, but rather, that a consensus among economists was building which indicated that economics

was on the *verge* of becoming a normal science. More striking, however, is the contrast between Redman and Bliss. Writing only five years apart, Redman and Bliss come to fundamentally different assessments as to the degree of consensus among economists and thus the normal science status of economics. Redman contends that even mainstream economists lack the consensus necessary for a normal science, while Bliss concludes that economics is a normal science given the fundamental unanimity he finds among economists in general, *including both heterodox and orthodox economists*.

Among economists who locate normal science in economics (or some part of it), we find that most who provide an indication of the degree of consensus among members of that normal science (Bøhren, Bliss, Canterbury and Burkhardt, Gordon, Osberg, Swaney and Premus, Ward and Whitley) assert that normal economic scientists share a common paradigm or agree as to fundamentals.<sup>148</sup> These economists, however, differ as to their accounts of overall agreement in the normal science they identify. Gordon lays stress almost exclusively upon the high level of consensus among economists. Similarly, Canterbury and Burkhardt and Whitley provide little to no indication of disagreements among normal economic scientists. Ward and Bliss acknowledge disagreements among economists, but stress that these disputes are of minor importance. On the other hand, Swaney and Premus, discussing economics in general, and Bøhren, a subfield within it, both point to substantial disagreement among normal economic scientists sharing the same paradigm. Along similar lines, we will recall that Johnson and Ley, while identifying Classical economics as a normal science and finding its members gathered around a common paradigm, also locate significant divergences among Classical normal scientists. Thus, while most economists commenting upon the consensus among normal economic scientists find them in agreement as to fundamentals, these commentators diverge in their accounts of the degree and nature of the general consensus among normal economic scientists.



Economists' use of Kuhn's trichotomy of normal science puzzle solving also varies. Some, such as Gordon and Argyrous, cite examples of such puzzle solving activity as evidence/examples of normal economic science. Coats, on the other hand, in examining economists' accomplishments along these lines, raises questions as to economics' status as a normal science. Vredevelde employs the trichotomy without even broaching the question as to whether econometrics constitutes a normal science.

All this taken together points to a conclusion similar to the one we reached in our discussion of economists' specifications of paradigms. Economists have offered multiple interpretations as to the status of economics as a normal science. Economists - even ones writing around the same time, even ones employing similar criteria as to what constitutes a normal science -- have disagreed as to whether economics is a normal science. Further, those who depict economics as a normal science have often highlighted different aspects of it. Finally, even those providing descriptions of normal economic science along similar lines (e.g., consensus), have provided divergent characterizations of normal economic science.

Economists also offer divergent normative assessments of normal economic science. A few applaud the ways in which economics resembles a normal science. Argyrous lavishes praise upon the normal science puzzle solving he locates in economists' work in consumption theory. Reder, similarly, looks favorably upon the reluctance of normal scientists in the Chicago School to countenance novel theories or evidence running contrary to their theories. Cohen's call to post-Keynesians to behave more like normal scientists as well constitutes an endorsement of the practice of normal science in economics. On the other hand, Lind contends that there are grounds neither to exalt nor disparage the model-building normal science he finds in theoretical economics.

However, most economists (Boland; Brinkman; Canterbury and Burkhardt; Cornwall; Dillard; Jalladeau; Miller; Rousseas; Swaney and Premus, and Ward) who

offer a normative assessment of normal economic science are highly critical. Many of their criticisms follow along the parallels we have drawn between economists' descriptions of normal economic science and normal science in general. For instance, many economists are alarmed by the strong hold economics' dominant paradigm has (may have) over normal economic science. As we saw, Dillard expresses serious concern that this strong hold squelches innovation in the profession, and Swaney and Premus charge that the regnant paradigm's hold upon economics has, to the detriment of economics, marginalized the role of inductive inquiry. Miller similarly argues that, given the lack of critical spirit in normal economic science, economic theories will be true and/or relevant only by accident, as does Jalladeau charge that strict allegiance to economics' dominant paradigm has led economists to overlook exactly those problems which matter most in modern-day economies.

Several economists also criticize normal economic science for basing its theories and policies upon unrealistic conceptions of the economy. Cornwall, for instance, argues that normal economic science's lack of concern for either the predictive ability or the realism of its theories has resulted in theories which provide us with no or an incorrect understanding of the world and with policies irrelevant to modern-day problems. Miller similarly castigates normal economic science for its indubitable ability to overlook disconfirming evidence.

Along similar lines, economists also criticize normal economic science for ignoring many pressing social issues and concerns. As we saw, Brinkman intimates that initiates to normal economic science are taught to eschew consideration of economic institutions before they have the wherewithal to make an informed decision as to their relevance. Jalladeau finds normal economic science's eschewal of social matters highly problematical; mainstream economists' neglect of social issues renders them unable to provide the basis for formulating relevant theories and policies. Similarly, as we saw, Rousseas holds the eschewal of social problems partly

responsible for the conspicuous lack of imagination among social scientists, economists included.<sup>149</sup>

Finally both Ward and Boland criticize puzzle solving activity among economists for failing to solve really all that much. However, we must draw a distinction between Ward and Boland. While Boland is critical of puzzle solving *per se*, Ward's censures apply more narrowly to the sort of puzzle solving which economists do. More generally, we find that while some economists censure normal science in general, and thus normal economic science in particular, others more narrowly direct their criticisms only at normal economic science, but not necessarily normal science in general. For instance, both Jalladeau and Dillard find the strong hold which the current economic paradigm exercises over normal science as problematical. However, while Dillard indicates that the strong hold of *any* paradigm impairs the practice of normal economic science, Jalladeau does not foreclose the possibility that the practice of normal science under a different paradigm (in particular one which took proper account of social concerns) would be desirable. Along these same lines, Swaney and Premus do not criticize economists for practicing normal science *per se*, but, rather, for practicing bad normal science. Finally, while Miller is critical of economics' resemblance to a Kuhnian normal science, she is even more critical of its differences.

We should perhaps not be surprised that so many of the economists we located are critical of normal economic science. As we noted in Chapter Two, many of the articles which employ Kuhn's schema have been published in heterodox economics journals. Given that so many economists ally normal science with the economics orthodoxy, the preponderance of criticism against normal science (i.e., orthodox economics) is to be expected. Further, the relative lack of praise we found for normal economic science is perhaps also to be expected. If normal economic scientists resemble many economists' depictions of normal scientists in general, they would care

little about either their discipline's history or methodology.<sup>150</sup> However, as we found in Chapter Two, almost two-thirds of a sample of journal articles citing Kuhn were concerned with these two areas. This being the case, we would expect very few of the articles discussing (and assessing) normal economic science to be written by normal economic scientists -- who, we might assume would have the greatest interest in defending (and would look most favorably upon) normal economic science.

Further, if normal economic scientists, as many economists characterize normal scientists in general, practice without questioning the fundamental structure (paradigm) under which they work, then we would expect that most normal scientists would see little point in expending much energy defending normal science. Rather than *analyzing* normal science, they would be *practicing* it. Thus, in some sense, the relative dearth of economists found praising normal economic science provides us with the final parallel we draw between economists' descriptions of normal science in general and normal economic science in particular.

## NOTES

1. Colander and Koford: "The 'realitic' method aims at successful description of regularities for a particular place and time. It wants to be 'conceptually' consistent with the results of any existing 'analytic' research program to the extent that such results aid in explanation, but it is willing to give up rigor, tidiness, or elegance of theory for gains in practical application. It is usually adopted by those with an intermediate discount rate and a concern with affecting practical affairs. Realitic theory usually is important in building up a more complete analytic theory, and so it often has major long-term *theoretical* payoffs, as Thomas Kuhn argues" [Colander and Koford, 1979, page 710].  
Colander footnotes his reference to Kuhn in this paragraph to Chapter Three of *The Structure of Scientific Revolutions*, "The Nature of Normal Science" [Colander and Koford, 1979, page 727, footnote 12].
2. DeGregori: "When the realm of discourse is shifted from the level of generalization of large civilizations undergoing long-term change to the level of seeking to understand specific transformations over shorter periods, greater specificity is needed for the concept of invention or scientific discovery than is found in Ayres. It is one thing to argue that the heliocentric theories of astronomy developing in the 1500s were a movement in the right direction away from the more prevailing geocentric notions; it is another to understand at what point navigation practices predicated upon heliocentric theory were in fact operationally superior to those in practice that were derived from a Ptolemaic conception of the universe. *As Thomas Kuhn has noted, after the emergence of a new paradigm, it takes some time for 'normal science' to work out the practical implications of the discovery*" [DeGregori, 1977, pages 864-865, emphasis added].
3. Along similar lines, Sachiko Matsui, Choorchiro Asano and Yoshiro Matsuda assert that, "the normal science standardizes a set of theories as paradigms to be used as routine procedures for practical applications and textbook knowledge for a classroom" [Matsui, Asano and Matsuda, 1989, page 123].
4. Rowley asserts that normal science "epitomizes" a "preoccupation with 'puzzle-solving'" [Rowley, 1991, page 164].
5. Karsten: "He [Kuhn] defines 'normal science' as research which, based upon past scientific achievements, leads to the formation of a field of study acknowledging the science as the basis for further practice and permitting its 'scientists' to resolve problems or 'puzzles'" [Karsten, 1973, page 401].
6. Backhouse: "Puzzle-solving is research where the results are generally known beforehand, where it is known that there is a solution, and which operates within certain rules [Backhouse, 1985, page 4].
7. Caldwell: ". . . a failure to reach a solution to a particular problem usually is taken more as a reflection of the [normal] scientist's competence than of the nature of the problem or methods used" [Caldwell, 1982, page 71].
8. Along similar lines, Miller notes that, "Kuhn himself notes a fundamental disagreement between the Kuhnian and the Popperian formulations." That

disagreement boils down to the fact that, following Popper, problems scientists encounter reflect badly on the theory, not the scientist. For Kuhn, just the opposite is true [Miller, 1991, page 996].

9. Johnson and Ley: "Both empirically and theoretically, the activity of normal science involves three broad categories: (1) the nature of the facts and theory of interest to the original formulators of the paradigm, which reveal its scope and purpose, (2) the facts and theory that it is hoped will yield new predictions and applications of the paradigm, and (3) efforts designed to reformulate the paradigm in such a way as to resolve its ambiguities and clarify its qualitative dimensions, while leaving its basic structure unchanged" [Johnson and Ley, 1990, pages 27-28].

Argyrous: "He [Kuhn] argues that normal science consists in the actualization achieved by (1) extending the knowledge of those facts that the paradigm displays as particularly revealing, (2) by increasing the extent of the match between those facts and the paradigm's predictions, and (3) by further articulation of the paradigm itself" [Argyrous, 1992, page 238].

Pheby describes the three different areas of puzzle-solving as: "the determination of significant facts," "the articulation of the theory" and "the matching of facts with theory" [Pheby, 1988, page 39].

Gordon quotes Kuhn directly on this matter: "Thus from basic models [paradigms] 'spring particular coherent traditions of scientific research' which develop the promise inherent in the basic model 'by extending the knowledge of these facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself'" [Gordon, 1965, page 123, quoting Kuhn, 1962, pages 10 and 24, respectively].

Coats [Coats, 1969, page 292], Stanfield [Stanfield, 1974, page 98], Peabody [Peabody, 1971, page 2], L.E. Johnson [Johnson, 1980, page 57] note the same threefold division of Kuhnian normal science work.

10. Pheby: "All experiments within normal science can only be conducted within the confines of a particular paradigm" [Pheby, 1988, page 39].

Johnson and Ley: "Broadly speaking, normal science refers to the research carried on within the context of an accepted paradigm" [Johnson and Ley, 1990, page 27].

Caldwell: "Normal science requires the existence of a paradigm . . . The concepts of normal science and paradigm are intertwined, for the archetype of mature scientific activity is normal science research taking place within the framework provided by a paradigm" [Caldwell, 1982, page 71].

Hirsch describes Kuhnian normal research as "work which fits within the bounds of a given paradigm" [Hirsch, 1976, pages 205-206].

11. Similarly, Cohen asserts "Most observers agree that the bulk of scientific activity instead fits Kuhn's (1970, p. 10) characterization of 'normal science': 'research firmly based upon . . . past scientific achievements . . . that some particular scientific community acknowledges for a time as supplying the foundation for its further practice'" [Cohen, 1984, page 616-617].
12. According to Negishi, for Kuhn, "The history of science . . . is marked by long periods of steady refinement, normal science or problem-solving activity in the context of an accepted theoretical framework, a paradigm . . ." [Negishi, 1985, page 4].

According to L.E. Johnson's interpretation of Kuhn, "*normal science* mean[s] the research carried on within the constraints of the accepted paradigm" [Johnson, 1980, page 57].

13. Glass and Johnson assert that "while normal science will involve research work that is aimed at improving the theory/observation fit, and while this work will, in turn, involve empirical testing to check this improved fit, it must be noted that this testing is *not* viewed as an attempt to either confirm or falsify existing theories. Rather this testing is viewed as a way of checking whether or not a proposed puzzle solution is successful, *while still maintaining implicit trust in the prevailing paradigm*" [Glass and Johnson, 1989, page 155].

While not using the term paradigm, Lind's description of "normal research" closely parallels the understanding that such research is conducted with "implicit trust in the prevailing paradigm." Lind asserts that in "normal research, that is, highly qualified research made within a certain tradition[,] [b]asic theories and basic research strategies are taken as given" [Lind, 1992, page 85].

14. Argyrous' above remark represents his explication of the second of normal science's three activities ("increasing the extent of the match between those facts and the paradigm's predictions") [Argyrous, 1992, pages 238-239].
15. Gordon cites as one of normal science's "weaknesses," that fact that "normal research does not 'test' the basic model, the soundness of which is taken for granted" [Gordon, 1965, page 123].
16. Williams: "Those who read Popper's published work which predates the Kuhnian critique will derive the impression that the controls of experimental test and critical discussion operate to constrain the subject wherever and whenever theories are objectively in a relation of competition. . . . For, in the debate with Kuhn, the Popperians have admitted the existence of paradigms by accepting that 'normal science,' in Kuhn's sense exists." (Popper, 1970: 52). This is to admit large organisations of knowledge which are the property of the subject and which inevitably get in the way of the operation of these controls" [Williams, 1975, page 327].
17. Similarly, Jalladeau and Ward remark:

Standardized research favors the deepening and widening of knowledge pertaining to specific points rather than a fundamental questioning of the central theoretical core agreed upon by the community of scholars. [Jalladeau, 1975, page 3]

The problems on which they typically work are problems of detail. An individual researcher is working at any one point in time on some relatively minor aspects of the science. He may be trying to improve somewhat the accuracy with which the value of some constant is known, or he may be trying to modify some portion of the theory to make it fit a new range of data. He will definitely *not* be seriously engaged in answering the question, "What's wrong with science X?". [Ward, 1972, page 6]

In this regard, we should note that Pheby stresses that "Kuhn does not accept that normal science need necessarily become a dogmatic exercise." Still

further, Pheby notes, Kuhn does not "feel that normal science should be treated as a case of 'might being right'" [Pheby, 1988, page 47].

18. Again, if normal science were understood to concern itself with more than the answering of questions and the solving of problems left incompletely specified by the paradigm, then the open-endedness of a paradigm would not constitute a necessary condition for normal science in economics. If normal science's ken extended beyond the bounds of the paradigm, a closed-ended paradigm would not preclude normal science. It might, in fact, by providing a tightly knit conceptual basis, facilitate rather than render impossible scientific activity. To quote Coats on this matter again: "Obviously no paradigm is complete; if it were, 'normal' scientific activity would cease, for there would be no unsolved puzzles" [Coats, 1969, page 291].
19. Pheby makes much the same point: "The concepts of paradigm and normal science are virtually synonymous, however, normal science constitutes the 'actualisation' of the promise provided by the paradigm" [Pheby, 1988, page 39].
20. Along these lines, Caldwell remarks, "What is the nature of normal science? Much of the research that practitioners of normal science engage in involves 'mopping up activities' which extend and articulate the paradigmatic structure assumed; in a phrase, it is 'an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies'" [Caldwell, 1982, page 71, quoting Kuhn, 1970c, page 24].  
 Along similar lines, John Wells notes "Like Thomas S. Kuhn's notion of 'normal science,' programmed research is devoted to working out the implications and solving the puzzles presented by the current technological paradigm" [Wells, 1986, page 535].
21. Swaney and Premus: "Normal science, where most science is conducted, consists of progress by accretion. Inquiry is directed toward finding the missing pieces of the paradigm-defined puzzle. In the terminology of logical positivism as enunciated by Carl Hempel and Paul Oppenheim the 'General Laws' of the science, when applied logically to the 'Antecedent Conditions,' allow one to deduce accurate descriptions (as *ex post* explanations or *ex ante* predictions) of relevant empirical phenomena. The paradigm, then, consists of the General Laws, which, in combination with alternative antecedent conditions, define the realm of inquiry" [Swaney and Premus, 1982, page 715].
22. Miller: "Normal science, according to Kuhn, is science in the grip of a prevailing paradigm. The research it encourages is the reiteration in novel ways of phenomena already specified by that paradigm; that is, the working out of puzzles" [Miller, 1991, page 994].
23. Peabody: "Normal scientific research is a highly cumulative enterprise and therefore fits the normal image of scientific work with one major exception -- it does not aim at novelty in fact or theory" [Peabody, 1971, page 2].  
 Stanfield, Caldwell, Folbre and Dunn make assertions along the same lines:  
 Stanfield: "Significantly, normal science is characterized by a *lack of intent* to uncover phenomenal or theoretical novelties" [Stanfield, 1974, page 99].



Caldwell: "Normal science does not seek to produce novelties; rather it is a 'puzzle-solving' activity . . . ." [Caldwell, 1982, page 71].

Folbre: "Even more important, normal scientific research agendas are often limited to questions that can be answered simply by means of technical ingenuity. As Kuhn writes, 'normal science does not aim at novelties of fact or theory, and, when successful, finds none'" [Folbre, 1986, page 246, quoting Kuhn, 1970c, page 52].

Dunn: "One of the striking things about normal science is that it does not aim at producing novelties and when it is successful on its own terms it finds none" [Dunn, 1970, page 353].

24. Tollison: "Without meaning to enter the debate about Kuhn's theory, I note that while Kuhn focuses attention on the *conservative forces of normal science*, my approach stresses the forces affecting those revolutionary individuals who force paradigm shifts. That regular scientists are conservative is not remarkable; what is remarkable is that some individuals are able to destroy the human capital stock of normal scientists" [Tollison, 1986, pages 921-922, emphasis added].
25. Pheby: "The main purpose of normal science is to 'force nature into the preformed and relatively inflexible box that the paradigm supplies'; it is not the purpose of normal science to bring forth new discoveries. Indeed, phenomena that do not fit into the paradigm are unlikely to be 'seen' . . . ." [Pheby, 1988, page 39, quoting Kuhn, 1970c, page 24]. Gordon makes the same points [Gordon, 1965, page 123].
26. At least in the natural sciences.
27. Swaney and Premus: "Potentially available data is prescreened by the view of reality implicit in the paradigm, but the remaining quantity of conceivably relevant data is nevertheless immense due to the complexity of reality. . . . Thus, certain facts associated with the phenomenon being analyzed will be discarded in the abstracting process used to organize the protocol data" [Swaney and Premus, 1982, page 716].
28. While Cole, Cameron and Edwards never use the term "normal science," their interpretation of scientists' usual response to facts should be mentioned here. They allow that scientists under Kuhn's description sometimes ignore the facts, whereas under other situations, they modify theory in response to "new experience," without "changing the core theory." At bottom, however, they like neither prospect [Cole, Cameron and Edwards, 1983, pages 134].
29. Piore: "But 'normal science' is in fact largely a set of practices in which members of a given scientific community *customarily* engage. Students seeking to follow their professors in careers in which the latter will judge them and ultimately determine their fate are not encouraged to undertake projects which depart radically from those which their professors conceive for them. When suggestions for such projects arise, either from students or competing members of the community, they are as often treated by sarcasm and ridicule as the subject of seasoned discourse and debate. In this process, what students acquire is less an abstract understanding of what they are doing than a set of habits, or instincts, about what constitutes a legitimate mode of inquiry or a plausible explanation" [Piore, 1983, page 249].

Still again, Piore describes Kuhn as seeing "the abnormal and ~~a~~paradigmatic in a discipline" being met with "fury, disdain, resentment, sarcasm, and condescension" by normal science [Piore, 1983, page 249].

30. Wisman, though not allying the notion with Kuhn's normal science concept, does link the notion of the excommunication of heretics with Kuhn's theory of science: "Those who challenge the dominant orthodoxy are treated harshly; they are socially ostracized" [Wisman, 1979, page 27].
31. Gordon, similarly, cites normal science's impressive productivity as its strength: "Research, which consists in the further development of such basic models, and which defines scientific communities, has both strengths and weaknesses. It has a criterion for choosing problems 'that . . . can be assumed to have solutions' and for gathering data which are relevant; for these reasons, normal research can be highly productive" [Gordon, 1965, page 123, quoting Kuhn, 1962, page 37].  
Willett speaks of the "cumulative progression of puzzle solving" [Willett, 1970, page 449]. Likewise, Peabody remarks: "Normal scientific research is a highly cumulative enterprise . . ." [Peabody, 1971, page 2].
32. Two other articles make very much the same observation. Swaney and Premus asserts that "Normal science, where most science is conducted, consists of progress by accretion" [Swaney and Premus, 1982, page 715], and William Guthrie maintains that ". . . incremental extensions of knowledge . . . occur during the course of (Kuhnian) 'normal science'" [Guthrie, 1982, page 113].
33. And, as we just saw Dillard and Hardaker also directly contrast normal science with major scientific advance. Boland as well describes normal science as "not very progressive" [Boland, 1982, page 161].
34. Glass and Johnson point out that progress under normal science is measured in terms of puzzle-solving success: ". . . normal science research activity has as its aim the steady, cumulative extension of the precision and applicability of the existing theoretical framework. Consequently, in a Kuhnian normal science period, progress is defined in terms of puzzle-solving advances within the context of the prevailing paradigm" [Glass and Johnson, 1989, page 157].  
Caldwell makes similar remarks about the benefits of the restricted nature of normal science: "Thus, normal science involves a highly restrictive sort of scientific activity. But there are advantages which accompany this narrowness of focus, for without it, the subtlety and depth of scientific investigation that also characterizes normal science would not be possible" [Caldwell, 1982, page 71].
35. Rousseas provides an example of this description of the effect of normal science:

A key element within Kuhn's analysis of normal science is the phenomenon of paradigm shifts. As the paradigm polishers work, as we are wont to say, "on the frontiers of science," eventually certain unexpected novelties or anomalies crop up that cannot adequately be explained by the existing paradigm and which violate paradigm-induced expectations. Rather than deny the existing paradigm, those with a vested interest in it (the older scientists and professors) resist change. Ad hoc adjustments, or

paradigm-patching, become the order of the day which only serve to increase the brittleness of the paradigm. A state of crisis emerges followed by a technical breakdown because of the "persistent failure of the puzzles of normal science to come out as they should." The accepted paradigm then cracks, spewing forth a proliferation of new competing schools, each with its own theory. The established paradigm is declared *invalid* (not untrue) and is replaced by one of the competing theories which succeeds in becoming the new universally accepted paradigm. [Rousseas, 1973, page 76]

Similarly Dunn remarks that, while, "One of the striking things about normal science is that it does not aim at producing novelties" and "seeks the progressive testing and refinement of the paradigm until its correspondence with nature is perfected. "Yet this very process serves to generate novelty." Attempts to integrate the novelty leads not simply to incremental change, but to a "paradigm shift" [Dunn, 1970, page 353].

We should note that according to the chronologies used by economists that once a new paradigm replaces the old paradigm, a new period of normal science ensues during which anomalies will again be uncovered, some of which may eventually lead to the dissolution of the newer paradigm and so on.

36. As we noted last chapter, one of the functions which economists see paradigms performing is the establishment and maintenance of consensus.
37. Jalladeau: "Normalized science is not simply a fund of knowledge; as Kuhn specifies, the scientific community itself constitutes a social system. The researchers form a kind of 'invisible college' founded in their commonly shared interests. They agree on the fundamental structure of their discipline, on the boundaries of their field of study, on the general theoretical approach to scientific problems, on the criteria used to validate their work" [Jalladeau, 1975, pages 2-3].  
Stanfield: "Normal science is achieved when the discipline more or less universally accepts the dominant paradigm, which then directs the practitioner as to the key questions and appropriate methods of normal research" [Stanfield, 1974, page 98].
38. Ward: "The researchers, though they are widely scattered over universities and research institutes around the country or even the world, form a sort of invisible college, based on common interests, shared commitments and frequent interaction" [Ward, 1972, page 6].
39. Redman: "'Normal' in Kuhn's terms assumes the existence of . . . a consensus supporting the theory that is *not* imposed or legislated but is natural . . ." [Redman, 1991, page 150].
40. Glass and Johnson: "Whereas in the Kuhnian account competing paradigms only appear during the period of revolutionary science (since, during normal science, the prevailing paradigm has a monopoly position), in Lakatos's account competing programmes are viewed as both a necessary and everpresent feature of science" [Glass and Johnson, 1989, page 167].

L.E. Johnson: "Lakatos's 'hardcore' of SRP's can be interpreted as one possible set of paradigm defining characteristics, where Kuhn's concept of

'normal science' is analogous to a Lakatosian SRP that has gained a monopoly" [Johnson, 1983, page 1108, footnote 6].

Peabody: "Mature science is characterized by an unwavering commitment to a paradigm on the part of the relevant research community. In the natural sciences there are seldom competing schools addressing the same problem" [Peabody, 1971, page 1].

41. Brinkman: "In the Kuhnian framework the concept of 'normal science' is crucial, and central. Normal science is the science practiced by the majority of scientists in a given discipline at a given moment of time; it is what scientists, as members of a given scientific community, normally do" [Brinkman, 1981, pages 35-36].
  
42. Along the same lines, Mark Blaug notes that according to Kuhn's schema, "'Normal science', or problem solving activity in the context of an accepted theoretical framework, is said to be the rule" [Blaug, 1976, page 152].  
 Canterbury and Burkhardt similarly see Kuhn as defining science as "whatever scientists do": "Taking their clue from historical studies of science, especially the 'model' science of physics, philosophers of science like Kuhn and Lakatos argue that science never actually adheres to the positivistic canons. Instead, the canons are ex post facto creations of the positivists. In fact, the historian-pragmatists contend, science is really definable not by either its content or its empirical methodology but rather by what scientists who are accepted as scientists by other scientists hold it to be. Science is whatever scientists do" [Canterbury and Burkhardt, 1983, page 19].
  
43. Describing Kuhnian normal science, Backhouse observes, "Education in the subject becomes learning to solve the puzzles produced by the paradigm, and because of the shared assumptions within the group, textbooks can become important" [Backhouse, 1985, page 4].  
 Along similar lines, Matsui, Asano and Matsuda assert, "The normal science standardizes a set of theories as paradigms to be used as routine procedures for practical applications and textbook knowledge for a classroom" [Matsui, Asano and Matsuda, 1989, page 123].
  
44. O'Brien: "The second set of questions that we have to ask ourselves about the use of Kuhn's theory of the history of science in explaining the history of Smith's bequest, relates to the whole collection of 'normal' science 'puzzle solving' and 'anomalies' that are so important to Kuhn in his view of natural science. It does seem that these questions can be answered even less satisfactorily. . . . For it does seem clear that very little 'puzzle solving' was engaged in by the classical economists" [O'Brien, 1983b, page 104].
  
45. O'Brien: "But if we reduce him [Ricardo] to the role of a puzzle solver within the Smithian paradigm we achieve generality as the expense of meaning, for we then have nothing by which to delineate the phenomenon of the Ricardian explosion" [O'Brien, 1983b, page 104].
  
46. Deane: "It is generally accepted that the British classical economists of the first half of the nineteenth century constituted an identifiable school of economic thought. They shared a distinctive framework of economic ideas, shaped by a particular set of axioms and theories and generally characterised by a strong bias towards economic policies favouring economic individualism and laissez-faire. Whether this school of thought constituted a 'scientific community' in the sense

that T.S. Kuhn uses the term in his analysis of the structure of scientific revolutions or whether it is better described as a 'pre-paradigm school' may be open to question. In Kuhn's view a scientific community consists of the practitioners of a scientific specialty who share a common paradigm. . . .

"Certainly nineteenth-century economists drew their basic assumptions and techniques from the same textual sources -- Adam Smith, David Ricardo, Nassau Senior and John Stuart Mill being the main links in a clearly perceptible continuity of thought -- though there was as yet no *formally* recognised education as an economist. The doubt, however, is not whether the nineteenth century community of economists shared 'similar educations and professional initiations' but whether they were practitioners of a *scientific* specialty" [Deane, 1978, pages 93-94].

47. Dillard: "Thomas Kuhn's view of normal science as essentially a closed society is illustrated in a statement by Francis A. Walker, first president of the AEA: 'Laissez-faire . . . was not made the test of economic orthodoxy, merely. It was used to decide whether a man was an economist at all'" [Dillard, 1986, page 358, quoting Ely, 1938, page 127, ellipses in Dillard].
48. DeVroey: "the rise of classical economics witnessed the formation in England of normal science, although still at an incipient stage. On the one hand, a distinct scientific community of economists was developing and becoming recognized as such, even if not yet professionalized. On the other hand, a consensus existed among these people about the field in which they inquired, the questions they asked, and the main concepts and categories which they used in order to answer the questions. Thus, one already may speak of a 'paradigm in dominance'" [DeVroey, 1975, page 421].  
 DeVroey: "During the classical period economic science had not yet reached the stage of a fully developed normal science. The community of economists was still small, and its boundaries were vague. Consequently, intellectual production was neither abundant nor specialized" [DeVroey, 1975, page 425].
49. That Johnson and Ley see classical economists practicing normal science we derive from two statements which they make about the progressiveness of classical economists' work: (1) "However, as one would expect in the early stages of normal science, intellectual progress occurred" [Johnson and Ley, 1990, page 95]. and (2) "We have also seen that political economy evolved via the practice of normal science. The more rigorous approach of Ricardo, for example, set a precedent that has lasted in economics to the present day" [Johnson and Ley, 1990, page 116].
50. Johnson and Ley: "We will see that in spite of their working within a common framework, different political economists reached very different conclusions regarding the potential for capitalism to maximize social welfare over time" [Johnson and Ley, 1990, page 97].
51. Brinkman: "Practitioners of normal science may disagree on the solution of a given puzzle but they strongly agree as to which economic problems shall be submitted as puzzles to be resolved in the context of their given disciplinary matrix. Say's Law as a case in point was part of the overall neoclassical disciplinary matrix and consequently the economic problem of unemployment was not viewed as a puzzle to be resolved by economists. For the practitioners of the normal science of economics the puzzle to be explained was not

unemployment, but rather, the puzzle became one of explaining how supply created its own demand and generated full employment. The problem of unemployment was ultimately introduced as a puzzle to be resolved by economists with the innovation of Keynesian economics as the disciplinary matrix in dominance. Unemployment was not permeable as a puzzle given Say's Law and neoclassical economics as normal science" [Brinkman, 1981, page 37].

52. In this regard, Pheby observes that installation of Keynes' theory as economics' new paradigm resulted in a proliferation of puzzle solving, both theoretical (given the incompleteness of Keynes' theory) and empirical ones (e.g., the consumption function, money demand, multipliers). In addition, there was an agreement over symbolic generalisations (the Keynesian cross, the IS-LM model, in particular). Having noted all this Pheby asserts that, despite current crisis-like conditions in economics *today*, that there was a "period of normal science that showed strong similarities with the work of Kuhn" following the Keynesian revolution [Pheby, 1988, pages 52-53].
53. Deane: "In the event then, the macroeconomic concepts and theories associated with the Keynesian revolution inspired a variety of theoretical and empirical research programmes and there are wide divergences between the basic assumptions and analytical techniques adopted by economists who would regard themselves as working within a Keynesian tradition" [Deane, 1978, page 188].  
Deane, clarifying the foregoing assertion explains, "That is to say the Keynesian revolution did not lead the economics profession into the kind of narrowly constrained research tradition that is implied in Kuhn's concept of Normal Science and which in the Kuhnian model of scientific revolutions marks the success of a new paradigm" [Deane, 1978, page 188, footnote 30].
54. For example, while the neo-Keynesians regard Keynes' theory as "merely a special case of the traditional neo-classical theory of markets," post-Keynesians take issue with this understanding. Still again, while the latter stressed uncertainty's influence over investment, the former minimized its impact [Deane, 1978, pages 187-188].
55. Stanfield: "Second, Keynes provided new fundamental behavioral functions and tools of analysis, such as the consumption and liquidity functions and the multiplier, and new applications of the discipline in the areas of policy and econometrics. Third, as we have seen, the Keynesian revolution was a change in world view, a redefinition of not only the purposes, tools, and problems of the discipline, but also of the world itself. . . ." [Stanfield, 1974, page 104].
56. Bornemann: "The practitioners, with avoidance of unemployment their objective, meanwhile, failed to perceive the relationship between the forces involved in attempting to push the economy beyond its production possibility curve and excess demand, inflation, accumulating dislocations, and ultimate depression. The normal science situation in economics reflected what Kuhn described as the development in a science of esoteric vocabulary, skills, and refined concepts 'that increasingly lessens their resemblance to their usual common-sense prototypes,' a 'professionalization' leading 'to an immense restriction to the scientist's vision and to a considerable resistance to paradigm change' in the course of which he becomes 'increasingly rigid.' In their general explanations of instability and inflation, professional economists always repeated as fundamental only such eventually customary general expressions and phrases

as stagnation and the shortage of aggregate demand, even though they attributed immediate developments at any particular time to current short-range exogenous influences including drought and other natural phenomena, monetary policy, political negotiations including crop deals, foreign petroleum and other cartel practices, and international economic and financial developments. They always said additional government expenditures or a lower-bracket tax cut were necessary not only to sustain demand and offset the tendency (claimed to exist most of the time) of prices and output to fall but also to remedy rising prices because they adversely affected those with lower incomes. The unvarying full employment, inadequate demand, slump, and stagnation slogans were in due course echoed by others including the radical left" [Bornemann, 1976, page 132, quoting Kuhn, 1970c, page 64].

57. MacLennan: "It was Jevons's intention to establish the general laws of economics in the sense, not of laying out a broad outline within which further development of the theory could take place but, rather, of setting out trivially true and certain axioms on the basis of which different branches of the subject could be built. The axioms were intended to be universally true in contrast to, say, the laws of property which are very different in different countries and would therefore just form part of the data. Any idea of wider generalisation about society was rejected, and even ridiculed, because of the impossible complexity of social phenomena. Thus Jevons appears to think that knowledge of society can only be piecemeal; yet, his reason for asserting the impossibilities of wide generalisation about the development of societies is that 'A nation is not a mere sum of individuals whom we can treat by the method of averages; it is an organic whole, held together by ties of infinite complexity.' If society is an organic whole, then an attempt at wide generalisation would seem to be at least as appropriate an approach as the piecemeal approach proposed by Jevons. But however that may be, the envisaged development of separate areas of study, divided according to method of treatment as well as by subject-matter, but all to some degree based on the unquestionable axioms, constitutes an essentially non-revolutionary programme of development for the science which once again belies the role of the bold hypothesis" [MacLennan, 1972, page 70, quoting Jevons, 1958, page 761]. In a footnote to this quote, MacLennan equates the notion of "an essentially non-revolutionary programme . . ." to Kuhnian normal science [MacLennan, 1972, page 70, footnote 6].
58. DeShon: "Webb asks if the history and philosophy of science offer any examples of new theories coming into being by means of the processes I suggest. He answers that he thinks not, and puts forth Stigler's view of intellectual autonomy. Actually the history and philosophy of science contain cases where both views are defensible; depending upon the stage of development of a discipline at a given time. In terms of Thomas Kuhn, Stigler's view would be appropriate for periods of 'normal science' and my view would be appropriate for periods of 'revolutionary science'" [DeShon, 1971, page 249].
59. In all, Argyrous identifies and examines 72 articles published in the *American Economic Review* between 1957-1987 dealing with these two economists' hypotheses of consumption [Argyrous, 1992, page 238].
60. Argyrous: "Houthakker then tried to collect the facts needed to test the hypothesis. However, available data rarely come in exactly the form needed. An important aspect of normal science is thus to extract the 'meaningful' facts

from the information sources that are available. In this instance, Houthakker took cross-section data from the 1950 budget survey of the U.S. Bureau of Labor Statistics and divided households by occupation of the head. He then subdivided these occupational groupings by income. . . ." [Argyrous, 1992, pages 239-240].

61. Argyrous: "The three types of normal scientific activity almost always occur simultaneously, and by looking at some of the puzzles that have been addressed in the pages of the *American Economic Review* one can see normal science at work in the PIH/LCH. . . ." [Argyrous, 1992, page 239].
62. Argyrous continues: "For example, in terms of fact gathering and testing, which often occur simultaneously, we could look in more detail at the way in which Modigliani and Ando (1963) explore the use of alternative formulations of nonproperty income in testing the LCH; or the use of cross-section and time series data by Simon and Aigner (1970) and Laumas and Mohabbat (1972) to test various aspects of the LCH; or the use of probate records by Menchik and David (1983) in order to measure the share of wealth that derives from intergenerational transfers as opposed to life cycle saving; or . . . ." [Argyrous, 1992, pages 241-242].

Similarly Argyrous describes the research generated by the question as to whether the MPC out of transitory income was zero in terms of the three normal science activities (without here making the link explicit).

63. We, not Argyrous, make the distinction between normal science in general and Kuhnian normal science in particular. Argyrous speaks only of normal science, which he describes in terms of his interpretation of Kuhn.
64. Argyrous here refers to economists' initial findings (1) that consumption was not independent of transitory income (Houthakker) and (2) that the MPC out of transitory income was much closer to one than it was to zero (as Friedman's hypothesis predicted) [Argyrous, 1992, page 240].
65. In the case of the independence of consumption and transitory income, there was Eisner's painstaking "filling-in" of Friedman's argument. The "filling-in" produced results favorable to Friedman's hypothesis [Argyrous, 1992, page 240].

In the case of determination as to whether, as Friedman's hypothesis maintained, the MPC out of transitory income was zero, economists excluded consumer durables from the definition of consumption and added the assumption that the MPC out of transitory income falls as the proportion of one's income that is transitory rises [Argyrous, 1992, page 241]. These modifications made it possible to reconcile divergent findings with one another within a modified paradigm [Argyrous, 1992, page 241].

66. Argyrous: "A number of conclusions can be drawn from this overview of the development of the consumption function. . . .  
"The second conclusion involves dispelling a myth that is often attributed to Kuhn's notion of normal science. Some commentators have ridiculed normal science and puzzle solving as a slavish and unimaginative activity that engages only the hack scientists. . . . Although Kuhn agrees that hack science occurs, it is not this activity that he sought to identify as the hallmark of mature normal scientific activity. For him, normal science is creative . . . ." [Argyrous, 1992, page 242].



67. Baumberger: "In a sense, economics provides the counter-example *par excellence* to physics. But, as does physics, it fails to illustrate and support Kuhn's schema. Whereas in physics we look in vain for the period of the revolutionary mode of behavior, in economics we are at a loss to find the period of the normal science mode of behavior" [Baumberger, 1977, page 10].
68. These five classics are K.J. Arrow, 1951; D. Black, 1958; A. Downs, 1957; M. Olson, 1965; and J.M. Buchanan and G. Tullock, 1962. [Blankart, 1987, pages 5 and 12-14].
69. While Arrow, Downs and Olson forward pessimistic assessments as to the prospects of collective decision-making, Black and Buchanan and Tullock advance optimistic ones [Blankart, 1987, pages 5-6].
70. The paths, in large part, correspond to the field's conflicting classics: "It is normal science that most of the research within the European Public Choice Society is done. The work by scholars within this group may be conveniently characterized according the five classic works by Arrow, Black, Downs, Olson and Buchanan/Tullock mentioned before and by keeping in mind their *divergent* views on the political process" [Blankart, 1987, page 6, emphasis added].
71. Blankart details normal science in public choice economics solely by providing five separate accounts of the work conducted under the aegis of each of the five classics; there is no discussion of any efforts at cross-fertilization or conciliation. His discussion of work following Arrow's is typical:

First, Arrow's Impossibility Theorem has always been a fascinating subject for European scholars. The thrust of research in this field was on two sides. Some scholars have investigated the consequences of relaxing Arrow's assumptions. Gaertner (1979) deduced the preference restrictions necessary for achieving transitivity under majority rule. Breyer (1980), however, showed that cyclical majorities can occur even in the presence of identical tastes of all voters. Others inquired into consequences of voters cycles, e.g Baigent (1982) and Güth (1985).

Further impossibility theorems have been analyzed, particularly Sen's theorem of 'The Impossibility of a Paretian Liberal' (1970), e.g., by Bernholz (1974a), Breyer (1977), Gaertner and Krüger (1981). Rowley and Peacock (1975) have applied the liberal dilemma to the problem of constitutional choice. In case of conflict between Pareto optimality and liberalism, Rowley and Peacock have a clear priority for the latter. Their study also shows that there are limits to a purely positive analysis in constitutional choice. Eventually it may become unavoidable to enter values in a constitution. [Blankart, 1987, page 6]

72. Bliss: "It is true that different economists in different places are working on different problems and with different priorities, and it is true that they do not always agree about what counts as good work. There is considerable disagreement at the present time, for example, concerning the legitimacy of so-called *ad hoc* assumptions. However, these disagreements, embarrassing though they may be, span no more than the normal range of a normal science.

As I have argued above, what Professor Pasinetti does, despite what he writes in his Forward [Pasinetti, 1981], is normal economics" [Bliss, 1986, page 375].

Earlier in this regard, he asserts that "Sraffa's production model, for example, is quite consistent with Debreu's general equilibrium theory, if the appropriate assumptions are included in the latter" [Bliss, 1986, page 374].

73. Baumberger: "Thus the current fashion in economic 'theory' methodology is to incorporate all givens in the 'universe of discourse' and provide a proof for anything else that is introduced. This means that apart from the terms introduced in the 'universe of discourse' the only things we are allowed to take for granted are the rules of logic, since everything else will be proven by the economic 'theorist' within the 'universe of discourse.'  
". . . The contribution provided by the given article is a 'new' proof or an 'alternative' proof demonstrating that the theorem or proposition can be proven using only the specified 'universe of discourse.' Anything novel or informative will have to be provided in the 'universe of discourse.' What we are saying here is simply that economic 'theory' today is nothing but exercises in puzzle-solving -- along the lines described by Thomas Kuhn" [Boland, 1982, pages 132-133].
74. Boland: "In a 'universe of discourse' consisting only of non-inferior (i.e., 'normal') goods and utility-maximizing consumers, upwardly-sloping demand curves are logically impossible. In such a hypothetical world, Giffen's observations would not be empirically possible, since they are logically impossible. But this question of possibility depends on the special characteristics of our invented hypothetical world. There is no reason why the real world has to correspond to this restricted hypothetical world. In other conceivable worlds it is quite possible for there to be upward-sloping demand curves (i.e., Giffen goods).  
"The point of all this complexity and perversity is that a statement which some might consider to be a tautology may only be a statement for which the hypothetical world has been designed logically to rule out all counter-examples. In fact, in economics there are very few pure tautologies (statements which are true regardless of definitions). But there are many theories and models which invent hypothetical worlds that provide what we might call 'pseudo-tautologies'" [Boland, 1982, page 135].
75. Boland: "If it matters whether our explanations are true, it is because we want our theories to be true while at the same time allowing the possibility that our theories might be false. If they cannot be false (for purely logical reasons), not much will ever be at stake and thus nothing much can be gained" [Boland, 1982, pages 133-134].
76. Brinkman: "It is not quite correct to assume that if you ask a thousand different economists the same question you will get a thousand different answers. It depends on the question. If for example the question is raised concerning the relevance of institutional analysis as an integral and substantive part of economic analysis, the majority of economists would answer in the negative. And such a negative response is not necessarily based upon systematic inquiry or specific knowledge of the Veblen-Ayres disciplinary matrix and, consequently, its rejection on scientific grounds but rather reflects that in their training the majority of economists by definition have absorbed the neoclassical synthesis. The neoclassical synthesis views the institutional approach as '. . . having withered away as an effective counterforce in economics. Who can

explain it when a movement turns sterile?' Learning the neoclassical synthesis, as normal science, means that the economist in his training learns to exclude the institutional and the cultural as integral parts of his theoretical explanation. The young economist learns the behavior pattern of exclusion long before he accumulates the necessary knowledge which would warrant such a judgment" [Brinkman, 1981, page 37, quoting Samuelson, 1976, page 847].

77. Canterbury and Burkhardt: "Economics, however, is an illuminating instance of how the values of the positivists, even as ideals and not as descriptive criteria of real science, are not always attained in disciplines that claim to be scientific. In fact, economists seem to pay only lip service to the positivist ideals, going off and doing their own thing irrespective of whatever normative criteria philosophers of science -- or indeed other kinds of scientists -- have found to be sound, or at least functional. *In this respect, economics may be a science, but only in the bold-faced Kuhnian sense of there being a paradigm within which economists practice and according to which they self-referentially define their activity as science.* Economics may thus be a pre-positivist 'system of organized cognition' -- much the same way, perhaps, as Ptolemaic geocentrism" [Canterbury and Burkhardt, 1983, page 22, emphasis added].
78. Canterbury and Burkhardt: "Economics as a field has, to a great extent, been sheltered from the winds of social change. . . . the hierarchical 'democratic centralism' of the American Economic Association has insulated it from the student revolt in relative contrast to what has happened in other academic professional societies. . . .  
". . . 'Radicals,' including Marxists and post-Keynesians, presume that their submissions [to mainstream journals] will not be acceptable to the normal science or mainstream reviewers of the 'leading' journals and therefore they do not submit" [Canterbury and Burkhardt, 1983, page 25].
79. Canterbury and Burkhardt: "In fact, economists seem to pay only lip service to the positivist ideals, going off and doing their own thing irrespective of whatever normative criteria philosophers of science -- or indeed other kinds of scientists -- have found to be sound, or at least functional" [Canterbury and Burkhardt, 1983, page 22].
80. Coats's exact words are: "Does this not mean that by comparison with the natural sciences economics has not yet passed beyond the 'developmental' or pre-paradigm stage?" [Coats, 1969, page 292].  
The paradigm stage, however, in Kuhn's schema, is that time in a science's history prior to its congregating around a single paradigm and initiating the practice of normal science [Kuhn, 1970c, pages 10ff.].
81. More precisely, Cornwall identifies economics' current normal science with that group of theories connected with the regnant paradigm: "If this paradigm is the one most widely adopted by the profession, we can designate the group of interconnected theories emanating from the paradigm as 'normal science.' If at the same time there exists a competing paradigm that is not widely accepted, the theories connected with this paradigm can be thought of as 'revolutionary science.' Clearly, neoclassical economics must be considered the normal science of our profession toady . . ." [Cornwall, 1979, pages 70-71].  
Later, however, Cornwall equates the field's dominant paradigm with normal science: "An accepted paradigm -- in other words, normal economic science . . ." [Cornwall, 1979, page 75].

82. Cornwall focuses his attention upon growth theory [Cornwall, 1979].
83. Cornwall: "Neoclassical growth models cannot explain the changing patterns of growth in real economies either, since multisectoral neoclassical growth models assume balanced growth . . ." [Cornwall, 1979, page 79].
84. In this regard, we may cite three passages from Cornwall: "In the neoclassical framework, for example, the Great Depression would be seen as the result of production in the consumption goods industries becoming less capital intensive than production in the capital goods industries. Such an explanation would indeed be viewed as somewhat novel by economic historians" [Cornwall, 1979, page 77].  
Cornwall: "Since in the neoclassical approach growth rates are determined by unexplained supply factors, there is only one way this theory can be used to explain such interesting historical phenomena as why growth rates accelerated in the postwar period . . . this is to assume that one or more of the exogenous forces determining growth varied over time or across countries. This approach certainly qualifies as an explanation of growth, but hardly as an economic one" [Cornwall, 1979, page 78].  
"In neoclassical growth analysis, the long-run equilibrium (if it exists) is always one where the economic variables grow at the same constant rate. . . . Yet studies . . . indicate that, for the United States and the United Kingdom, such key variables as capital stock and output, respectively, have grown at quite different rates for extended periods of time" [Cornwall, 1979, page 80].
85. Cornwall: "There is thus no sense of history in these models . . . Once again, this leads to the neglect of a number of interesting and important problems linked to the development of capitalism . . ." [Cornwall, 1979, page 79].
86. Cornwall: "A final note. Continuous stagflation with no end in sight can do little but call into question the ability of capitalism to successfully organize economic activities. . . . Adherence to the neoclassical paradigm has only delayed the development of the kinds of theories we need to help us deal with the unsettling experiences of today" [Cornwall, 1979, page 87].
87. Cornwall: "It has been argued here that the anomalies in growth theory are derived from the neoclassical framework. Balanced growth, the unimportance of investment, technical progress and labor force growth unrelated to economic events, and fixed tastes and technologies -- important assumptions and predictions that are clearly at variance with real-world events -- are basic elements of neoclassical growth theory, now the normal (economic) science" [Cornwall, 1979, page 86].
88. Dillard: "The Walrasian general equilibrium theory . . . is not a significant theory in our sense. Its weakness as a scientific model for twentieth-century American capitalism lies in its institutional and technological premises. Economic theory is necessarily abstract and cannot correspond to details of actual experience, but its chief purpose is to help organize empirical reality in a systematic fashion that will suggest hypotheses for the solution of important problems. . . . the Walrasian model . . . cannot be very helpful in organizing empirical reality in a world totally different from the one hypothesized in the model. As a theory about a monetary economy (capitalism), it has no useful theory of money. As a theory about a profit-seeking economy, it has no real theory of profits, except that they disappear under perfect competition. The

Walrasian theory by itself is the wrong one of an American capitalism dominated by giant firms, merger mania, the financing of a casino economy" [Dillard, 1986, pages 359-360].

89. Dillard: "Its [Walrasian general equilibrium theory's] domination rests in no small part on the content of graduate courses in economic theory in leading graduate schools. It is taught there, learned there, and passed on from one generation of theorists to another. One who aspires to become a professional economist, and especially an academic one, is required to master neo-classical Walrasian general equilibrium theory. For one who rebels against learning this doctrine there is little place in the profession because it is the chief test of competence as a professional economist" [Dillard, 1986, page 360].
90. Dugger: "'Normal' science in neoclassical economics consists of gathering price and quantity data to test demand and supply predictions generated by deductive models of the consumer and producer" [Dugger, 1979, page 906].
91. Dugger: "In institutionalism what Thomas Kuhn would call 'normal' science consists of conducting case studies and using them to elaborate on or extend a basic pattern" [Dugger, 1979, page 906].  
       "The 'normal' science of institutional economics involves cases studies along the lines of, say, Adolf Berle and Gardiner Means" [Dugger, 1979, page 906].
92. Gordon makes no comments about normal science in connection with the history of economic doctrines (i.e., economic policy and philosophy). However, the article's central focus is with the history of analysis -- not doctrines. Thus, Gordon's exclusive focus upon history of economic analysis in his normal science discussion should not be read as *necessarily* implying that Gordon sees no normal science in the history of economic doctrines.
93. This activity corresponds to what Coats refers to as "actualizing the promises inherent in their paradigm" [Coats, 1969, page 292].
94. Thus, it appears that Gordon sees a high level of consensus as necessary for the practice of normal science.
95. Several other economists have commented upon the parallels between the unimportance which economists attach to they study of the history of economic thought with Kuhn's observation that scientists are the ones least likely to be interested in their field's past (e.g., Bernard Corry [Corry, 1975, page 252] and Margaret Schabas [Schabas, 1992, page 196]).
96. Gordon conspicuously contrasts the high level of consensus among economists in the mid-1960's with past periods during which such widespread agreement was lacking in order to explain the lack of importance of the history of economic thought to economic analysis in 1965:

I conclude that economic theory is very much like a normal science, and that, like a normal science, it finds no necessity for including its history as a part of professional training. But I know no reason to suppose that the study should or will disappear. Why then does it appear to be declining? I conjecture that this is a decline from what might be called an "abnormal"

level. During the "crisis" which was apparent in American economics as late as the 1920's, competing models flourished. As in other fields where there is no consensus, it is natural for people to turn to the classics of the field. . . . With the reestablishment of a consensus, with the decline in controversy, it was only to be expected that historical interest would decline to what might be called a normal level for a normal science. [Gordon, 1965, page 126]

Gordon grants that the consensus he locates is predicated upon a certain definition as to what counts as economics. He, however, argues that this consensus is "an empirical proposition," based upon what one finds to be the case in economics departments at major universities [Gordon, 1965, page 125].

97. Jalladeau: "Yet by virtue of the normalized character of the science, a question will be deemed economic only if it can be formulated in terms of the dominant paradigm. Neoclassical theory reasons in terms of stable structures. Relations of a type other than those of exchange are excluded therefrom. Its limitations stem from the fact that by reducing its subject matter to the calculus of economic efficiency, its field of inquiry is being cut off from its social stratum. . . .  
"Excluded from the 'economy' are the mechanism of want creation in a social system, the process by which preferences are formed, the distribution of power and its role in economic society, the interplay of social classes, the forces that tend to disrupt equilibrium, the conflicts, the contradictions, the structural changes" [Jalladeau, 1975, page 4].
98. Jalladeau: "Normal economic science dodges the question of the evolution of systems, rejecting it as lying outside the scope of its scientific concerns. It thereby bars itself as much from facing the problems of structural transformation in the industrialized economies as from tackling the question of the genesis of underdevelopment. Conventional economics does not enlighten us to any great extent on these problems because they are thrust aside by the dominant paradigm" [Jalladeau, 1975, pages 4-5].
99. Jalladeau: "The theoretical structure of a science depends upon the consensus of the interested scientific community; all the same, it appears that a theory of economic society and of social change is not incompatible with economic theory. The contemporary institutionalists do not gainsay the value of the fundamental economic calculus; what they dispute is that the axiomatic quality of the logicomathematical kind of reasoning constitutes all there is to political economy. . . ." [Jalladeau, 1975, page 11].
100. Jalladeau: "In conclusion, the reciprocal relations between the two traditions of political economy -- economic theory and theory of social change -- are less of an alternative than of a complementary nature" [Jalladeau, 1975, page 12].
101. Jalladeau never excludes the possibility that a normal economic science could, *qua* normal science, address itself to social problems (such as Miller does). He however does not explicitly argue that it could.
102. Jensen on Marshall: "Did he [Marshall] meet the criteria for a practitioner of social economics that were listed in the first section of the present paper? In my opinion, he did. He based his economics on clearly enunciated ethics; he did

have a well focused vision; he developed a conception of reality that had both historical, behavioral, and structural dimensions, and he formulated a corpus of policy recommendations that was designed to improve the socioeconomic condition of humankind" [Jensen, 1987, page 34].

Jensen on Keynes: "Keynes emphasized" "the subject of reform" "because of his conviction that it was historically evolved institutional perversities that were responsible for the onset of the great depression" [Jensen, 1977, page 255].

103. In addition to his remarks concerning normal science's dismissal of the social economics elements of Jevons, Marshall and Keynes, Jensen similarly contrasts John Maurice Clark's social economics with normal economic science [Jensen, 1984, page 73].
104. Lind provides only a one-phrase description of normal research: "highly qualified research made within a certain tradition" [Lind, 1992, page 85], and only mentions Kuhn once. He, however, entitles his article, "A Case Study of Normal Research in Theoretical Economics," and makes explicit that his analysis centers upon an examination of Kuhnian normal science: "In the case study to be presented, we look at what Kuhn (1962) would call normal research . . ." [Lind, 1992, page 85], and sets out conclusions about "normal research in mainstream theoretical economics" [Lind, 1992, pages 98-99]. In this way, his article may be regarded as a major treatment of normal science in economics.
105. Lind, however, does not maintain that the findings he draws from examination of Svensson's articles are, in any way, conclusive. He maintains that studies of randomly selected articles from a broad range of economists are necessary to assay the representativeness of his case study [Lind, 1992, page 85].
106. Along similar lines, Lind affirms that "the idea behind many studies resembles a strategy of successive approximations, where increasing realism is believed to increase the probability that the results for the model-economy say something about the real economy" [Lind, 1992, pages 98-99].
107. Lind: "There is no trace of such a[n] [instrumentalistic] view in the articles analyzed in this case study. Simplifications are not argued for with that type of argument, and there are no discussions about testing the predictive ability of the model-economies. When simplifications are described as harmless, it is for reasons unrelated to predictive ability. The introduction of more realistic assumptions are generally viewed as a step in the right direction, most likely because the more realistic assumptions are believed to increase the likelihood that the results for the model-economy carry over to the real economy" [Lind, 1992, page 95].
108. Lind: "The study here is concerned with the last of these questions. ["How do economists actually use idealized models? . . . "] It is based on the view that what is needed in the philosophy of economics today are detailed studies of how economists really argue" [Lind, 1992, page 84].
109. The present paragraph constitutes a summary of Miller's discussion entitled, "*Normal Economic Science*" [Miller, 1991, pages 995-996].
110. Miller: "Standard economic theory joins a platonic veneration of abstract, universal, and eternal truths with a reliance both upon cartesian rational thought

and newtonian mechanics. *A priori* reasoning is employed logically to deduce from its model of the world truths about reality; that is, regularities and uniformities in nature that are taken as universal laws. The model is driven by first principles, absolutes, ultimate realities, truths given from outside the system. Solutions to problems are specified by a prior belief set, most particularly by regularities perceived as axiomatic -- for example, rationality, maximization, -- and that lead inexorably to a puzzle-like, pre-imposed, free market conclusion. Hypotheses thus logically deduced from general law, then, may be tested against experience; that is, validity is to be judged by the concordance of predictions with reality. The validity of assumptions (that is, the world view) is irrelevant" [Miller, 1991, page 995].

111. Miller: "Kuhn's view of normal science has been disparaged as a description of 'a closed society of closed minds' [Watkins, 1970, p. 27]. Despite the intended criticism, it is a characterization that, in my mind, hits the mark for normal economic science. Normal economics constructs a model of the world that admits of one conclusion only, irrespective of the problem (puzzle) posed. It is characterized precisely by that 'abandonment of critical discourse' that Kuhn apprehends as a hallmark of normal science. . . . If the model chosen is irrelevant or otherwise incorrect, even very sophisticated manipulation will be for naught when it comes to research and policy prescription; that is, in the solution of problems that are more than or other than Kuhnian puzzles" [Miller, 1991, page 995, bracketed reference is Miller's].
112. Directly following the claim, Miller contrasts Kuhn's notion of normal science with Paul Feyerabend's position that "anything goes" [Miller, 1991, page 997]. Miller's description of Feyerabend stands in direct opposition to Miller's understanding of normal science as a "closed society of closed minds." Further, normal science's lack of critical discussion bears directly upon the nature of the scientific community, which McCloskey holds out as the arbiter of truth.
113. Directly prior to discussing the implication of the concomitant applicability of McCloskey's understanding of scientific truth and Kuhn's notion of normal science, Miller points out that, according to Kuhn, empirical contradictions reflect badly upon the scientist finding them, not the theory they "contradict." Miller, however, draws no link. Nor, as in the case of lack of critical discussion, is an implicit link as easily forged with McCloskey's contention that the scientific community established truth.
114. D.P. O'Brien questions the applicability of Kuhn's normal science concept to the social sciences (including economics) on similar grounds: "The truth seems to be that the concept of puzzle solving does not transplant from natural to social science any more easily than Popper's 'testing' or 'falsification' and 'crucial experiments.' Moreover, it is extremely hard, even today, to envisage a situation in economics when a research failure would reflect on the researcher" [O'Brien, 1983b, pages 104-105].
115. Miller: "For example, in the face of concentrated power, the theory of contestable markets and of inter-product rivalry is introduced, translating oligopoly into competition. High unemployment rates are translated into full employment by means of the theory of the natural rate of unemployment. Monopoly power is translated into economic democracy by the use of a version of the social contract. And so it goes. The facts are ignored, or assumed away.



Prediction[s] are taken as in accord with reality, by the simple token of changing reality" [Miller, 1991, page 1002].

116. Osberg: "Today, most economists would agree that human capital theory has become the 'normal science' (in Kuhn's sense) of earnings behaviour. Textbooks in fields as diverse as labour and development economics apply its approach, conferences assemble its wisdom and articles in learned journals explore intricate theoretical subtleties within its framework. Even anomalies which are painfully apparent to highly educated college professors as their own declining real incomes (both absolutely and relatively to less-educated groups) find explanations -- perhaps somewhat forced -- within the paradigm" [Osberg, 1976, page 93].
117. Despite regarding human capital theory as a normal science, Osberg concedes all is not quiet in the subfield: A crisis does, now, exist in the study of earnings. Some anomalies have remained particularly intractable and alternative theories are emerging now to take account of them [Osberg, 1976, page 93].
118. Piore does not draw the parallel outright. However, the parallel between his descriptions are striking and strongly suggestive. Compare Piore's following description of normal science in general to his above description of the treatment of non-orthodox viewpoints in economics: "But 'normal science' is in fact largely a set of practices in which members of a given scientific community *customarily* engage. Students seeking to follow their professors in careers in which the latter will judge them and ultimately determine their fate are not encouraged to undertake projects which depart radically from those which their professors conceive for them. *When suggestions for such projects arise, either from students or competing members of the community, they are as often treated by sarcasm and ridicule as the subject of seasoned discourse and debate*" [Piore, 1983, page 249, emphasis added].
119. Dutton and King: "Economic heretics are generally ignored rather than burned at the stake. The practitioners of 'normal economic science' rarely engage dissidents on their own or any other terrain. Heretics ask embarrassing questions, investigate problems which are not generally accepted as legitimate, and provide answers which rely upon unusual concepts, unfamiliar reasoning, and inadmissible evidence" [Dutton and King, 1986, page 259].
120. Reder does not formally introduce Chicago economics as a Kuhnian normal science. He does, however, ally it with the notion of a Kuhnian "sub-culture" [Reder, 1982, page 19]. Then, *en medias res*, he refers to Chicago economics as a normal science: "As in any 'normal science,' there is resistance to innovation, and ambivalence toward would-be innovators . . ." [Reder, 1982, page 20]. Still again, in discussing the Chicago school's treatment of evidence, Reder allies the school with normal science [Reder, 1982, page 21].
121. Reder's definition of "Tight Prior Equilibrium": "In essence the Chicago View, or what I term 'Tight Prior Equilibrium' theory (TP), is rooted in the hypothesis that decision makers so allocate the resources under their control that there is no alternative allocation such that any one decision maker could have his expected utility increased without a reduction occurring in the expected utility of at least one other decision maker" [Reder, 1982, page 11].

122. Reder: "Success is achieved by mastery and application of certain tools and concepts to obtain correct answers to analytical problems (Kuhnian puzzles). Correct answers must conform to definite criteria which are the fundamental characteristics of TP, e.g.: competitive markets must clear, decision makers must optimize, money illusion must be absent. However, imaginative answers that violate any maintained hypothesis of the paradigm are penalized as evincing failure to absorb training" [Reder, 1982, page 19].
123. Reder: "As in any 'normal science,' there is resistance to innovation and ambivalence toward would-be innovators . . .  
"The greater the merit to which a given contribution aspires, the greater the resistance it encounters. This is partly because greater merit is associated with need for more fundamental adjustments in the research program to which the contribution is made. But it is also because merit increases with the breadth of the contribution's influence -- the number and importance of the research programs that it disturbs. . . ." [Reder, 1982, page 20].
124. Reder: "The paradigmatic nature of TP gives its adherents a particular perspective upon empirical evidence. A new finding is, and should be, screened to see how it bears upon the findings of research programs in a number of related fields. Because in 'normal science' it is presumed that the currently accepted theory is valid, new findings are accepted far more readily if they are consistent with the theory's implications, than if they are not. . . .  
"This posture of TP causes its adherents to distrust reports . . . of behavior incompatible with the implications of economic theory. The resulting scepticism sharply distinguishes TP adherents from other economists who are willing, sometimes even anxious, to credit accounts of irrational and/or non-competitive behavior" [Reder, 1982, page 21].
125. Reder cites three major examples of paradigm extending/preserving theories in Chicago economics. (1) The Coase Theorem, (2) Becker's human capital theory and time allocation theories, and (3) Stigler's theory of search. Each theory broadened the applicability of economic theory (i.e., was "paradigm extending"). Coase broadened the applicability of the economic notions of "transaction" and "commodity" to the buying and selling of the right to injure; Becker's addition of a time constraint to the consumer budget model opened up a slew of new areas of research for economists and Stigler's work broadened TP's "range of applicability (to such phenomena as search, turnover, and frictional unemployment)" [Reder, 1992, pages 22-23]. Stigler's work, in addition, provided an explanation for the troublesome lack of a single price for like economic goods and resources and thus was also "paradigm preserving" [Reder, 1982, pages 21-23].
126. Reder: "The pros and cons of this matter cannot be usefully discussed in the abstract. Foolish or unlucky scientists can easily slide from 'properly tight' to dogmatic priors and appear, ex post, to have been insensitive to evidence. But a mixture of misfortune and bad judgment can just as easily make an open-minded investigator appear a credulous bubble head willing to believe anything. There is no formula for 'optimal tightness;' what is required is judgment and luck" [Reder, 1982, page 21].
127. Redman: "In economics there is no paradigm or program (theory is often what Kuhn means) that is unquestioned by all economists. Not even the *problem* is defined unanimously -- inflation being more of a problem for monetarists,

unemployment for the various Keynesians, stochastic disturbance for the rational expectations theorists. (And these groups all belong to orthodox economics)" [Redman, 1991, page 150].

128. Rouseas does not draw an explicit link between his observation that economics has come to focus upon narrow technical problems and eschewed consideration of social problems to Kuhn's notion of normal science. Nonetheless, he does, as we see in the two following quotes, liken economists to "paradigm polishers," (his derisive characterization of normal scientists), and does speak of economists being engaged primarily in "puzzle solving," an activity which he links with the practice of normal science [Rouseas, 1973, pages 75-76].
129. Swaney and Premus: "In short, economists all too frequently leap from their oversimplified view of the nature of man and their naive view of the institutional framework as a God-given constant to elegant and sophisticated theories . . ." [Swaney and Premus, 1982, page 726].
130. In this respect, Swaney and Premus's description of normal economic science comports with Miller's depiction of normal economic science (along with Dugger's account of the practice of normal science in neoclassical economics): economists first employ deductive methods to derive hypotheses (predictions), which they then test empirically.
131. Swaney and Premus regard economic practice in migration and monetary economics as representative of economic practice in general. In this respect they assert, "These subdisciplines are characterized by extensive theoretical and empirical work that we take to be indicative of the nature of the scientific process in other subdisciplines of economics" [Swaney and Premus, 1982, page 721].
132. Swaney and Premus: "In general, the sub-quantum leap method would yield, in comparison to quantum leap theorizing, superior policy recommendations" [Swaney and Premus, 1982, page 721].
133. Vredeveld: "[Barbara] Bergmann's illustration of the potential abuses of econometric studies seems to apply to the activities described in Kuhn's second category [matching fact with theory]. There is little reason to include the results of those studies that lend little to knowledge. But this is a long way from saying that econometric studies are valueless. Econometric studies that use facts to describe the nature of things (Kuhn's first category) provide good information for the precollege student. These studies may be essential in 'finding out things in economics.' We should at least sketch out how these data were obtained so that students know how we go about reaching the understandings we do" [Vredeveld, 1987, page 205].
134. Ward: "To the layman nothing is more obvious than that economics journals are exclusively concerned with problems of detail" [Ward, 1972, page 12].
135. Ward: "if one judges it by the standards which Kuhn has used in categorizing natural sciences, then economics passes muster as a science with flying colors" [Ward, 1972, page 15].
136. Ward: "Nevertheless, there are important aspects of its [economic science's] procedure that are rather troubling. The fact that a science should be so closely

tied to an ideology is perhaps the most important of these troubling aspects" [Ward, 1972, page 32].

137. Ward cites two examples: (1) Seeking to explain the differences in the relationship between income and consumption in time series versus cross section studies, and (2) explaining the discrepancy between microeconomic theory, which predicts that proportionate business taxes will not be shifted onto consumers, and empirical evidence which indicates otherwise [Ward, 1972, pages 17-19].
138. Ward: "Any fears aroused by this are bound to be enhanced by recognition of the possibility that though economics generates new puzzles, it often, perhaps typically, does not solve the old ones, and that methodological sophistication may often substitute for solution in the eyes of the most respected practitioners" [Ward, 1972, page 32].
139. Ward; "The important thing becomes not so much to solve the puzzle as to make an ingenious attempt at solving it within the conventional framework of puzzles. There is thus some risk that economic science may degenerate into a series of self-contained methodological explorations which are not closely tied to that real world which is the nominal subject of investigation" [Ward, 1972, page 19].
140. Ward: "Most economic problems deal with concepts whose fuzziness is a major handicap to a solution. In the tax case, the problem is that economists don't really know how to distinguish profit maximization from other modes of behavior over a wide range of situations. Furthermore, the data that would be relevant are quite hard to come by. . ." [Ward, 1972, pages 18-19].
141. Ward: "If Marxism is to qualify as a Kuhnian science, perhaps the principal test it must pass is the possession of a set of puzzles that define the ways in which economic problems are to be analyzed. These puzzles do exist, all right, or perhaps one should speak not so much of puzzles as of puzzle-forms -- of types of puzzles that can be applied fairly generally to whole classes of problems" [Ward, 1972, page 58].
142. Ward summarizes his assessment of the scientific status of Marxism as follows: "In summary, Marxism passes most of the tests necessary for a Marxist economic science to exist in the Kuhnian sense, but in practice has failed because of the virtual absence of an integrated social system of scientists oriented toward the systematic development of the science through study of problems of detail. It has the puzzles and the network of commitments sufficient to develop as a science, but in fact its development has been distorted and spotty" [Ward, 1972, page 70].
143. Whitley: "Thus many textbooks are filled with exercises and problem pages, or are combined with books of problems, which the neophytes have to work through to learn the prescribed procedures and correct ways of dealing with problems. Indeed, economics training manifests many characteristics of Kuhn's (1977) account of training in 'normal' paradigm-bound science: as 'a dogmatic initiation in a pre-established tradition that the student is not equipped to evaluate,' it develops a capacity to solve analytical problems in the prescribed manner with standardized techniques and formalisms. As a result, economists share common analytical skills, a standardized symbol system for

communicating the results of analytical research, a strong consciousness of the boundaries of economics and of appropriate ways of formulating intellectual problems in the field, and an overwhelming commitment to theoretical goals and priorities since none of the skills they have acquired deal with empirical research or the problems of turning data into information" [Whitley, 1986, page 193].

144. Lind: "the internationally respected Swedish theoretical economist Lars E.O. Svensson, currently professor at the Institute for International Economic Studies at Stockholm University" [Lind, 1992, page 85].
145. See our analysis of Foster-Carter's appraisal as to whether development economics constitutes a normal science.
146. We might also include Bliss, who conceives of the heterodoxy as practicing under the same normal science umbrella as the orthodoxy.
147. In this respect, Lind is an exception. As we saw, he finds that the realism of assumptions did matter considerably to normal scientists in theoretical economics.
148. Blankart, who describes public choice normal science as *founded upon fundamental disagreement*, constitutes the only -- though very notable -- exception to this rule which we found.
149. We may perhaps also include Jensen here. Although we found no direct remarks by Jensen as to whether normal economic science's pushing the social dimensions of prominent economists' work to the side was problematical, his tone (and the fact that Jensen himself is a social economist) strongly suggest that Jensen is displeased with the excising of social concerns from their works.
150. Assuming for the sake of argument that a normal science exists in economics.





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**ECONOMISTS' INTERPRETATIONS AND APPLICATIONS OF THOMAS S.  
KUHN'S THEORY OF SCIENTIFIC REVOLUTIONS**

**Volume II**

**By**

**Thomas Walter Patchak-Schuster**

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## CHAPTER FIVE: SCIENTIFIC REVOLUTIONS

In this chapter, we discuss the last of Kuhn's three major notions: scientific revolutions. First, we review the different ways in which economists have defined scientific revolutions. We find that while all economists examined associate a scientific revolution with a major change in a science, they differ as to the nature of that change. In particular, the lines of disagreement are drawn around the following questions, which can be grouped into two categories: (1) How discontinuous is the change effected by a scientific revolution? How much (if any) of a science's past is preserved in a scientific revolution? How permanent are the changes wrought by a scientific revolution? (2) Does a scientific revolution imply more than a major change? In particular, does a scientific revolution also connote a period during which scientists' behavior and the context in which they work changes? Must a scientific revolution occur quickly?

Second, we look at economists' understandings as to the causes of a scientific revolution. Most economists we find agree that anomalies play a role in effecting a scientific revolution, but that anomalies *per se* are not sufficient to induce a scientific revolution. Further, most agree that, according to Kuhn's model, anomalies are generated via the internal workings of normal science, and that, if important and persistent enough, anomalies will effect a crisis. However, economists provide varying understandings as to what constitutes an anomaly and are split as to whether a crisis must necessarily produce a scientific revolution.

Third, we consider economists' contentions that revolutions in economics are spurred by factors other than those which Kuhn, according to their interpretations of him, argued caused scientific revolutions.

Fourth, we examine those economists who assert that economics has never undergone a Kuhnian scientific revolution. As we shall see, many of these economists cite significant continuities in economics' history as evidence against the existence of scientific revolutions. At the same time, however, they disagree as to the implications of these continuities for the applicability of Kuhn's schema to economics. While some dismiss the applicability of Kuhn's model out of hand given the continuities they find, others identify modifications or alternative interpretations of Kuhn's theory of scientific revolution which render it applicable to economics. Further, economists diverge as to the continuities they identify and the reasons why those continuities have persisted in the history of economic thought.

Fifth, we briefly explore economists' assessments as to the implications of the differences between the natural and social sciences for the applicability of Kuhn's theory of scientific revolutions to economics. Here, we find that most -- but not all -- economists contend that these differences compromise the applicability of Kuhn's theory for economics. Further, we find that economists differ as to the significance they lend to these differences.

We next turn our attention to examination of economists' interpretations of economics' most well-known revolutions: the marginal (utility) and the Keynesian revolutions. Here, we find significant and fundamental disagreement. In particular, economists differ in their identification of the changes the marginal (utility) and/or Keynesian revolution effected in economics and their determination as to whether those changes constituted a scientific revolution/paradigm shift. These differences, we argue, relate to economists' divergent specifications of economics' paradigm.

We conclude our discussion of economists' applications of Kuhn's scientific revolution concept with brief discussions of some less often cited revolutions in economics' history. As with the marginal (utility) and Keynesian revolutions, we find disagreements as to whether a particular revolution constituted a scientific revolution and the changes it effected.

#### A. DEFINITIONS OF SCIENTIFIC REVOLUTION

Before entering into a discussion of economists' applications of Kuhn's theory of scientific revolutions, we first consider economists' various interpretations of the term, "scientific revolution." As we noted, all economists ally the term with some sort of change; however, they differ as to the nature, extent and context of that change.

##### 1. *Paradigm Shift*

For many economists, a scientific revolution constitutes a science's replacement of an old paradigm with a new one, i.e., a paradigm shift. O'Brien, for instance, defines a Kuhnian scientific revolution as "The process of paradigm replacement, when a new paradigm comes to be accepted by the scientific community" [O'Brien, 1983b, page 102]. Likewise, Mehta defines a scientific revolution as "the replacement of one paradigm by another," while Johnson and Ley equate a scientific revolution with "paradigmatic change," which they, in turn, ally with a change in a discipline's "purposive function"<sup>1</sup> [Mehta, 1978, page 5; Johnson and Ley, 1990, pages 28 and 36]. Finally, both Dow and Blaug associate a scientific revolution with the "overthrow" of a science's regnant paradigm by a new one [Dow, 1987, page 337; Blaug, 1976, page 152].<sup>2</sup>

##### 2. *Change in Worldview*

As we saw in our earlier examination of economists' definitions of paradigm, many economists characterize a paradigm as a worldview. Thus, it is not surprising that economists also describe a scientific revolution as a change in worldview. Vincent

Tarascio links the description of a scientific revolution as a paradigm shift with that of a change in worldview and describes a Kuhnian scientific revolution as "a displacement of one scientific paradigm, or way of seeing the world, by another fundamentally different" one [Tarascio, 1971, page 102, footnote 6]. Likewise, Wible equates a scientific revolution with both a paradigm shift and a worldview change, which transforms all aspects of a science:

In contrast, revolutionary science is characterized by a paradigmatic shift or change in world view. The change in world view constitutes a scientific revolution since it changes the conceptual, theoretical, and empirical aspects of the science in question. [Wible, 1984, pages 94-95]

Similarly, Johnson and Ley point out that the newly emergent paradigm constitutes "an altered way of perceiving that portion of the universe which is its area of interest" [Johnson and Ley, 1990, page 26], and Mehta allies a scientific revolution with a gestalt switch [Mehta, 1978, page 5].<sup>3</sup> Jalladeau, as well, indicates that a scientific revolution alters a science's worldview: "In Kuhnian terms, a scientific revolution is a displacement of the conceptual frame from which scientists view the world" [Jalladeau, 1978, page 589].<sup>4</sup>

Stanfield goes further. Not only does the new paradigm scientists adopt out of a scientific revolution change their view of the world, it changes the world itself: "The new paradigm is a change in world view to the extent that the world itself is changed: The perception and cognition of data and even the data to be collected are redefined" [Stanfield, 1974, page 100].

### 3. *Change in Problem Focus*

Along with the paradigm (worldview) change, some economists point out that a scientific revolution also alters a science's priorities. As some economists' interpret Kuhn, a scientific revolution leads scientists to focus their energies on a new set of problems and adopt a new set of criteria for evaluating prospective solutions. In this regard, Jalladeau asserts that, "At the time of a replacement of paradigms there is thus

a significant change in the standards determining the legitimacy of the problems as well as of the offered solutions . . . " [Jalladeau, 1978, page 590]. Similarly Chase maintains that under Kuhn's model, "with the acceptance of a new paradigm there is a shift to a new source of methods, problem-field and accepted standards of solution" [Chase, 1983b, pages 820-821].

However, Cristina Bicchieri, while acknowledging that the overlap is not complete, points out that under Kuhn's schema the new and old paradigms do share certain problem areas in common with one another [Bicchieri, 1989, pages 245-246].

#### 4. *Non-incremental, Discontinuous Change*

Numerous economists stress that a scientific revolution marks a discontinuity in a science's history. The new paradigm emerging from a revolution is not the cumulation and extension of past science (and past paradigms) and does not simply build upon a science's past; to the contrary, it provides scientists an entirely new basis upon which to establish their work. Along these lines, Rouseas describes a Kuhnian scientific revolution as "non-cumulative" [Rouseas, 1973, page 76]. Similarly, Stanfield asserts that a scientific revolution is "not simply a cumulative process of attaching a new layer to the old foundation. Rather, it is the construction of a new foundation involving new fundamental laws, generalizations, and behavioral functions, often new methods and applications, and a redefinition of the character and standards of the science" [Stanfield, 1974, page 100]. Likewise, Dunn remarks,

In short, a paradigm shift emerges. Scientists have to turn their attention to the formulation of a new contextual theory to guide their work. It is important to realize that this is not just a linear extension of established theory. It involves a fundamental change in perspective and perception. . . . There is a change in the rules and standards that govern the practice of normal science in that discipline. [Dunn, 1970, pages 353-354]<sup>5</sup>

Remenyi and Matsui, Asano and Matsuda characterize a scientific revolution as a rupture with the past. Remenyi allies a scientific revolution with "a sharp break with the verities of previous theories," and Matsui, Asano and Matsuda describe a scientific

revolution as the "process of breaking through the traditional thinking" [Remenyi, 1979, page 31; Matsui, Asano and Matsuda, 1989, page 123].<sup>6</sup>

Other economists provide more complex understandings of the (dis)continuity of Kuhnian scientific revolutions. Deane identifies two interpretations of Kuhn's theory of scientific revolutions. The first contains "a strong element of 'rhetorical exaggeration'" and implies a "total paradigm-switch," indicating a dramatic and complete break from a science's past paradigm [Deane, 1978, page xii]. The other "narrower sense of Kuhn's concept" describes a less complete break with the past: "a pervasive change in the typical criteria, exemplars and procedural rules accepted as normal . . ." [Deane, 1978, page 97]. Johnson and Ley contend that, because Kuhn took only *implicit* account of a paradigm's purposive function, the only scientific revolutions possible under his specification are total gestalt switches.<sup>7</sup> However, they argue, if one *explicitly* takes account of the purposive function, scientific revolutions need not imply complete breaks with the past paradigm. While the new and old paradigms are still fundamentally different from one another (i.e., possess distinct purposive functions), they still may share other, less fundamental elements in common. The purposive function "view admits of certain continuities that may exist with respect to the content of other paradigm characteristics, such as focal variables or method, as well as the persistence of some secondary issues" [Johnson, 1983, page 1104].

##### 5. *Treatment of the Old Paradigm*

Related to the (dis)continuity of a scientific revolution is scientists' treatment of the old paradigm. Economists' positions on this issue vary. Some interpret Kuhn to say that the new paradigm completely displaces the old. According to Bronfenbrenner, "Thomas Kuhn's 'catastrophic' theory of scientific revolution" implies "the complete disappearance of a paradigm, or a mode or framework of thought and language in some branch of science, following a revolutionary upheaval" [Bronfenbrenner, 1971, pages

136-137]. Likewise, according to Rugina, a Kuhnian scientific revolution implies the "total negation" of the old paradigm and Khalil assents that "there is textual support [in Kuhn] for the total displacement thesis" [Rugina, 1989, page 17; Khalil, 1987, page 120]. While not indicating that the old paradigm is fully dispensed with, Zinam argues in a Kuhnian scientific revolution the old paradigm "is rendered obsolete" [Zinam, 1978, page 181]. Worland, however, maintains that a paradigm shift does not render the old paradigm wholly useless; instead the old paradigm "can be said to retain its validity over a limited range of phenomena" [Worland, 1972, page 281].

On the other hand, Johnson and Ley provide no indication that the old paradigm becomes unusable, but do assert that it is "left unused" after the revolution [Johnson and Ley, 1990, pages 26-27]. They, however, allow that "the rise of the new paradigm does not necessarily destroy the old one" [Johnson and Ley, 1990, pages 26-27]. Likewise, Worland finds that, after a paradigm shift, the old paradigm "may be taken as a special case of its successor" [Worland, 1972, page 281]. And, Ekelund and Hébert maintain that the old paradigm is not destroyed by, but rather "absorbed" into the new one. Under Kuhn's schema, they assert that in scientific revolutions, "Old paradigms are often absorbed into new ones, however, as illustrated by examples from chemistry and physics, e.g., the emergence of quantum mechanics in reaction to Newtonian physics, and absorption of the latter into the former" [Ekelund and Hébert, 1990, page 11].

Some economists contend that, according to Kuhn's theory, once a paradigm has been displaced, it is displaced forever. John Vint describes the displacement of an old theory by a new one in Kuhn's schema as "final," while E.G. West asserts that "Kuhn's system includes no provision for old paradigms to come back and replace new ones" [Vint, 1971, page 285; West, 1978, page 349]. Bronfenbrenner, however, makes this point most forcefully -- and colorfully:



The difficulty with the catastrophic theory is that, if I understand Kuhn correctly, it maintains that paradigms, once displaced, are displaced definitively and relegated to the antiquarian's dustbin. [Bronfenbrenner, 1971, page 137]

As does Bøhren who asserts that in a Kuhnian revolution, "the new paradigm kills the old one once and for all" [Bøhren, 1990, page 12].

Chase, however, takes direct issue with Bronfenbrenner's "catastrophic" interpretation of Kuhn's theory of scientific revolution. The former contends that "Kuhn is quite explicit on the issue that he views a new emergent paradigm as a synthesized amalgam of that which has preceded it," including the prior paradigm [Chase, 1983b, page 822].

#### 6. *Incommensurability of the Old and New Paradigm*

As many economists interpret Kuhn, the old and new paradigms are "incommensurable" to one another. That is, it is impossible to make meaningful comparisons between the two paradigms. Rousseas describes paradigm shifts as "incommensurable" [Rousseas, 1973, page 76]. Khalil and Caldwell, likewise, understand the new and old paradigm in a Kuhnian scientific revolution may be incommensurable with one another [Khalil, 1987, pages 120 and 126; Caldwell, 1982, pages 73-74].

Economists explain the incommensurability of the new and old paradigms along a number of different lines. At bottom, all hinge upon the fundamental differences between the old and new paradigms. Chase, even while acknowledging that the new paradigm "embodies key elements of its antecedents," stresses that, despite links with the past, the new paradigm "is nonetheless a separate entity, distinct and unique from that which has come before" [Chase, 1983b, page 822]. Further, given that the new paradigm concerns itself with problems and facts the old one did not, the new paradigm will be incommensurable with the old one:

since a new and competitive paradigm will incorporate various different facts and problems -- the anomalous findings of normal research -- it

will be fundamentally incompatible with the existing paradigm. Thus the ensuing debate will tend to take place on different sets of premises, and participants in paradigmatic debate will tend to talk through one another . . . [Chase, 1983b, page 820]

Jalladeau, likewise, links the incommensurability of the old and new to their different problem foci, along with a host of other fundamental differences between the two:

Kuhn describes the successor paradigms as being "incommensurable." In the frame of a new conceptual model, the knowledge elements -- vocabulary, analytical equipment, experiences -- will interact differently (even if they are partly the same). At the time of a replacement of paradigms there is thus a significant change in the standards determining the legitimacy of the problems as well as of the offered solutions since, as we have seen with Kuhn, "in learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture. [Jalladeau, 1978, page 590]

For Mehta, the way of thinking required by the new paradigm renders comparisons between it and the old one impossible:

The incommensurability is due to the fact that the new paradigm cannot be simply grafted on to the existing stockpile of scientific knowledge. To use Butterfield's felicitous phrase, the new paradigm requires that a new "thinking cap" be donned by the scientists. Changing the "gestalt" in which scientists view the world is not an easy process. It is for this reason that new paradigms meet with stiff resistance from adherents of the old paradigms. [Mehta, 1978, pages 5-6]

Similarly, Stanfield maintains that the new and old paradigms are incommensurable because they imply "different world views, standards, delineations of the science, and connotations of terminology" [Stanfield, 1974, page 100].<sup>8</sup>

Alternatively, others describe the new and old paradigms as "incompatible" with one another. Stigler, in explicating Kuhn's theory of scientific revolutions, observes that "The new theory explains some phenomena differently than the older theory explained them, and hence the two theories are not logically compatible" [Stigler, 1969, page 225]. Phillips directly quotes Kuhn as to this point:

"Scientific revolution" is used here in the same sense as in T.S. Kuhn, *The Structure of Scientific Revolutions*. Kuhn restricts the term to . . . "non-cumulative development episodes in which an older paradigm is replaced in whole or in part by an incompatible new one." [Phillips, 1966, page 302, quoting Kuhn, 1962, page 91]

As Bicchieri interprets Kuhn, however, incommensurability is antithetical to his framework. To the contrary, under Kuhn's schema, old and new theories arising from a revolution are commensurable. The grounds for comparison, she explains, lie with the common sets of problems with which the old and new theories concern themselves:

With [Larry] Laudan and Kuhn, we have an intermediate position,  $T_2$  [the new theory] may not accommodate all the explanatory successes, solved empirical problems, or observational consequences of  $T_1$  [the old theory], nevertheless it remains possible to assess the merits of new sets of guiding assumptions with respect to old ones. To this effect, it is not necessary to uncritically assume a neutral or shared domain of facts. It is enough to identify common problem-areas, and questions that both  $T_2$  and  $T_1$  consider relevant . . . [Bicchieri, 1989, page 245]

#### 7. *Losses as Well as Gains*

Bicchieri further holds that, in a Kuhnian revolution, there are losses as well as gains. While the new paradigm possesses desirable characteristics which the old one did not (such as greater generality), the new one inevitably also sacrifices certain highly serviceable elements of the old paradigm (such as a well-performing theory) [Bicchieri, 1989, pages 245-246 and 253]. Similarly, B. Sandelin points out that Kuhn affirmed that there were both losses as well as gains in a scientific revolution [Sandelin, 1986, page 84].

#### 8. *Scientific Revolutions and the Scientific Community*

Interpreting Kuhn's schema, economists identify two major consequences of a scientific revolution for the scientific community involved. First, in their search for an alternative to the previous paradigm, prior to the revolution, scientists suspend the practice of normal science, and scientific practice enters into a phase of "extraordinary" or "revolutionary" science. During this time, the prevailing paradigm's hold on the community weakens and scientific research becomes increasingly random, ad hoc and/or contentious, as these descriptions indicate:

while there is still a paradigm, Kuhn argues that the proliferation of competing, divergent articulations of this paradigm will lead to a blurring of both the paradigm and the rules of normal science puzzle-

solving, in the sense that there will be less and less unanimity within the scientific community with regard to the legitimacy or otherwise of these articulations. [Glass and Johnson, 1989, page 159]

There occur a relaxation of the rules of normal science and more speculative, random research. Increasingly divergent articulations occur which may involve the formation of schools of thought. [Stanfield, 1974, page 99]

Instead of forcing nature into the preformed conceptual box provided by the paradigm, they begin to tamper with the structure and dimensions of the box itself. The original paradigm becomes blurred and the rules for its interpretation and employment are loosened. [Worland, 1972, page 276]

Further, in contrast with normal science, scientists on the verge of a scientific revolution enter into methodological and philosophical debates with one another [Johnson and Ley, 1990, page 30; Blaug, 1976, page 153; Deane, 1978, page 100].

Second, during the transition from the old to the new paradigm, communication among scientists breaks down. Advocates of the old and new paradigms are often described as talking past one another. This disruption, according to many economists' interpretations, results from the paradigms' incommensurability:

At the group level, there is bitter controversy between adherents of the old and new paradigms, which is rendered all the more frustrating by the fact that inevitably both groups argue from within their own paradigms, and there can be no "neutral observation language" with which they could communicate. Hence in a sense they do not communicate at all: Kuhn actually says that they "talk past" one another. [Foster-Carter, 1976, page 170]

the ensuing debate [between adherents of the new and old paradigm] will tend to take place on different sets of premises, and participants in paradigmatic debate will tend to talk through one another; that is, proponents of competing paradigms are always to some extent at cross-purposes. . . . Since each group will tend to use its own paradigmatic model to argue that paradigm's case, the argument will necessarily be circular. [Chase, 1983b, page 820]

Baumberger stresses that these behavioral changes are essential attributes of a Kuhnian scientific revolution. A Kuhnian revolution, he argues, though entailing a paradigm/proposition change, connotes significantly more than the change itself:

We must take it that breakdown of both communication and mutual intelligibility, the desperate search for *ad hoc* amendments of existing theories, the disruption of the social fabric of a discipline, and the

increased reliance on extrascientific standards, increased behavioral uncertainty, and so forth, are what mark off a revolution from normal science. [Baumberger, 1977, pages 5-6]

Blaug, as well, cites the "pervasive failure of communications" prior to a paradigm shift as one of the "distinctive feature[s] of Kuhn's methodology" [Blaug, 1976, page 153].

#### 9. *Violent Change*

Many economists characterize the transition from the old to the new paradigm in violent terms. As we saw earlier, Dow and Blaug describe the replacement of the old with the new as an "overthrow" [Dow, 1987, page 337; Blaug, 1976, page 152]. Further, Blaug asserts, in a scientific revolution, the new "conquers" the old paradigm [Blaug, 1976, page 153]. We have seen as well that Bøhren asserts that the new paradigm "kills" the old one. He further describes the old as being "attacked" by the new one [Bøhren, 1990, page 12]. Similarly, DeVroey affirms that "Kuhn's main thesis is that a change of the paradigm in dominance occurs in a rather brutal way, comparable to political revolution" [DeVroey, 1975, page 420]. And, Stanfield stresses that the "importance of . . . the analogy of scientific revolutions to political revolutions" "cannot be overstated" [Stanfield, 1974, page 105].

#### 10. *Conversion Experience*

Economists, as well, interpret the shift from the old to the new paradigm as amounting to a conversion experience, in which scientists transfer their allegiance to the new paradigm. They, however, offer varied understandings as to the reasons why the shift constitutes a conversion. Blaug characterizes the conversion as "a religious experience" [Blaug, 1976, page 153]. Coats draws no parallel to the religious; he instead maintains that the shift from old to new constitutes a conversion because ". . . the change is dependent on the possibilities inherent in the new paradigm rather than any demonstrable proof of its superiority" [Coats, 1969, page 291]. Chase, on the other hand, explains that the switch amounts to a conversion because adherents of the

old and new paradigm lack "access to neutral or objective argument" [Chase, 1983b, page 820].

#### 11. *Speed of a Scientific Revolution*

Economists also disagree as to the rate at which a science, undergoing a Kuhnian scientific revolution, moves from the old to the new paradigm. Some maintain that Kuhn's theory requires the shift to be rapid. Vint for instance asserts that the displacement of the old paradigm must be "abrupt," as does Stigler [Vint, 1971, page 285; Stigler, 1969, page 225].

Others, however, dispute this contention and argue that, in determining whether a revolution is a Kuhnian one, time is irrelevant. For example, Khalil points out that, "Kuhn, however, did not give a concrete time span in order for a revolution to take deep roots" [Khalil, 1987, page 120]. DeVroey goes further and specifically warns against misinterpreting Kuhnian revolutions as "instantaneous events" [DeVroey, 1975, page 431].

#### 12. *A Scientific Revolution Must Come to an End*

Others, while not requiring that a Kuhnian revolution be abrupt, stress that to comport with Kuhn's schema, a revolution must come to an end. Baumberger affirms strongly that, "In the Kuhnian account it is fairly explicit that the periods of revolution sooner or later come to an end so that the canvas is cleared for another period of normal science . . ." [Baumberger, 1977, page 7]. Similarly, Jalladeau, contrasts Kuhn's notion of a scientific revolution with Popper's understanding of a revolution in permanence:

By contrast, for Kuhn, revolutions by their very nature cannot be the whole of science. Frameworks must be "lived with and explored," that is, articulated before being eliminated. For him, the model of production and growth of knowledge is relative; for the Popperians, it is absolute, unchanging. [Jalladeau, 1978, page 592]

### 13. *Summary*

In sum, economists regard a scientific revolution as entailing a fundamental (as opposed to incremental) change within a science. In particular, we find that numerous economists understand a scientific revolution as a paradigm change. Further, as many economists understand Kuhn, the fundamental nature of this change renders the new paradigm arising out of the scientific revolution incommensurable to its predecessor. While noting this broad agreement, however, we must recall from Chapter Three that economists have interpreted the paradigm concept in a wide variety of different ways. Thus, lying behind economists' agreement that a scientific revolution implies a paradigm change are significant differences as to what a "paradigm change" itself is. Further, while many economists understand a scientific revolution as implying a discontinuity with the past, they disagree as to the degree of that discontinuity. Some economists contend that a scientific revolution marks a complete and permanent break with the past, whereas others see continuities existing within a science before and after the revolution. Further, while many economists understand a scientific revolution as a period during which the normal rules of science are suspended and scientists experience significant difficulties communicating with one another, only a few explicitly identify these behavioral changes as a necessary attribute of a scientific revolution. Finally, economists disagree as to whether a scientific revolution must, by definition, occur quickly.

### B. CAUSES OF SCIENTIFIC REVOLUTIONS

We next turn our attention to economists' understandings of the causes of scientific revolutions under Kuhn's schema. As with economists' interpretations of these revolutions, we find that while agreeing as to certain aspects of the causes of scientific revolutions, economists disagree about others.

# 1. *The Definition of Anomalies*

According to most economists' explanations, "anomalies" play a significant role in spurring a Kuhnian scientific revolution. However, the majority of economists who refer to the role which anomalies play in instigating scientific revolutions leave the term undefined. Among those who do define "anomaly" the most common understanding is an unexpected empirical result. Peabody, for instance, defines anomalies as "novelties of fact that occur with an unanticipated empirical observation," and Glass and Johnson characterize them as "discrepancies between theory and observation" [Peabody, 1971, page 3; Glass and Johnson, 1989, page 155]. Similarly, the anomalies which both H.E. Conklin and W.R. Bryant and John Cohen and David Lewis identify in economics employing Kuhn's schema constitute observations not expected under the regnant paradigm. For Conklin and Bryant, it is "prices far above farm value when farming is the next alternative use" [Conklin and Bryant, 1974, page 608]. For Cohen and Lewis, it is the case of Yemen where, counter to paradigmatic expectations, "commercial and manufacturing systems . . . do not seem to be serving the needs of a progressive rural structure," even while "a significant proportion of the rural population already has substantially more money than it has ever had before" [Cohen and Lewis, 1979, page 527].

Conversely, others define anomalies as expectations of the paradigm which run counter to observed fact. Cornwall, for instance, describes anomalies as

inconsistencies between empirical observations and the predictions and assumptions of the given theories which the practitioners consider important but which they find they cannot resolve within the framework of their paradigm. [Cornwall, 1979, page 71]

Likewise, Worland characterizes anomalies in economics as "instances when expectations derived from the paradigm of conventional economics are disconfirmed," and Pheby affirms that "The significance of anomalies is that our 'paradigm-induced expectations' of the world are violated . . . ." [Worland, 1972, page 277; Pheby, 1988, page 40].



Still another related understanding of anomaly is as a fact which cannot be incorporated into the paradigm's framework. Jalladeau, for instance, defines anomalies as "stubborn facts [which] cannot be subdued with the help of supplementary (the Popperian would say auxiliary) hypotheses within the existing paradigmatic structure" [Jalladeau, 1978, page 589]. Green similarly describes anomalies as "features of the data which are observed but cannot be explained by existing theory" [Green, 1977, page 268]. Finally, Backhouse describes anomalies as "awkward facts which cannot be explained in terms of the paradigm" [Backhouse, 1985, page 4].

However, while most who define "anomaly" ally it with empirical observation, some economists stress that "anomaly" in Kuhn's framework applies more generally. There are, they note, not only empirical anomalies, but theoretical ones as well. Stanfield, for instance points out that

an anomaly may be associated with conflicting experimental or empirical discoveries *or* with an insistent theoretical ambiguity which defies resolution by paradigm articulation. [Stanfield, 1974, page 99, emphasis added]

And, Pheby speaks of there being "'anomalies' in both fact and theory" in Kuhn's model [Pheby, 1988, page 40].

Still more generally, others define an anomaly as a normal science puzzle that has gotten out of hand. Here, we find Mehta describing anomalies as "intractable puzzles" and Khalil characterizing them as, "unmanaged 'puzzle[s]' for people who are practising normal science" [Mehta, 1978, page 5; Khalil, 1987, page 120]. Still again, we see Johnson and Ley defining anomalies as "unsolvable problems encountered in the practice of normal science" [Johnson and Ley, 1990, page 28]. Finally, DeVroey defines anomalies as "puzzles which the existing paradigm cannot resolve" [DeVroey, 1975, page 420].

With the possible exception of the last understanding, all these understandings of anomaly imply that anomalies have either empirical and/or analytical basis. None

draws any explicit affinity between Kuhnian anomalies and ideological questions. Khalil starkly distinguishes between anomalies and undesired ideological consequences of a paradigm (e.g., the implication of the Classical paradigm's labor theory of value that labor is being exploited under capitalism):

An anomaly could not be liking or disliking certain explanations, but would be rather the inability of a paradigm to handle a pressing counter-instance. [Khalil, 1987, page 120]

## 2. *The Source of Anomalies*

As we indicated last chapter, many economists interpret the practice of normal science as that which generates/uncovers anomalies under Kuhn's schema. As they see it, normal scientists, despite their intentions to the contrary, produce/discover anomalies as a result of their work aimed at refining and extending the paradigm.

Economists provide varied, though related explanations as to how normal science creates/locates anomalies. Gordon, though not explaining how, notes simply that "in the further development of a basic model, empirical anomalies appear" [Gordon, 1965, page 123].<sup>9</sup> Caldwell links anomalies' appearance to normal scientists' "intense efforts" [Caldwell, 1982, pages 91-92].<sup>10</sup> According to Rousseas, anomalies are the product of normal scientists' working "on the frontiers of science":

A key element within Kuhn's analysis of normal science is the phenomenon of paradigm shifts. As the paradigm polishers work, as we are wont to say, "on the frontiers of science," eventually certain unexpected novelties or anomalies crop up that cannot be adequately explained by the existing paradigm and which violate paradigm-induced expectations. [Rousseas, 1973, page 76]

Finally, Worland attributes the emergence of anomalies to "[t]he deepening vision attained by employing a successful paradigm" [Worland, 1972, page 276].

Many economists take note of an irony. Even while normal science seeks to limit its work to the confines of the regnant paradigm, it uncovers anomalies, which may lead to the paradigm's replacement:

Finally, though normal science is a cumulative enterprise, it has unintentional noncumulative effects. By its very nature, normal science leads its practitioners to awareness of anomalies, which are a prerequisite to new discoveries that ultimately can produce paradigm change. [Caldwell, 1982, page 72]<sup>11</sup>

For others, the irony is even more profound. The more normal scientists succeed at their work aimed at refining and broadening the prevailing paradigm's application, the greater the likelihood that they will encounter paradigm-threatening anomalies. As Worland notes, "the very success with which a science 'articulates' its existing paradigm guarantees that, sooner or later, inadequacies in the paradigm itself [anomalies] will begin to emerge" [Worland, 1972, page 276]. Johnson and Ley make the point most strikingly:

. . . there is a bit of irony here that Hegel and Marx would no doubt appreciate: the initial success of the paradigm encourages its wider application and sophistication, leading to the emergence of anomalies. These, in turn, cause dissatisfaction with the paradigm and touch off a search for a new one. *Nothing fails like success.* [Johnson and Ley, 1990, pages 28-29, emphasis added]

Chase interprets Kuhn's theory of scientific revolutions as a dialectical theory of scientific development and identifies normal science as the twin force of intra-paradigmatic progress and inter-paradigmatic change. As normal science makes progress within the current thesis (the regnant paradigm), it also brings to light contradictions (anomalies), out of which antitheses (alternative paradigms) may be formed:

Furthermore, it is the "base" -- the exemplary paradigm in the Kuhnian case and the mode of production in the Marxian -- that incorporates the forces of progress and change *within* the system and which also generates those contradictions which in turn bring about the fundamental change in the system itself through conflict and revolution. In Marx, it is technology expressing itself through the mode of production that so importantly powers the process of change; while in Kuhn it is the puzzle-solving activity of normal science that brings forth the forces of change. [Chase, 1983b, pages 822-824]

Explicating Kuhn in dialectical terms, Chase explains that the thesis and antitheses may, in turn, be incorporated and reconciled within a synthesis -- a new paradigm [Chase, 1983b, page 821].

### 3. *The Role of Anomalies*

Economists interpret anomalies as a prerequisite for a scientific revolution under Kuhn's model. Wisman, for instance, refers to anomalies as "the necessary prelude to revolutions" [Wisman, 1979, page 27]. However, while agreeing that anomalies are a *necessary* condition for a Kuhnian scientific revolution, economists do not regard them as a sufficient one.<sup>12</sup>

Economists point out that the existence of anomalies may lead to outcomes other than a scientific revolution under Kuhn's model. Alternatively, scientists may devise ad hoc modifications to the current paradigm in order to accommodate the anomalies. As Worland notes, "Many times, an 'anomaly' is eventually cleared up and assimilated into the existing body of theory," while Stanfield allows that "the paradigm may be adjusted to resolve the anomaly" [Worland, 1972, page 276; Stanfield, 1974, page 99].

Still further, scientists may ignore an anomaly or push it off to the side: "it may exist and be recognized but be considered peripheral" [Stanfield, 1974, page 99]. Along similar lines, Gordon allows that "the resolution of" anomalies "can normally be postponed" [Gordon, 1965, page 123].<sup>13</sup> In either case, the anomalies (at least in the near term) do not generate scientific revolutions.<sup>14</sup>

However, many economists acknowledge that while anomalies, *per se*, do not produce scientific revolutions, sufficiently numerous, persistent and/or important anomalies will induce a crisis, in which there comes to be growing dissatisfaction and/or discomfort with the regnant paradigm. As Stanfield interprets Kuhn, in order for anomalies to produce a crisis they must "question explicit, fundamental generalizations of the paradigm, be important to the solution of a pressing practical problem, or involve a long history of persistently defying resolution within the paradigm" [Stanfield, 1974, page 99]. Worland and Chase provide a similar interpretation:

On other occasions, however, the anomalies persist and accumulate. When this happens scientists begin to question the adequacy of the

current paradigm. Instead of forcing nature into the preformed conceptual box provided by the paradigm, they begin to tamper with the structure and dimensions of the box itself. The original paradigm becomes blurred and the rules for its interpretation and employment are loosened. When anomalies persist and accumulate to this extent, the science in question undergoes a "transition to crisis." [Worland, 1972, page 276, quoting Kuhn, 1970c, page 82]

But as anomalies grow in number and/or importance, their existence becomes more critical. And a "sense of crisis" will develop, particularly if anomalies call into question explicit and fundamental generalizations of the paradigm itself. [Chase, 1983b, page 819]<sup>15</sup>

Likewise, Gordon notes that, according to Kuhn, "if enough such anomalies occur, or if they are particularly critical, a crisis emerges" [Gordon, 1965, page 123].

As many economists interpret Kuhn, the crisis thereby generated (along with the availability of an alternative paradigm)<sup>16</sup> will inevitably effect a paradigm shift. For example, as Albert Arouh interprets Kuhn's model, if anomalies produce a crisis within a normal science, "a gestalt switch is generated eventually" [Arouh, 1987, page 417].<sup>17</sup> Klant's interpretation of Kuhn, likewise, implies the ineluctability of a paradigm shift given anomalies sufficient to effect a crisis:

If the anomalies increase, a number of scientists are ultimately prompted to practice what Kuhn calls, "extraordinary research," to resolve the crisis. They work at proclaiming a new paradigm, to which they manage to convert the community of researchers after a time, so that a new stage of normal science can be initiated. [Klant, 1984, page 43]

In addition, both Ronald Heiner and Barrie Pettman contend that anomalies of "sufficient" number and/or import will invariably result in a paradigm shift:

The work of Thomas Kuhn . . . has emphasized a systematic pattern of resistance in the behavior of scientists to quick and sensitive reaction to new ideas and theories. Yet, when sufficient anomalies and awkwardly interpreted evidence about a previous theory build up, a major shift in ideas (a "scientific revolution") will relatively quickly occur. [Heiner, 1983, page 576]

Given the paradigm, change is by accretion, elaboration and moderate extensions (his "normal science") until the paradigm is subverted by research anomalies of sufficient scope to shift professional allegiances to a new paradigm. [Pettman, 1977, page 105]

Contrariwise, many economists stress that crises and the availability of alternative paradigms need not result in a paradigm shift under Kuhn's model. For

instance, while Webb posits the existence of a crisis and an alternative as a necessary condition for a scientific revolution, it is unclear whether he regards their existence as a sufficient condition:

It is only when the number of anomalies grows to such proportions that a "crisis" arises in a science and -- provided some alternative body of theory exists -- that a "scientific revolution" occurs, overthrowing the old theory for the new theory that helps to explain the previously unexplained anomalies. [Webb, 1987, page 405]<sup>18</sup>

Gordon, in contrast to these authors who portray paradigm shifts as the inevitable result of scientific crises, asserts only that "a crisis . . . *may* shatter the tradition," i.e., regnant paradigm [Gordon, 1965, page 123, emphasis added].

Others directly question the understanding that a crisis and an alternative paradigm must lead to a paradigm shift under Kuhn's theory. Reynolds does identify "crises, coupled with new discoveries," as that which "force[s] a paradigm shift" under Kuhn's model. However, as he interprets Kuhn, the philosopher allowed that scientists confronting a crisis situation may be able to make the adjustments to the regnant paradigm necessary to dispel the anomaly [Reynolds, 1976, page 30]. Caldwell, along similar lines, explains that a crisis need not invariably lead to a scientific revolution because "normal science may successfully handle the apparent anomaly" [Caldwell, 1982, page 72].<sup>19</sup> Ekelund and Hébert, likewise, point out that a crisis need not result in a paradigm shift. Instead, adherents to the orthodoxy may find the means by which to subsume a competing paradigm as a special case of the regnant paradigm, and thus avert its replacement [Ekelund and Hébert, 1983, page 446].

Stanfield contends that a crisis-ridden paradigm may be able to fend off its replacement because it may be "sufficiently institutionalized to *suppress* alternative paradigms" [Stanfield, 1974, pages 105-106]. Likewise, DeVroey points out that a crisis does not necessarily result in a paradigm shift because adherents to the ruling paradigm, who wield considerable power, will oppose the shift:

If the anomalies are not resolved and the uneasiness tempered, and if approaches involving a radically different way of looking at the problems are suggested, then the chances of a revolution will grow. However, cognitive factors are not sufficient in themselves to cause the move. Indeed, such a change does not occur in a social vacuum. The power elite within the profession is linked to the existing paradigm. Since they feel, rightly, that changes within the power and prestige structure probably will follow a scientific revolution, they may very well use their power to oppose the changes. Thus a scientific revolution is the result of forces in tension. On the one hand there are forces, usually of a cognitive order, pushing for a break; on the other there are forces, either cognitive (the ability of an existing paradigm to broaden its scope in order to integrate anomalies) or social, which will work against the successful occurrence of the scientific revolution. [DeVroey, 1975, page 420]

Finally, many economists also point out that, according to Kuhn's framework, an alternative paradigm will replace a crisis-ridden paradigm only if the new paradigm effectively addresses the anomalies which plagued its predecessor. As Harry Landreth and David Colander explain only "an alternative paradigm better able to deal with the anomalies can" "overthrow the reigning paradigm" [Landreth and Colander, 1989, page 13]. According to Caldwell one reason why a Kuhnian scientific revolution "does not follow automatically from the crisis situation" is that "on occasion, no new replacement that can solve the old problems is forthcoming" [Caldwell, 1982, page 72]. Similarly, Everett Burt and Phyllis Deane remark:

In order to supplant the old normal science, however, the new view must open up previously unrecognized areas for research as well as shed new light on the interpretation of old problems. The new paradigm must be a theory capable of development, not simply a critique of the present way of thinking. [Burt, 1972, page 282]

Among the reasons which Kuhn in his study of scientific revolutions in the natural sciences has identified as typically important in inducing a scientific community to reject one disciplinary matrix in favour of another are: (i) the emergence of a "methodological crisis" due to the failure of the current orthodoxy to deal effectively with problems that have come to be regarded as crucially important; (ii) the ability of the new paradigm to resolve the problems that led its predecessor into crisis; and (iii) its superior quantitative precision. [Deane, 1978, page 100]<sup>20</sup>

#### 4. *Summary*

To summarize the foregoing: (1) As economists interpret Kuhn, scientific revolutions are internally generated. The practice of normal science gives rise to anomalies which may, if of sufficient number and importance, lead to a crisis, which may in turn effect a scientific revolution. Coats makes this point explicit:

According to Kuhn, paradigm breakdowns occur as a result of conditions *internal* to the scientific community, for example, weaknesses inherent in the structure of theory, unexpected or anomalous "results," or the failure of predictions based on the ruling paradigm. [Coats, 1977, page 6]

And, as we shall see in our treatment of economists' assessments of Kuhn's applicability to economics, Kunin and Weaver, Johnson and Ley and DeVroey all implicitly identify Kuhn's theory of scientific revolutions as an internalist account of scientific change [Kunin and Weaver, 1971, page 394-395; Johnson and Ley, 1990, page 36, DeVroey, 1975, page 416 and 420]. We must, however, note an exception. Mehta, while providing a similar account as to how the practice of normal science gives rise to anomalies, which may in turn precipitate a paradigm shift, stresses that "Kuhn's thesis does not require that the crisis should be produced by the work of normal scientists working on the basis of the old paradigm . . ." [Mehta, 1978, page 62].

(2) Economists offer varying understandings as what constitutes an "anomaly." While many regard anomalies as problematical mismatches between a science's paradigm and empirical observation, others also link anomalies to theoretical difficulties vis-a-vis the paradigm, and still others, more generally, to intractable puzzles.

(3) As economists interpret Kuhn's schema, anomalies are a necessary, but not sufficient condition for a scientific revolution. While anomalies play an indispensable role in leading a science to a scientific revolution, normal science may find ways to deal with them so as to avert a revolution.<sup>21</sup>



(4) However, economists are split as to whether crises spurred by pressing anomalies must necessarily result in a paradigm shift. Some speak as if crises automatically give rise to scientific revolutions; others, while identifying crises as a step towards a paradigm shift, argue that under Kuhn's schema, crises need not effect a scientific revolution.

#### 5. *Kuhn Does not Provide a Cogent Explanation for Scientific Revolutions*

In concluding this section, we must note that many economists question whether Kuhn's theory provides an adequate explanation as to why scientific revolutions occur. Blaug, for instance, cites as one of the weaknesses of Kuhn's model its failure "to provide insight as to why 'paradigms' are ever replaced" [Blaug, 1976, page 157]. Likewise, Solo maintains that "Kuhn does not explain" why "a shattering transformation of the established paradigm" occurs [Solo, 1991, page 33].<sup>22</sup>

More specifically, others point out that Kuhn's model provides no clear indication as to how and when anomalies spur a crisis. Kuhn, Lofthouse argues, leaves the question as to why "only some anomalies cause crisis" unanswered [Lofthouse, 1973, pages 412-413]. Foster-Carter likewise maintains that

All we can say, on Kuhn's account, is that for a time there is confidence in a paradigm, such that apparent anomalies are cheerfully postponed for future puzzle-solving. . . whereas at a later stage there is decreasing confidence that anomalies will constitute puzzles ever soluble within the paradigm. [Foster-Carter, 1976, page 169]

#### C. NON-KUHNIAN CAUSES OF REVOLUTIONS IN ECONOMICS

As we just laid out, many economists see scientific revolutions, under Kuhn's schema, as spurred by factors arising within a scientific community: namely, crises borne out of anomalies generated by the practice of normal science.<sup>23</sup> However, in assessing Kuhn's applicability to economics, economists contend that revolutions in economics are not (solely) an outgrowth of activity within the discipline itself. Events

outside the realm of "economics proper," they argue, play a key role in effecting fundamental change in economics.

While agreeing that anomalies engender scientific revolutions in economics, many contend that, very often in economics, anomalies emerge for reasons lying outside the bounds of the practice of normal science. Kunin and Weaver do not deny that anomalies may arise from the practice of economic normal science [Kunin and Weaver, 1971, page 395]. They, however, contend that changes in the subject matter under investigation provide another source for anomalies in economics. Kunin and Weaver point out that Kuhn's explanation presupposes that the "nature" which scientists study is "a historically unchanging one."<sup>24</sup> While Kunin and Weaver allow that such an assumption may be reasonable when examining the dynamics of paradigm change in the natural sciences, they point out that social scientists, in general, and economists, in specific, study "a universe which changes historically." As a result, "Normal economic research is vulnerable to anomalies not only from the internal dynamics of the scientific enterprise itself but also from sources external to the science as such -- stemming from changes in the economic universe being studied" [Kunin and Weaver, 1971, page 395]. Thus, changes in the "universe" under study, *in addition to* the research process itself, may produce anomalies which lead to a paradigm change.

Worland concedes that Kuhnian anomalies (understood as "instances of disconfirmation") have arisen in the history of economics (e.g., the water-and-diamonds paradox, the Giffen good case and Keynesian underemployment). However, he observes, none of these anomalies has engendered crisis; instead, each has been incorporated into economics' regnant paradigm as a special case. More generally, he implies that Kuhnian anomalies are unlikely ever to spur crisis (and thus scientific revolution) in economics by virtue of the Duhem-Quine thesis. Any potential instance of disconfirmation may be explained away as a case in which the antecedent conditions

required to test a theory, not the theory itself, are disconfirmed [Worland, 1972, pages 277-278].

However, while Kuhnian anomalies are unlikely to engender a crisis in economics, Worland contends that "policy anomalies" (inconsistencies between different economic policies) may. Policy anomalies arise, Worland explains, when policy makers implement policy on the basis of a theory at wide variance to the real world. Given the divergence between reality and theory, policy makers will find themselves without a means by which to coordinate policymaking. As a result, policy is devised and implemented on an ad hoc basis. The ad hoc policies, however, need not be consistent with one another, and thus policy makers may find themselves constantly having to re-adjust one policy because other ad hoc policies counteract its effect. These readjustments, in turn, are likely to work at cross purposes with still other policies, requiring further tinkering. In the end, the mounting policy anomalies may, Worland observes, result in "cumulative social disorder" and "crisis -- the practical counterpart of the kind of theoretical crisis which, according to Kuhn, is encountered by a developing science" [Worland, 1972, pages 278-279].

Similarly, Craufurd Goodwin observes that *ceteris paribus* conditions, as well as the inaccessibility of controlled experimentation, protect economists from the sorts of "crucial experiments" which Kuhn finds in the natural sciences.<sup>25</sup> Nonetheless, "major social or economic convulsions" such as the Great Depression have served as "*external* crucial experiments" [Goodwin, 1980, page 615, emphasis added]. Public outcry during these "experiments" has forced economists to recognize and come to grips with the anomalies whose existence they had previously denied. This recognition has in turn led to fundamental, though not revolutionary, change in economics.<sup>26,27</sup>

Some economists argue that factors other than anomalies provide the impetus for scientific revolution in economics. Johnson and Ley, while assenting that anomalies have played a role in inducing scientific revolution in economics, contend

that other factors may effect revolutions in the discipline. They cite three additional causes: "(1) emulation by economics of other currently fashionable disciplines, (2) changes in societal values when they become part of the economists' consciousness, and (3) major social and economic convulsions" [Johnson and Ley, 1990, pages 35-36].<sup>28</sup> Each of these external factors, they explain, impact upon an economic paradigm's purposive function and thus may result in a paradigm shift (i.e., a scientific revolution) [Johnson and Ley, 1990, pages 35-36].

Many economists point out that in the application of Kuhn's theory of scientific revolutions to economics, his framework needs to be augmented by an appreciation for the central role which ideology in society at large plays in revolutions in economics. As we noted, Johnson and Ley cite "changes in societal values" as one non-Kuhnian cause of scientific revolution in economics. Further, DeVroey, in applying Kuhn's model to the marginal revolution, argues that it is necessary to supplement the model with an understanding of the vital role played by politics in the revolution [DeVroey, 1975, page 416]. Similarly, Deane cites ideology as a key factor in the marginal revolution [Deane, 1978, page 109]. More generally, she contends that the success of a scientific revolution in economics, in contrast to Kuhn's schema,<sup>29</sup> often hinges upon the ideological desirability of switching paradigms: "The philosophical and ideological premises of an economic theory thus play an important role both in its initial acceptance and in its tenacity" [Deane, 1978, page xiii]. Along similar lines, Kunin and Weaver, in opposing Kuhn's explanation of revolutions with revolutions in economics, point out that "Ideas, in the social sciences at any rate, gain the general acceptance of the scientific community only when they address some widely recognized social phenomenon in terms congenial to the times" [Kunin and Weaver, 1971, page 396]. Finally, Burtt points out that social values play a significantly greater role in economics than in the natural sciences to which Kuhn's theory spoke:

Our purpose here is . . . to indicate some of the features of the process by which new theoretical systems have been formed in the past. In that process there is a fundamental difference between the natural sciences discussed by Kuhn and economics. Value judgments affect all social-science categories; concepts such as income, wealth, and well-being are open to different interpretations. These interpretations are not "given," but arise out of different viewpoints concerning social objectives. [Burt, 1972, page 282]

Reynolds highlights the importance of ideology and policy in explicating the modifications which must be made to Kuhn's model to make it applicable to economics.<sup>30</sup> In explaining crises, Kuhn, according to Reynolds, treated a discipline's paradigm as a unitary entity. However, Reynolds contends that in order to understand crises in economics, the economics paradigm must be treated as a trichotomous framework, composed of ideology, science and practice. According to him, crises in economics arise because the three elements, growing at different rates from one another, may in time become incompatible with one another.<sup>31</sup>

The process that Kuhn uses to explain the nature of scientific revolutions must be modified in its application to economics. The nature of the crisis must be modified. The three elements -- ideological, scientific and practice -- must maintain some minimum degree of compatibility. When these elements reach a degree of inconsistency that cannot be rationalized, the first step toward a revolution in economics has occurred. [Reynolds, 1976, page 31]

In sum, many economists take issue with what they see to be Kuhn's internalist explanation of scientific revolutions. Some charge that anomalies generated within the practice of normal economic science have not effected or are largely incapable of effecting fundamental change in economics. More generally, all the economists analyzed here contend that in contrast to their interpretation of Kuhn, scientific revolutions and crises in economics depend, to a significant degree, upon influences lying outside the realm of normal science or the bounds of economic science such as ideology, economic policy, social distress, and changes outside economics either in other disciplines or in the economy at large. Coats makes this general point explicit:

According to Kuhn, paradigm breakdowns occur as a result of conditions *internal* to the scientific community . . . Social scientists, however, are generally less insulated from society than their counterparts in the natural

sciences; hence, in explaining a crisis period in economics, due weight must also be given to external, extra-scientific or exogenous influences on the discipline. [Coats, 1977, page 6]

#### D. NO SCIENTIFIC REVOLUTIONS IN THE HISTORY OF ECONOMICS

Many economists also take issue with the notion that economics has ever undergone a Kuhnian scientific revolution and/or paradigm shift. In the main, they point to significant continuities in the history of economics as evidence that economics has never experienced a Kuhnian revolution.

##### 1. *Change in Economics is Less Discontinuous than in a Kuhnian Revolution*

As we saw in our discussion of economists' interpretations of scientific revolutions, many conceive of a Kuhnian scientific revolution as a complete break with the past. In large part, it is these economists who argue that change in economics is far less discontinuous than implied by Kuhn's theory of scientific revolutions. Many of these economists point out that in economics new paradigms, *contra* Kuhn, never wholly displace old ones:

Advances in economic thought have been brought on not only by jolts or "revolutions" but also by non-uniform accretions, without necessarily rejecting existing economic paradigms or fundamental postulates. Old and new "paradigms" continue to coexist in economics. For example, elements of mercantilist, classical, and neoclassical thought still play a role in the science, side by side with modern theories. Kuhn's proposition, therefore, does not yield a satisfactory explanation for the evolution of economic thought. [Karsten, 1973, page 402]

But economics, as everyone knows, does not proceed according to a series of successive paradigmatic shifts by virtue of which a Copernican or Einsteinian formulation comes totally to dominate the discipline. In economics, one scientific tradition does not completely replace another scientific tradition. [Breit, 1987, page 827]

Similarly, Goodwin, who contends that "one must struggle very hard to identify candidates for truly revolutionary episodes (in the Kuhnian sense)," affirms "Certainly, there were no . . . total gestalt switches as Kuhn perceived them in the physical sciences" [Goodwin, 1980, page 612].

Others, while allowing that economics may have undergone a revolution of some sort, make much the same point. Economics has never undergone a Kuhnian revolution whereby the new paradigm fully sweeps away the old one. Zinam, for instance, grants that economics may have undergone a revolution, if the term "is given a broader meaning" than implied by Kuhn, but affirms that economics has never undergone a Kuhnian revolution, "whereby the old paradigm is rendered obsolete" [Zinam, 1978, page 181]:

throughout history one can identify a mainstream of economic thought from Adam Smith until the present time. This main trunk was never completely interrupted by catastrophic revolutions envisioned by Kuhn. Yet it was not a very smooth evolutionary process of growth by mere accretion but rather a succession of restatements of the major paradigm. [Zinam, 1978, pages 161-162]

Likewise, Bronfenbrenner identifies three revolutions in economics history. None of them however, he contends, fits with Kuhn's notion of a scientific revolution because in each case change occurred via *accretion*, rather than by the complete and permanent displacement of the old with the new. The incorporation of new theories into the field (e.g., utility theory) left many old ones largely in tact (e.g., classical cost theory) [Bronfenbrenner, 1971, page 150]. Further, many formerly displaced theories have re-emerged later in the discipline's history. The theory of the just price, for example, has found new life in current-day incomes policies.<sup>32</sup>

We find still other economists, however, who maintain that even while revolutions in economics are not marked by complete discontinuity, Kuhn's model in modified form may be applicable to the profession's experience. Johnson and Ley, for instance, discern strands of continuity running throughout economics' history (e.g., the rationality assumption and deductive method) [Johnson and Ley, 1990, pages 139-144]. Nonetheless, they maintain that one may locate scientific revolutions, i.e., paradigm shifts, in economics' history employing an adjusted Kuhnian framework which takes explicit account of the purposive function (P-F) of economics' paradigms. Such a

model, they argue, allows one to detect paradigm shifts in economics by locating switches in the field's P-F (the necessary and sufficient condition for a paradigm shift). But, as we noted in our discussion of the definition of paradigm, the P-F understanding requires only that the field's P-F change; a new paradigm may share other paradigmatic elements in common with its predecessor. Thus, employing the modified Kuhnian framework, Johnson and Ley simultaneously locate both strands of continuity (e.g., deductive method and rationality assumption) as well as two scientific revolutions (i.e., P-F shifts) in economics' history (the emergence of neoclassical economics and the rise of Keynesian economics) [Johnson and Ley, 1990, pages 139-144, 119ff., and 132ff.].

Along similar lines, Deane maintains that economics has never undergone a "total paradigm switch" (the extreme understanding of Kuhn's concept) [Deane, 1978, pages xii-xiii], but does identify at least one paradigm shift, understood in the "narrower sense of Kuhn's concept," the marginal revolution [Deane, 1978, page 97].

## 2. *Economics has Been Dominated by One and Only One Paradigm*

Many economists contend that economics has not undergone any major scientific revolutions (at least since Adam Smith's publication of the *Wealth of Nations* in 1776). Several argue that economics' dominant paradigm has been called into serious question over the course of the field's history. Nevertheless, they maintain that despite these challenges, economics has been dominated by one and only one paradigm, which has yet to be displaced:

From this standpoint economics may be regarded as more "uniformitarian" than the natural sciences, for despite persistent and often penetrating criticism by a stream of heterodox writers (e.g. socialists, evolutionists, institutionalists) it has been dominated throughout its history by a single paradigm . . . [Coats, 1969, page 292]

Similarly, Gordon, while agreeing that economics has had major "rebellions," led by groups such as the "historicists of the nineteenth and the institutionalists of the early twentieth centuries," "economics has never had a major revolution; its basic . . . model



has never been replaced" [Gordon, 1965, page 124]. Reynolds, likewise, cites two serious challenges to economics' orthodox paradigm (one mounted by Marx, the other by Veblen). However, in each case, Reynolds contends economics was able to preserve the one and only paradigm which has dominated economics since the mid-1700's [Reynolds, 1976, pages 30 and 32]. Zinam identifies several "dissenting" schools which have challenged orthodox economic theory, but maintains that none of them displaced economics' orthodox paradigm. These schools were either "absorbed" into the mainstream or continue to co-exist along side of it [Zinam, 1981, pages 72-73]. As we have seen, Zinam contends that economics has never undergone a Kuhnian scientific revolution whereby "an old paradigm is rendered obsolete and replaced by a new one" [Zinam, 1978, page 163].

Economists who contend that economics has not undergone a paradigm shift since the mid 1700s, however, disagree as to what economics' long-standing paradigm has been. According to Coats, the paradigm is "the theory of economic equilibrium via the market mechanism," whereas for Gordon, it is "Smith's postulate of the maximizing individual in a relatively free market and the successful application of this postulate" [Coats, 1969, page 292; Gordon, 1965, page 123]. Reynolds, on the other hand, includes an analog of the paradigm Gordon identifies ("maximizing individuals in a relatively free market") as only one of five elements which comprise the conceptual framework which has dominated economics since the mid-1700s. Reynolds cites as the other four elements, "(2) private property, (3) acquisitiveness, (4) the work ethic, [and] (5) the mechanical analogy" [Reynolds, 1976, pages 28-29].

Still again, Routh, who contends like Gordon that "the paradigm which provides the inner framework for economic thought has not changed since the seventeenth century" [Routh, 1989, page 27], provides yet another description of economics' orthodox paradigm: a "set of useful tools" along with a commitment to a value-free economics:

What the Kuhnians call a paradigm consists, in the case of economics, of a set of useful tools that are essential if a beginning is to be made to the handling of economic phenomena: the initial division into demand and supply, the intermediary of price, the elasticities that determine the shape of the demand and supply curves, marginalism as the operational element in elasticity and maximisation, factor substitution as the medium for optimisation of production, product substitution as that for the optimisation of consumer satisfaction. As for political bias: the economic-technician should be no more concerned with moral values than the motor mechanic with the route of the but he is required to repair. As soon as moral values obtrude, he has ceased to be a technician and has become politician, propagandist, moralist, preacher, interfering with the prerogative of the sovereign consumer. [Routh, 1989, pages 27-28]

Canterbery and Burkhardt also deny the existence of a paradigm shift in economics in the last 200 years and ally economics' orthodox paradigm with Adam Smith. They, however, identify the "great truth" upon which Smith's work is premised as "the self-regulating nature of the market" [Canterbery and Burkhardt, 1983, page 22-23]. Finally, Zinam, like many of the economists discussed here, maintains that "throughout history one can identify a mainstream of economic thought from Adam Smith until the present time," which was "never completely interrupted by catastrophic revolutions envisioned by Kuhn" [Zinam, 1978, pages 161-162]. But, as we saw in our discussion of paradigms in economics, Zinam provides yet another accounting of economics' orthodox paradigm (the classical/neoclassical paradigm) [Zinam, 1978, page 171].

### 3. *Explanations as to Why Economics Has Not Undergone a Scientific Revolution/Paradigm Shift*

In addition to identifying different paradigms, economists also differ in the reasons they offer as to why economics' central paradigm has not been displaced in over 200 years.

Coats attributes the "less rigid and compelling" nature of theories in economics as compared with the natural sciences as the reason why "the structure of scientific revolutions is much less readily discernible in economics than in the natural sciences" [Coats, 1969, page 293].

According to Zinam, numerous differences between the natural and social sciences help to explain why economics has not undergone a Kuhnian scientific revolution. The greater complexity of social science analysis and more ambiguous relationships among economic phenomena significantly reduce economists' ability to arrive at clear-cut results. Further, stronger emotional and cultural ties to theory hinder paradigm change in economics, as does the greater power wielded by the powers that be in society at large. These differences, Zinam asserts, provide "at least a partial explanation of why Kuhn's catastrophic scientific revolutions are not registered in economic theory" [Zinam, 1978, pages 163-165].

Reynolds attributes the lack of a revolution primarily to two factors. First, despite pressures for change exerted by such factors as social immobility, adequate "escapements" -- such as population growth and abundant natural resources -- have allowed economics' conceptual framework to survive, even in the face of incompatibilities among its elements. Second, economists have been able to make technical adjustments to its framework that have successfully averted impending revolution [Reynolds, 1976, pages 31-32].

As Routh sees it, economics has yet to undergo a paradigm shift, despite a widening gap between economic theory and reality because the discipline founds its work not upon empirical reality, but rather upon the "imagination." Further, because the field is dominated by academics, and not those who must apply economic principles, the problems which the divergence between theory and reality present for the application of economics principles have mattered little:

the environment has gone through various transformations; the thought [in economics], in its methodology and ideology, has changed hardly at all. This is possible because, as designed by Petty, it was arrived at not empirically but by imagination. It has been used not to explain, but to explain away. Economists have been and remain largely a teaching order; when they enter government or business or field research in the UDCs, their tools become an encumbrance and are soon thrown away. But as long as the orthodox creed remains recognised by universities and professional examining boards, teachers of economics are under no

pressure to change. They are not seriously engaged in attempting to understand society, nor are they paid to do so. [Routh, 1975, page 295]

Many of the foregoing explanations for the lack of scientific revolutions in economics (especially Routh's and Reynolds's) imply that the regnant economics paradigm has eluded displacement despite all its deficiencies. Gordon, however, provides a wholly opposite accounting as to why economics has not undergone a major revolution. Rather than attributing the longevity of Smith's postulate as economics paradigm to the sociological power wielded by the powers that be in economics, the lack of precise testing or the availability of "escapements," Gordon asserts that the absence of a major revolution in nearly 200 years is a "tribute to the supremacy of purely positivistic intellectual forces" over "passion":

Since economic theory has obvious connections with economic policies over which economists' passions are so easily aroused and considering that these policies have been so vigorously debated by economists past and present, it is a tribute to the supremacy of purely positivistic intellectual forces that such has been the case. [Gordon, 1965, page 124]

#### 4. *Summary*

In summary, among many economists finding no scientific revolution or paradigm shift in economics, we identify a common thread: the contention that economics manifests substantially greater continuity than had it experienced a scientific revolution and/or paradigm shift. At the same time, however, we find significant disagreements. While some completely dismiss Kuhn's notion of scientific revolution by virtue of the continuities they locate, others identify modifications or alternative interpretations of Kuhn's thesis that render it applicable to economics and allow one to identify scientific revolutions in economics' past. Further, among those who identify the continuity of a single paradigm running throughout economics' history, there is disagreement as to what that paradigm is, and why it has never been displaced.

E. THE APPLICABILITY OF KUHN'S THEORY OF SCIENTIFIC REVOLUTIONS AND THE DIFFERENCES BETWEEN THE NATURAL SCIENCES AND ECONOMICS

Underlying much of the discussion concerning the applicability of Kuhn's model of scientific revolutions is a recognition of the importance of the differences between the natural sciences on the one hand, and economics and the social sciences on the other.

As to those who highlight differences in the causes of scientific revolutions between the natural sciences and economics, we may list several. Kunin and Weaver, for instance, point out that due to the greater changefulness of the phenomena that social scientists study as compared with the natural sciences, Kuhn's model must be modified to incorporate the role which changes in socio-economic reality play in effecting scientific revolutions. They warn against the "mechanical and uncritical attempt to transfer the Kuhnian apparatus to the arena of the social sciences," including economics. Given the differences between the natural and social sciences, they assert

One would expect at least a cursory examination of the different terrains investigated by these groups of disciplines before assuming that the same conceptual apparatus would yield results of equal pertinence in areas as different as the physical and social sciences. [Kunin and Weaver, 1971, page 392]

Reynolds echoes Kunin and Weaver's note of caution.<sup>33</sup> Similarly, Worland, in arguing that policy, not Kuhnian, anomalies spur crises in economics, highlights the distinction between science and art, under whose rubric he includes economic policy making.<sup>34</sup> And, as we saw, Coats points to the lesser insularity of the social, as compared to the natural, sciences as the reason why greater account needs to be taken of conditions external to the scientific community, while Burt contends that values play a significantly greater role in economics than the physical sciences.

Coats, also as we have seen, cites the lesser precision of economic theories, compared with their natural science counterparts as an explanation as to why it is more difficult to discern scientific revolutions in economics. And, also as we noted, Zinam lays out a raft of differences between economics and the natural sciences to account for

the lack of a Kuhnian revolution in economics' history [Zinam, 1978, pages 163-165]. Similarly, Breit prefaces his assertion that economics has not undergone complete paradigm shifts by pointing out that

Kuhn did not include the social sciences in his analysis. His conclusions were informed by his study of the natural sciences in which a Copernican revolution in astronomy had completely replaced the Ptolemaic view of the universe; in which Newtonian absolutes largely had been replaced by Einsteinian relativity. [Breit, 1987, page 827]

However, while many economists link differences between the natural and social sciences with difficulties in applying Kuhn's theory of scientific revolutions to economics, some economists intimate that the adjustments they make to Kuhn's schema to render it applicable to economics would facilitate a greater understanding of scientific revolutions in the natural sciences as well. Bronfenbrenner, though diffidently, speculates that his own crude dialectic may fit better not only with economics, but with all sciences.<sup>35</sup> And, Kunin and Weaver maintain that taking account of the role of factors external to a discipline would strengthen the ability of Kuhn's model to explain the nature of change in the natural, as well as the social, sciences.<sup>36</sup> Similarly, Wisman questions not simply the applicability of Kuhn's internalist account of science to economics, but to science in general.<sup>37</sup>

Further, we find at least one economist who contends that differences between the natural sciences and economics increases, rather than decreases, the applicability of Kuhn's theory of scientific revolutions to economics. Dow maintains that the greater changefulness of economic phenomena heightens the relevance of Kuhn's theory to economics because it increases the likelihood that economics will undergo a scientific revolution.<sup>38,39</sup>

#### F. MARGINAL (UTILITY) REVOLUTION

Turning our examination away from broad questions of Kuhn's applicability to economics, we shift our attention to economists' applications of Kuhn's theory of

scientific revolutions, as well as their assessments of the applicability of that theory, to the two most often cited revolutions in economics history: the marginal (utility) and the Keynesian revolutions.<sup>40</sup> In particular, we are interested in addressing four interrelated matters: (1) economists' assessments as to the applicability of Kuhn's theory of scientific revolutions to the marginal (utility) and/or the Keynesian revolution, (2) economists' understandings of the causes of each revolution, (3) economists' interpretations as to the nature and extent of the changes effected by each revolution, and (4) economists' determinations as to whether (or not) the marginal (utility) and/or the Keynesian revolutions constituted a scientific revolution.

We analyze economists' interpretations of the marginal (utility) revolution in the present section and turn to consideration of their understandings of the Keynesian revolution in the next. In the case of each revolution, our discussion consists of an analysis of individual economists' interpretations of that revolution, followed by an examination of the major lines of disagreement, especially as to that revolution's status as a scientific revolution and the changes it effected in economics.

As we noted above, we begin with analyses of individual economists' interpretations of the marginal (utility) revolution. Arranging the discussion alphabetically, we begin with our interpretation of Alain Alcouffe's account of that revolution in France.

#### 1. *Alain Alcouffe*

Alcouffe questions whether a marginal revolution ala Kuhn occurred in late nineteenth century France. He finds little indication that the orthodox classical school in France was locked in a Kuhnian-type paradigm battle with an emergent marginalism.

To judge by the French example, the term marginal revolution in the Kuhnian sense is misleading; most importantly, it was not marginalism which was competing with classical economics; the revolution went in fact largely unnoticed by contemporaries . . . [Alcouffe, 1989, page 335]

One reason why marginalism made such a minimal impression upon orthodox French economists was that the former's theory of value differed little from the orthodoxy's Sayian theory of value in which utility and scarcity already had a place.<sup>41</sup> Further, while in broad brush, marginalists can be seen as favoring (or at least not opposing) the use of mathematics in economic analysis and the classicals as questioning math's role, neither side was, as a Kuhnian interpreter might expect, unified as to its position on the matter [Alcouffe, 1989, pages 341-343].<sup>42</sup>

The crux of Alcouffe's argument that there was no marginalist revolution ala Kuhn in France boils down to demonstrating that "it was not marginalism which was competing with classical economics." This line of reasoning, however, begs the question: then what *was* competing with classical economics -- if not marginalism. Alcouffe answers: the historical school.<sup>43</sup> Chiefly, the historical and classical schools opposed one another on the question as to what should be the proper scope of political economy. The latter held that economists *qua* scientists' concern should be with the examination of immutable natural laws which existed prior to and despite any action taken on the part of law makers [Alcouffe, 1989, page 336]. The historical school, on the other hand, maintained that economics dealt exclusively with legislated laws, which "as such . . . can be amended" [Alcouffe, 1989, page 337].

Beyond establishing that a conflict existed between the classical and historical schools, Alcouffe contends that the historical school had a hand in moving economics from Say's definition of political economy as the examination of the formation, distribution and consumption of wealth, toward Robbins' description of it as "the science of 'allocating decisions about scarce resources'" [Alcouffe, 1989, pages 336-337]. The school assisted in the redefinition by conceiving of economics as concerned with that which men (legislators) can change, rather than that over which men have no control.<sup>44</sup>



Given the admitted conflict between the historical school and the role which Alcouffe sees the school playing in redefining political economy, one might expect that Alcouffe, while failing to see a marginalist revolution in economics, might find evidence for an historicist (assisted) revolution ala Kuhn. Alcouffe, however, never addresses the question as to whether (or not) the existence of the conflict between the historical school and the role the school played in redefining political economy constitutes evidence for a Kuhnian revolution. He seeks only to argue that a *marginalist* revolution ala Kuhn did not take place [Alcouffe, 1989, pages 335ff.].

## 2. *Roger Backhouse*

Backhouse assents that events prior to the Jevonian revolution in England resembled, at least in some respects, a Kuhnian crisis. Faith in the orthodoxy had crumbled; consensus had evaporated.<sup>45</sup> He further maintains (1) that Jevons' economics did displace Classical economics in Britain<sup>46</sup> and (2) that, despite the long time required for the transition, "it seems reasonable to refer to the change as revolutionary" [Backhouse, 1985, page 124].<sup>47</sup> Despite this, he stops short of certifying the Jevonian revolution as a Kuhnian revolution [Backhouse, 1985, pages 123ff.].<sup>48</sup>

The situations in the other two countries traditionally associated with the marginal revolution (Menger's Austria and Walras' France) cannot, according to Backhouse, be properly be referred to as revolutionary -- even in the generic (not necessarily Kuhnian) sense of the term. Unlike Jevons, Menger and Walras were working *within*, not in *opposition to*, the prevailing orthodoxy:

The situation in Austria and France was very different. Walras was working in a long French tradition which stressed the role of demand and utility. Neither was Menger rebelling against any established orthodoxy. [Backhouse, 1985, page 125]

Looking at economics as a whole (not simply within a particular country), Backhouse identifies "the 1870s . . . as marking a decisive turning point in the

development of economic analysis" [Backhouse, 1985, page 123]. With publications by Jevons, Menger and Walras, "a theory of resource allocation based on marginal analysis" moved to the fore of economics and "has remained at the centre of economic theory" ever since. The period also marks the beginning of

the development and use of a system of economic equilibrium, in which maximizing behaviour on the part of individuals is brought into some sort of equilibrium through markets. [Backhouse, 1985, page 123]

Backhouse, however, makes these points not to bolster a claim that the "marginal revolution" was truly revolutionary, let alone a claim that the "revolution" was a *Kuhnian* scientific revolution. The points, instead, serve as qualifications to Backhouse's overriding thesis that the marginal revolution was the "revolution that wasn't" [Backhouse, 1985, page 123]. It wasn't a revolution, despite its palpable impact, because it did little to change the corpus of economic theory. The theory of value forwarded by the "revolutionaries" (marginal utility theory) was not that revolutionary. Economists had discovered the theory forty years earlier and, had already made significant strides toward heightening the importance of demand in their theories of value.<sup>49</sup> Further, while the use of marginalist techniques may have refined and facilitated economists' work in such subfields as trade and monetary theory, the "revolution" had minimal substantive impact:

An even stronger case can be made as regards other branches of economics. In the theory of trade, not only has Mill's theory of reciprocal demand remained an important part of the pure theory of trade, but so too has the Ricardian theory of comparative advantage. Though the application of marginal analysis and the use of mathematics contributed to the theory's being stated more precisely, and enabled it to be developed more fully, there was no discontinuity in the development of trade theory. Similar remarks can be made concerning monetary economics and the theory of the cycle. [Backhouse, 1985, page 123]

Thus, that certain theories and "line[s] of inquiry" gained a central importance they had not enjoyed prior to the 1870s does not constitute the marginal revolution as "revolutionary," but only a "turning point" [Backhouse, 1985, pages 123-124].

Directly speaking to Kuhn's applicability to the "revolution," Backhouse maintains that "the marginal revolution was not a scientific revolution in Kuhn's sense" [Backhouse, 1985, page 8]. Backhouse's reasons appear two-fold. First, the 1870s are disqualified as revolutionary for reasons already stated: marginalist theories had already been anticipated by past economists and were, in many cases, already part of the mainstream. Second, Backhouse questions Kuhn's relevance to the marginal revolution because economics lacked a "scientific community" which Kuhn's theories presume. Economics did not consist of a single, well-connected community of scholars who shared a common background and kept in close contact with one another. Instead, the "revolution's" three major figures worked essentially in isolation from one another [Backhouse, 1985, page 124]. "It was only after Walras and Jevons discovered that they had independently developed similar theories, that economic theory began to become more cosmopolitan, and even then this was a slow process" [Backhouse, 1985, page 124].<sup>50</sup>

### 3. *Jörg Baumberger*

Baumberger indicates that the "neoclassical 'revolution'" resulted in fundamental, not simply incremental, change in the propositions guiding the mainstream.<sup>51</sup> While he does not detail the nature of that change, he does oppose what had been the discipline's orthodoxy (Classical political economy) to what is now its current mainstream (neoclassical economics). The latter was not the refinement of the former, nor even its complement. Instead, Classical political economy and its current descendants are at odds with neoclassical economics.<sup>52</sup> Given this however, Baumberger strongly avers that the "revolution" did not constitute a Kuhnian scientific revolution. As we saw, Baumberger contends that such a revolution amounts to substantially more than a proposition change (even a major one). A Kuhnian revolution requires that a discipline's practitioners had worked under a *single* paradigm before the

event, and under a new and different *one* afterwards. Such, however, was not the case with the rise of neoclassical economics. While there may have been a change in the mainstream, the discipline as a whole, both before and after, was not guided by one and only one paradigm. Reviewing another economist's<sup>53</sup> line of reasoning, Baumberger asserts, "These points fail to show (and no one can show it) that *before* and *after* the period of the rise of neoclassicism, economics was operating in a normal science puzzle-solving way under a unique paradigm" [Baumberger, 1977, page 12]. The Classical tradition that guided much of economics before the "revolution," was too fluid and heterogeneous to count as a rigid Kuhnian paradigm [Baumberger, 1977, pages 9-10]. Further, after the rise of neoclassicism, the Classical tradition lived on in economics in many different schools (including Marxism, institutionalism, and post-Keynesianism).<sup>54</sup> Those schools continue to exist to this day in economics and influence *all* economists (neoclassicists included). The schools persist in their criticisms of the mainstream and the mainstream continues to react (often defensively) to those censures.<sup>55</sup> If the emergence of neoclassical economics produced any revolution,<sup>56</sup> it effected one that persists to the present; that is, economics has been in a state of perpetual revolution since the rise of neoclassical economics:

there has been less than complete peace in economics ever since the neo-classical 'revolution.' By any standards, the classical paradigm (if we may call the content of the tradition by that name) has been around all the time, and the battle is far from finished. If there was something like a revolution a hundred years ago, the intervening century clearly was not sufficient to consolidate it. [Baumberger, 1977, page 10]

But, such a revolution in permanence is not a Kuhnian scientific revolution, which must, by definition, come to an end:

What we have been witnessing in economics also fits poorly into the mold of a Kuhnian revolution. A revolution that is never completed may well be a permanent one in a colloquial sense, but it certainly is not a Kuhnian one. [Baumberger, 1977, pages 10-11]

As we have also seen, Baumberger finds that scientific revolutions necessarily entail that a discipline's members behave in a manner substantively different from the

way in which they behaved before and after the revolution. Baumberger, however, expresses doubts that economists' behavior differed in any substantive way during the revolution from the ways they acted before or after the rise of neoclassical economics [Baumberger, 1977, page 12].

#### 4. *Cristina Bicchieri*

Bicchieri censures those who associate the marginalist revolution with a change in economics' theory of value. Such an understanding, she charges, is predicated upon a myopic conception of the revolution. Taking a longer view, Bicchieri argues that though slower in coming, the advent of general equilibrium analysis constituted a much more fundamental change wrought by the revolution.<sup>57</sup>

As to Kuhn's relevance, Bicchieri never addresses the question as to whether the "marginalist revolution" constituted a scientific revolution *per se*. She does, however, assess the applicability of Kuhn's understanding of scientific change and progress to economics' transition from classical to marginalist economics. Broadly, she finds that the change comports with Kuhn's model. Consistent with his conception of scientific progress, the revolution entailed both gains as well as losses. On the minus side, the revolution cast aside, without replacing, a very viable Classical theory of growth:

The marginalist revolution, however, is not a traditional case of growth of knowledge, since to a greater precision and generality in the above sense there corresponded a reduction in scope. An important example is the abandonment of the theory of economic growth. . . . the loss of a theory of growth must be considered no small sacrifice for economic theory, as evidenced by the revival of interest in growth theory among neoclassical economists after 1950, as well as the continued prominence of the maintenance of a satisfactory rate of economic growth on the agendas of economic policymakers. [Bicchieri, 1989, page 251]

On the plus side, marginalist theory possessed greater generality than its predecessor in that

it offers explanations where the classics only stated tendencies (e.g., the case of equilibrium), it is potentially extendable to a larger class of phenomena than the old theory, and finally, it needs no restrictive

auxiliary hypotheses to obtain the results of classical models (albeit not in all cases). [Bicchieri, 1989, page 252-253]

Bicchieri's assessment implies that meaningful comparisons between marginalist and classical theory can be made. Here, too, is another sense in which Bicchieri finds that the marginalist revolution conforms with Kuhn's model of scientific progress. Comparison between these two economic theories is possible for much the same reason that Kuhn, according to Bicchieri, contends new and old scientific theories could be compared: the two share common areas of concern, and thus can be compared with one another in terms of their relative success in addressing those concerns. Given that so many associate Kuhn's theory of scientific change with the incommensurability of the old and the new paradigm, Bicchieri's contention that the commensurability of marginalist and classical theories comports with Kuhn's theory stands out.

One of those common areas, Bicchieri maintains, was with the formulation of a theory of value, which she allies with a theory of prices.<sup>58</sup> The marginalists' theory of value, Bicchieri concludes, possesses a greater measure of generality than its predecessor's theory. Her explanation: Marginalists preserved the classical's analysis of value. The former, however, subsumed its antecedent's analysis as a set of special cases within the context of a more general theory of value. Thus, for example, while acknowledging the validity of the classical labor theory of value under certain restrictive assumptions,<sup>59</sup> marginalists demonstrated that, without those assumptions, relative price was not -- as the classical theory proposed -- determined solely by the relative amount of labor used to produce a given good. Other factors needed to be taken account of (in particular, product demand and the shape of the production function) [Bicchieri, 1989, page 247].

As Bicchieri sees them, the marginalists crafted a theory of value which allowed for determinate solutions without recourse to the restrictive and often unrealistic assumptions required by classical analysis. So, for example, marginalists did not need

to assume, as Ricardo had to, that wages were governed solely by the level of subsistence in order to determine an equilibrium wage [Bicchieri, 1989, page 248].

Another indication of marginalism's greater generality was that it broadened the application of Ricardo's theory of rent. Ricardo had applied his theory only to land, and, still further, only to those instances where fertility declines as additional land is brought into cultivation. The marginalists, however, in their formulation of the theory of marginal productivity, expanded the reach of Ricardo's theory to the analysis of all factors of production.<sup>60</sup> Still further, marginalists could determine rent prices even in instances in which additional lands brought into cultivation are no less fertile than land already in use [Bicchieri, 1989, pages 249-250].<sup>61</sup>

Thus, Bicchieri finds it possible to compare the two theories on the basis of common areas of concern. She focuses, in particular, upon their common concern with price determination and concludes that marginalism possesses a greater degree of generality than classical doctrine in those areas.<sup>62</sup>

Bicchieri, however, finds the marginalist revolution's fit with Kuhn's model as far from perfect. Contrary to her interpretation of Kuhn's schema, marginalism did not succeed at solving most of the empirical problems which classical doctrine had solved.<sup>63</sup>

As to the causes of the revolution, Bicchieri finds that while numerous anomalies surrounded classical theory, not all of them endangered its existence. On the one hand, evidence running counter to the expectations of the school's Malthusian population theory did not pose a threat because "a population doctrine had ceased to be an essential part of what economics sought to explain" [Bicchieri, 1989, page 243]. On the other hand, empirical anomalies plaguing classical subsistence wage theory, along with theoretical ones besetting its wages fund doctrine, did play a role in classical theory's downfall. The primary problems facing the wages fund theory ((1) the implausibility that "a single wage fund" can account for "the relative wages of

heterogeneous types of labor" and (2) its ignoring the very real possibility of a capitalist's substituting between capital and labor) did present particular difficulty for classical doctrine. These anomalies threatened classical economics because the emergent marginalist economics provided cogent solutions for them [Bicchieri, 1989, pages 241-243]. Bicchieri does not indicate whether or not the role she sees anomalies playing in the marginal revolution comports with Kuhn's understanding.<sup>64</sup>

##### 5. *R.D. Collison Black*

Black finds Kuhn's model of scientific revolutions inadequate to take account of the circumstances surrounding the Jevonian revolution in England. Kuhn's model assumes either that a science is in possession of a paradigm *and* well insulated from the laity, or else lacks a paradigm *and* is significantly influenced by outside opinion and forces. British economics in the late 1800's fits neither of these descriptions. While the field was certainly in possession of a paradigm, it had not attained any significant degree of professionalization:

In the first place, Kuhn's concepts relate to a scientific community whose field of research is not generally accessible to the layman, and whose members essentially report to one another. Yet, as Professors Spengler and Eagly have argued, economics did not attain to this stage of professionalization until the post-1870 period. According to Kuhn, this would place the subject in a "pre-paradigm" phase; yet most of us would feel inclined to accept that classical political economy had established a paradigm.

In fact the state of economic thought in England from about 1850 to 1870 suggests that a discipline may very well have reached the stage of establishing a paradigm without being fully professionalized. [Black, 1972, pages 366-367]

##### 6. *Mark Blaug*

Economists, Blaug notes, were slow to accept marginal utility theory, as were historians of economic thought slow to recognize the profession's acceptance of it. This hesitancy, he agrees, suggests that the theory was anomalous to, and thus did not arise out of, classical economics. Given this, marginal utility theory's eventual



acceptance would imply a Kuhnian paradigm shift (presumably to a paradigm in which marginal utility theory did not constitute an anomaly) [Blaug, 1985, page 305].

Blaug, however, cites a number of difficulties with such an interpretation.

First, it is unclear as to what paradigm economics might have shifted:

Was it a new emphasis on demand rather than supply, on consumer utility rather than on production costs? Was it something as ambitious as a subjective theory of value, which was to supplant the objective labour-cost theories of the past? Was it rather the extension of the principle of maximization from business firms to households, making the consumer and not the entrepreneur the epitome of rational action? Was it perhaps the equimarginal principle, enshrined in the proportionality of marginal utilities to prices as the condition of consumer equilibrium? Was it instead, as Schumpeter liked to say, the explicit or implicit discovery of general equilibrium analysis? Or lastly, was it simply the first conscious recognition of constrained maximization as the archetype of all economic reasoning? [Blaug, 1985, page 305-306]

Further, Jevons, Menger and Walras (the economists traditionally credited with instigating the revolution), if advancing a new paradigm, were not advancing the same one. Menger set himself apart from Jevons and Walras in that he shunned the use of math and "the pure logic of extremum problems" and held a deep distrust of "all determinate theories of pricing and underlined discontinuities, uncertainties and bargaining around the market price." Further, Walras distinguishes himself from Jevons in that he, unlike the latter, integrated a utility-based demand analysis, a supply analysis predicated upon marginal productivity theory and a theory of market pricing into a general equilibrium framework [Blaug, 1985, page 306].

Despite noting these difficulties, Blaug both eliminates some possible candidates for the new paradigm and advances what he sees to be the most likely paradigm to which economics may have shifted in the late 1800's. First, he rules out utility theory. As he explains, in economics' progression from cardinal to ordinal utility and then to revealed preference, utility lost its significance. It was the adjective (marginal), not the noun (utility) that "proved important" [Blaug, 1985, page 306]. Nonetheless, Blaug also dismisses marginalism as the new paradigm because classical economists had

employed marginalist techniques as much as economists after the "revolution."<sup>65</sup> Instead, Blaug maintains that, if economics underwent a paradigm shift, it was a movement away from dynamic questions (such as growth) to more static ones, such as the pricing and allocation of a fixed set of resources:

If we are going to describe the last quarter of the 19th century as a period when economists developed a new "paradigm," the only defensible definition of that paradigm is the proposition that pricing and resource allocation with fixed supplies of the factors of production is *the* economic problem, largely or entirely dismissing all questions about changes in the quantity and quality of productive resources through time. [Blaug, 1985, pages 306-307]

Ironically, Blaug questions the revolutionary status of the paradigm shift on the same grounds that he cited as evidence for the shift in the first place: the "revolution's" considerable length:

Whether we label this shift to a new paradigm as a "revolution," given the fact that it took at least twenty to thirty years to complete and in some sense is still going on, is a matter of words. [Blaug, 1985, page 307]

In any case, Blaug, like Backhouse, questions the existence of a marginal revolution outside of England. His reasons are much the same. While in England there was widespread discontent with the Ricardian brand of classical economics, to which British economists adhered, economists outside of England, who followed Smith, had little difficulty with classical economics [Blaug, 1976, page 166].

## 7. *Martin Bronfenbrenner*

Bronfenbrenner identifies the "utility revolution" in the latter part of the nineteenth century as one of three revolutions in economics' history. He does not, however, consider the revolution to be a Kuhnian scientific revolution. He, instead, finds the revolution employing his own dialectical notions of economics' development. Under Bronfenbrenner's schema, Classical economics constituted the thesis. Numerous antitheses opposed this thesis: (1) criticisms from landowners against the Classical school's "class-disharmony implications," (in particular, the notion that the interests of

the renter class were at odds with those of other classes); (2) on the other end of the spectrum, censures from socialists and Marxists against the notion that a harmony existed between the worker and the capitalist; (3) utility economists' criticism that Classical economics neglected the demand side; (4) others' (including the historical school's) contention that the Classical school laid excessive dependence upon deductive methods, and (5) criticisms by those such as Carlyle against the school's underlying "materialistic or hedonistic psychology and philosophy" [Bronfenbrenner, 1971, pages 143-144].

According to Bronfenbrenner, a synthesis emerged out of the conflict between the Classical thesis and the various antitheses opposing it: neoclassical economics. Marshall, according to Bronfenbrenner, was the key architect of the synthesis. In particular, Marshall's theoretical framework synthesized classical real cost theory with one of Classical economics' major antitheses (utility theory) by founding his supply side conceptions on the former and notions of the demand side on the latter:

A second great synthesis came out of all this; it is known as neoclassicism. At the risk of offending some Continental economists, we might say that, like the classical school itself, neoclassicism was predominantly British. Its greatest name was Alfred Marshall; its great compendium of paradigms was Marshall's *Principles of Economics* (1890). The best-remembered synthetic feature of Marshall's theory is the "Marshallian scissors." The supply-side blade of the scissors is the classical real-cost theory of Ricardo, whom Marshall especially admired. It bases supply on costs, and cost on such "real" or "pain" elements as labor and the postponement of consumption. The demand-side blade of the Marshallian scissors, however, is a marginal-utility theory taken over more largely from Stanley Jevons than from any Austrian writer. But utility was there, along with real cost; so Marshall had synthesized the two antagonistic principles. [Bronfenbrenner, 1971, pages 144-145]

Thus, Classical economics was not, as Kuhn's "catastrophic" theory of revolutions would have it, completely and permanently replaced. Instead, economics developed by "accretion." Classical economics' theory of cost remained largely in tact on the supply side in the synthesis and was married with utility theory of the demand side.

Contrasting his own crude dialectical theory with Kuhn's theory of revolutions, Bronfenbrenner remarks:

The second difference between two dialectic structures is that, in the present case, important advances tend to be major *accretions* without any corresponding rejections of existing paradigms. Utility theories of value did not displace cost theories except in special cases of fixed supply of productive factors; more generally, utility and preference theory has taken over the demand side, leaving the supply side to cost. [Bronfenbrenner, 1971, page 150]

#### 8. *A.W. Coats*

Coats describes the marginal revolution as an "intellectual breakthrough" in economics' history and asserts that the changes that took place "may be regarded as revolutionary in their implications, if not in their novelty or in the speed of diffusion." The revolution produced a "major shift" in economics' focus away from "supply, production, and distribution" and towards "subjective factors," "demand and consumption." The "breakthrough" also systematized a large body of economics' theories,

including the elaboration and eventual completion of competitive price theory, the integration of value, production, and distribution theories, the refinement of economic logic, and the extension of mathematical modes of analysis. [Coats, 1972, page 304]

Coats leaves open the question as to whether the revolution left economics with a new theoretical system that was incommensurable with the old one. Such a determination, he notes, is largely a function of a given historian's interpretive framework:

Of course, no final answers to these questions can be given, not merely because there is no uniquely correct historical perspective but also because the historian's judgment is itself influenced by his philosophical and methodological preconceptions. [Coats, 1972, page 312]

However, we may infer from Coats's general contention that economics' basic paradigm, established in the latter 1700s, has never been displaced, that he does not regard the marginal revolution as a paradigm shift, at least at the discipline's broadest level. We may infer that for Coats, the marginal revolution left "the theory of

economic equilibrium via the market mechanism" undisturbed [Coats, 1969, page 292]. As we shall see, others (e.g., DeVroey and Jalladeau) cite the same sorts of changes (e.g., increased focus on subjectivity) as Coats does as major elements of a paradigm shift in economics. They, however, also specify economics' pre-marginal revolution paradigm along very different lines from Coats.

Not only does Coats imply that the marginal revolution was not a Kuhnian paradigm shift, he as well asserts that the revolution was not brought on, contrary to Kuhn's model of scientific revolutions, by an intellectual crisis:

There seems no reason to believe that the marginal revolution of the 1870's was the product of an acute sense of intellectual crisis; on the contrary, as Schumpeter observed, many of the cofounders' "fellow scientists felt no attachment to the old doctrines." [Coats, 1972, pages 310-311, quoting Schumpeter, 1952, page 570]

However, even though Coats questions whether Kuhn's theory of scientific revolution was borne out by economics' experience in the marginal revolution, he still regards the philosopher's model as a useful heuristic device for economists seeking to understand the nature of the revolution.<sup>66</sup>

#### 9. *Phyllis Deane*

Deane questions whether the marginal revolution constituted a paradigm shift in Kuhn's broader sense of the term: a total gestalt switch. To characterize the marginal revolution as a comprehensive change in economics' paradigm, she contends, is "to over-dramatise" the revolution. Along these lines, Deane argues that the revolution represented neither economics' "evasion" of pressing social concerns, nor the advent of "a genuinely scientific unified theory of economic behaviour" [Deane, 1978, pages 96-97].

She, like Blaug, questions whether those credited with founding the revolution (Jevons, Walras and Menger) shared a common paradigm. Menger, she notes, fits rather uncomfortably with marginalist notions. Further, Jevons' and Walras' influence

on economics, qua theorists, was limited. During his life, Jevons' reputation was largely as an empirical economist, and economists' accessibility to Walras was severely limited by his heavy reliance upon mathematics. Further, the changes which the revolution did effect were slow in coming [Deane, 1978, page 97].

She, however, allows that the revolution did bring about "a paradigm-shift in the narrower sense of Kuhn's concept," understood as a change in "criteria, exemplars and procedural rules," which in turn provided "new ways of formulating, ranking and tackling the critical unsolved problems on the academic research agenda" [Deane, 1978, pages 97-98]. Understood in this sense, the centerpiece of the marginal revolution's paradigm shift was "the application of marginal analysis" [Deane, 1978, page 98].<sup>67</sup>

Similar to Blaug, Deane acknowledges that "classical economists were thoroughly familiar" with marginal analysis. However, in contrast to Blaug who contends that economists employed marginalism as much before as after the revolution, Deane stresses that after the revolution the area to which economists *could* apply marginal analysis broadened. In addition to areas to which economists had applied the analysis before (production and distribution theory), they now found it practicable to apply it to value and exchange theory [Deane, 1978, page 98].<sup>68</sup> This, in part, may explain why she allies the paradigm shift with marginalist analysis and Blaug expressly rules out the analysis as economics' new paradigm.

Consistent with her definition of a paradigm shift "in the narrower sense," Deane indicates that marginalist analysis' broadened application induced a change in economics' scope as well as methodology. In particular, economists came to limit themselves almost exclusively to market analysis and focused upon abstract theory, while eschewing policy considerations. Similarly, marginalist analysis lent itself well to mathematical techniques and thus spurred the increased use of mathematics in economics [Deane, 1978, pages 98-100].

Deane finds evidence that two of the three factors which Kuhn identifies as inducing a paradigm shift played a role in the marginal revolution: (1) A methodological crisis brought on by classical economics' inability to handle "crucially important" problems. In this regard, Deane cites indications of growing dissatisfaction with classical economics. Significantly, she limits her discussion to the situation in England:<sup>69,70</sup>

The other recurrent methodological controversy which Neville Keynes effectively disposed of was the argument about whether inductive or deductive techniques were appropriate to political economy: that was an issue which had greatly exercised nineteenth-century economists who were anxious to establish the scientific credentials of their discipline, and its re-emergence could perhaps be regarded as evidence of a sharpening of the sense of professional insecurity which Kuhn finds characteristic of periods of methodological crisis. [Deane, 1978, pages 103-104]

and (2) The new paradigm possessed "superior quantitative precision."<sup>71</sup>

Deane does indicate that Kuhn's third criteria was also met: the new paradigm (neoclassical economics) was able to solve problems which had beleaguered the old (classical economics). However, she questions the importance of neoclassical economics' problem solving successes in inducing the shift. She observes that the new economics solved many of the old one's problems simply by defining them to lie outside economics' ken. Further, she contends that neoclassical economics' ideological appeal and ability to lend "scientific" legitimacy to the status quo played a significantly larger role in effecting the paradigm shift:

In the event, the fact that the neo-classical economists of the period up to 1914 were able to retain unimpaired the classical bias towards economic individualism and laissez-faire, and the ideological overtones which this gave to the policy conclusions deducible from their analyses, may have had more to do with the success of the neo-classical paradigm than its problem-solving qualities. The problems of value and distribution which had preoccupied the Ricardians were solved, or, more accurately, one might say swept under the carpet, by simple process of definition. The problems of growth were outside the effective range of marginal analysis and further consideration of them was consciously postponed. At the same time, the very jargon of pure economic theory, e.g. the notions of "rationality," or "perfect" competition, or an "optimum" allocation of resources, helped to accentuate its ideological overtones and to lend ostensible "scientific" support to a political *status*

*quo* which depended on accepting a philosophy of economic individualism and harmony. [Deane, 1978, page 101]

#### 10. *Michel DeVroey*

For DeVroey, the transition from classical to neoclassical economics most assuredly constituted a Kuhnian scientific revolution. The reason: the transition marked a paradigm change, that is, a change not simply in the answers economists gave, but in the questions asked and the premises upon which they based their analysis:

It seems clear from the above discussion that classical and neoclassical economics each constitute a coherent and specific paradigm. To repeat an earlier statement: They do not provide different answers to the same questions; they ask different questions. . . . The transition actually concerned really the premises of analysis, and, thus, it must be regarded as a scientific revolution a la Kuhn rather than as a scientific advance in a Popperian way through a process of criticism and falsification of existing laws or assumptions. [DeVroey, 1975, page 429]

As we saw earlier, DeVroey finds that the two paradigms differed along a number of lines: the object they envisioned for economics, the aim they posited for economic research, the institutional framework and unit of analysis shaping their study, the core of their theoretical structure, and their conceptions of value and profit [DeVroey, 1975, page 430].

The scientific revolution, DeVroey finds, began not in the latter part of the nineteenth century, but in the 1830s, almost immediately after Ricardo's articulation of his theoretical framework. During this time ("the negative or destructive phase of the revolution"), critics successfully removed the most ideologically objectionable aspects of Ricardian analysis. Then, after an "interregnum," the revolution entered into its "constructive phase" in the 1870's during which time the neoclassical paradigm was installed as the field's regnant paradigm. The lengthiness of the revolution does not, however, disqualify it from being a Kuhnian scientific revolution. For DeVroey, scientific revolutions are "processes," not "instantaneous events" [DeVroey, 1975, page 431]. What matters is not the length of the revolution, but instead, that the neoclassical



paradigm to which the revolution gave rise constituted a new, separable and fundamentally different paradigm as compared to the classical paradigm preceding it.

According to DeVroey, the neoclassical paradigm's rise was marked not so much by the heightened prominence of the theory of utility, as it was by the rise of subjectivism, the installation of "the individual and his needs" at the center of economic analysis and the "introduction" of marginal analysis into economics. Utility was only one form of the more general subjective frame of mind that came to shape economists' research and conceptions. In particular, it was not the rise of utility theory, but marginal analysis and subjectivism that marked the ascendancy of the neoclassical paradigm in economics:

Hence it is not utility theory in itself which is central for grasping the essentials of the neoclassical paradigm (and this specific theory has lost much of its prominence today). What was central are the categories on which it was built, namely, the definition of the economic process as a relationship between the individual and his needs, the absence of class consideration, the identification of economic behavior with an act of individual choice. The object of our attention must be the rise of subjectivism, a new social vision and set of methodological principles, rather than the particular form in which this subjectivist approach became embodied, that is, the theory of marginal utility. [DeVroey, 1975, page 432]

What spurred the revolution? DeVroey makes clear it was not the heightened professionalization of the field. Such an explanation, according to him, fails to take proper account of the fundamental differences between the two paradigms and "evades what appears to us as being the heart of the matter, namely: What was the rationale behind the sudden abandoning of the Ricardian concepts in the 1830s?" [DeVroey, 1975, page 433]. At bottom, he finds the revolution was spurred by an anomaly. That anomaly was not, however, any disagreement between the Classical paradigm and "fact," but instead, an incompatibility of the classical system's ideological implications with the interests of society's ruling class, the bourgeoisie: "The stumblingblock or anomaly within the classical paradigm lay in its political consequences" [DeVroey,

1975, page 435]. Particularly troubling was the inference which socialists drew from the paradigm's labor theory of value:

if one considered labor as the only source of value, it appeared logical that workers be entitled to receive the totality of the product of their work. Nothing justified an important portion of produce, namely profit, accruing to the capitalists. [DeVroey, 1975, page 434]

The emergent capitalist class, to whom the conclusion was anathema, sought to displace Classical economics (along with its emphasis upon class and class conflict) with a less politically disturbing brand of economics. The neoclassical paradigm provided that ideologically inoffensive economics and, thus, gained widespread acceptance and ultimately replaced the Classical paradigm. By shifting the focus from the social class to the individual, the new paradigm "eluded the dangerous topics of class interests . . ." [DeVroey, 1975, page 435].<sup>72</sup>

It is, however, open to question whether DeVroey would classify the anomaly spurring the revolution as a Kuhnian one. DeVroey defines anomalies simply as "puzzles which the existing paradigm cannot resolve" [DeVroey, 1975, page 420]. He does not, like so many, directly ally anomalies with empirical and/or theoretical difficulties -- as opposed to ideological ones. However, DeVroey never mentions Kuhn when discussing the causes of the revolution and only refers to the ideological tension as an anomaly at the article's close -- again drawing no direct connection with the philosopher. Most significantly, DeVroey explains at the outset that while he will frame his discussion in terms of Kuhnian analysis, the philosopher's schema will be "*supplemented* with the political connections which will be outlined hereafter" [DeVroey, 1975, page 416, emphasis added].<sup>73</sup> In that later discussion, DeVroey explains that Kuhn's understanding of the role of social factors affecting a discipline's development needs to be "*enlarged* in order to include factors relating to the power structure within society as a whole" [DeVroey, 1975, page 418, emphasis added].

Thus, a strong case can be made for DeVroey's understanding the anomaly which he identifies as non-Kuhnian.

DeVroey, however, at no point backs down from his contention that the marginal revolution was a *Kuhnian* scientific revolution. Again, the revolution led economists to change the questions they asked and the premises upon which they based answers to those new questions and, thus, constituted a scientific revolution ala Kuhn. That the revolution was spurred by causes outside the ken of Kuhn's framework does not invalidate its status as scientific revolution. The marginal revolution was a Kuhnian revolution -- though one whose understanding of which requires that we expand the realm of social factors beyond those Kuhn included.

#### 11. *Dudley Dillard*

Dillard identifies a revolution in economics in the late nineteenth century led by Alfred Marshall. As with each of the five revolutions Dillard finds in economics' history, he associates it with need for social reform. In the case of Marshall, the problem was the existence of poverty amidst prosperity.

Dillard indicates that Marshall's proposed solution to the poverty problem was the education of the poor and economic chivalry on the part of the business community. We find, however, no indication as to whether or not that objective was achieved [Dillard, 1978, pages 713-714]. Dillard characterizes the revolution as one of economics' "breakthroughs that result in far-reaching changes in the main body of economic theory" [Dillard, 1978, page 705]. He, however, provides no indication as to what effects the Marshall-led revolution had upon economic policy or theory, nor whether the revolution in any way comported with Kuhn's model of scientific revolutions.<sup>74</sup>

12. *Robert Ekelund and Robert Hébert*

Ekelund and Hébert allow that, employing Kuhn's theory of scientific revolutions, one might understand neoclassical economics as a new economics paradigm arising out of the decay of classical economics.<sup>75</sup> Alternatively, however, they contend that one could include classical and neoclassical economics under the umbrella of the same paradigm ("equilibrium economics").<sup>76</sup> While Ekelund and Hébert do not draw out the point, this line of reasoning implies that neoclassical economics added to or modified, rather than replaced, economics' prevailing paradigm. In general, however, Ekelund and Hébert are wary of employing Kuhn's schema because it begs a number of important questions, most notably: What counts as an (economics) paradigm?<sup>77</sup>

13. *Craufurd Goodwin*

According to Goodwin, the marginal revolution primarily impacted upon two spheres of economic theory: (1) the theory of price, where the individual's utility function gained greater prominence and (2) market analysis, where marginal techniques gained great importance.<sup>78</sup> Did the marginal revolution, with these changes, however, constitute a scientific revolution? It did, according to Goodwin, mark "a major change in the basic core principles upon which some or all the parts of the science operated" and "the changes were more fundamental than mere artifactual innovation in a protective belt" [Goodwin, 1980, page 616]. Yet, Goodwin asserts there was no Kuhnian revolution. In contrast to Kuhn's schema, economics did not undergo a "total gestalt switch" [Goodwin, 1980, page 612].<sup>79</sup> However, while explaining what change came out of the revolution, Goodwin provides no indication about what remained *unchanged*. His assertion thus begs many questions.

The revolution, Goodwin maintains, was motivated by an "external crucial experiment," which classical economics was failing badly. Confronted with such pressing socio-economic problems as doubts as to how income should be (fairly)

distributed, depression and unemployment, classical economics fell short at providing the guidance necessary to comprehend the meaning of those problems and determine the proper policies to deal with the difficulties. Thus, in response to mounting pressures (notably from Marxists and socialists), leading economists such as Jevons, Menger, Walras and Marshall forged an economics that both prescribed policy consonant with accepted practice and provided a compelling response to radical censures:

Acceptance of utility maximization by the marginalists was in accord with the political changes which had taken place in recent decades, while the precise exposition they were able to provide of a competitive market system afforded an effective response to radical critics in the important structural debates of the day. [Goodwin, 1980, page 615]

As we saw earlier, however, Goodwin differentiates such an external crucial experiment from the ones Kuhn envisions in the physical sciences. This crucial experiment arose not from within the discipline, but from the outside in response to public furor [Goodwin, 1980, pages 612 and 614].

#### 14. *Joel Jalladeau*

Jalladeau's interpretation of economics' move from classical to neoclassical economics closely parallels DeVroey's. Like DeVroey, Jalladeau affirms that the transition constituted a scientific revolution because neoclassical economics possessed/constituted a fundamentally different paradigm from Classical political economy. The shift from the old to the new paradigm did more than tinker with economics at the margin; it altered the field at its very core:

The subject of investigation shifted, and the transformation of "political economy" into "economics" was the sign. The nodal points were no longer the same. Classical political economy and neoclassical economics seem to present their own paradigmatic articulation. There was not only a modification of auxiliary hypotheses in the protective belt, but also a fundamentally new perspective in the realm of economic knowledge. There was scientific revolution. [Jalladeau, 1978, pages 597-598]

Jalladeau, similar to DeVroey, lays aside questions regarding the length of time required for the transition from Classical political economy to neoclassical economics in assessing its revolutionary status.<sup>80</sup>

He further enumerates numerous changes which the revolution effected in economics. Capital, defined in a very broad sense, held center stage in Classical political economy. Neoclassical economics, however, narrowed the conception of capital such that it held no greater importance than any other factor and replaced capital with price as the discipline's central analytical concept:

Attention "shifted increasingly to *price* which became the central construct of post-classical Theory. The behavior of participants in the economic process was viewed in terms of its relationships to price; and price data were considered to form a major input into the decision process of entrepreneurs and members of the general public" Walras searched for a mathematical explanation of the formation of market prices of commodities and productive services. He moved toward a fundamental reconstruction of theory and to the creation of a complete general equilibrium model of prices and exchange. [Jalladeau, 1978, pages 594-595, quoting Eagly, 1974, page 10]

Classical political economy had held to a labor theory of value based upon relations among social classes in a production setting. Neoclassical economics, however, promoted a fundamentally different theory of value in which exchange among individuals mediated value:

Breaking with the labor value theory, W.S. Jevons, Karl Menger, Walras, and their precursors founded exchange value on the marginal utility notion. They elaborated a subjective theory of value rooted in man. An interpretation of exchange value based upon the attitude of the consumer thereby concentrates attention on the individual situation and, from that, tends "to introduce a certain individualist or atomistic bias." This reveals a view of socioeconomic reality somewhat different from that of the classical writers inasmuch as for the latter the analysis of exchange value "necessarily started from those socio-economic conditions that shaped the class relations of society." [Jalladeau, 1978, page 595, quoting Dobb, 1973, page 168]

Likewise, as concerns distribution theory, neoclassical economics replaced Classical political economy's class-based theory of distribution with one predicated upon exchange and the productivity of classless factors of production.<sup>81</sup> "Classical political

economy and neoclassical economics appear as two distinct theoretical structures as far as determining exchange phenomena and income distribution is concerned" [Jalladeau, 1978, page 596].

The foregoing points to significant differences between Classical political economy and neoclassical economics. However, for Jalladeau, divergences in the two's purposes and scopes most clearly delineate the radical and discontinuous nature of the change and thus support the contention that the move marked a scientific revolution. First, as to changes in the field's purpose: Classical political economy centrally concerned itself with questions of economic growth. With the rise of neoclassical economics, however, "the interest shifts from growth to problems of efficiency and allocation of scarce resources to alternative uses" [Jalladeau, 1978, page 597]. "Viewed in this way," Jalladeau affirms, "the neoclassical system tends to detach itself radically from the classical structure" in that the "nodal point of this new analysis" departed from its predecessor.<sup>82</sup> It is, we will recall, this departure that distinguishes the transition as a scientific revolution for Jalladeau. He also notes that while Classical political economy preoccupied itself with policy questions, neoclassical economics slighted practical concerns in favor of the development and refinement of theory.

Second, as Jalladeau sees it, the rise of neoclassical economics was linked with a significant narrowing of the scope of economics. Questions of class, class conflict and social power figured prominently in Classical political theory and analysis. Neoclassical economics, however, ruled such broad socio-economic factors out of bounds for economists. Such matters were left for other fields, such as sociology, to tackle [Jalladeau, 1978, page 597]. Given neoclassicism's omissions, economics cannot be understood to have progressed continuously from lesser to greater generality. Rather, the field experienced a discontinuous transition from one to another

fundamentally different paradigm, which excluded significant aspects of the former.

That is, it underwent a scientific revolution:

The transition from classical to neoclassical economics is not merely explicable as the rational choice of T' over T because the former explains everything the latter did and more; rather, and above all, the transformation leaves outside the field of observation certain key concerns and concepts by calling them noneconomic. The real shift involves a revolution by omitting from the sphere of discourse critical questions posed by classical economics, that is, by excluding the relations between economy and society from the scope of analysis.

If we admit this interpretation of the structure of marginalist analysis, the hypothesis of discontinuity of the historical development of economics is corroborated. Classical and neoclassical economics are then susceptible to paradigmatic articulation; the transition between the two schools of thought constitutes a scientific revolution. [Jalladeau, 1978, pages 603-604]

Along these lines, Jalladeau asserts that it was never the intention of the new paradigm's founders to preserve the older paradigm and/or build upon its foundations.<sup>83</sup>

Jalladeau reviews a number of different interpretations as to what instigated the revolution. He, like DeVroey, dismisses Stigler's identification of the revolution with the professionalization of economics because, "this interpretation tends to obscure the differences between the classical and neoclassical theoretical systems" [Jalladeau, 1978, page 599]. Jalladeau however also lays aside DeVroey's notion that ideology lay at the heart of the impetus for change. In the end, Jalladeau simply puts aside the question as to whether forces inside and/or outside spurred the revolution.<sup>84</sup> He does note that "The marginalists did not want to extend classical economic thought . . . [b]ecause they regarded the classical theory as a failure . . ." [Jalladeau, 1978, page 601]. Jalladeau, however, leaves unanswered the reasons why the "marginalists" saw Classical political economy and its theory of value as a failure and why they founded neoclassical economics -- as opposed to any other possible alternative. But, most important, Jalladeau leaves *unasked* why the discipline as a whole (not simply the marginalists)



would abandon Classical political economy and adopt neoclassical economics in its place.

15. *L.E. Johnson and Robert D. Ley*

Economics did, according to Johnson and Ley, undergo a paradigm shift and thus, a scientific revolution in the latter part of the nineteenth century.<sup>85</sup> According to them, "By the 1870's the classical paradigm largely collapsed in the face of the 'marginal revolution' initiated by Menger, a German, and Jevons, an Englishman" [Johnson and Ley, 1990, page 121]. This revolution brought marginalists<sup>86</sup> to the fore in economics. It was, however, another twenty years -- with Alfred Marshall's work -- before the "real emergence" of economics' new paradigm: the neoclassical paradigm. Like Bronfenbrenner, Johnson and Ley note that Marshall "sought to reconcile elements of classical and marginalist thought" and pointed out that price was determined by both utility (as the marginalists held) as well as cost (as the Classical economists maintained) [Johnson and Ley, 1990, page 123]. However, Marshall's integration of utility and cost theory does not hold the same significance for Johnson and Ley as it did for Bronfenbrenner. They, unlike Bronfenbrenner, do not conclude that neoclassical economics constituted (simply) a synthesis (dialectical or otherwise). Given that economists' adoption of the neoclassical paradigm entailed the replacement of Classical economics' purposive function, focusing upon "total social welfare," with a new P-F, centering its attention upon "individual subjective satisfaction" and allocative efficiency, it constituted a paradigm shift and thus, for Johnson and Ley, a scientific revolution:<sup>87</sup>

As much as anything, the seizing of these alternatives and the development of a new paradigm had to wait for the emergence of a new P-F. In the case of neoclassical economics, the paradigm shift involved a rejection of total social welfare measured by the wealth of the nation as the concern of the economics profession, and its replacement with a concern for the efficient allocation of resources and individual subjective satisfaction as the focus of inquiry. [Johnson and Ley, 1990, page 121]

Neoclassical economics did not abandon all elements of its antecedent. The neoclassical paradigm, for example, preserved the former's "assumption of rational, self-interested behavior" [Johnson and Ley, 1990, page 119], and preference for the deductive method [Johnson and Ley, 1990, page 142]. But again, despite these continuities, neoclassical economics still represented a paradigm shift from classical economics because of its new and fundamentally different purposive function.

While applying Kuhn's schema in laying out the events in economics in the late 1800's, Johnson and Ley caution that the period's fit with the philosopher's theory of scientific revolutions is not perfect. First, counter to their interpretation of Kuhn's model, they note that marginalist notions had existed years before the marginalist revolution. Second, the battle between the advocates of the classical school's "'real' cost doctrine" and the forerunners to the neoclassical paradigm persisted for more than thirty years after the new paradigm's acceptance. And, finally, some economists continue to practice outside the neoclassical paradigm, even after economics' shift to that paradigm [Johnson and Ley, 1990, page 123].

Johnson and Ley see anomaly, consonant with their interpretation of Kuhn's theory, playing a role a role in the revolution. In particular, according to them, the classical paradigm confronted a vexing anomaly in the ethical and ideological implications of the labor theory of value. The theory jeopardized both the existing social order and social peace:

One difficulty with classical analysis was ideological. Marx's extension of the labor theory of value into the ethical sphere led to the conclusion that labor has the only legitimate claim to output, and that the shares of capital and land result from exploitation made possible by the institutional framework of market capitalism.

These views were not those of respectable society. Indeed, the ethical version of the labor theory was seen as a threat to the established order; a threat taken very seriously after the revolutions of 1848. [Johnson and Ley, 1990, page 120]

This anomaly contributed, in part, to the concomitant decline of the classical paradigm and rise of the neoclassical one. Johnson and Ley never explicitly ally, nor do they

contrast the anomaly with Kuhn's understanding. However, given that they accept Marxists as part of classical normal science [Johnson and Ley, 1990, page 112-116], and credit them with having drawn out the disturbing implications from the classical paradigm, the labor theory of value fits well with their description of Kuhnian anomalies as "unsolvable problems encountered in the practice of normal science" [Johnson and Ley, 1990, page 28].<sup>88</sup>

The failed predictions of the Classical paradigm which Johnson and Ley enumerate also resemble their definition of a Kuhnian anomaly: (1) wages rising even in the face of rising population over 150 years, in contradiction to the Classical paradigm's "Iron Law of Wages," and (2) the failure of industrial legislation to stifle economic growth, counter to Classical economists' expectations. They, however, never refer to these failures as anomalies [Johnson and Ley, 1990, page 120].

Johnson and Ley, in any case, find that Kuhn's model provides an incomplete understanding of the causes motivating the revolution. The philosopher's schema allows for only one cause of scientific revolution: anomalies. However, they maintain that while anomalies may have been a contributing factor leading to the change, they were "not the direct cause of change" [Johnson and Ley, 1990, page 153]. Instead, broader social and philosophical movements (Lockean political philosophy, utilitarian ethics, and Romanticism) played the leading role in giving rise to economics' new paradigm and purposive function. Indeed, neoclassical economics' emergence was both the product of, and a participant in, the larger social shift from "social welfare" to "individual satisfaction":

The shift to the neoclassical concern with individual welfare and allocative efficiency was part of a broader change in western values. Throughout the eighteenth century, the spread of Locke's ideas concerning natural rights led to a political philosophy which emphasized individual liberty. In utilitarian ethics, this individualistic outlook led to the assertion that there is a direct connection between individual and social welfare. This focus on the individual was reinforced by the Romantic movement in the arts, which emphasized individual, subjective experience. Thus, the humanism of the renaissance -- which had stalled

during the age of the divine right of kings -- reasserted itself in the nineteenth century as classical liberalism, of which neoclassical economics is one aspect. Henceforth, the purpose of society (as well as scholarly inquiry) was individual well-being. [Johnson and Ley, 1990, page 121]

16. *Elias Khalil*

Khalil assents that, at least in England, "[i]t is not far-fetched if we called the rise of marginalism and neoclassical economics a 'revolution'" [Khalil, 1987, page 121]. However, he contends Kuhn's model fits poorly with economics' experience during the revolution. First, according to Khalil, it is questionable whether Kuhn's model can explain the revolution's causes. If DeVroey is correct, then the revolution was spurred by ideological uneasiness about the labor theory of value. As Khalil reads Kuhn, however, scientific revolutions are brought about by anomaly/ies, understood as a "pressing counter-instance," not "liking or disliking certain explanations," such as the labor theory of value.<sup>89</sup> Thus, it is up to serious question whether the revolution was "'a la Kuhn'" [Khalil, 1987, page 120], and using Kuhn's framework, "leaves us hunting for an answer to the question of why such a change took place" [Khalil, 1987, page 121].

Still further, the relationship between the classical and the neoclassical type<sup>90</sup> does not comport with Kuhn's understanding of the relationship between new and old paradigms. Khalil assents to the major differences which others have identified between the two, such as their theories of value. He however maintains that in contrast with Kuhn's theory, the types are commensurable; that is, meaningful comparisons between them is possible -- even in the face of strictly paradigm-laden facts.<sup>91</sup> Paradigm-laden facts may mitigate against comparison in the empirical realm; however, one is still able to relate the two in the conceptual arena. In particular, Khalil seeks to demonstrate that the two may be meaningfully compared and contrasted in terms of their understandings of the concept of time [Khalil, 1987, pages 126-127].

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We need to make an important distinction here. Khalil affirms that the classical and neoclassical types can, in principle, be meaningfully compared with one another. He, however, does not speak to the question as to whether adherents of the respective types have, in practice, made such comparisons, nor to the question as to whether the two sides have engaged in intelligible discourse with one another.

17. *Patrick Murray*

Murray posits a Kuhnian explanation for the crisis which beset classical economics in the nineteenth century: the work of Karl Marx *qua* normal scientist. Murray contends that Marx worked squarely within the confines of the classical paradigm and addressed himself to solving various puzzles which the paradigm posed (e.g., "What is the difference between productive and unproductive labor? How are prices governed? What leads to a falling rate of profit? What is value of labor?") [Murray, 1988, page 99]. However, in providing solutions to these and other classical puzzles, Marx's work produced a number of anomalies which, in turn, provoked a crisis in economics:

Using the language of Thomas Kuhn, we can say that Marx often organizes these investigations in terms of scientific puzzles, many of which turn out to be crisis-provoking anomalies for classical political economy. [Murray, 1988, page 99]

In solving the puzzles which lead to these anomalies, Marx works with Smith and Ricardo, within the classical paradigm. For example, Marx shows that the divergence of value from price of production follows from the labor theory of value. Nevertheless, Marx's solutions to puzzles both *known* and *unknown* to the tradition relativize political economy to a theory of bourgeois wealth. These anomalies undermine the classical theorists' naturalization of the economic and political categories of capitalism. [Murray, 1988, pages 99-100]

However, while indicating that Marx's work induced a crisis in classical economics, Murray provides no indication as to whether the crisis, in turn, spurred a revolution (marginal or otherwise).

18. *D.P. O'Brien*

While casting serious doubt upon the utility of Kuhn's theory of scientific revolutions during other periods of economics' history, O'Brien finds the theory helpful in gaining insight into events in economics after 1870. First, consonant with Kuhn's understanding of a paradigm shift as a gestalt switch, "the Smithian spectacles *were* gradually replaced" as economists moved away from a view of the world centered around growth toward one focused upon "static optimisation" [O'Brien, 1976, page 142].<sup>92</sup> As an indication of this shift, O'Brien cites economists' waning concern about population [O'Brien, 1976, page 143].

As to events surrounding the change, O'Brien cites three which mesh with Kuhn's portrayal of scientific change. First, the new framework which displaced the old had been "anticipated" by some economists many years prior to the switch. Second, a crisis of sorts -- at least in Britain -- preceded the change. And finally, adherents of the new and old brands of economics experienced serious difficulties in understanding each other, as evidenced by the classical economist Cairnes' inability to understand Jevons' work.<sup>93</sup>

19. *Michael Perelman*

Perelman finds that the downfall of Ricardianism -- which he allies with Ricardian rent theory -- parallels Kuhn's theory of progress. Just as with Kuhn's explanation for the demise of Ptolemaic astronomy, Ricardianism eventually fell under the weight of excessive complexities and detail which had been added to the theory over the course of its development:

In his thoughtful survey of the development of post-Ricardian thought, Wesley Clair Mitchell began with Malthus' observation that the theory presumed land that was yielding a surplus. Senior added that the law of diminishing returns must be operative for the theory to hold. Jones stressed that the system was only valid within a definite form of social organization. As qualification after qualification was loaded onto the system, Ricardianism lost its vigor.

According to the Kuhnian perspective, Ricardianism may be said to have declined as its abstract powers became weighted down with too much detail. [Perelman, 1985, page 101]

However, Perelman contends that the rise of neoclassical economics fits poorly within Kuhn's schema. According to Perelman, neoclassical economics' foundations had been fully laid by 1830.<sup>94</sup> Following 1830, marginalists then concentrated their efforts upon the details involved in neoclassicism. Their efforts, however, did not lead to the result one would expect, Perelman argues, under Kuhn's understanding of scientific development. Instead of leading to the destabilization and demise of neoclassical economics, their work led to its refinement. Events failed to play out as Kuhn's theory would expect because marginalists sloughed off exceptions to their theories as "disturbing causes" rather than seeking to incorporate and/or reconcile them with their notions:

According to Kuhn, the concentration on details should have resulted in the accumulation of anomalies that would finally raise serious doubts about the system. In point of fact, the work of the marginalists was to refine the work, to make it more elegant. The actual social relations were a matter of indifference. What did it matter whether the workers hired the means of production or the capitalists hired the workers? Even when the economic process itself resulted in outcomes that had been ruled out theoretically, the confidence of the theorist need not be shaken. It could always be explained by "disturbing factors." [Perelman, 1985, page 101]

20. *Larry Reynolds*

As we noted earlier, Larry Reynolds argues that there have been no scientific revolutions in economics' history. The "'Utility Revolution' of the 1870s," as he sees it, constituted merely an "adjustment" "of a technical nature" and left the five propositions constituting mainstream economics' "conceptual framework" undisturbed.<sup>95</sup>

21. *Ben Seligman*

Likewise, Seligman claims that the regnant Classical paradigm founded by Smith was not displaced by events in economics in the nineteenth century. To the contrary, he maintains that numerous economists of the mid and late 1800's -- many of



whom are traditionally associated with the fall of the Classical and the rise of the neoclassical school -- actually strengthened Smith's paradigm. They did so by proffering economic theories predicated upon the paradigm which appeared to be descriptively accurate, predictively powerful, as well as comprehensive in their reach.<sup>96</sup>

## 22. *Joseph Spengler*

Spengler assumes that "marginalism does not represent a *complete* break with the past in Kuhn's paradigmatic sense" [Spengler, 1972, page 469, emphasis added]. However, unlike Deane, he does not consider whether economics underwent a paradigm shift of a less extreme sort. Spengler does, however, cite many changes which others have taken as indicative of a scientific revolution. First, he finds that marginalists adhered to a different view of the world:

The marginalists qua marginalists viewed the economic world from inside man through the medium of themselves rather than from outside man through the medium of his self-revealing behavior. [Spengler, 1972, page 496]

Second, he maintains that economists came to lay less emphasis upon dynamic questions such as growth and population after the revolution. Attention toward these matters waned, Spengler explains, because economists came to assume any problems that arose in these areas would take care of themselves. Marginalists "put less direct stress upon economic growth than did their predecessors, though in the belief that action in keeping with their principles would assure progress if men wanted it" [Spengler, 1972, page 496]. Likewise, population was viewed as less of a problem because

Presumably the marginalists assumed that if, as seemed likely, the tastes of populations became more future-oriented, the derived demand for future-oriented goods and services would rise, with the result that output per head would eventually increase. . . . [Spengler, 1972, page 496]

23. *George Stigler*

According to Stigler, the marginal revolution did result in significant changes in economics. In particular, it altered their conception of man:

To be concrete, the marginal utility revolution of the 1870s replaced the individual economic agent as a sociological or historical datum by the utility-maximizing individual. [Stigler, 1969, page 225]

Similar claims serve as a major plank in other economists' (e.g., Jalladeau) arguments that the revolution constituted a paradigm change. The claim, however, does not lead Stigler to the same conclusion. The change, while "major," did not impact the "essential elements of the classical theory."<sup>97</sup> Therefore, Stigler implies, there was no paradigm change.<sup>98</sup> However, given Kuhn's ambiguity, whether the change amounted to a scientific revolution is open to question:

Until Kuhn gives us criteria of a revolution (or a paradigm) which have direct empirical content, it will not be possible to submit his fascinating hypotheses to test. [Stigler, 1969, page 225]

24. *Vincent Tarascio*

Tarascio finds that the marginalist revolution followed Kuhn's description of "a displacement of one scientific paradigm, or way of seeing the world, by another fundamentally different" one, despite attempts to forestall such change [Tarascio, 1971, page 102, footnote 6].

25. *James Thompson*

Thompson remarks that "The so-called 'value revolution' of the 1870's possibly marks the closest that economics has approached a Kuhn-type upheaval" [Thompson, 1975, page 191]. Just how close, Thompson does not indicate.

He does, however, find evidence of Kuhnian incommensurability in economics during this period in economics' history. In particular, he argues that incommensurability may explain, in part, the largely negative reaction which Mill's fourth fundamental proposition of capital<sup>99</sup> received at this time. Mill had formulated his proposition from within the Classical paradigm. Many (most notably marginal

utility theorists), however, interpreted and critiqued the proposition from within a different paradigm [Thompson, 1975, page 186]. "Consequently, the inability of marginal utility theorists of the period to perceive any appreciable truth in Mill's fourth proposition could easily be interpreted as a typical example of incommensurability" [Thompson, 1975, page 191].

While forwarding this thesis, Thompson provides no example of a specific economist whose censures of Mill may be attributed to incommensurability. At the same time, he does cite counterinstances to the claim: economists whose reactions to the proposition are better understood as stemming from reasons other than a communication gap. Thompson, for instance, issues caution about viewing either Jevons' or Canaan's criticisms as symptomatic of incommensurability. Better explanations, Thompson argues, exist. Jevons, for example, more likely questioned the proposition's validity as part of an effort to weaken Mill's hold on the profession. "Similarly, since Canaan is known as an individualist who opposed some aspects of the Marshallian system, his acerbic comments concerning Mill could easily be interpreted as a declaration of independence from a newer orthodoxy" [Thompson, 1975, page 186].

According to Thompson, the incommensurability was not insuperable. However, he maintains this does not invalidate the applicability of Kuhn's incommensurability thesis. Thompson notes that Kuhn's thesis conceives of incommensurability as a temporary -- not permanent -- phenomena:

It is necessary, of course, to explain the awkward fact that this reaction proved to be a temporary one. Within a decade or two, such writers as Marshall, Wicksell, and Newcombe were able to consider Mill's theorem with moderation and objectivity. This fact is not believed to present a serious barrier to the application of the incommensurability thesis, however. Incommensurability was pictured by Kuhn as primarily a phenomenon of the crisis period when a paradigm shift was in progress. With the passage of sufficient time, the work of translation could be accomplished, making a valid reinterpretation of an old proposition in terms of a new system entirely possible. [Thompson, 1975, page 191]

Further, Thompson argues that the manner in which the incommensurability dissolved comported with Kuhn's theory. Consistent with his interpretation of the philosopher's schema, economists acting as translators served to bridge the gap between old theory and present-day practitioners. Marshall and Pigou are two examples. Each may be understood as translating Mill's proposition by spelling out what Mill had implied, but left unsaid:

Another and probably more accurate interpretation, however, is that Marshall and Pigou -- in specifying the assumptions needed to make the corollary follow strictly from the theorem -- were merely making explicit what was really implicit in Mill. If this second interpretation is accepted, each of these critics can be regarded as having played the role of translator in the Kuhnian sense. [Thompson, 1975, page 187]<sup>100</sup>

26. *E.G. West*

West takes direct issue with the parallels which O'Brien draws between economics' situation during and following the 1870s and Kuhn's model of scientific change. First, West maintains the population theory whose fall O'Brien cites as evidence of Smith's demise was not Smith's, but instead, Malthus's theory. The latter's contention that population growth equated wages with workers' subsistence living expenses was discredited, but the former's theory, which postulated that wages were positively related with capital growth, was not [West, 1978, pages 348-349]. But, more fundamentally, the history of Smith's paradigm directly conflicts with Kuhn's contention that paradigm replacement is irrevocable.<sup>101</sup> If Smith's paradigm ever had been submerged, West implies, it resurfaced in twentieth century general equilibrium models.<sup>102</sup>

27. *Oleg Zinam*

As Zinam sees it, nineteenth century economists came to concern themselves almost exclusively with supply and cost, to the detriment of explorations of demand. This increased narrowness, he contends, helped spur the marginalist revolution, which he allies with Jevons and Gossen, champions of demand side analysis. Countering the

increased narrowness, the revolution led to the Marshallian synthesis of classical and marginalist thought. In sharp contrast with Jalladeau's conception of the revolution, Zinam maintains that the synthesis broadened economics' scope [Zinam, 1981, pages 72-74].

Zinam does not identify the synthesis as a new paradigm, nor the revolution as a Kuhnian scientific one. Instead, following Bronfenbrenner, Zinam characterizes the synthesis and revolution in dialectical terms. In these terms, the synthesis represents the reconciliation of a thesis (classical economics' "objective theory of value") and an antithesis (the marginal revolution itself) [Zinam, 1978, page 182].<sup>103</sup>

## 28. *Summary and Conclusions*

The foregoing indicates that an economist's determination as to whether the marginal (utility) revolution constituted a scientific revolution depends upon significantly more than appeal to the "facts." It, instead, depends substantially upon an economist's understanding of what a Kuhnian scientific revolution is, and in particular, upon his/her understanding of the nature, degree and permanence of the change implied by such a revolution. It depends as well upon an economist's perception as to what changes the revolution effected in economics, and finally, upon his/her judgment as to whether those changes are of sufficient magnitude and/or importance to constitute a revolution. Economists have employed varying interpretations of "scientific revolution" and understandings as to the nature and degree of the changes entailed by a scientific revolution. They have provided differing understandings as to whether there was indeed a marginal revolution in economics and, if so, as to what the marginal (utility) revolution did/did not change in economics.<sup>104</sup> Finally, economists have arrived at divergent judgments as to whether the changes effected by the revolution comprise fundamental or only incremental change.<sup>105</sup> Consequently, they have arrived

at divergent assessments as to whether the marginal (utility) revolution was a scientific revolution.

Lying behind economists' perceptions as to what the marginal (utility) revolution changed, as well as their judgments as to whether the changes were revolutionary, are varying understandings regarding what fundamentally defined economics -- in Kuhnian terms, what constituted economics' paradigm. Given the complexity and heterogeneity of economics, along with economists' selective perception of it, economists have arrived at a multiplicity of understandings as to what did/does comprise economics' paradigm (or indeed as to whether economics ever even possessed a paradigm). In defining economics' paradigm along different lines, economists' attentions have been drawn in different directions, towards those aspects of economics bearing upon the paradigm they define. Looking in different directions, they have perceived the revolution as effecting different changes. Further, economists' definitions of economics' paradigm also provides them with their yardsticks against which to determine whether a given change constitutes a scientific revolution. That which fundamentally alters what is perceived to be economics' paradigm is a scientific revolution, that which does not, is not. Having identified different paradigms and thus employed different yardsticks, economists have arrived at different understandings as to whether a given change constitutes a major departure (i.e., *interparadigmatic* change) and thus a scientific revolution, or only a minor departure (*intraparadigmatic* change) and thus not a scientific revolution.

#### G. KEYNESIAN REVOLUTION

As with the marginal revolution, economists diverge in their interpretations of the Keynesian revolution and its status as a scientific revolution. Their disagreements run along similar lines as they did in the case of the marginal (utility) revolution and may be traced to many of the same sources: economists' selective perceptions of

Kuhn's theory of scientific revolutions and the definition of economics (i.e., specification of economics' paradigm). In addition, as we shall see, selective perceptions of Keynes' economics also figure prominently in their disagreements as to whether and how Keynes' paradigm was incorporated into economics. Again arranging our discussion alphabetically by author, we begin our discussion with an economist whose evaluation of the Keynesian revolution significantly depends upon his understanding of Keynes and his relationship to the economics' orthodoxy, Albert Arouh.

1. *Albert Arouh*

According to Arouh, economics in the 1930s manifested all the symptoms of a Kuhnian scientific crisis, which according to Kuhn's schema, should have, in time, induced a "gestalt switch."<sup>106</sup> However, Arouh argues, a gestalt switch occurred only at the level of policy and failed to impact upon economics' methodology or substance [Arouh, 1987, page 417]. Keynes had, in Arouh's view, enunciated an economics which radically departed from classical conceptions. Methodologically, he censured classical economics for its "unrealism and naive empiricism" [Arouh, 1987, page 395]. On the theoretical side, Keynes shunned classical economics' timelessness and certainty and introduced a new conception of rationality based upon the recognition that economic actions and decisions take place in historical time and in the face of uncertainty.<sup>107</sup> However, Arouh observes, economists did not embrace Keynes' novel precepts. While they did abandon Say's Law, they replaced it with "hydraulic Keynesianism." In so doing, they retained classical economics' timelessness and marginalization of uncertainty [Arouh, 1987, page 417].<sup>108</sup> And, though "various attempts to come to grips with the problem of time and uncertainty within the neoclassical framework" were made, all "were inevitably abortive" [Arouh, 1987, page 401], and most

evaded the methodological and substantive repercussions of using real time and ineradicable uncertainty. They have been ad hoc adjustments when faced with the anomaly of time and uncertainty. They have not actually answered the thorny questions of a truly dynamic-historical economics. [Arouh, 1987, page 402]

Still further, the neoclassical synthesis which emerged in the wake of Keynes' economics adopted methodological positions which were close cousins to the classicals' "unrealism and naive empiricism:" "positivism, instrumentalism, and naive falsificationism" [Arouh, 1987, pages 395-396].

Had economists fully taken account of implications of Keynes' conceptions of the role of time and uncertainty, Arouh implies they would have effected a gestalt switch, both substantively as well as methodologically. Economists did not, Arouh maintains; they instead reduced time and uncertainty to special cases within a largely unaltered framework.<sup>109</sup> Consequently, in terms of economics' substance and methodology, "the so-called Keynesian revolution . . . was more of an 'ad hoc modification'" than a Kuhnian gestalt switch [Arouh, 1987, page 417].<sup>110</sup>

## 2. *Mark Blaug*

While assenting that the Keynesian Revolution constitutes the best candidate for a scientific revolution in economics, Blaug argues that the revolution was not a Kuhnian scientific revolution.

His reasons are two-fold. First, Blaug indicates that it would be inaccurate to characterize economics' adoption of Keynesianism as a paradigm shift. The reason: neither the economics which Keynesianism replaced, nor Keynesianism itself amounted simply to a paradigm. Rather, they both comprised "a network of interconnected sub-paradigms; in short, . . . a Lakatosian SRP [scientific research programme]" [Blaug, 1976, pages 160-161; 161ff.]. Second, a Kuhnian depiction of the revolution grossly misrepresents the state of economics in the 1930s. Such an account

creates the image of a whole generation of economists, dumbfounded by the persistence of the Great Depression, unwilling to entertain the obvious remedies of expansionary fiscal and monetary policy, unable to



find even a language with which to communicate with the Keynesians, and finally, in despair, abandoning their old beliefs in an instant conversion to the new paradigm. [Blaug, 1976, page 164]

Blaug, however, provides indication that this characterization is misleading on all points. Far from being dumbfounded, "[O]rthodox economists had no difficulty in explaining the persistence of unemployment" [Blaug, 1976, page 162]. Further, most economists favored expansionist policies, questioned the wisdom of certain non-expansionary measures (e.g., wage cutting and balanced budgets) and "dismissed the policy conclusions of the book [Keynes' *General Theory*] as 'old hat'" [Blaug, 1976, page 163]. Finally, the switch to Keynesianism was rational, not from one incommensurable paradigm to the next, but from a degenerating research programme to a progressive one [Blaug, 1976, page 163].

Significantly, Blaug, while forcefully denying the applicability of Kuhn's model of scientific revolutions, portrays the Keynesian Revolution as effecting fundamental change in economics. Employing Lakatosian terminology, Blaug identifies at least three major changes which Keynes made to economics' "hard core." First, while Keynes kept with economics' traditional reliance upon methodological individualism in some areas of his analysis (e.g., investment and money demand), he deviated measurably from this practice in others, such as in his consumption theory, where he based his analysis upon economic aggregates [Blaug, 1976, page 161]. Second, Keynes' allowance for an "'underemployment equilibrium'" stood in sharp contrast to "nineteenth-century economics['] . . . faith that competitive forces drive an economy towards a steady-state of full employment" [Blaug, 1976, page 162]. Finally, Blaug indicates that Keynes' introduction of "pervasive uncertainty and the possibility of destabilising expectations" represented a fundamental departure from past economic theory [Blaug, 1976, page 162].

Blaug, unlike Arouh, provides no indication that the profession failed to adopt any of Keynes' fundamental alterations. Indeed, Blaug cites one of these changes

(Keynes' notion of an "underemployment equilibrium") as the major reason that economists' abandoned their old "degenerating" research programme, which could only provide ad hoc explanations of a persistent economic downturn, and replaced it with Keynes' "progressive" research programme, which, Blaug implies, could explain such slumps without recourse to ad hoc theorizing [Blaug, 1976, page 163].

### 3. *Alan Booth*

Booth does not address the applicability of Kuhn's theories to economics in the early 1900's. However, looking at the Keynesian Revolution in Great Britain from a policy standpoint, Booth finds it difficult to characterize events in the 1930s and 1940's as "revolutionary," given the considerable length of time required for the British government to warm up to stimulative fiscal policy. Even with the publication of the White Paper, "demand management was still regarded as a relatively blunt instrument . . . the confidence, or the pressure, to 'fine tune' the economy had not emerged by 1947" [Booth, 1983, page 123]. Nor, by this time, had many British politicians demonstrated significant "economic literacy" [Booth, 1983, page 123].

### 4. *Alfred Bornemann*

Bornemann contends that Keynes' *General Theory* replaced Say's Law and classical economics as economics' paradigm. Primarily, as Bornemann sees them, the old and new paradigms diverged in that the former saw economic disruptions as temporary, posited the entrepreneur as the economy's motive force and laid emphasis upon production, while Keynes' saw a necessary role for government in assuring full employment, and stressed the role money demand and aggregate spending play in influencing economic stability.<sup>111</sup>

While not directly referring to the Keynesian paradigm's emergence as a scientific revolution, Bornemann explicitly couches his description of its rise in terms of Kuhn's schema. In doing so, he maintains that the paradigm "gained its status

because it purported to offer a solution to the acute unemployment problem" [Bornemann, 1976, page 129]. Further, he notes that once the few initial adherents to the paradigm demonstrated its abilities, most new economists adopted the paradigm, while its older aged opponents died away. Eventually, via persuasion, most economists converted to the new paradigm or were consigned to the status of non-economist [Bornemann, 1976, pages 129-130].

##### 5. *Martin Bronfenbrenner*

Bronfenbrenner does identify a revolution in economics in the early twentieth century. However, it is not the "Keynesian," but the "macroeconomic" revolution and the revolution's final outcome was not (solely) economics' adoption of Keynesianism (let alone Keynes' economics), but rather the neoclassical synthesis. Further, according to Bronfenbrenner, the macroeconomics revolution is not best understood as a revolution in the sense of Kuhn's "catastrophic" model of scientific revolutions. Instead, Bronfenbrenner argues that the macroeconomics revolution fits significantly better with his own crude dialectical framework.

In this case, Bronfenbrenner cites neoclassical economics as the revolution's thesis. He lists a number of antitheses to neoclassicism. The "Keynesian antithesis"<sup>112</sup> (alternately referred to as "cyclical depression or stagnation") was a central one. This antithesis objected to Say's Law's contention that the economy automatically tended to full employment and argued that fiscal and monetary policies were needed to keep the economy fully utilized [Bronfenbrenner, 1971, pages 145-146]. Imperfect competition theory represented another important antithesis. This theory, in contrast to neoclassical economics' theory of perfect competition, regarded monopoly (of varying degrees) as the rule and perfect competition as only one special case [Bronfenbrenner, 1971, page 145]. Bronfenbrenner identifies still other antitheses: (1) Numerous heterodox schools of economics, including historicism, Marxian socialism and institutionalism. (2)

Various objections to certain aspects of neoclassical economics, including (a) its founding utility theory on unrealistic psychological notions (hedonism); (b) neoclassical analysis' static (non-evolutionary) nature, and (c) its tendency to value "technique" above all else and to eschew consideration of social problems [Bronfenbrenner, 1971, pages 145-146].

As noted earlier, Bronfenbrenner regards the neoclassical synthesis as the revolution's outcome. As he describes it, this synthesis combined Keynesian macroeconomics with a recognition of the need to take account of "imperfect along with atomistic competition." Its methodology directs economists to formulate "theories in refutable form" and to test them via statistical methods [Bronfenbrenner, 1971, pages 146-147].

Bronfenbrenner's description of the synthesis implies that economics laid aside Say's Law in the realm of macroeconomics, while in microeconomics, the field concomitantly lessened the importance of perfect competition and heightening stress on imperfect competition. As we shall see, many economists conclude that these changes (notably putting Say's Law aside) comprise a fundamental change indicative of a Kuhnian revolution. Bronfenbrenner, however, does not. Instead, he stresses that counter to Kuhn's schema, the Keynesian revolution effected few significant *permanent* changes. Keynesian economics did not "permanently" replace any "important tenet of pre-Keynesian economics . . . except as a consequence of price, or wage, or interest-rate rigidity, or of some critical elasticity being zero or infinite" [Bronfenbrenner, 1971, page 151]. Likewise, he earlier points out in indirect reference to the macroeconomics revolution, that "The quantity theory of money, once considered moribund, has been resuscitated . . ." [Bronfenbrenner, 1971, page 138]. Significantly, he provides no assessment as to the permanence of the replacement of Say's Law -- though we should acknowledge that his central task, with respect to the

macroeconomics revolution, is to render a crude dialectical accounting of the revolution rather than critique a Kuhnian portrayal of it.

#### 6. *A.W. Coats*

Coats finds that the Keynesian Revolution resembled Kuhn's description of a scientific revolution in at least eight different ways. Crisis-like conditions preceded the emergence of Keynesian economics and the transition resembled, as Kuhn's scientific revolutions do, a conversion experience. The change was resisted by the field's elders, championed by its youngsters, yet occurred very quickly. The impetus for the revolution came from within the economics community, as did the criteria against which Keynes' (and others' theories) were judged -- again as is the case in a Kuhnian scientific revolution [Coats, 1969, page 293]. Nonetheless, the Keynesian Revolution, unlike a Kuhnian revolution, failed to produce a new paradigm incompatible to the regnant paradigm: "it is now clear that the Keynesian paradigm was not 'incompatible' with its predecessor" [Coats, 1969, pages 293-294].<sup>113</sup>

Given Coats's general assertion that economics has been dominated by one and only one paradigm, we may further conclude that the Keynesian paradigm did not displace economics' basic paradigm: "the theory of economic equilibrium via the market mechanism" [Coats, 1969, page 292]. But, even while not regarding the Keynesian revolution as a paradigm shift, Coats still finds the philosopher's schema helpful in understanding what went on during the revolution:

Yet he [Keynes] had undoubtedly provided his professional colleagues not only with a new "map, but also with some of the directions essential for map-making;" and the whole process would repay systematic study in terms of the cognitive and regulative functions attributed to Kuhn's paradigms. [Coats, 1969, page 294, quoting Kuhn, 1962, page 108]

#### 7. *Phyllis Deane*

According to Deane, the proximate cause of the Keynesian Revolution was Keynes' publication of his *General Theory* in 1936. She, however, stresses that

Keynes' work spurred the revolution because of the confluence between its theory and prevailing attitudes, needs and concerns. In a world in which laissez-faire precepts were waning, the *General Theory*, though for different reasons, appealed to a broad range of ideological positions, from Marxists, to liberals, to those in between [Deane, 1978, pages 184-185]. Further, by virtue of its amenability to empirical research and policy application, Keynes' work attracted the growing number of empirical and policy minded economists [Deane, 1978, page 185]. Most fundamentally though, Keynes' work spurred the change it did because, by centering its analysis upon aggregate economic activity, it addressed itself to concerns more pressing than those of the neoclassical orthodoxy, which focused its attention upon the individual:

It is doubtful whether the new system could have seduced so many theorists and empirical research workers away from the neo-classical orthodoxy in which they had been trained, or could have inspired so many practical policy makers with confidence that economics, had, after all, something useful to contribute to their decisions, if it had not been focused on a more urgent set of questions than the Marshallian ones. . . . By starting from the national, aggregative level of analysis Keynes focused on problems that academics and politicians alike regarded as important and he tackled them by making assumptions drawn from a close and acute observation of the way the economic system was working in his own time. [Deane, 1978, pages 185-186]

It was, Deane notes at the outset, the macroeconomic "problem of intense, persistent, trade depression, associated with widespread, unprecedentedly heavy unemployment" "that dominated the mature capitalist economies" [Deane, 1978, page 175]. The gravity of the problem, in turn, stemmed from prevailing economics' inability to alleviate or, even, tendency to exacerbate the stagnation and unemployment [Deane, 1978, pages 175-176].

As to the revolution's effect upon economic science, Deane distinguishes between the *General Theory's* radical departures from the neoclassical orthodoxy and the significantly less dramatic changes the Keynesian Revolution made to economics as a whole. According to Deane, Keynes' economics did, in numerous respects, represent a break with neoclassical economics. While neoclassical economics likened economics

with the natural sciences in which phenomena could be modelled mechanistically and be predicted, Keynes held that economics was a moral science, whose universe was neither constant, nor predictable. In this spirit, Deane observes Keynes laid stress upon the role "of psychological propensities and on expectations under uncertainty" [Deane, 1978, page 182]. Likewise, in contrast to neoclassical economics' focus upon equilibrium, Keynes' "primary concern was with states of *disequilibrium*"<sup>114</sup> [Deane, 1978, page 182, emphasis added].

Keynes also departed from neoclassical economics in that he focused his attention upon answering a question which neoclassicals largely ignored: "what were the determinants of the supply and demand for aggregate output" [Deane, 1978, page 182]. Further, he reached conclusions at wide variance with orthodox economics. In particular, he concluded that "there was no invisible hand translating private self-interest into social benefit," and, still more, that cutting wages would not reduce unemployment, but "could actually increase it by reducing the level of effective demand" [Deane, 1978, pages 182-183]. So too did the means he employed to answer those questions and reach those conclusions set him apart from past economic practice: the construction of a highly relevant and easily understood "aggregative model of the economy as a whole," with which he, in unprecedented fashion, "demonstrated . . . the dependence of aggregate expenditure on itself in its income-generating capacity" [Deane, 1978, page 183].

The Keynesian Revolution did, as Deane sees it, supplement neoclassical economics' largely microeconomic viewpoint with a macroeconomic one (along with "a new integrated set of theories, concepts and tools for analysing macroeconomic problems") [Deane, 1978, pages 205-206]. The Revolution, as well, facilitated a growing movement toward greater governmental intervention in the economy [Deane, 1978, page 187]. The Revolution, however, did not displace (let alone replace) the neoclassical paradigm.<sup>115</sup> In particular, despite Keynes' emphasis upon

*disequilibrium*, orthodox economics continues to hold to its general equilibrium precepts. Indeed, by virtue of an equilibrium interpretation of Keynes' economics, the precepts have been strengthened [Deane, 1978, page 208].

Ultimately, by interpreting Keynes' economics in a manner wholly antithetical to Keynes' perception of his own model, orthodox economics, Deane asserts, has absorbed Keynes' economics as a special case in -- rather than an alternative to -- neoclassical economics ("the case of unemployment equilibrium"). Under the neoclassical interpretation of Keynes, unemployment is due to fixed wages, the liquidity trap and interest insensitivity (all of which Keynes used only as expository devices and none of which was essential to the findings he reached) [Deane, 1978, pages 180 and 187].

Looking at the economics discipline as a whole, Deane draws only one conclusion about the Keynesian Revolution with direct reference to Kuhn's theory of scientific revolution. The revolution has not, as Kuhn's model requires for the "success of a new paradigm," led to the practice of normal science (i.e., a "narrowly constrained research tradition"); it instead has given rise to multiple lines of research. In addition to the orthodoxy's equilibrium version of Keynesianism, post-Keynesians follow a disequilibrium understanding of Keynes in their work [Deane, 1978, page 188, note 30].

#### 8. *Dudley Dillard*

Like Deane, Dillard argues that Keynes' *General Theory* instigated a revolution in economics because the work addressed itself to the pressing problem of unemployment and proposed a solution:

What quality of Keynes's *General Theory* created the sole revolution in economic theory in the twentieth century to date? The answer is that his theory was addressed to the central issue of his generation and was based on an insight that saw a workable resolution of the problem, which reached crisis proportions during the Great Depression. [Dillard, 1978, page 712]



Dillard cites two central features of the *General Theory*. (1) Keynes argued that employment and output are a function of effective demand, which in turn depends upon "the propensity to consume and the magnitude of investment." (2) Keynes broke with regnant theory, which applied only to a full employment equilibrium, and put forth a theory which applied to all levels of employment [Dillard, 1978, page 712].

Given Dillard's definition of revolution and his identification of Keynes as the revolution's revolutionary, it is clear that he sees the *General Theory* as having wrought "far-reaching changes in the main body of economic theory" [Dillard, 1978, page 705]. Dillard, however, never explicitly indicates what those far reaching changes were, nor whether (or to what degree) Keynes' innovations were accepted by the profession. Still further, while identifying the Keynesian Revolution as one of the field's revolutions, Dillard provides no indication as to whether or not the revolution was a Kuhnian one.

#### 9. *Sheila Dow*

According to Dow, neoclassical economics underwent both a microeconomic as well as macroeconomic crisis in the 1930s. On the macro side, it confronted "the palpable existence of involuntary unemployment which could not be eliminated by reductions in wage levels." On the micro side, the crisis was "provoked by observation of the absence of perfect competition" [Dow, 1981, page 330]. Further, she maintains that in each crisis, a viable alternative paradigm to neoclassical economics arose: Keynes' in macroeconomics and Robinson's in microeconomics. Dow acknowledges that each alternative had a substantive impact upon economics. However, in both "attempted scientific revolutions" she finds that the alternative paradigm was co-opted by the orthodoxy (via an IS-LM interpretation of Keynes' and a cost and revenue function depiction of Robinson's); and thus the neoclassical paradigm was preserved on both fronts [Dow, 1980, pages 377-378].<sup>116</sup>

In the case of Keynes' economics, Dow argues that the development of the microfoundations literature constitutes an example of "the Kuhnian process of paradigm defense," in which "the prevailing paradigm is amended to contain the criticism to which it is subject" [Dow, 1981, page 330]. Here, as with the IS-LM and cost and revenue function defenses, Dow indicates that the microfoundations defense serves to protect the regnant neoclassical paradigm only by reducing the alternative to those of its elements amenable to neoclassical analysis, and thus, she implies, grossly distorting Keynes' economics.<sup>117</sup>

10. *Robert Ekelund and Robert Hébert*

Like Dow, Ekelund and Hébert argue that Keynes' paradigm did not displace economics' orthodox paradigm. Like Arouh and Deane, they maintain that Keynes' economics was absorbed into the field's orthodoxy as a special case as a result of efforts to preserve the regnant paradigm. However, unlike Arouh and Deane, Ekelund and Hébert provide no indication that Keynes' economics needed to be, or was, distorted in the process [Ekelund and Hébert, 1990, pages 530-531]. Adding to the irony, Ekelund and Hébert portray Keynes' explanation of unemployment precisely in the manner Deane regarded distortive. According to the two, persistent unemployment in Keynes' model is due to downwardly inflexible real wages, an inelastic investment function and the liquidity trap [Ekelund and Hébert, 1990, page 523ff.]. Given this interpretation of Keynes' economics, Ekelund and Hébert find that Keynesian unemployment reduces to a special case of the pre-Keynesian paradigm, once account is taken of the Pigou effect. Even with the liquidity trap and interest insensitive investment, increases in wealth resulting from falling prices will eventually bring the economy back to full employment [Ekelund and Hébert, 1990, pages 529-531].

Though seeing no paradigm shift, Ekelund and Hébert indicate that orthodox economics' reaction to Keynes' paradigm was consistent with Kuhn's theory.

Consistent with their interpretation of Kuhn, the orthodoxy rose to protect economic's regnant paradigm in the face of an alternative, showed that the alternative was reducible to a special case within the prevailing paradigm and, they imply, revitalized the old paradigm.<sup>118</sup>

11. *Craufurd Goodwin*

Goodwin describes the Great Depression as an "external 'crucial experiment'" in economics' history. The Depression, coupled with public pressure for economists to address the widespread economic stagnation in a forthright manner rather than allow economists to hide behind a set of *ceteris paribus* conditions, spurred widescale adoption among economists of Keynesian economics and, in particular, "a new scientific core principle," which eschewed the notion that full employment was "the natural state of an economic system" and thus allowed for the possibility that governmental intervention into the economy may be necessary [Goodwin, 1980, pages 615-616].

As we noted earlier, Goodwin maintains that economics' "external 'crucial experiments'" differ from the laboratory controlled crucial experiments which Kuhn envisions spurring a revolution [Goodwin, 1980, pages 612 and 614-615]. Also, as we have already pointed out, Goodwin argues that economics has never undergone a Kuhnian scientific revolution. Here is no exception. Though describing economists' recognition that full employment is not a "natural state" as a "new scientific core principle" and a "fundamental" change, Goodwin contends that there was no Kuhnian revolution [Goodwin, 1980, page 616].

12. *L.E. Johnson and Robert D. Ley*

According to Johnson and Ley, economics underwent a scientific revolution in the 1930s. The revolution, they contend, constituted the "partial abandonment" of the neoclassical paradigm and its purposive function (centered around economic efficiency)

and economists' acceptance of the Keynesian paradigm, whose P-F concerned itself with the "maintenance of full employment" [Johnson and Ley, 1990, pages 132-135].<sup>119</sup>

As Johnson and Ley portray it, the revolution did not make a complete break with neoclassical economics. First, as with the neoclassical paradigm's replacement of the classical paradigm, economics retained the assumption of rational economic behavior and widespread use of the deductive method [Johnson and Ley, 1990, pages 140-143]. But, even more fundamentally, while the Keynesian paradigm may have largely replaced the neoclassical one in *macroeconomics*, Keynesianism never displaced the neoclassical paradigm and its corresponding P-F in *microeconomics*. "The neoclassical paradigm has been quite hardy: it continues to evolve and to dominate the branch of contemporary theory known as microeconomics" [Johnson and Ley, 1990, page 132]. As a result, to this day, the two paradigms, though possessing "distinct P-F's" continue to co-exist with one another.<sup>120</sup> However, despite the continuity and continued co-existence of the neoclassical with the Keynesian paradigms, Johnson and Ley still regard the partial replacement of the neoclassical paradigm by the Keynesian as a scientific revolution. In this respect, Johnson and Ley set themselves apart from Bronfenbrenner. For the latter, the lack of a complete break constitutes evidence against a scientific revolution.

Portraying the Keynesian revolution in the light of Kuhn's theory of scientific revolutions, Johnson and Ley affirm that prior to the acceptance of the Keynesian paradigm, economics underwent a crisis generated by (1) an anomaly: Unemployment during the Great Depression persisted at very high levels, despite falling prices and interest rates -- which, according to the neoclassical paradigm, should have alleviated the unemployment, and (2) the lack of a good alternative. They further indicate that the crisis spurred a period of extraordinary science during which numerous different groups competed to replace the failing paradigm. In the end, they maintain, the

Keynesian paradigm won out and, in part, replaced the neoclassical one [Johnson and Ley, 1990, pages 132-133].

While asserting that the anomaly of persistent unemployment resulted in the partial displacement of the neoclassical paradigm, Johnson and Ley stress that the anomaly, contrary to their interpretation of Kuhn's model, was not the "direct cause of change" and that "strong pressures for change also came from outside the profession." Further, they note the Keynesian P-F, which in part replaced the neoclassical's P-F, was "supplied to the profession from outside" [Johnson and Ley, 1990, page 153].

### 13. *Elias Khalil*

Khalil concedes that in certain respects, Keynes departed from orthodox economics: (1) by regarding unemployment as a permanent, rather than a temporary, phenomena; (2) by positing that money demand and supply, rather than savings and investment, determined the interest rate; (3) by modelling effective demand, rather than the "market forces of supply and demand," as the determinant of output and employment, and (4) by advancing a non micro-based theory of economic aggregates. Despite these concessions, however, Khalil challenges the contention that the Keynesian revolution constituted a Kuhnian paradigm shift [Khalil, 1987, page 121]. He argues his case along two fronts. First, he questions the paradigm shift thesis by arguing that Keynes did not completely break from the past, but rather laid his novel conceptions over "some of its [the orthodoxy's] basic postulates," to which he continued to adhere. Alternatively, Khalil maintains that even if Keynes' work itself constituted a radical departure from the economic orthodoxy, his economics was quickly co-opted by the orthodoxy via the neoclassical synthesis. Keynes thus failed to effect fundamental change in the discipline<sup>121</sup>:

If Keynes is a revolutionary, he was reduced to an episode by orthodoxy. Since the profession did not change "its view of the field, its method and its goals," as Kuhn states, then the rise of Keynesian

economics cannot be explained as a paradigmatic shift. [Khalil, 1987, page 121, quoting Kuhn, 1970c, page 85]

14. *Ghanshyam Mehta*

Mehta argues that the Keynesian Revolution fits well with Kuhn's theory of scientific revolutions. He agrees that the impetus for the revolution laid outside the realm of normal economic science. The crisis which eventually gave birth to the Keynesian Revolution, Mehta contends, arose out of a growing perception among those working outside economics' regnant macroeconomic paradigm that unemployment constituted an anomaly for Say's Law [Mehta, 1978, pages 60-3]. These investigators' perception of the anomaly led many of them to directly challenge the orthodox paradigm and forward theories antithetical to Say's Law's contention that supply creates its own demand [Mehta, 1978, pages 63ff.]:

All the writers studied in this chapter came from different backgrounds. They had different interests and orientations. The details of their theory were as varied as the men themselves. But one thing they had in common: their perception of an anomalous and ambiguous situation. They were aware that the existing doctrine that productions can only be bought by productions, that supply creates its own demand could not explain the breakdown of the financial or distributive mechanism of economies in which money is used. The fact that the distributive mechanism had broken down and that the existing theory could not explain it, was, to them, indubitable. [Mehta, 1978, page 91]

Taken collectively, these authors' theories helped set the stage for the displacement of the classical paradigm and the installation of Keynes' by creating an atmosphere in which other economists (including Keynes) felt freer to question the classical paradigm which, hitherto, had enjoyed inviolable status.<sup>122</sup> That the crisis did not originate in economics' normal science, according to Mehta, presents no difficulty for Kuhn's application to the Keynesian Revolution.<sup>123</sup> As we noted earlier, Mehta takes issue with the contention that Kuhn's theory requires that revolutions emanate from within normal science [Mehta, 1978, pages 62-63].

Mehta does, however, allow that normal scientific activity -- while not initially spurring a crisis in economics in the 1920s -- did heighten the crisis [Mehta, 1978,

pages 92-93]. Along these lines, he maintains that both Myrdal and Robertson sought to address the unemployment anomaly within the confines of economics' prevailing paradigm, but, in the process, ended up forwarding notions which led economics closer towards a paradigm shift. Myrdal, for instance, claimed only to be explicating what was "potentially" in Wicksell's economics. Nonetheless, Mehta asserts, Myrdal's explication advanced many propositions which ran counter to Wicksell's theory and more closely resembled Keynes' paradigm toward which economics was moving:

Despite his disavowals of originality, Myrdal had succeeded in making considerable advances over the older way of looking at things. His account of the inducement to invest is almost the same as the one given by Keynes in the *General Theory*. He clearly recognized that saving and investment are independent. He showed how the ex-ante difference between saving and investment becomes an ex post equality. He showed that an increase of voluntary saving leads, not to increased investment as in the Wicksell-Hayek system, but to losses for entrepreneurs due to the fall in prices. He systematically introduced expectations into economic analysis. [Mehta, 1978, page 106]

Out of the revolution, Mehta contends, Keynes' *Treatise on Money* replaced the classical paradigm as the field's prevailing paradigm. Mehta identifies at least two results of the shift to Keynes' paradigm. First, in contrast to the classical paradigm, which had stressed the importance of the relationship between the quantity of money and the general price level, the *Treatise* employed equations containing the general price level, but not the stock of money [Mehta, 1978, pages 53 and 149]. Second, Mehta indicates that the "most important" deviation from the past was Keynes' putting forth "a theory of the forces determining output and employment" [Mehta, 1978, page 149].<sup>124</sup> Keynes advanced a theory to determine the "equilibrium level of income," a notion that was irrelevant to the Classical paradigm, given its contention that "supply creates its own demand" [Mehta, 1978, page 23].<sup>125</sup>

Finally, Mehta asserts, in contrast to Blaug, that the Keynesian Revolution was marked by a breakdown in communications between adherents of the old (classical paradigm) and the new paradigm (Keynes' *Treatise*):

The post-*Treatise* debates exemplify Kuhn's assertion about the incommensurability of paradigms. According to Kuhn, adherents of different paradigms have a tendency to "talk thru each other." In our chapter on the post-*Treatise* developments in monetary theory it is this aspect of Kuhn's theory (i.e. the tendency of scientists during a crisis to "talk thru each other") that we shall try to bring out. [Mehta, 1978, page 7]

In his chapter discussing economics following Keynes' publication of the *Treatise*, Mehta cites several instances in which Keynes and adherents to the old paradigm speak past one another and concludes:

. . . economists in the "classical" tradition experienced great difficulty in understanding the central message of the *Treatise*. Their belief in certain "classical" propositions was so strong that they could not accept the fact that Keynes was saying something both valid and original. Hayek started out by assuming that the approach of the *Treatise* is not so novel as it appears to the author. Having imputed what he regarded as irrefragably correct views to Keynes he is startled to find that Keynes' most important conclusions are inconsistent with those views. [Mehta, 1978, page 170]

#### 15. *Mark Pernecky*

Pernecky contends that one's determination of the revolutionary status of Keynesianism (and thus the applicability of Kuhn to the Keynesian Revolution) depends upon one's interpretation of Keynes. Under many interpretations, Pernecky observes, Keynesianism constitutes an extension of past economics, rather than a revolutionary break from it. Interestingly, one interpretation which many employ to explicate the Keynesian Revolution as a Kuhnian paradigm shift, Pernecky identifies as implying an evolutionary conception of Keynesianism: "Fiscalist Keynesians" who disavow Say's Law and the quantity theory of money and stress the need for fiscal intervention during economics slumps. The Fiscalist Keynesian position constitutes an extension of pre-Keynesian economics by virtue of the Keynesian cross model it employs. With the model, Fiscalists are able to explain the full employment equilibrium which pre-Keynesian theory posited, along with equilibrium income levels below full employment. The same, Pernecky argues, is true of the "IS/LM" interpretation of Keynes. The IS/LM position, he contends, even more closely resembles classical



macroeconomics in that it "underemphasizes the role of imperfect expectations" [Pernecky, 1992, pages 126-127].

Likewise, he affirms that the "Monetarists," "Disequilibrium Approach," "Rational Expectations" and "New Keynesianism" depictions of Keynes all imply an evolutionary understanding of Keynesianism [Pernecky, 1992, page 127-128]. Under the monetarist rendering, Keynes supplemented pre-Keynesian macroeconomics with a set of additional notions, namely "wage rigidity, money illusion," and different perceptions as to slopes of the IS and LM curves [Pernecky, 1992, page 127]. Under the disequilibrium approach interpretation, Keynes' model is able to take account of both equilibrium and disequilibrium and thus subsumes the classical general equilibrium framework as a special case [Pernecky, 1992, pages 127-128]. As Rational Expectationists would have it, Keynes extended pre-Keynesian economics, though along ill-advised lines: by providing an explanation for involuntary unemployment (which does not exist) and further, by basing that explanation upon the presumption of sticky wages, which is without microfoundations [Pernecky, 1992, page 128]. On the other hand, New Keynesians contend that micro-based explanations of rigid wages and prices do exist (e.g., efficiency wages) and therefore regard Keynes' economics as "a positive expansion of classical economics" [Pernecky, 1992, pages 128-129].

Pernecky identifies only one interpretation which implies a revolutionary understanding of Keynes' economics: post-Keynesianism. Under this portrayal, Keynes' use of uncertainty marks a revolutionary departure from orthodox economics. Under orthodox theory, individuals make decisions in "logical reversible time" and thus have complete knowledge of the past, present and future. Under Keynes' theory however, decisions are made and actions undertaken in the context of historical time, which implies a radically unknowable future, for which persons lack even a rational basis upon which to assay [Pernecky, 1992, page 129].

As to his own interpretation of Keynes, Pernecky sides with the post-Keynesian interpretation. It is wrong, he contends, to view Keynes' work simply as an extension of pre-Keynesian economics. Keynes' use of uncertainty and the central importance which he attributes to wages "in the determination of prices when there is significant unemployment of labor and raw materials" establish Keynes as a revolutionary [Pernecky, 1992, pages 130 and 131]. Thus, he concludes, "Kuhnian emphasis on the revolutionary nature of paradigm change," as compared with an evolutionary conception, provides a "superior" understanding [Pernecky, 1992, page 131]. Pernecky asserts the propriety of Kuhn's revolutionary schema despite acknowledging that the "macro orthodoxy" never accepted either of Keynes' notions cited as evidence of the revolutionary nature of Keynesian economics [Pernecky, 1992, page 130].

Instead, Pernecky argues that consistent with Kuhn's schema, orthodox economists misconstrued Keynes' revolutionary notions. They mistook uncertainty (which implies the lack of a rational basis upon which to determine the unknown) for risk (which presumes a known rational basis). Likewise, they misinterpreted Keynes' contention that, in downturns, price was largely a function of wages, as "tantamount to marginalist pricing." Counter to the marginalist understanding, however, Keynes held that prices did not depend upon marginal revenue, but only marginal costs. And, again following Kuhn, orthodox economists dismissed Keynes' radical ideas, despite their empirical corroboration, on what must have been largely subjective grounds [Pernecky, 1992, page 131].

#### 16. *John Pheby*

Pheby finds that in many respects, the Keynesian revolution resembled Kuhn's portrayal of a scientific revolution: (1) Heightened Perception of Anomalies: Those elements of classical macroeconomics which minimized the prospect and importance of unemployment (namely, Say's Law (the proposition that supply creates its own demand

and that money is demanded strictly for transactions needs) and the assumption that price flexibility assured that any economic gluts only briefly kept the economy away from equilibrium) "were becoming increasingly anomalous" [Pheby, 1988, page 50].

(2) "Extraordinary Research" of the Anomalies: A significant number of economists began conducting research aimed at explaining cyclical depressions. (3) A New Paradigm Arose: With the publication of Keynes' *General Theory*, a new economic paradigm emerged that, at least in some respects, significantly differed from current attitudes. Whereas the prevailing view seriously questioned government's ability to stimulate the economy, Keynes' "book provided the conceptual basis for some form of government counter-cyclical policy." In addition, given its greater stress upon economic aggregates and the distinction it drew between different types of money demand, "there were a number of significant achievements that began to attract an enduring group of adherents" [Pheby, 1988, page 51]. (4) Acceptance of Keynesian Paradigm for Kuhnian Reasons. Keynes' paradigm possessed the characteristics which Kuhn saw leading practitioners to adopt a new paradigm. Further consistent with Kuhn's model, the Keynesian paradigm found its greatest support among the young and its greatest opposition from older economists, which "tended to 'fade away,' as Kuhn predicted" and were eventually overwhelmed. (5) The Keynesian Paradigm Developed a Normal Science Tradition. Keynes left economists with theories which still required a significant amount of further development. "This meant that there were plenty of 'puzzles' to occupy researchers." And, more significantly, his work left economists with a sizeable amount of empirical research to conduct. As a result, development of Keynes' economics came to occupy the bulk of economists' time and effort. Finally, most economists came to accept a common set of Keynesian "symbolic generalisations" in their work, most especially "the 'Keynesian Cross' and the IS-LM versions" [Pheby, 1988, page 52]. Thus, Pheby concludes, despite the fact that economics currently finds itself in disarray, "it would *seem* to be the case that there was a revolution and a

subsequent period of normal science that showed strong *similarities* with the work of Kuhn" [Pheby, 1988, page 53, emphasis added].

The reader will note the tentativeness of Pheby's remarks and, still further, that he stops short of declaring (or even intimating) that the Keynesian Revolution was a Kuhnian scientific revolution. Pheby later questions the revolutionary status of Keynesianism and, concomitantly, whether Kuhn's model provides the most accurate accounting of the Keynesian "revolution." Lakatos, he contends, provides a superior framework. Employing the latter philosopher's schema, Pheby sets forth two non-revolutionary descriptions of the Keynesian "revolution." First, Pheby argues that Keynes built his model upon neo-classical foundations (e.g., "general equilibrium, perfect competition and comparative statics") and contends that "The *General Theory* was grafted on to the neo-classical system," and that "Therefore in a conceptual sense it was never completely revolutionary, although some of the policy implications drawn from it were" [Pheby, 1988, page 66]. Alternatively, Keynes' economics might be conceived of as having been "safely absorbed" into orthodox economics. While Keynes' economics may have possessed elements antithetical to orthodox economics (such as its stress on uncertainty and expectations), it was subsequently defeated and co-opted by the older economics. IS-LM and 'neoclassical synthesis' interpretations of Keynesian economics whose approaches contradict the importance of expectations and uncertainty are indication of the defeat of Keynes' economics [Pheby, 1988, pages 66-67].

#### 17. *Larry Reynolds*

According to Reynolds, the emergence of Keynesianism did not constitute a paradigm shift (i.e., a scientific revolution). Its introduction had negligible impact on four of the five elements of economics' conceptual framework ("notion of the maximizing individual, private property concepts, acquisitive desires and the

mechanical analogy"). Still further, Keynesian economics' concern with maintaining full employment bolstered the fifth element ("work ethic") [Reynolds, 1976, page 31].

However, even while seeing no Keynesian revolution nor any other revolution at this period in economics' history, Reynolds still finds Kuhn's framework applicable. In particular, Kuhn's model helps explain why a revolution in the face of crisis was averted. Keynes' work, Reynolds contends, constitutes an example of a practitioner devising "ad hoc modifications" in order to forestall a paradigm's displacement:

The process of paradigm shift may be postponed because "They will devise numerous articulations and ad hoc modifications to their theory in order to eliminate any apparent conflict." Is this an explanation of Keynes' purpose in writing the *General Theory*? I would argue that this is, in fact, the case. Joan Robinson argues that Keynes saw that capitalism was a going concern and "felt it worthwhile to patch it up and make it work tolerably well." [Reynolds, 1976, page 30, quoting Kuhn, 1970c, page 78]

#### 18. *Guy Routh*

Routh acknowledges that Keynesian economics represented a departure from what had been a central and prevailing understanding in economics for over one hundred years: that all unemployment was voluntary. Keynes, Routh notes, rejected the proposition that all those unemployed are without work by choice and sought, as his primary mission, "to show how *involuntary unemployment* can be reconciled with equilibrium" [Routh, 1989, page 294]. However, Keynesian economics' did not constitute a Kuhnian revolution. To the contrary, it was "the means by which the survival of the existing paradigm was ensured" [Routh, 1989, page 27]. In the face of waning public confidence precipitated by economics' inability to cope with, explain, or even acknowledge the possibility of involuntary unemployment,<sup>126</sup> Keynes provided economics a "conversion kit whereby the existing theory could assimilate the phenomenon of unemployment" [Routh, 1989, page 296]. In so doing, Keynes restored people's faith in economics, "for at last it seemed to have prescriptions for positive action instead of only warnings of what must not be done" [Routh, 1989, page

313]. But, while Keynesian economics lent credibility to economics, its introduction did not displace economics' orthodox paradigm, nor even -- Routh's remarks strongly imply -- the notion that all unemployment is voluntary:

Ironically, the *General Theory* has now been absorbed into the corpus of the orthodox creed, where it co-exists happily with the fallacies that it purported to refute. In this it shares the fate of monopolistic competition and revealed preference, both of which were invented in response to crises of credibility, and in whose gardens perfect competition and marginal utility continue to flourish. [Routh, 1989, page 313]

Thus, overall, Keynesian economics' impact on economics' regnant paradigm was much more cosmetic than it was substantive.

#### 19. *Anghel Rugina*

As we noted earlier, Rugina allies Kuhn's notion of "paradigm" with his own concept, "system of reference." Further, Rugina characterizes Keynes' work as having "shifted the system of reference" in economics [Rugina, 1986, page 41]. Thus, we can read Rugina as maintaining that Keynes' economics represented a paradigm shift. In particular, for Rugina, the shift from classical to Keynesian economics was one that shifted economics along two dimensions: (1) from the "abstract" and "hypothetical" to the "more realistic"; (2) from "stable equilibrium" to "disequilibrium or unstable equilibrium."<sup>127</sup> While inferring that Rugina conceives of Keynes' economics as a paradigm shift might be justified; we can not infer from this that he sees Keynes' economics marking a Kuhnian scientific revolution. Here, we need to recall that for Rugina, a Kuhnian scientific revolution implies a total negation of the prior system of reference. We, however, find no indication from Rugina that Keynesian economics totally negated Classical economics. Still further, an advocate of Rugina's position, Clem Tisdell, contends that Keynes' economics did not amount to a scientific revolution because "the Keynesian paradigm does not negate the classical one. Each has its own area of relevance" [Tisdell, 1987, page 42].<sup>128</sup>

20. *J. Ron Stanfield*

Stanfield finds the match between Kuhn's model of scientific revolution and the Keynesian Revolution impeccable. First, activity in economics prior to the revolution meshed with Kuhn's portrayal of a science on the verge of a scientific revolution. Economists had long been aware of an anomaly: fact was at variance with Say's Law and its assurances that the economy will rarely deviate from full employment. However, any serious examination of the anomaly was relegated to the economics "underworld" [Stanfield, 1974, pages 101-102]. But, in the late 1800s, economists stepped up their research of the anomaly area. And, as they focused more and more attention upon it, there came to be a growing sense of crisis in economics. Here, we should note that Stanfield portrays the crisis as arising as a result of increased attention to the anomaly. The Great Depression might have "accelerated the crisis and shortened the paradigm battle after the appearance of the *General Theory*," but economics was in a state of crisis prior to the Depression [Stanfield, 1974, page 102].<sup>129</sup>

Continuing to comport with Kuhn's depiction of a science of the verge of scientific revolution, research became much less structured and, more and more, economists concerned themselves with methodological and philosophical matters. Further, out of their increasingly random inquiries, a number of potential replacements to the regnant paradigm emerged (e.g., "sunspots, monetary, and innovational theories"); each offered its own solution to the anomaly that beset economics [Stanfield, 1974, page 102]. Finally -- also as Kuhn's model predicts -- the old paradigm was not displaced until a replacement for it (Keynesian economics) was found [Stanfield, 1974, page 103].

Most significantly, Stanfield maintains that the revolution effected the type of fundamental, non-incremental changes which Kuhn's model of scientific revolutions envisions:

Keynes provided new fundamental behavioral functions and tools of analysis, such as the consumption and liquidity functions and the

multiplier, and new applications of the discipline in the areas policy and econometrics. [Stanfield, 1974, page 104]

Keynesian economics also represented a new world view. Whereas prior to the revolution economists' chief concern was with efficiency, afterwards their attention shifted to the level of employment. In Stanfield's opinion, this shift constitutes "the *fundamental* change in world view evoked by the Keynesian revolution" [Stanfield, 1974, page 104]. Further, whereas before savings had been considered the key to economic well-being, now aggregate demand was [Stanfield, 1974, page 103]. But, Stanfield maintains, not only had the "purposes, tools, and problems of the discipline" changed, the world had as well. The world was no longer one in which the economy took care of itself:

The social happiness derived from laissez-faire, thrift, and competition was replaced by the social specter of stagnation and/or cyclical depression and the vice of oversaving. [Stanfield, 1974, page 104]

Further, Stanfield argues, the conversion to Keynesian economics was complete. On this point, he contends that the present-day "monetarist counter-revolution" does not contradict this fact. Here, he questions whether the ties between today's monetarists and pre-Keynesian monetarists are strong enough to merit use of the term "counter-revolution"<sup>130</sup> and intimates that monetarism does not constitute an eschewal of the Keynesian revolution's "*fundamental* change in world view . . . the attention centered on the level of employment, income, and output" [Stanfield, 1974, page 104].<sup>131</sup>

Finally, still following with Kuhn's schema, economists' conversion to Keynesian economics gave birth to a new economic normal science:

As for Keynesian normal science, despite its fundamental simplicity, Keynes's theory was sufficiently open-ended to allow substantial articulation. For example, note the surge of econometrics and national income accounting, the consumption function debates, the stagnation theories, and the portfolio balance approaches to liquidity preference. [Stanfield, 1974, page 105]



Thus, Stanfield may be read as regarding the Keynesian Revolution as a scientific revolution. Economists behaved before the revolution as Kuhn's model predicts; the revolution resulted in the type of fundamental change Kuhn allies with a scientific revolution, and after the revolution, economists engaged in the practice of normal science.

## 21. *Benjamin Ward*

Ward assents that, in many ways, economics was much the same after the Keynesian Revolution as it was before it. (1) Microeconomic and macroeconomic theory remain unintegrated with one another. (2) Not unlike their neoclassical predecessors, many Keynesians identify price and wage rigidity as the principal cause of persistent unemployment. (3) Economics still possesses no theory which cogently accounts for the interrelations between real and monetary factors [Ward, 1972, pages 38-39]. (4) Over time, Keynesian methodology has reverted to pre-Keynesian rationalism:

Thus has Keynesianism, the policy-oriented discipline *par excellence*, been transformed into something very like its neoclassical predecessor of the 20's, a field in which rationalism tends to substitute for empiricism, theoretical sophistication for common sense. [Ward, 1972, page 39]

(5) Still further, according to Ward, in the 20 years during which Keynesian economics was incorporated into economics, "much of what Keynes proposed in the General Theory had either been dropped or remained controversial" [Ward, 1972, page 40].

As we have seen, many economists, on the basis of points similar to the five listed above, argue that the Keynesian revolution did not constitute a scientific revolution. However, Ward -- notwithstanding these concessions -- still contends "Within economics the Keynesian revolution was definitely a Kuhnian revolution." Despite everything Keynes did not change, Keynes did forever change the nature and status of "aggregative economics and most of its key concepts, such as money and savings and investment" [Ward, 1972, page 40]. Macroeconomics, which had existed

on the field's periphery, moved to center stage. Keynes also succeeded in solving one of the anomalies confronting pre-Keynesian economics by bringing the theory of money within supply and demand analysis. Thus, "quite aside from the political and policy impact, Keynesianism has dramatically changed some of the major ways in which economists view their subject" [Ward, 1972, page 38].<sup>132</sup>

Setting out to portray the Keynesian Revolution in Kuhnian terms, Ward identifies a number of anomalies which came to the fore in economics in the decades directly prior to Keynes' *General Theory* [Ward, 1972, pages 34-37]. We have already alluded to one of these: there was a growing recognition of the need to provide a demand and supply analysis of money. More generally, economists increasingly sought to find a way to devise a theory which integrated the real and monetary sectors of the economy. A second related anomaly growing in importance involved the need to explain how monetary factors influence investment decisions. A third anomaly confronting economics in the early twentieth century stemmed from pre-Keynesian economics' providing no widely accepted explanation of business cycles.

Finally, Ward identifies "the great factual anomaly of the period" as the "persistence of massive unemployment." The anomaly posed no theoretical difficulties for neoclassical economics, since neoclassical theory could offer a number of explanations for the unemployment:

This fact did not contradict the neoclassical theory: it would be explained there in terms of frictions and resistances to wage and price changes, particularly by labor. A neoclassical economist could satisfy himself theoretically with a slogan such as, "The longer the unemployment, the stronger the monopoly unions." [Ward, 1972, page 36]

The factual anomaly, however, still posed a very serious political threat. The massive unemployment called for more than simply a theory consistent with its existence; the unemployment demanded action. In the face of the unemployment, "unconventional theories about what to do" and "unconventional political groupings with even more

drastic remedies to propose" proliferated. Given this, what was needed was a theory which provided more conventional and safer solutions:

Nevertheless a problem remained: what to *do* about the unemployment. Not only was it politically infeasible to do nothing, it was positively dangerous. . . . [the unemployment] was not just a case of misbehaving atoms that could be studied at one's leisure. What was needed politically was a theory that explained what was wrong, explained what to do about it, and whose policy prescriptions were politically feasible for the existing political parties. [Ward, 1972, pages 36-37]

22. *E. Roy Weintraub*

According to Weintraub, application of Kuhn's schema to events in economics since the 1930s yields a distorted picture of what went on in economics during the 1930s and thereafter. Kuhn's model excludes the possibility that more than one well-defined paradigm can exist "for any length of time." However, contrary to this understanding, two scientific programs (Keynesian and neo-Walrasian) did emerge simultaneously in the 1930s and have co-existed with one another ever since [Weintraub, 1983, pages 297-298]. Consequently, he notes one (such as Sheila Dow) who attempts to portray economics' post-1930s developments in Kuhnian terms, must, in conflict with actual fact, fabricate a series of revolutions and counter-revolutions in which one and then the other program gained temporary dominance.<sup>133</sup> Among those Kuhnian-type revolutions that Weintraub implicitly denies ever occurred is the Keynesian revolution.

23. *Stephen Worland*

Even while "the ambiguity of Keynesian underemployment equilibrium" constituted an anomaly for economics, Worland contends that economists were able to integrate the anomaly "as a special case" within the field's pre-existing paradigm. As a consequence, the paradigm was not replaced; instead, the anomaly's incorporation "served to clarify and extend the basic paradigm" [Worland, 1972, page 276].<sup>134</sup>

24. *Oleg Zinam*

Like Routh, Zinam contends that the Keynesian revolution rescued economics' orthodox paradigm:

Macro economic theorizing has also been neglected. The Great Depression caught classical economists completely unprepared to deal with world-wide unemployment. It took the Keynesian revolution to save orthodox paradigm from being discarded. [Zinam, 1981, page 73]

Further, like Routh, Zinam argues that the revolution did not displace economics' orthodox paradigm. The revolution, instead, was absorbed into and broadened the scope of economics' mainstream [Zinam, 1981, pages 72 and 73].

25. *Summary and Conclusions*

As with the marginal (utility) revolution, economists offer multiple interpretations of the Keynesian revolution. In particular, they disagree as to whether a Keynesian revolution ever took place, and, if it did, whether it constituted a Kuhnian scientific revolution. While some economists argue that, without question, there was a Keynesian revolution that indeed amounted to a Kuhnian revolution, others, with equal certainty, deny that there was a Keynesian revolution, let alone one which comprised a scientific revolution ala Kuhn. And, again, as with the marginal (utility) revolution, economists' determinations as to the existence of the Keynesian revolution and its status as a scientific revolution largely hinge upon economists' selective interpretations of "scientific revolution," the changes which the Keynesian revolution effected, and the nature and degree of those changes. The selectivity of economists' interpretations of those changes, in turn, depends upon their divergent understandings as to what comprises economics' paradigm. These divergent understandings influence where economists look for change, as well as their assessments as to whether the changes they find comprise the sort of fundamental changes constitutive of a scientific revolution. For example, many who regard Say's Law as a central element of economics' paradigm before the revolution regard its eschewal as revolutionary, while many of those who

regard the Law as incidental to other elements they regard as fundamental, such as time and uncertainty, do not. Looking different places for changes and applying different standards in assessing the nature and extent of those changes, economists reach divergent conclusions as to whether there was a Keynesian revolution, and, if so, whether it constituted a scientific revolution.

Likewise, economists' multiple interpretations of the paradigm Keynes advanced in *The Treatise* and/or *The General Theory* give rise to divergent understandings as to whether Keynes' paradigm was ever incorporated into economics, and, if so, how. Identifying different elements as the fundamental substance of Keynes' paradigm, economists look toward and lend importance to different aspects of economics (those which bear upon their definition of the Keynesian paradigm) in seeking to determine whether economics ever adopted his paradigm. Given their multiple interpretations of Keynes -- along with their multiple interpretations of economics paradigm itself -- economists have arrived at a variety of different positions: economics' paradigm prior to Keynes already encompassed Keynes' paradigm; Keynes paradigm was adopted following the publication of *The Treatise/General Theory* and its adoption constituted a scientific revolution; Keynes paradigm was adopted following the publication of *The Treatise/General Theory* and its adoption modified (or even strengthened) economics' regnant paradigm, but did not replace it; Keynes' paradigm was not adopted by economists, but, instead was co-opted by normal economic science to preserve/strengthen the regnant paradigm.

#### H. ADVENT OF CLASSICAL ECONOMICS

In reviewing the economics revolution literature which addresses itself to Kuhn's theory of scientific revolutions, it is clear that the most often analyzed revolutions in economics history are the marginal (utility) and the Keynesian revolutions. We do, however, find some discussion examining whether paradigm shifts

and/or Kuhnian revolutions occurred at other points in the field's history. A few economists seek to apply Kuhn's notion of scientific revolutions to events in economic methodology since the emergence of Keynesianism. Still further, several economists assess the applicability of Kuhn's schema to the advent of classical economics with Adam Smith in the late eighteenth century and the subsequent ascendancy of Ricardo's economics in the early nineteenth century. It is to these economists to which we now turn our attention.

1. *Martin Bronfenbrenner*

Bronfenbrenner asserts that economics underwent the "laissez-faire" revolution in the mid to late 1700s. He links the revolution with the publication of Adam Smith's *The Wealth of Nations* in 1776 and David Hume's *Political Discourses* in 1752 [Bronfenbrenner, 1971, page 138]. In addition, Bronfenbrenner allies the revolution with the field's transition from mercantilist to classical economics. In these respects, his account of the advent of classical economics parallels that of other economists to be discussed in this section. However, he sets himself apart from these other authors in that he directly argues against understanding the emergence of Classical economics in Kuhnian terms. As we noted earlier, Bronfenbrenner contends that all revolutions (including the "laissez-faire" revolution) in economics are better understood in terms of a crude dialectic, rather than Kuhn's catastrophic theory of scientific revolutions [Bronfenbrenner, 1971, page 151]. In contrast to Kuhn's notion that paradigms are "displaced definitively and relegated to the antiquarian's dustbin," Bronfenbrenner points out that "few elements in anyone's form of mercantilism have missed revival in connection with my generation's dollar-shortage, dollar-glut, and world-liquidity crises" [Bronfenbrenner, 1971, pages 137-138]. Further, Bronfenbrenner casts doubt upon understanding the transition from mercantilism to classical economics as a

paradigm shift by expressing reservations as to whether mercantilism amounted to/constituted/possessed a paradigm:

I am not sure whether what preceded it [classical economics] was an orthodoxy of the "normal science" or "paradigm" variety. My conception of pre-Smithian or pre-Humean economics is an odd collection of ethical preachments, bullionism, mercantilism, and physiocracy, plus a considerable body of embryonic laissez-faire. [Bronfenbrenner, 1971, page 141]

Depicting the transition in dialectical terms, Bronfenbrenner identifies mercantilism as the revolution's thesis and Physiocracy as the "principal offsetting antithesis" [Bronfenbrenner, 1971, page 141]. The revolution's outcome was the "classical or laissez-faire synthesis," whose principal contention was that "there was no economic class whose interests represent those of society in any unique manner. Society should therefore refrain from encouraging any class at the expense of any other, and treat the interests of all classes as complementary rather than competitive" [Bronfenbrenner, 1971, page 142].<sup>135</sup>

## 2. *Dudley Dillard*

Dillard identifies Adam Smith as the revolutionary heading up a revolution in economics in the late eighteenth century. Further, as with all revolutions he locates in economics' history, Dillard argues that the one led by Smith arose in response to a pressing social problem. Dillard asserts that that problem was "mercantilist restrictions on private enterprise" and Smith's solution to the problem was "laissez-faire in domestic and international economic relations" [Dillard, 1978, page 713]. Smith's theory, Dillard asserts, can be understood as an argument for this policy [Dillard, 1978, page 706]. However, unlike many of the authors discussed in this section, Dillard draws no link between the revolution and Kuhn's framework.

## 3. *L.E. Johnson and Robert D. Ley*

Like Bronfenbrenner, Johnson and Ley do not identify the emergence of classical economics as a scientific revolution. They do, however, contend that classical

economics' ascendancy constituted a paradigm shift. In particular, it represented a shift away from the mercantilist paradigm to the classical one. The transition involved a rejection of the prior paradigm's *political* purposive function, "the maximization of the relative world political power of the nation state" [Johnson and Ley, 1990, page 86], and concomitant acceptance of the field's first *economic* P-F, "an examination of the extent to which market directed capitalism leads to the maximization over time of total social welfare, defined in material terms" [Johnson and Ley, 1990, page 89-90]:

In summary, the classical political economists not only rejected the mercantilist P-F, they provided in its place the first economic P-F, one based on the growing view that society's welfare was something apart from royal or even national political power. [Johnson and Ley, 1990, page 90]

This distinction between the political and economic helps explain why Johnson and Ley do not identify the paradigm shift with a scientific revolution in economics, for it was only with the acceptance of the classical paradigm that economics became a discipline in its own right: "The emergence of the classical paradigm marks the first economic paradigm and, as such, the beginning of economics as a discipline" [Johnson and Ley, 1990, page 89].

As Johnson and Ley portray the shift, it was motivated by practical concerns and had its roots in France. In France, they note, mercantilism's rejection was spurred by the failure of a heavily entrenched system of state controls and taxation to generate economic growth, coupled with governmental extravagance [Johnson and Ley, 1990, page 91]. As a result of mercantilism's failures, society turned its attention to the "social welfare of the populace" [Johnson and Ley, 1990, page 91], rather than the state at large and Physiocracy arose.<sup>136</sup> The installation of Physiocracy marked the replacement of mercantilism's P-F with the classical P-F, "examining the extent to which market capitalism maximised social welfare" [Johnson and Ley, 1990, page 95].



Adam Smith, according to their depiction of events, did not found the new paradigm. Instead, he constituted a normal scientist, who, nonetheless did contribute significantly to the paradigm's progress:

Smith, like the Physiocrats, was interested in examining the extent to which market capitalism maximised social welfare. He was therefore concerned with the creation of the economic surplus, economic growth, and their relationship to the class income distribution pattern. However, as one would expect in the early stages of normal science, intellectual progress occurred. Smith's work exhibits a more systematic and rigorous quality than does that of the Physiocrats. [Johnson and Ley, 1990, page 95]

#### 4. *D.P. O'Brien*

O'Brien, like Johnson and Ley, identifies a Kuhnian "paradigm switch" in economics in the latter half of the eighteenth century and allies the "switch" with economics' movement away from mercantilism toward classical economics. However, unlike them -- and like Dillard -- O'Brien links the "switch" with Adam Smith.<sup>137</sup> Further, O'Brien describes the shift in different terms from Johnson and Ley: as a shift in economists' world view away from mercantilism's "balance-of-payments spectacles" to "economic-growth-through-decentralized-decision-taking spectacles" [O'Brien, 1983b, page 116].

#### 5. *Larry Reynolds*

Like Bronfenbrenner, Reynolds asserts that economics underwent the "Hume-Smith Revolution in that last half of the 1700s" [Reynolds, 1976, page 30]. However, Reynolds provides no direct indication as to whether the revolution comported with Kuhn's model of scientific revolutions. He also provides no indication as to what the revolution displaced. Reynolds does, however, as we noted earlier, lay out the five propositions of a conceptual framework, which he asserts have remained unchanged since the revolution. The first of these, "maximizing individuals in a relatively free market" resonates with many here who link the revolution with increased reliance upon the market and upon individuals, rather than a nation state in general. However, the

other four elements constitute, at very least, much more distant cousins to the "paradigms" others find arising out the revolution: "(2) private property, (3) acquisitiveness, (4) the work ethic, (5) the mechanical analogy" [Reynolds, 1976, pages 28-29].

#### 6. *Ben Seligman*

Seligman does not indicate whether the emergence of Smith's economics constituted a revolution (Kuhnian or otherwise). He does, however, maintain that the discipline's acceptance of Smith's economics in the 1700s may be understood in terms of Kuhn's model of scientific change. Applying the philosopher's schema, Seligman explains that economics' adoption of Smith's economics may be attributed to (1) pre-eighteenth century economics' increasing difficulties in dealing with "newly evolving fact" and (2) the facility with which Smith's economics handled those facts.<sup>138</sup>

#### 7. *Summary*

None of the foregoing accounts explicitly identifies the advent of classical economics as a scientific revolution in economics' history. Nonetheless, they diverge in the changes they identify with the emergence of classical economics. As with the marginal (utility) and Keynesian revolutions, economists' divergent definitions of economics play a key role. Highlighting different aspects of economics around the turn of the eighteenth century, economists arrive at different conclusions regarding the implications of the rise of classical economics.

### I. EMERGENCE OF RICARDO'S ECONOMICS

In our research, we found no economist who identified the emergence of Ricardo's economics, in and of itself, as a scientific revolution. Dillard does identify Ricardo as the leader of a revolution in economics' history. The revolution, Dillard maintains, was spurred by the problem of "high labor costs and low profits associated with import restrictions on food;" Ricardo's solution was "repeal of the Corn Laws"

[Dillard, 1978, page 713]. Underlining the central role with pressing social problems played in the revolution, Dillard asserts, "Ricardo's is one of the clearest cases of a theory that emerged from a preoccupation with policy" [Dillard, 1978, page 707]. However, as we noted earlier, Dillard provides no assessment of Kuhn's applicability to this, or any of the other four revolutions he identifies in economics' history.

Some economists do assay the applicability of the philosopher's model to the rise of Ricardianism. Their evaluations, however, vary. William Breit -- though not elaborating on the point -- asserts that the period in economics directly following Ricardo's publication of his *Principles* "represents the closest episode I know to a Kuhn-type scientific revolution in economics" [Breit, 1987, page 829]. On the other hand, Michel DeVroey describes the period immediately after the *Principles'* publication not as a scientific revolution itself, but, rather, the beginning of a different scientific revolution: the marginal revolution. As we saw earlier, DeVroey characterizes this period in economics' history as the "destructive" phase of that revolution [DeVroey, 1975, page 431]. Finally, O'Brien raises serious question as to Kuhn's applicability to economics' experience in the years following the *Principles'* publication. O'Brien points out there are problems with interpreting the rise of Ricardian economics as a Kuhnian paradigm shift from a Smithian paradigm to a Ricardian one. First, he finds little indication that a Kuhnian crisis preceded Ricardo's replacement of Smith's economics [O'Brien, 1983b, page 103]. Second, Kuhn's model, O'Brien maintains, does not allow for counter-revolutions. Nonetheless, Smith's paradigm did subsequently re-emerge into prominence and replace Ricardo's.<sup>139</sup> Here, we should note that West concurs with O'Brien on this point [West, 1978, page 348]. Second, there is little indication that the two classical economists' paradigms were incommensurable with one another. While the concept of "value" did take on a new meaning under Ricardo's model, all other key terms

including "*wages, subsistence, capital, or population*" had substantively the same meaning under both systems [O'Brien, 1983b, page 104].<sup>140,141</sup>

In sum, the economists discussed here lend different significance to the emergence of Ricardo's economics. While Dillard regards it as revolutionary, DeVroey, West and O'Brien do not. While Dillard identifies Ricardo's rise as the consummation of a revolution, DeVroey depicts it as the starting point of one.

#### J. EMERGENCE OF JOHN STUART MILL'S ECONOMICS

We located only one economist who cited the emergence of J.S. Mill's economics in the mid 1800s as a revolution in economics: Dillard. As with all the revolutions he identifies, Dillard contends that pressing social problems were in the background of the revolution led by Mill. In Mill's case, the motivating problem was the "frustrations of the laboring classes in an age of rising aspirations and threatened revolution" [Dillard, 1978, page 713]. In this regard, Dillard affirms:

Importance should also be attached to the concurrent publication of Mill's *Principles* and Marx's *Communist Manifesto* in the same year, 1848, in which social revolutions rocked the capitals of western Europe. All three of these 1848 events were responses to the unsatisfied and rising aspirations of the working classes of Europe and England. [Dillard, 1978, page 709]

The solution Mill proffered to address present and prospective social upheaval was "cooperative workshops, peasant proprietorship, trade unions, emigration, and ultimately socialism" [Dillard, 1978, page 713]. Dillard, however, draws no connection between Kuhn and the revolution.

Given Dillard's identification of the rise of Mill's economics with a revolution in economics, we should note that DeVroey characterizes that period as part of another scientific revolution in economics history (the marginal revolution). Instead of describing Mill as a revolutionary leading his own revolution, he depicts the classical economist as an unknowing participant in the marginal revolution. Further, DeVroey describes the period during which Mill's economics dominated economics not as a

revolution (or the result of one), but rather, as an "interregnum" between the classical and neoclassical paradigms:

In between there was a long period of hybrid equilibrium. It was a sort of interregnum between the *ancien* and *nouveau regime*. The dominant figure was J.S. Mill, whose *Principles of Political Economy* . . . Mill himself thought he was only qualifying Ricardo and was not aware of the long-term consequences which would result from the infiltration of subjective elements into the Ricardian system. More precisely, he did not realize that the labor and the subjective theories of value, which he tried to synthesize, belonged to opposed methodological approaches. [DeVroey, 1975, pages 431-432]

## K. METHODOLOGICAL REVOLUTIONS IN ECONOMICS AFTER THE KEYNESIAN REVOLUTION

### 1. *Formalist Revolution*

Ward tentatively advances the notion that economics may have undergone a Kuhnian revolution in the 1950s and 1960s. He labels the revolution the "formalist revolution" and identifies its major impacts upon economics as (1) the integration of theoretical and empirical economics, which prior to the revolution had been largely separate from one another, (2) increased reliance by theorists upon mathematics and (3) a movement away from historical studies and towards statistical analysis and testing on the part of empirical economists [Ward, 1972, pages 40-41].

Ward points out that, casting the revolution in Kuhnian terms, one may identify the increasingly critical anomalies which set the revolution off: criticisms as to the lack of integration between theory and empirical work in economics and, concomitantly, charges that economic theory lacked empirical content. Still further, he grants that the revolution "happened rather suddenly" and can be seen as having given birth to a new normal science in economics [Ward, 1972, page 43]. However, the nature of the changes effected by the revolution causes Ward to step back from understanding the revolution as a Kuhnian one. The revolution, he notes, while significantly altering the field's methodology, had little to no impact upon its substance (i.e., theory). Referring to the formalist revolution, Ward indicates

This sounds like about as classic a case of a Kuhnian scientific revolution as one could imagine. And yet there is one striking and anomalous feature to the whole transformation of economics: it was essentially methodological rather than substantive. . . . The core of theory remains the analysis of the price system, and right at its heart is still to be found the competitive régime of production and exchange. Associated with this theory is Keynesian economics and growth theory, whose basic ideas were well-known by 1950. In applied work much the same is true, though the very great increase in the amount of data available describing the economy has, in combination with the manifold increase in the number of practitioners, given contemporary economists a much more detailed picture of certain aspects of the economy than was available to our predecessors. But again, dramatically new ideas are just not there. It is as if the interwar economists had some sort of uncanny ability to intuit the features that are now being traced out in more detail. The great methodological puzzle in economics is why a great methodological revolution should make so little substantive difference. [Ward, 1972, pages 43-44]

## 2. *Econometrics Revolution*

Tong-eng Wang is another who identifies a paradigm shift in economic methodology after 1930. In his case, he expresses no doubt that economics underwent a paradigm shift<sup>142</sup> toward "an econometric approach in economic teaching and research" some time between 1930, the year the Econometric Society was founded and the latter 1960s, when two econometricians, Ragnar Frisch and Jan Tinbergen received the discipline's first Nobel Prizes [Wang, 1973, page 151]. What is up to question for Wang is when the revolution occurred. He determines the date of the revolution by fitting annual data on the number of econometric and quantitative articles published by thirty-one prominent econometricians between 1930 and 1965 to an exponential curve. He postulates that the inflection point on a curve fitted to the data will represent "the date of successful econometric 'revolution' when the number of economists remaining to be converted is on the decline" [Wang, 1973, page 160]. His fitted curves of the annual data suggest the revolution took place in 1957 (quantitative articles) or 1959 (econometric articles) [Wang, 1973, page 161].<sup>143,144</sup>

In closing, we should note that the methodological developments in economics after Keynes, which Wang and Ward link to Kuhn's notion of a paradigm shift

(implying fundamental change), others ally with the philosopher's understanding of normal science (implying only incremental change). Stanfield, for instance, identifies the "surge of econometrics and national income accounting" as elements of Keynesian normal science [Stanfield, 1974, page 105].<sup>145</sup>

#### L. SUMMARY AND CONCLUSIONS

The foregoing examination of economists' interpretations of Kuhn's theory of scientific revolutions and their application of that theory provides further evidence of the multiple interpretations to which economists have subjected Kuhn's notions. Even while many economists identify a scientific revolution with a fundamental change and still more, a paradigm shift, they disagree as to the degree of discontinuity implied by a scientific revolution. In addition, as we noted earlier, economists' general agreement that a scientific revolution constitutes a paradigm shift must be juxtaposed with economists' significant lack of agreement as to what constitutes a paradigm, and thus, by implication, what comprises a paradigm shift itself. Further, economists disagree as to whether scientific revolutions must, by definition, occur rapidly; and, they differ as to the importance they lend to behavioral changes surrounding a revolution in characterizing scientific revolutions. Regarding the causes of a scientific revolution, economists do generally agree that anomalies comprise a necessary, but not sufficient, condition for a scientific revolution. However, they disagree as to what anomalies are. Likewise, most -- though not all -- economists agree that according to Kuhn, scientific revolutions are the product of crises generated via the internal workings of normal science. However, they disagree as to whether, following Kuhn, crises *must* necessarily generate scientific revolutions. Thus, despite partial agreement as to what constitutes and causes a scientific revolution, economists by no means share a common understanding of "scientific revolutions." What comprises Kuhn's theory of scientific revolutions for one economist need not comprise that theory for another.

Economists also provide differing assessments of Kuhn's applicability to economics. As we have noted, economists vary regarding the significance they lend to the differences between the natural and social sciences in their assessments of Kuhn's applicability to economics. While many argue that the differences weaken the applicability of Kuhn's theory of scientific revolutions, at least one economist contends certain differences strengthen that theory's applicability. Further, among those highlighting the differences between the social and natural sciences, some suggest that the need for the modifications they propose in order to render Kuhn's theory applicable is not based strictly upon the differences they identify between the two realms of science. Instead, they indicate the need for the changes they propose crosses over the boundary between the natural and social sciences. Similarly in assessing Kuhn's applicability to economics, economists differ as to the significance they lend to continuities in the history of economic thought. While some economists dismiss the applicability of Kuhn's theory of scientific revolutions out of hand on the basis of continuities they locate, others do not. They instead identify modifications or alternative interpretations of Kuhn's theory which allow for such continuities and permit the identification of scientific revolutions in economics.

In addition, economists in applying Kuhn's theory of scientific revolutions to the history of economics offer multiple interpretations of the discipline's past. Even among those arguing that there have been no major paradigm shifts in economics in over two hundred years, there is disagreement regarding what constitutes that paradigm as well as the reasons why it has not been displaced.

In addition, economists offer multiple interpretations of the two most often cited revolutions in economics: the marginal (utility) and Keynesian revolutions. They disagree as to whether these revolutions ever took place, what changes these revolutions effected in economics, the nature and degree of those changes, and whether these revolutions constituted scientific revolutions. As we have argued, these multiple



interpretations themselves are the product of economists' multiple interpretations of "scientific revolution" and economics paradigm. Some economists interpret a scientific revolution as a complete break with a science's past; others interpret the break as less severe. As a result, while the former cite continuities between economics before and after a revolution as evidence against its being a scientific revolution, the latter does not.

Disagreeing as to what constitutes economics' paradigm, economists have focused their attention upon different aspects of economics. As a result, economists have identified different changes arising out of these revolutions. Disagreeing as to what constitutes economics' paradigm, economists have employed different standards in assessing the nature and degree of those changes. As a result, while some economists argue that the changes effected by these revolutions constituted fundamental, inter-paradigmatic change, i.e., a scientific revolution, others contend that these revolutions resulted only in incremental intra-paradigmatic changes (or no substantive change at all), and thus not a scientific revolution. In the case of the Keynesian revolution, disagreements as to what constitutes Keynes' paradigm -- together with disagreements as to what comprises economics' paradigm -- give rise to differences in economists' assessments as to whether economics adopted that paradigm and, if so, the implications of that adoption.

Brief consideration of economists' depictions of other less well-noted revolutions in economics have led to a similar conclusion: economists have offered multiple interpretations of the changes effected by these revolutions, as well as their status as scientific revolutions.

As with economists' treatment of paradigms in general and in economics in particular, as with economists' treatment of normal science in general and in economics in particular, economists have subjected both Kuhn's notion of "scientific revolution"

as well as revolutions in the history of economics to multiple and selective interpretations.

## NOTES

1. In this respect, Johnson and Ley assert that "a change in the P-F represents the necessary and sufficient condition for inter-paradigm change hence, scientific revolution" [Johnson and Ley, 1990, page 36].
2. Jalladeau similarly allies the "the structure of scientific revolutions" with "the transition from one paradigm to another" [Jalladeau, 1978, page 589], and Tarascio describes a Kuhnian scientific revolution as "a displacement of one scientific paradigm, or way of seeing the world, by another fundamentally different" [Tarascio, 1971, page 102, footnote 6]. Wang describes a scientific revolution as "consist[ing] of the replacement of an established paradigm by a new one" [Wang, 1973, page 152], and Stanfield describes "the ascension to dominance of a new paradigm" as "the consummation of a scientific revolution" [Stanfield, 1974, pages 99-100]. Finally, Kunin and Weaver, succinctly and in passing, define a scientific revolution as "the demise of outworn paradigms and the adoption of new ones" [Kunin and Weaver, 1971, page 392].
3. Like Mehta, Remenyi associates a scientific revolution with "a 'gestalt switch' in how the scientist sees the puzzles to be solved" [Remenyi, 1979, page 31].
4. Similarly, Canterbury and Burkhardt assert that in a scientific revolution, "the fundamental world view, is challenged -- and rejected" [Canterbury and Burkhardt, 1983, pages 20-21]. As well, Foster-Carter notes that once the "scientist puts his 'faith' in the new paradigm," he "sees the world differently as a result" [Foster-Carter, 1976, page 170]. Finally, Dunn describes a paradigm shift as a "fundamental change in perspective and perception" [Dunn, 1970, pages 353-354].
5. Along similar lines, Tony Lawson argues that for Kuhn, non-normal science (i.e., scientific revolutions) involves "qualitative" as opposed to simply "quantitative" change [Lawson, 1987, pages 967-968], and Karsten observes, "for Kuhn, a new theory, [is] 'seldom or never just 'an increment of prior theory,' but is the result of a basic reconstruction of the field from new fundamentals" [Karsten, 1973, page 402, quoting Kuhn, 1970c, page 7].
6. Similarly, Blaug asserts that a "distinctive feature of Kuhn's methodology is . . . that of 'scientific revolutions' as sharp breaks in the development of science . . ." [Blaug, 1976, page 153].
7. Johnson and Ley: "Kuhn admits in the 'Postscript' that scientific revolutions should not imply absolute discontinuities or cataclysmic shifts of scientific activity. This misinterpretation of Kuhn results primarily from his treatment of the P-F. If Kuhn held the implicit P-F discussed above, there are no scientific revolutions. This form of historical objectivism Kuhn clearly rejects. If, then, we assume that he simply ignores the P-F, the alternative interpretation is that Kuhnian scientific revolutions do indeed seem to imply total gestalt shifts, his assertions to the contrary notwithstanding" [Johnson and Ley, 1990, page 175, note 39].
8. See also Dow, 1985, page 28.

9. Coats remarks "As 'normal' research proceeds unexpected or anomalous results appear" [Coats, 1969, page 291].
10. Likewise, Perelman understands scientists' "concentration on details" as resulting "in the accumulation of anomalies," under Kuhn's model [Perelman, 1985, page 101].
11. Similarly Dunn remarks: "One of the striking things about normal science is that it does not aim at producing novelties and when it is successful in its own terms it finds none. It seeks the progressive testing and refinement of the paradigm until its correspondence with nature is perfected. Yet this very process serves to generate novelty" [Dunn, 1970, page 353].
12. In addition to those listed below, we may also include Yong Yoon who observes that under Kuhn's schema, "A scientific paradigm can hold on through anomalies, until a new paradigm replaces it" [Yoon, 1991, page 567].
13. Likewise, Chase observes that under Kuhn's model, "One way or another, dealing with anomaly is avoided or, at least postponed" [Chase, 1983b, page 818].
14. Similarly, Arouh assents that the existence of anomalies *per se* need not, under Kuhn's model, produce a scientific revolution [Arouh, 1987, page 417].
15. Along these same lines, Coats asserts that it is when "the anomalies grow in number and importance" that "they eventually become critical" [Coats, 1969, page 291].
16. Indeed, this condition is required by definition of a scientific revolution as a paradigm shift.
17. Arouh: "Anomalies will precipitate a revolution, when they evoke crisis. Crisis is characterized by a proliferation of competing theories, persistent questioning of the foundations of the paradigm, disagreement, loosening of the rules for normal research, and speculation. As a consequence of these crisis conditions, a gestalt switch is generated eventually" [Arouh, 1987, page 417].
18. Negishi as well specifies the existence of repeated anomalies and an alternative as a pre-requisite to a scientific revolution, but provides no indication that such conditions must generate a paradigm shift: "It should be emphasized that a paradigm cannot be overthrown by a single empirical refutation. It is overthrown in consequence of repeated refutations and mounting anomalies only when a competing, alternative paradigm is ready" [Negishi, 1985, page 4].
19. See also Glass and Johnson, 1989, page 159.
20. Similarly, Jalladeau asserts: "If we grant that the evaluative criterion of scientific discoveries is their problem-solving capacity, then the new candidate for the function of paradigm must be able to resolve an essential question, recognized as such, and which could not be approached in any other way. There will be scientific revolution only if the new candidate succeeds in asserting itself to the detriment of its predecessor" [Jalladeau, 1978, page 590]. See also Webb, 1987, page 405.

21. In this context, we may note that Willett asserts that under Kuhn's mode "Confrontation with an increasing number of anomalies is not in itself sufficient to cause rejection of a paradigm . . ." [Willett, 1970, page 449].
22. Solo: "Each science exists as an island of the explicable within a sea of anomalies and contradictions. Occasionally, very rarely there occurs, for reasons Kuhn does not explain, a shattering transformation of the established paradigm. . . ." [Solo, 1991, page 33].
23. And, we should add, the availability of an alternative to the present paradigm.
24. Kunin and Weaver use the term "historically unchanging" "nature" to connote that "the structure and behavior of the physical universe toward which these sciences [the physical sciences] are directed do not exhibit change on a time scale which would alter significantly any important characteristics of the population of that universe" [Kunin and Weaver, 1971, page 394].
25. Reviewing the history of economics, Goodwin observes, "Certainly there were no crucial experiments . . . as Kuhn perceived them in the physical sciences" [Goodwin, 1980, page 612].
26. Goodwin: "This third type of phenomenon is in some respects analogous to the physical scientist's crucial experiment which jolts the consciousness of a discipline and becomes the trigger for a scientific revolution. An obvious feature of economic science is that opportunities for controlled, laboratory experiments are few and far between. Moreover, use of *ceteris paribus* conditions, and conditional forecasts, protects the scientists from decisive falsification where an hypothesis is unquestionably disproved. However, the lay public in the society in which economists live do not accept these rules for scientific protection. Occasionally they rise up and declare that an anomaly exists, and in effect announce that the Emperor has no clothes" [Goodwin, 1980, page 614-615].
27. Similarly Coats, linking the notion of intellectual crisis with Kuhn's in a footnote, asserts that "Unlike natural scientists, economists are rarely confronted with crises resulting from an accumulation of experimental results which conflict with existing theories; indeed, their theories have rarely been subjected to rigorous empirical testing, and it is consequently more difficult for the historian to determine the precise reasons why one economic theory displaced another" [Coats, 1972, pages 310-311].

In addition, Wisman questions whether empirical testing in economics will generate Kuhnian anomalies because the field's testing does not yield unambiguous results:

Although the extent to which these theories of science might be applicable to economics is questionable, at least two lessons seem pertinent. First, both Kuhn and Lakatos, recognize the importance of empirical testing capable of yielding relatively unambiguous (highly exact) results. Even though testing may not lead to the rejection of theories through falsification, such testing aids in uncovering "anomalies" (Kuhn) or in indicating that the research program is "degenerative" (Lakatos) -- the necessary prelude to revolutions (highly significant scientific progress). But since empirical testing in economics can only yield highly

ambiguous results, it is incapable of acting as an internal motor force for scientific progress. [Wisman, 1979, page 27]

28. Goodwin cites the same three factors listed here as spurs to fundamental change -- though not scientific revolution -- in economics. In addition, as we have seen, Goodwin, unlike Johnson and Ley, allies the third factor "major social and economic convulsions," with anomalies [Goodwin, 1980, pages 614-615].
29. We may recall from last section that Deane interprets Kuhn as requiring both the existence of a methodological crisis and an alternative paradigm, which both successfully deals with problems which had plagued its successor and possesses superior quantifiability.
30. Dillard similarly argues that the genesis of the five revolutions which he identifies in economics history lies with the need for social reform. He, however, makes the observation divorced from any discussion of Kuhn or his applicability to economics [Dillard, 1978].
31. Reynolds: "Both Kuhn's and Bronfenbrenner's explanations of the method of change in economic thought considers the entire body of thought as a single element. But, in fact, there are at least three elements of economics which may change at different rates" [Reynolds, 1976, page 26].
32. Bronfenbrenner: "The difficulty with the catastrophic theory is that, if I understand Kuhn correctly, it maintains that paradigms, once displaced, are displaced definitively and relegated to the antiquarian's dustbin. Ptolemaic astronomy, phlogiston chemistry, and humoral medicine are examples from natural sciences; Social Darwinism may be an example from anthropology and sociology. But in economics, where are their equivalents? Currently fashionable incomes-policy proposals are based on elements of the medieval *justum pretium*. Synonyms for 'forestalling,' 'engrossing,' and 'regrating' grace contemporary trade-regulation decisions by both courts and administrative agencies. Few elements in anyone's form of mercantilism have missed revival in connection with my generation's dollar-shortage, dollar-glut, and world-liquidity crises. A French physiocrat or *économiste* of the eighteenth century is brain brother to an American agricultural fundamentalist of the twentieth" [Bronfenbrenner, 1971, pages 137-138].
33. Echoing Kunin and Weaver's concerns, Reynolds asserts that "since economics is not a 'hard' science, it is a questionable process to uncritically and mechanically apply Kuhn's paradigm shift to economics" [Reynolds, 1976, page 26].
34. Worland: "The distinction between a *policy anomaly* and the kind of theoretical anomaly envisioned by Kuhn as characteristic of a developing physical science, parallels the classic methodological distinction between science and art. Whereas art established an end and searches for the means to achieve it, science takes a cause and tries to uncover its effects. In an open system such as a society, where causal factors embraced by one science interact in a non-systematic fashion with causal factors embraced in another, the cause-to-effect connections discovered by science will not convert directly into the means-to-end rules required for art" [Worland, 1972, page 278].

35. Bronfenbrenner hints that his own crude dialectic framework provides a superior framework than Kuhn's in understanding science in general at both the outset as well as the conclusion of his article. At the outset he remarks, "All this may mean only that economics is a branch of study insufficiently developed, insufficiently innovative, or insufficiently scientific to have undergone scientific revolutions in Kuhn's sense. It may mean that people's individual and class interests have shunted economics off into 'ideology' in the pejorative sense of that term. It may also be that Kuhn has generalized too rapidly from too small a sample of revolutions within the natural sciences which he treats. I shall not venture upon so controversial a controversy as these three subtheses would require. My immediate sympathies are with the third and last" [Bronfenbrenner, 1971, page 136].  
And, in closing he maintains, "About the natural sciences, with their longer history, their easier resort to crucial laboratory experiments, and their more sharply defined paradigms, I am incompetent to venture any similar guess. But perhaps there too, purposeful synopses of the complete history of particular branches of science might yield results more similar to our own for economics than to Kuhn's for science as a whole, or with a capital S" [Bronfenbrenner, 1971, page 151].
36. Kunin and Weaver: "Having drawn attention to the necessity for the historian of social science to be concerned with a social-historical component in tracing intellectual development, one might draw the implication from our argument that the natural sciences may with impunity be treated in a more ahistorical manner. In one sense this may be so . . . But in a different sense these sciences also become historical -- in the sense that the perceptions of scientists take place within and are conditioned by paradigms. Not having direct and complete access to some independently existing material universe, the problem of paradigm relativity remains, and the only road out of this quandary it to introduce a historical dimension into paradigm succession as found in the natural sciences" [Kunin and Weaver, 1971, page 397].
37. Wisman: "The work of Kuhn and of Lakatos has expanded the definition of science to include the context of discovery. However, both, and especially Lakatos, still view progress in science as an internal affair. That is, they do not expand their scope to encompass the dialectical relationship between scientific enterprise and the larger social environment to which it belongs. Consequently, their framework of analysis stops short of an inquiry into the motivating human interests which steer science" [Wisman, 1979, page 27-28].
38. Dow: "The historical dimension of economics alters also the application of the Kuhnian concept of scientific revolutions. If economics must adapt to take account of changes in the economic system, then there will be a continual source of fresh anomalies, and thus impetus towards paradigm shift for that reason alone. *Rather than weakening the applicability of Kuhn's approach to economics, however, this additional feature of economics would seem to strengthen it.* Paradigm shifts have perhaps been more frequent in economics than in the physical sciences. . ." [Dow, 1985, page 36, emphasis added].
39. Here, however, we should note that Zinam maintains that *despite* the greater changefulness of economic phenomena, the field has never undergone a Kuhnian paradigm shift for the reasons we listed above [Zinam, 1978, pages 164ff.].

40. We employed the expressions "marginal (utility) revolution" and "Keynesian revolution" because of their currency in the history of economic thought literature. The terms, however, undoubtedly may be read as carrying with them certain presumptions (e.g., that economics underwent a revolution; that economics underwent a revolution in which marginal techniques or notions (or utility theory or notions) played a significant role or were the outcome of the revolution; that Keynesian economics played a central role in a revolution in economics or was the outcome of a revolution in economics). However, given that such presumptions are counterproductive to the interpretive process, we seek, as much as our own preconceptions allow, to suspend judgment on these and other related matters.
  
41. While Alcouffe assents that the classical school's founder (Adam Smith) espoused a labor theory of value, he asserts "the orthodox economists generally follow[ed] J.B. Say more faithfully" [Alcouffe, 1989, page 337].
  
42. Alcouffe: "The relations between the marginal revolution and mathematics are very complex. Walras and Jevons claimed loudly that the orthodox school's hostility towards marginalism originated in their use of mathematics. But, on closer examination, marginalism went rather unnoticed when it first appeared, and the two sides do not have the clearly defined positions that the reader of Kuhn would be led to expect. Although the Austrians were not hostile to mathematics being used, they questioned its use by Jevons and Walras. . . . In the orthodox camp, the views expressed about mathematics were not clearly defined either. . . ." [Alcouffe, 1989, page 341].
  
43. Alcouffe points out that the French economist Gide, despite being familiar with Walras's work, cites the German Historical School, and not marginalism, as the newly emerging competitor to the classical school [Alcouffe, 1989, page 335].
  
44. Alcouffe: "The historical method contributed to the redefinition of political economy. . . . The historical school, insisting naturally on the historical character of economic categories, tended to abolish this distinction [between science and art] as the 'socialisme de la chaire,' and did more in 'assigning a practical aim to science.' Thus, Cauwès, as early as 1878, insisted on the 'inseparability of science and art.' But as a consequence 'the laws that political economy is interested in, are not the laws of nature,' he wrote, 'but those which are decreed by the legislator,' and as such they can be amended. It was that kind of consideration that led E. de Laveleye to define economics as the 'science which determines which laws men must adopt in order to be able to procure with the least possible effort the greatest number of things to satisfy their needs, in sharing them in accordance with justice, and consuming them according to reason.' It may be cant, but this definition shows that the historical school played a role in the abandoning of the classical definition and in the forming of the conception of economics as the science of 'allocating decisions about scarce resources' as it has often been defined from L. Robbins onwards" [Alcouffe, 1989, pages 336-337, quoting Cauwès, 1893, page 21; and DeLaveleye, 1882, respectively].
  
45. Backhouse: "British political economy, in the 1860s, exhibited some of the symptoms of a Kuhnian crisis. Confidence in the classical system of political economy collapsed, and there was little agreement even on how economic inquiry should be conducted. Economists bemoaned the lack of any consensus on fundamental doctrines" [Backhouse, 1985, page 124].



46. Backhouse agrees "that it was Jevons who played the most distinguished role, both in the attack on the classical regime, and in laying the foundations of what was, eventually, to become a new system of thought" [Backhouse, 1985, pages 124-125].
47. Backhouse: "Although the emergence of a new orthodoxy to replace classical political economy was a slow process, Jevons' ideas taking a long time to gain acceptance, it seems reasonable to refer to the change as revolutionary" [Backhouse, 1985, page 124].
48. Backhouse also never declares that the revolution was *not* a scientific revolution.
49. Backhouse: "Though the work of Jevons, Menger and Walras does, in the light of subsequent developments, mark an important turning point in the history of economic analysis, it is important not to exaggerate the change which occurred. The theory of marginal utility was discovered in the 1830s, and its significance was seen by several economists in the 1850s. In addition there were many economists who stressed demand as a determinant of value, even in England, the home of Ricardian economics. Longfield and Senior, for example, both argued the case for a subjective value theory. The stress on demand as well as on costs in Mill's theory of value marked a significant departure from the abstractions of Ricardian theory. It is, therefore, in some ways misleading to refer to a revolution in the theory of value occurring in 1870" [Backhouse, 1985, page 123].
50. Backhouse notes that despite the fact that the marginal revolution was not a Kuhnian scientific revolution, he acknowledges that "we may learn something in the process of coming to this conclusion" [Backhouse, 1985, pages 8-9].
51. Baumberger concedes that he is "far from arguing" about what he sees to be Michel DeVroey's exposition of the neoclassical revolution which includes the understanding that "the period in question displays a large propositional change" [Baumberger, 1977, pages 11-12].
52. As we shall see, Baumberger describes the classical tradition and neoclassical economics as opponents of one another [Baumberger, 1977, page 10].
53. That economist is Michel DeVroey. See below for discussion of DeVroey's portrayal of the transition from classical to neoclassical economics as a Kuhnian scientific revolution.
54. Baumberger: "On the other hand, 'classical-economics-including-Marx' by no means died after Marx. Just as there is a clear genealogical line from Smith to Marx, there are multiple lines leading beyond them that no less clearly carry on the tradition. To be sure, this tradition is much more dynamic than a rigid Kuhnian 'paradigm,' but this 'tradition in progress' nevertheless constitutes a historical whole. The classico-Marxian ancestry cannot be denied, and its descendants are well known. The first that comes to mind is the Marxist tradition with all its orthodox and revisionist factions, but there is also American institutionalism as exemplified by Thorstein Veblen. Then there is contemporary neo-Marxism. The peace of the neoclassical era has also been somewhat disrupted by the Cambridge post-Keynesians, where the classical tradition is mediated by a group of post-Ricardians" [Baumberger, 1977, page 9].

55. Baumberger: "By refraining from looking at things through the Kuhnian looking glass we notice that (1) both of the traditions in question have been around ever since the birth of the neoclassical approach; (2) the conflict between the two has flared up in various shapes at different moments and is still with us; . . . and (5) the changes in both camps were not independent from each other. Rather, they occurred within a tough and continuing *bataille rangée*" [Baumberger, 1977, page 11].
56. One might read Baumberger's assertion that economics has never experienced a period of normal science as suggesting that the field has been in a continual state of revolution even prior to the neoclassical revolution. In this case, the neoclassical revolution continued (rather than provoked) the revolution in economics. Baumberger, however, provides no discussion concerning any battles that may have raged among schools of thought prior to the 1870s. The lack of normal science under a single paradigm may have been the result of the lack of *any* paradigm in the field prior to the revolution. Baumberger, as we noted, makes clear that the Classical tradition did not comport with Kuhn's description of a paradigm. What is, however, relevant in the present discussion is that if the neoclassical revolution spurred (not simply continued or transformed) any revolution, it was a revolution in permanence.
57. Bicchieri: "Moreover, identification of a scientific 'revolution' with a particular historical moment can easily lead to misplaced emphasis on certain changes in beliefs and practices that seem most clearly to indicate what it was that changed at a definite point in time . . . This may occur at the expense of a failure to recognize truly fundamental changes in guiding assumptions whose full significance only became apparent after a much longer period of development. In my view, much scholarship relating to the 'marginalist revolution' has tended to overemphasize the change in the theory of value, because this change can be observed in several authors at nearly the same time, while the birth of modern general equilibrium analysis -- to which the early marginalists did not equally contribute and the lasting importance of which has become clear only many decades later -- has been relatively underrated" [Bicchieri, 1989, page 237].
58. Bicchieri: "In order to 'test' the adequacy of these different approaches in accounting for our example, it is useful to compare the theories of value classical and the marginalist, because a theory of value (i.e., of the prices of the various produced goods and the rewards of the various factors of production) is regarded as central to the subject matter of economics, both by the classical economists and by the marginalists" [Bicchieri, 1989, page 246].
59. Namely that "the production coefficients are assumed to be independent of the scale of production and . . . labor is the only factor of production" [Bicchieri, 1989, page 247].
60. Bicchieri: "The striking thing about this doctrine [Ricardo's theory of rent] . . . is that it is an early and quite sophisticated example of exactly the kind of marginal analysis that became the basis for a new theory of general factor rewards in the work of Jevons, Menger and Walras. It is possible to argue that the theory of marginal productivity amounts to an extension to all factors of production of Ricardo's treatment of rent" [Bicchieri, 1989, page 249].
61. Bicchieri: "The above account suggests that the neoclassical theory of value is more general than the classical one. This is true even in the special case of the

theory of rent. . . . Ricardo would not be able to obtain determinate prices were he to consider the case of a fixed quantity of homogeneous arable land (exactly the kind of assumption that his method requires him to make about labor), whereas neoclassical theory can handle this case as well" [Bicchieri, 1989, pages 249-250].

62. Here, we need to be careful as to what Bicchieri is arguing. She is not contending that marginalism is more general than classical economics in all respects. To the contrary, she explicitly affirms that classical economics is not simply a special case in marginalist economics. She further acknowledges that marginalism possesses significantly less testable empirical content than classical economics. What she is, however, affirming is that the two theories are not wholly incommensurable, but can be compared with one another in terms of the their treatment of common problem areas. Judged in terms of this overlap, she finds marginalism to possess greater generality.
  
63. In this respect, Bicchieri points out that "it is quite evident that the new theory does not accommodate all the classical observational consequences, nor all of the explanatory successes or solved empirical problems of its predecessor" [Bicchieri, 1989, page 252].  
 Along the same lines, she remarks that "[Marginalist theory] is more precise, not in the Kuhnian sense of accommodating previously solved empirical problems, but in that of being able to give a precise meaning to concepts such as 'equilibrium' . . ." [Bicchieri, 1989, pages 250-251].
  
64. Bicchieri does, however, indicate that their role fits well with the philosopher of science Larry Laudan's distinction between threatening and non-threatening anomalies and corresponding contention that not all anomalies cause problems for a regnant paradigm [Bicchieri, 1989, page 252].
  
65. Blaug: "Not for nothing do we speak of a 'marginal revolution' and not a 'marginal utility revolution' but marginalism as a paradigm of economic reasoning is a 20th-century invention; there is as much marginalism in Ricardo as in Jevons or Walras but it is applied to different things" [Blaug, 1985, page 306].
  
66. Coats: "And while there was no disposition to apply either Kuhn's paradigm concept or his theory of scientific revolutions to the new economic theories of the 1870's, the Kuhnian framework helped to sharpen the focus of our discussion by pinpointing relevant questions. For example, how far did the marginal revolution constitute a break with the past? How far were any or all of the key concepts already present in the classical literature? Was there an intellectual 'crisis' resulting from the acknowledged deficiencies of the cost-of-production approach to value? Did the new theories eliminate inherited theoretical or empirical anomalies, or did they merely entail a shift of attention to new, hitherto unrecognized problems? The list could, of course, be greatly extended . . ." [Coats, 1973, page 338].
  
67. Deane: "Briefly, the marginal analysis is designed to find the most efficient allocation of competing resources, of scarce means with alternative ends. At the optimum position marginal values are equalised . . ." [Deane, 1978, page 98].

68. It was, Deane explains, the quantification of marginal analysis that expanded its applicability [Deane, 1978, page 98].
69. Deane asserts that "As for the incidence of 'methodological crisis,' there is plenty of evidence for a sharpening of the debate on the scope and methodology of mainstream economics in the 1870s and 1880s" and cites Jevons' and Bonamy Price's discontentment with orthodox economics as indications of this crisis. While Deane assents that controversies had existed among Classical economists earlier, she asserts that directly prior to the marginal revolution, "the area over which the protagonists would admit agreement was generally narrower, the loss of professional prestige was greater and the resolution of the conflict had wider implications for the orthodox view of the methodology and scope of economic science" [Deane, 1978, page 100].
70. Backhouse, we will recall, draws a distinction between Britain, where a crisis was evident and the Continent, where there is little indication of a crisis.
71. Deane: "Armed with this technique the neo-classical economists were able to produce a logically consistent explanation of the determination of commodity and factor prices in a market system and to define the conditions for maximising consumer satisfactions. They were even capable in principle of quantifying inputs and outputs into the economy at micro and macro levels, for by defining value as equivalent to price in a perfectly competitive market they could measure the value of consumers' satisfactions and the marginal product of labour or capital in objective additive terms. The analytical power and range of the new technique, the plausible simplicity of its basic assumption -- that consumers and producers would naturally behave so as to maximise their satisfactions or profits in a competitive market -- was immensely attractive to students of 'pure' economics . . ." [Deane, 1978, pages 98-99].
72. Given DeVroey's explanation that it is social factors that spurred the revolution, it is interesting to recall that he saw social factors under Kuhn's model as forestalling (not pushing for) change. DeVroey, however, makes no comment about the discrepancy.
73. DeVroey: "The analysis will be structured in Kuhnian terms, but *supplemented* with the political connections which will be outlined hereafter. We will argue that this transition exhibits the characteristics of a scientific revolution, in the Kuhnian sense, and we will defend the view that the occurrence of this revolution is fully understandable only if account is taken of its political dimension" [DeVroey, 1975, page 416, emphasis added].
74. This is perhaps not surprising given that Dillard's chief objective is to demonstrate the link between calls for social reform and revolutions in economics (not to fully characterize those revolutions, nor their effects, nor assess the applicability of Kuhn's model to them).  
Nonetheless, given that Dillard cites Kuhn in another context in the same article (in assessing the likelihood of future revolutions in economics) [Dillard, 1978, pages 715-716], it is significant that he does not bring the philosopher's schema to bear upon this or any of the four other revolutions he identifies in economics' history.
75. Ekelund and Hébert: "Interpreting the development of economics along these lines, we might be tempted to argue that neoclassical analysis . . . emerged

around 1870 as a result of the intellectual bankruptcy of classical economics. By this reasoning, the breakdown of the wages-fund doctrine fore-shadowed the emergence of a new paradigm" [Ekelund and Hébert, 1990, pages 11-12].

76. Ekelund and Hébert: "Thus, one could conceivably lump seemingly diverse approaches, e.g., classical, neoclassical, Keynesian, and others, into a single paradigm called 'equilibrium economics.'" [Ekelund and Hébert, 1990, page 12].
77. Ekelund and Hébert: "This explanation is appealing on the surface, but it glosses over certain gnawing problems. For example, how does one appropriately identify a paradigm in economics? What precisely is 'a body of interrelated principles'?" [Ekelund and Hébert, 1990, page 12].
78. Goodwin: "By the term 'marginal revolution' is meant in particular two characteristics of the work of the English, Austrian, and French 'marginalist' writers: first, a new emphasis on the place of the individual utility function in the theory of price, and second, use of precise incremental analysis for the study of human behavior and markets" [Goodwin, 1972, page 551].
79. Goodwin: "But where does the history of economics most obviously not conform to the models of Kuhn and Lakatos? First, with respect to Kuhn, one must struggle very hard to identify candidates for truly revolutionary episodes (in the Kuhnian sense): perhaps marginalism, maybe Keynes, but arguably not even these. Certainly there were no crucial experiments or total gestalt switches as Kuhn perceived them in the physical sciences" [Goodwin, 1980, pages 611-612].  
Commenting upon various revolutions in economics (including the marginal revolution), Goodwin states: "What is common to all these incidents is the circumstance that events outside the discipline precipitated a major change in the basic core principles upon which some or all of the parts of the science operated. The changes were more fundamental than mere artifactual innovation in a protective belt. Yet no Kuhnian revolutions took place or Lakatosian research programs replaced less successful competitors" [Goodwin, 1980, page 616].
80. Jalladeau: "The advent of a paradigm must be considered as revolutionary in its theoretical implications independently of the time it takes to assert itself. The revolutionary character of marginalism lies more in the method of advanced analysis and in the identification of its subject matter than in its genesis and the rapidity of its diffusion" [Jalladeau, 1978, page 598].
81. Jalladeau: "While the classical economists viewed distribution as the result of the interplay of institutions and social relationships as well as market forces, the neoclassicists saw it as determined by the conditions of exchange. General equilibrium theory shows that remuneration for factor services is determined in the same way as the price of goods, that is, simultaneously in markets where competition tends to equalize permanently supply of and demand for each kind of service" [Jalladeau, 1978, page 596].

Jalladeau: "In the new theory of distribution which was going to be developed, income depends on the productivity of the factors of production. As a consequence, taking into account the socioeconomic relationships between the contributors of the means of production became useless. Classical political economy and neoclassical economics appear as two distinct theoretical structures

as far as determining exchange phenomena and income distribution is concerned" [Jalladeau, 1978, page 596].

82. Jalladeau: "Viewed in this way, the neoclassical system tends to detach itself radically from the classical structure. The nodal point of this new analysis is no longer the dynamic problem of the long-term development of the economic system but the static question of economic efficiency" [Jalladeau, 1978, page 603].
83. Jalladeau: "The marginalists did not want to extend classical economic thought. In fact, the neoclassical system is completely different from the previous theoretical structure. Because they regarded the classical theory as a failure, the marginalist authors devised another value theory and another model. We know that for the classical writers value had an objective foundation; the marginalists substituted a subjective foundation. The classical premise according to which production cost determines final value is turned around by the marginalists; producer goods derive their value from final goods" [Jalladeau, 1978, page 601].
84. Jalladeau: "The question of what role and importance must be accorded to various exogenous influences that may be found relevant to the historical formation of economics is always controversial. In my view, it is the *discontinuity* discernible in the transformation from classical to neoclassical economic thought which must be emphasized here" [Jalladeau, 1978, page 601].
85. While Johnson and Ley do directly speak of a paradigm shift during this period, they never directly refer to events comprising a scientific revolution. However, previewing their discussions of events in economics at the turn of the century and in the 1930's, they affirm: "In the past century or so, the discipline has undergone two scientific revolutions" [Johnson and Ley, 1990, page 119].
86. Johnson and Ley set forth two major suppositions of the marginalist economics: (1) "the assumption that people acquire commodities only because they expect to receive some utility for using them" and (2) "marginal utility (the extra satisfaction received from an additional unit of a good) diminished" [Johnson and Ley, 1990, page 122].
87. The reader will recall that a change in a discipline's P-F is both a necessary as well as a sufficient condition for a paradigm shift, which, for Johnson and Ley, amounts to a scientific revolution.
88. Johnson and Ley's contention resonates to some degree with DeVroey's depiction of events in economics in the latter end of the 1800's. A number of differences do, however, exist. First, Johnson and Ley portray neoclassical economists responding to the labor theory of value anomaly as neither apologists nor handmaidens to apologists of the status quo. Instead, they see advocates of the new paradigm as advocates of gradual social reform as opposed to radical change. There is, however, a gray line separating supporters of slow social change from those seeking to bolster the status quo. Indeed, given that the status quo does not constitute a static entity, those advocating gradual reform might just as well be understood as supporters of the course of change mediated by the extant social superstructure.

89. Khalil: "Assuming that ideology actually was behind the desertion of classical economic thought, could this be a la Kuhn? DeVroey sees the political implications of a labour theory of value as the anomaly. However, an anomaly in Kuhn is an unmanaged 'puzzle' for people who are practising 'normal science.' An anomaly could not be liking or disliking certain explanations, but would be rather the inability of a paradigm to handle a pressing counter-instance" [Khalil, 1987, page 120].
90. Khalil uses the term "type" in place of both Kuhn's term "paradigm" given the presumptions and problems attached to the paradigm notion.
91. Khalil: "The ground of judgement can be argued after a proper understanding is reached of the difference and identity of the two systems of thought. The over-simplified exercise just sketched attempts to show that one could compare and choose between competing types by providing conceptually constructed arguments" [Khalil, 1987, page 127].
92. O'Brien, unlike so many others, makes no comment as to the relevance of the lengthiness of the switch in assessing Kuhn's applicability to it.
93. O'Brien: "Moreover there was, at least in the British Isles, some sense if not of 'crisis' at least of *decay* in the subject of economics before the publication of Jevons' *Theory*. . . . Another of Kuhn's characteristics of paradigm change is present during this era: that the solutions had all been at least partially anticipated (by Senior, W.F. Lloyd, and Longfield). In addition there really does seem to have been a major communication problem between the old and new schools during this era, as Cairnes's total incomprehension of Jevons' work makes very clear . . ." [O'Brien, 1976, page 143].
94. Perelman: "Why was so much time required to work out the details of neoclassical or neo-Ricardian theory? Everything had already been laid in place by the 1830s" [Perelman, 1985, page 101].
95. Reynolds: "The adjustments can be of two types. First, they may be of a technical nature, which would allow the original conceptual framework of, say, the maximizing self-interest to continue (as was the 'Utility Revolution' of the 1870s and the 'Keynesian Revolution' of the 1930s). Secondly, they may be of a truly revolutionary character, in which case propositions in the conceptual framework are displaced" [Reynolds, 1976, page 31].
96. Seligman: "Though challenged later by Karl Marx and others, the classical paradigm appeared invulnerable, especially after it was buttressed by the Austrians and by such 'analysts' as Cournot, Walras, Jevons, Pareto and Wicksell. It seemed that their economic analyses most effectively and realistically described what occurred in the market and that their theories suggested an extraordinary capacity to predict the most likely outcome of any course of action. These theories were declared valid because they could ingest the universe of economic fact. Quite simply, the economic order functioned autonomously once the Prime Mover had acted. A kindly providence insured corroboration everlasting for the lucubrations of the economist" [Seligman, 1971, pages 2].
97. What constitutes Classical theory's "essential elements," Stigler does not specify here.

98. Stigler never explicitly claims that the change did not amount to a paradigm change. However, the following strongly makes that implication: "If, on the contrary, large changes in the science per se constitute a revolution, Kuhn asserts that there will be an abandonment of the previous paradigm which in actual fact may never have taken place. To be concrete, the marginal utility revolution of the 1870s replaced the individual economic agent as a sociological or historical datum by the utility-maximizing individual. The essential elements of the classical theory were affected in no respect" [Stigler, 1969, page 225].
99. The proposition, Thompson notes, is known in abbreviated form as "Demand for commodities is not demand for labor" [Thompson, 1975, page 174]. As a more complete statement of the proposition, Thompson quotes Mill: "'What supports and employs productive labor, is the capital expended in setting it to work and not the demand of purchasers for the produce of the labor when completed. Demand for commodities is not demand for labor. The demand for commodities determines in what particular branch of production the labor and capital shall be employed; it determines the *direction* of the labor; but not the more or less of the labor itself, or of the maintenance or payment of the labor. These depend on the amount of the capital, or other funds directly devoted to the sustenance and remuneration of labor'" [Thompson, 1975, page 176, quoting Mill, 1965, page 78].
100. Marshall and Pigou had done so in the process of demonstrating that the corollary to Mill's proposition ("that spending on the direct hire of labor is more advantageous to the laboring class than spending it on commodities" [Thompson, 1975, page 176]) did not logically follow from the proposition [Thompson, 1975, page 187].
101. West: "There seems to be no provision within Kuhn's apparatus for the old paradigm to come back and replace the new one" [West, 1978, page 348].
102. At the outset of his discussion, West asserts that "Several other writers have recently been defending Smith against neo-classical 'victory claims,' and these will be referred to later in this survey. Most of them would argue the return or resilience of the Smithian paradigm in the *twentieth* century. Smith's champions will find more significant O'Brien's first, not second thoughts, namely that Kuhn's system includes no provision for old paradigms to come back and replace new ones" [West, 1978, page 349].  
Then, in apparent reference to this contention, he later remarks, "Samuelson, in contrast, concludes that Smith's 'pluralistic supply-and-demand analysis in terms of all three components of wages, rents and profit is a valid and valuable anticipation of general equilibrium modelling.' Samuelson's use of the word 'anticipating' is interesting. It suggests that he would not be among the 'supporters' of the model of the sudden Kuhnian scientific revolution described earlier, at least as it applies to Smith" [West, 1978, page 355].
103. Zinam: "Periods between these restatements have been punctured by the emergence of several dissenting schools challenging orthodox theory. However, they were absorbed into the mainstream of economic theorising in an unmistakably dialectical pattern. At least, the integration of some mercantilist with physiocratic thought by Adam Smith and the events called Marginalist and Keynesian 'revolutions' can be interpreted in dialectical terms" [Zinam, 1982, page 370].



104. A partial listing of the different changes which the economists discussed here have argued the marginal (utility) revolution effected include: a redirection of economists' attention away from the examination of immutable natural laws to the study of the allocation of scarce resources; a movement away from policy concerns toward efforts to identify universally applicable laws; the heightened prominence, importance and/or utilization of marginal techniques; the emergence of general equilibrium theory; a move away from dynamic to static questions; the marriage of real cost theory with utility theory; the rise of subjectivism; a shift of concern away from social welfare toward individual satisfaction; a shift from questions of growth towards ones of efficiency/optimization; a narrowing in economics' focus in which consideration of class, class conflict and social power were cast aside; a shift in economics' central concept from capital to price; some combination of the foregoing; a minor adjustment to economics' extant paradigm; a strengthening of that paradigm.
105. In this respect, we may cite economists' differing assessments of the extent and nature of change effected in economics by the heightened prominence, importance and/or utilization of marginal techniques; the rise of subjectivism; and the marriage of real cost theory with utility theory.
106. In this respect, Arouh remarks: "Given the force of Keynes's criticism and the impact of his message at the theoretical, methodological, and policy levels, it seemed likely that economics was ready for a Kuhnian revolution. After the 'scientific crisis' had died out, the discipline should have established a new, genuinely dynamic paradigm that recognized time and uncertainty as the focus of its being" [Arouh, 1987, page 401].
107. Arouh: "If, therefore, time and uncertainty constitute the message of Keynes's (as opposed to the Keynesian) revolution, then there is no doubt that it undermined the methodological and substantive assumptions of 'classical' economics. Once time is accepted as a historical and real dimension that has unidirectional irrevocability, then models that rely on certainty and equilibrium become incompatible. . . ." [Arouh, 1987, pages 400-401].  
Arouh: "Keynes's MSP is not only critical, but offers an alternative to the 'as if' timelessness in economics that is based on uncertainty. It substitutes 'rational,' postulated behavior for sensible, real behavior. The rational economic man becomes the real-life entrepreneur who faces complete uncertainty and thus falls back on what is commonly sensible and conventional to expect. He may be surprised, but given complete uncertainty it is the only sensible, but not necessarily rational, thing to do" [Arouh, 1987, page 399-400].
108. Arouh: "The neoclassical synthesis, by suppressing Keynes's methodological and substantive criticism, regressed to the 'classical' MSP. . . . This new classicism takes the methodological form of positivism, instrumentalism, and naive falsificationism, while at the substantive level it takes new forms of rationalism. Keynes's contribution is seen as a macro-view of the economy that implies imperfections and rigidities in market mechanisms and their coordination. Once these are removed, hydraulic Keynesians enter the 'classical' world of timelessness" [Arouh, 1987, page 396].  
Arouh: "As for the anomaly of uncertainty, it remained a nagging puzzle in the periphery of the paradigm" [Arouh, 1987, page 417].

109. Arouh: "This reluctant acknowledgment of time and uncertainty has not created a methodological and substantive gestalt switch among economists to another MSP, essentially different from the 'classical' one that Keynes criticized. . . . Time and uncertainty became a special case of the Walrasian tâtonnement mechanism. Very much as competition became a special case of 'situational determinism,' the Cambridge critique on capital a mere technicality, so Keynes's time and uncertainty became a special case of the Samuelsonian and Hicksian neoclassical synthesis. As in imperfect competition and reswitching, however, Keynesian uncertainty cannot be accommodated within the neoclassical synthesis, but can only critically undermine its methodological and substantive foundations" [Arouh, 1987, page 403].
110. Canterbury and Burkhardt, likewise, maintains that economics' orthodoxy had pre-empted the Keynesian revolution by having "co-opted" Keynes' economics: "Nor do we need to note how numerous are the research puzzles and problems that have been generated at the protective belt, so that challenges to the fundamental world view have rarely if ever been taken seriously, at least by 'real economists.' Even the so-called 'Keynesian revolution,' a presumed paradigm shift, has been successfully aborted insofar as Keynes's vision has been co-opted to a large extent by the dominant neoclassical orthodoxy" [Canterbury and Burkhardt, 1983, pages 22-23].
111. Bornemann: "*The General Theory of Employment, Interest and Money* published by Lord Keynes (John Maynard Keynes) in 1936 replaced classical economics and Say's Law as the leading paradigm. In classical economics the individual entrepreneur's output based on the firm's least-cost combination of factor inputs initiated the circular flow of economic activity which culminated in a general equilibrium of maximum output for the entire market economy. Any break in the circular flow was caused by misdirected production and only temporary. In contrast, Keynesian macroeconomic employment theory emphasized aggregate income and money demand rather than enterprise and production as essential both to initiate and to close the circular flow. Deficiency of aggregate demand resulted in the economy's chronic failure to operate at the level of full employment. Since excess saving resulted from lack of investment opportunity, government fiscal policy involving spending was essential to achieve full-employment Gross National Product (GNP)" [Bornemann, 1976, pages 125-126].
112. Bronfenbrenner does, however, point out that the antithesis has much earlier roots in the economics of Marx, Malthus and even the mercantilists [Bronfenbrenner, 1971, page 145].
113. Coats provides no explanation as to how it is that the Keynesian paradigm was "not 'incompatible' with its predecessor." Nor does he cite any authority to support this contention; he rather affirms its incompatibility as a matter of established fact [Coats, 1969, page 293].
114. While Deane agrees that Keynes "made frequent use of the concept of equilibrium in his *General Theory*," she stresses that "it was a short-period equilibrium rather than a long-period equilibrium that he had in mind" [Deane, 1978, page 182].

115. Deane: "Whatever the Keynesian Revolution did then it did not displace the neo-classical paradigm in the standard textbooks for that remains the foundation of a wide area of orthodox economic theory today" [Deane, 1978, page 205].
116. Dow: "The persistence of the neoclassical orthodoxy in the face of the development of thought in the 1930s with respect to imperfect competition and the operation of the macrosystem can be seen to derive from the technical framework in which they came to be expressed. To state the former in terms of cost and revenue functions and the latter in terms of IS-LM functions is to obfuscate the alternative paradigms in the *Weltanschauung* sense originally intended by Robinson and Keynes, respectively" [Dow, 1980, pages 377-378].
117. Dow: "Weintraub does not interpret that Keynesian/neoclassical split as reflecting competition between paradigms. This perspective results directly from his concentration on only that part of macroeconomics which is susceptible to general equilibrium analysis -- neoclassical macroeconomics. The macroeconomics he considers, together with neoclassical microeconomics, constitute one paradigm: a common ideology expressed in a common, general equilibrium, framework" [Dow, 1981, page 330].  
     Dow: "If Keynesian macroeconomics were to be taken at all seriously, it would be clear that it contains the denial of neoclassical economics at the micro, as well as the macro, level" [Dow, 1981, page 331].
118. Ekelund and Hébert: "Thomas Kuhn, in his seminal work entitled *The Structure of Scientific Revolutions* . . . notes that the introduction of new paradigms of thought, such as the Keynesian model, comes about when old paradigms are no longer capable of providing good answers to the questions posed to them. But old paradigms may be firmly entrenched, and their defenders may rise to protect them and perhaps to show that a paradigm that is considered new may really be just a subset of an old one (thereby, of course, renewing and rejuvenating it [the old paradigm]). It appears that, to a certain extent, Kuhn's theory of the nature of ideational progress fits the case of J.M. Keynes" [Ekelund and Hébert, 1990, page 529].
119. Johnson and Ley: "The central feature of the rejection of neoclassical economics was the abandonment of its P-F. While Keynesians continued to be concerned with human welfare, they saw well being as depending far more on the provision of adequate employment opportunities than on allocative efficiency" [Johnson and Ley, 1990, page 133].
120. Johnson and Ley: "Contemporary economics, as taught in most principles' courses, is the product of a 'marriage' between Keynesian and neoclassical economics. The micro/macro distinction is based on this division. The marriage is made possible by the existence of some of the common features . . . . At the same time, the distinct P-F's of the two paradigms makes the union an unhappy one" [Johnson and Ley, 1990, page 144].
121. Khalil: "Even if Keynes represented a major rupture, he was immediately absorbed by the orthodox profession. . . . Keynes novelties were subverted from the start to the extent that the 'neo-classical synthesis' was called 'bastardized Keynesian' economics by Joan Robinson" [Khalil, 1987, page 121].

122. Mehta: "In presenting the theory of Soddy the object is not to disinter another obscure anticipator of Keynes. On the contrary, the object is to relate the work of Soddy to the work of many other writers who contributed to the formation of a group-licensed perceptual style that made it possible for other economists to question the 'objective' view of reality represented by the classical paradigm. Instead of saying that many of these writers anticipated Keynes it is more accurate to say that Keynes was influenced by them. This is not merely logomachy. The individualistic approach with its emphasis on anticipators implies that an individual scientist has a *tete-a-tete* with nature. If he is clever he will be able to penetrate her secrets. Keynes never had such an encounter with nature. He did not approach economic reality with a Lockean *tabula rasa*. He studied economic theory in a definite social environment. He internalized group-licensed perceptual styles. As late as 1922 or 1923 Keynes had not made the Gestalt switch. During the twenties he read many of the writers that have been mentioned here. There is no doubt that it is the influence of all these writers that made him consider whether the 'objective' view was so 'objective' after all" [Mehta, 1978, page 86].
123. Mehta: "Consequently, the fact that the attacks on the classical theory of employment and output were first made by men who could not be regarded as normal scientists does not attenuate the force of Kuhn's arguments" [Mehta, 1978, page 62].
124. Mehta: "The main reason why the *Treatise* created so much confusion and bewilderment is that it advanced a theory of the forces determining output and employment" [Mehta, 1978, page 149].
125. Mehta: "As we have seen, the main pillar of the Smith-Say-Mill paradigm was the proposition that supply creates its own demand. A corollary of this view is the denial of the existence of equilibrium income. In the Keynesian system, there is a unique equilibrium level of income determined by the intersection of the aggregate demand and supply schedules. The concept of equilibrium income, and the idea of the relationship between saving and investment on which it is based cannot be meaningfully formulated within the domain of Sayian economics" [Mehta, 1978, page 23].
126. Speaking with reference to the state of economics around the time of Keynes, Routh remarks "While the inhabitants of the world were suffering this disaster, economists, as we have seen, were preoccupied with conditions of maximisation under perfect and imperfect competition and the distinction between cardinal and ordinal utility" [Routh, 1989, page 286].
127. Rugina: "In economics it was Keynes who in this century shifted the system of reference from the classical, abstract, hypothetical model of stable equilibrium to a new, for his time, more realistic and relativistic model of disequilibrium or unstable equilibrium conditions" [Rugina, 1986, page 41].
128. Tisdell: "In terms of methodology, Rugina differs from Kuhn's interpretation of scientific revolutions. While Kuhn considers that a new paradigm negates the earlier one or ones, Rugina rejects this view. In Rugina's view all scientifically true theories are valid in their own habitat, that is in circumstances where their assumed environmental or surrounding conditions are satisfied. It follows that different scientifically true economic theories have applicability in different

- contexts. Thus the Keynesian paradigm does not negate the classical one. Each has its own area of relevance" [Tisdell, 1987, pages 41-42].
129. Stanfield: "Prior to the Great Depression, then, there was a crisis in evidence. It probably was caused by the sheer momentum of the time period in which the anomaly resisted resolution and by the articulation of the existing paradigm by the neoclassicists. The Great Depression probably accelerated the crisis and shortened the paradigm battle after the appearance of the *General Theory*" [Stanfield, 1974, page 102].
  130. On the basis of a quantitative analysis, Galen Burghardt reaches much the same conclusion. The monetarism which prevailed prior to Keynes differed from the monetarism which emerged after him; earlier monetarists more widely espoused the Fisherine brand of monetarism, whereas the more recent monetarism follow the Cambridge line [Burghardt, 1975]. For discussion of the "monetarist counterrevolution," see also Herman, 1984 and H. Johnson, 1971.
  131. Stanfield: "Nor should the monetarist counter-revolution be viewed as a negation of this point. Linkages between the pre-Keynesian monetarists and the modern monetarists are not so strong as the term *monetarist counter-revolution* suggests. The *fundamental* change in world view evoked by the Keynesian revolution is the attention centered on the level of employment, income, and output, not on any particular explanation of this level nor on any particular policy to achieve a desirable level. Indeed, with "stagflation" the primary anomaly, the current scene is best viewed as a new period of extraordinary science with the monetarist and structuralist paradigms challenging the Keynesian orthodoxy" [Stanfield, 1974, pages 104-105].
  132. Ward: "The 'Keynesian revolution' did clearly bring the study of variations in the level of aggregate output into the center of conventional economics. In our asserted hierarchy of contemporary fields in Chapter 1, macroeconomics, which embraces this topic as its central problem, is now right up there with the leaders, whereas a decade or two before Keynes it was close to being a Class D field. Furthermore, Keynes played a very important role in developing a theory of money in terms of supply and demand, so that one major anomaly of the older theory has virtually disappeared. Quite aside from the political and policy impact, Keynesianism has dramatically changed some of the major ways in which economists view their subject" [Ward, 1972, page 38].  
 Ward: "As an intellectual byproduct of this, his greatest essay in persuasion, mainline economics was forced to rethink the whole area of aggregative economics, money, and capital theory, so as to incorporate changes in the level of output into the picture. This was a difficult process and took the better part of two decades. At its end much of what Keynes proposed in the *General Theory* had either been dropped or remained controversial, but aggregative economics and most of its key concepts, such as money and savings and investment, would never be the same again. Within economics the Keynesian revolution was definitely a Kuhnian revolution, though revolution is too strong a word to apply to the Keynesian impact on western economies and politics" [Ward, 1972, page 40].
  133. Weintraub: "Kuhnian science, a developed science at any rate, cannot have coherent paradigms coexisting for any length of time. Dow, believing Kuhn's framework to be the way to tell my story, ignores the simultaneous burgeoning of the neo-Walrasian analysis *and* Keynesian analysis in the 1930s. She must,

and does, tell a story of a Walrasian (Classical) view, a Keynesian revolution, a neo-Walrasian counterrevolution, and a post Keynesian (counter-counter (?) revolution) restoration.

"Such a view does some violence to the modern history of our discipline. Many facts don't fit the story. The *Ergebnisse* papers, for instance, appeared *before* the *General Theory*. Kurt Gödel (!), in 1934, had already suggested the kinds of questions that would be asked in the 1970s. Hicks, Lange, Klein, and Patinkin formed a neo-Walrasian line which was perceived, correctly at the time, to support Keynesian ideas. In Dow's Kuhnian terminology Hicks must have been a fifth-columnist or a (counter) revolutionary vanguard.

"I reconstructed the history differently. Having identified the neo-Walrasian and Keynesian programs, I could describe the development of the neo-Walrasian program without having to deny the simultaneous existence of a Keynesian alternative" [Weintraub, 1983, pages 297-298].

134. Worland: "In instances such as these [including the ambiguity of the Keynesian underemployment equilibrium], articulation of the basic paradigm has eventually resolved the difficulty so that the anomalous came to appear as a special case, recognition of which served to clarify and extend the basic paradigm" [Worland, 1972, page 276].
135. Bronfenbrenner does, however allow, "A partial exception to this generalization is the Ricardian landlord, who gains by the 'niggardliness of nature,' and whose rising claims will eventually choke off both profits and progress" [Bronfenbrenner, 1971, page 142].
136. Johnson and Ley: "From this setting came the economic analysis of the Physiocrats, designed to explain the general laws governing the workings of the economy as a whole. This analysis was thought to imply policies which would maximize social welfare. Based on their conception of natural law as determined by divine providence and implemented by the Crown, the Physiocrats sought to develop a theory of an ideally functioning capitalist economy which, freed from mercantilist constraints, would achieve the greatest possible welfare for France's populace" [Johnson and Ley, 1990, page 91].
137. O'Brien: "It is perfectly possible to regard *The Wealth of Nations* as providing a paradigm -- that of self-interest pursuit and decentralized decision taking in a growth context viewed as producing a relatively best state of affairs and relatively efficient allocation of resources" [O'Brien, 1983b, page 103].
138. Seligman: "Employing a model like Kuhn's, it would not be difficult to demonstrate that pre-18th century economic doctrine gradually became 'Ptolemaic' in its incapacity to deal with newly evolving fact. Adam Smith was so well received -- even eagerly awaited -- because he offered in place of earlier theory a new conception that seemed to exhibit a Copernican power to explain the wealth of nations. It was a new paradigm in economics that successfully incorporated new facts into its model and gave economists new rules for research" [Seligman, 1971, page 2].
139. O'Brien: "But, supposing that we do accept all this, there seems to be no provision within Kuhn's apparatus for the old paradigm to come back and replace the new one. Yet, as we have seen, this is effectively what happened in

the course of the development of classical economics with the return of Smith's influence to predominance" [O'Brien, 1983b, pages 103-104].

140. O'Brien does, however admit "*an element* of incommensurability; for when McCulloch attempted to introduce Ricardian corn model elements into the Smithian growth model, they failed to graft" [O'Brien, 1983b, page 104].
141. Along similar lines, Blaug, while finding that "The Ricardian system was itself a 'progressive problem-shift' in the Smithian research programme," asserts that "The 'hard core' of Ricardo is indistinguishable from that of Adam Smith" [Blaug, 1976, page 165].
142. In using the term, Wang references Kuhn [Wang, 1973, page 151].
143. Wang does allow two alternative interpretations of the inflection point: (1) "the date of maturity in the economics profession when the majority of economists have made realistic appraisals of the econometric approach and achieved proper recognition of its merits and limitations in differing problem areas" and (2) "the date when the economists start to feel or actually confront the declining number of interesting subjects still left for econometric analysis" [Wang, 1973, page 160].
144. In contrast to Wang, who assumes that a revolution occurred in economics and employs quantitative methods to locate the revolution's date, Michael Lovell maintains that various quantitative indicators (annual number of articles published, AEA membership, number of PhDs) "are so gross as to conceal" any revolutions in the field's history [Lovell, 1973, page 27, note 2].  
Here, it is important to note a difference between Wang's and Lovell's operational definitions of revolution. The former operationally defines a revolution as the inflection point on a curve, which must by design have an inflection point. The latter, on the other hand, characterizes a revolution as a short time over which there is a rapid increase in the number of articles, AEA members, etc. -- which need not have occurred. Thus, while under Wang's analysis one will necessarily locate a revolution, under Lovell's, one need not find any evidence of one.
145. However, we must note that Ward does assert that there were connections between the formalist and the Keynesian revolutions [Ward, 1972, page 40].

## CHAPTER SIX: CONCLUSION

The foregoing chapters provide striking evidence that Kuhn's *The Structure of Scientific Revolutions* has held a variety of different meanings for economists. Having interpreted Kuhn's text from different vantage points, economists have provided divergent characterizations of each of Kuhn's three major concepts: "paradigm," "normal science," and "scientific revolution." Further, in applying those varying conceptions to broadly different understandings of "economics," economists have forwarded starkly different "Kuhnian" portrayals of their discipline. In short, owing to their selective perception of Kuhn's text and economics, economists have forwarded multiple interpretations of Kuhn's notions as well as of economics in their application of those notions.

For anyone hoping that examination of economists' interpretations and applications of Kuhn's notions to economics would provide a clear and straightforward understanding of Kuhn's text, the economics discipline and/or Kuhn's relevance to economics, these findings are rather disappointing.

Economists offer a host of different definitions of "paradigm." They identify a wide range of different mainstream economics paradigms. They disagree regarding the paradigmatic status of a given notion, method or worldview. They diverge in their specifications of a given heterodox school's paradigm -- and even their judgments as to whether some heterodox schools comprise/possess a paradigm. They differ in their assessments as to whether paradigms in general and economics paradigms in particular help or hinder scientists (economists) in their work. They disagree as to the



implications of Kuhn's ambiguous use of "paradigm" and the differences between the natural and social sciences for Kuhn's applicability to economics.

Economists highlight different aspects of Kuhnian normal science. They disagree as to whether economics constitutes a normal science. Economists who do identify economics as a normal science provide divergent accounts of economics as a normal science.

Economists disagree regarding the degree and permanence of change implied by a Kuhnian scientific revolution. While agreeing that anomalies spur scientific revolutions under Kuhn's schema, economists offer several different explanations as to what constitutes an anomaly. While agreeing that crises precede scientific revolutions under Kuhn's model, economists disagree as to whether a crisis must necessarily induce a Kuhnian scientific revolution. Economists who agree that economics has not undergone a scientific revolution/paradigm shift since the 1700s disagree as to what economics' paradigm has been and why that paradigm has never been displaced. Further, given economists' multiple interpretations of economics' paradigm as well as "scientific revolution," economists have diverged in their identification of the changes a given putative revolution in economics has effected, in their assessments as to the nature and degree of those changes, and their determinations as to whether those changes comprise a scientific revolution.

These findings are not peculiar to economists' application of Kuhn. Economists' applications of other philosophers of science have also given rise to multiple interpretations of economics. For example, just as economists have provided a host of different specifications of mainstream economics' paradigm employing Kuhn's model of science, they have likewise described economics' mainstream *research programme* along very different lines employing Lakatos' model [e.g., Diamond, 1988; O'Brien, 1983a; Blaug, 1976]. Second, economists are not the only ones who have arrived at different specifications of their discipline employing Kuhn's

model. The same, for instance, is also true of political scientists [Planinc, 1992]. Third, economists' multiple specifications of economics employing Kuhn's concepts must be juxtaposed with the diverse ways in which they have understood such commonly employed notions as "competition," "rationality," and "value."

Viewed in this larger context, the finding that economists have proffered multiple interpretations of Kuhn's notions, as well as of economics in their application of those notions, raises serious questions about the conventionalist model of science. Counter to this model's conception of scientists converging toward consensually agreed-upon truths, economists' application of Kuhn has done little to mediate differences among economists. Rather than providing economists a means by which to arrive at agreement regarding the meaning and significance of economics, Kuhn's model has provided them yet another forum within which to express their differences.

One seeking clarity amidst this diversity might suggest that we limit our focus to "accurate" interpretations of Kuhn and economics. This suggestion, however, forgets that that which gives rise to the multiplicity of interpretations in the first place (the selective perception of Kuhn's notions and the economics discipline) renders impossible the conclusive determination of the accuracy of an interpretation. No one -- including anyone seeking to determine the accuracy of another's interpretation -- operates from an objective and/or omniscient vantage point. Just as economists' interpretations of both Kuhn and economics are the product of their selective perception of Kuhn and economics, so too is one's evaluation of the accuracy of economists' interpretations the product of one's selective perception of Kuhn, economics, economists' interpretations of Kuhn and economics, *and* "accuracy." Consequently, in seeking to determine the "accuracy" of economists' interpretations, one invariably opens up a Pandora's Box of questions, which beg still further questions: "On what grounds does one certify that his/her (explicit or implicit) interpretations of Kuhn, economics, economists' interpretations of Kuhn and economics are 'accurate'?" "On

what grounds does one certify the grounds upon which he/she asserts that his/her interpretations are 'accurate'?" "On what grounds does one certify the grounds upon which he/she certifies the grounds upon which he/she asserts the 'accuracy' of his/her interpretations?" . . . . This is the well-known problem of the hermeneutic circle.

To employ Kuhnian terminology, one's interpretation of Kuhn and economics is relative to the paradigm from within which one interprets them; there exist no extra-paradigmatic means by which to determine the truth of one's interpretation.

Having said all this, it is important to recognize that even if economists were in complete agreement regarding the meaning and significance of Kuhn and economics, this agreement would, in no way, conclusively demonstrate their position's Truth. We still would be faced with the problem of the hermeneutic circle: "On what grounds do we justify that agreement among economists constitutes a foundation for Truth?" "On what grounds do we justify these grounds?" . . .<sup>1</sup>

Consequently, one should not regard Kuhn's notions as a means by which to uncover the "Truth" about economics. Instead, these concepts can properly only be regarded as a set of tools, which -- depending upon one's perspective and objectives -- may give rise to a multitude of different understandings of economics and serve a wide range of different purposes.

The present discussion has largely focused upon economists' use of Kuhn's concepts designed as tools with which to understand economics. However, as we alluded to on occasion, economists also employed Kuhn's model of science as a rhetorical tool. In addition to regarding Kuhn's notions as a means by which to understand economics, many economists regarded his notions as a means by which they might transform their discipline. Kuhn's model challenged the complacent acceptance of economics as a science and a necessarily progressive science. Prior to the introduction of Kuhn's philosophy of science into economics, opponents of economics' mainstream and the positivistic methodology it espoused were largely able to voice only

criticisms of that mainstream. However, Kuhn's model of science provided them with the wherewithal to couch their discussion in affirmative -- as opposed to only negative -- terms. Heterodox economists could not only contend that they disagreed with orthodox theory and method; they could argue that orthodoxy proffered only one of several different paradigms and forward their own theories and method as an alternative paradigm on par with the orthodoxy. Instead of simply contending that economists did not -- as the mainstream contended -- abide by positivistic precepts, they could argue that economics as practiced constituted a normal science in which the truth and falsity of economic theories were not always at issue. Instead of arguing that their theories and methods should be adopted, they could argue that economics was due for a scientific revolution. To the multiplicity of different ways in which economists have understood economics may then be added the multiplicity of different ways in which economists have employed Kuhn's concepts seeking to change economics.<sup>2</sup>

The multiplicity of ways in which economists have understood and applied Kuhn's notions, coupled with a recognition of the relativity of truth,<sup>3</sup> need not imply the futility of employing Kuhn's concepts in economics -- except for one for whom the attainment of "Truth" is an imperative. These findings, however, do instruct one seeking to apply Kuhn's concepts to economics to recognize that he/she is not applying notions with a univocal understanding to a set of undisputed facts. Rather, he/she is applying his/her *interpretation* of Kuhn's concepts to his/her *interpretation* of economics. Given this, he/she should not regard his/her Kuhnian portrayal of economics as definitive, but rather should recognize that his/hers is only one among many multiple interpretations of Kuhn and economics and that he/she should see his/her interpretation as part of a larger matrix of interpretations.

Numerous factors account for economists' diverse interpretations of Kuhn and of economics in their application of Kuhn's model of science. First, Kuhn did not articulate his notions in a straightforward, univocal fashion. Instead, he offered

multiple interpretations of his paradigm notion. He characterized normal science in multifaceted terms. He forwarded different understandings of the extent of the change effected by a scientific revolution. In sum, Kuhn forwarded a heterogeneous model of science which afforded economists broad latitude in their interpretation and application of that model.

Not only was Kuhn's model heterogeneous, so too were the perspectives from which economists interpreted and applied that model. Having different understandings as to what science and economics are/should be, economists arrived at divergent understandings of Kuhn's model and of economics in terms of it. Economists defined "theory" along a number of different lines and diverged in their understanding of the role which theory played in science and economics. Consequently, economists forwarded different understandings as to the relationship between theory and paradigm. Much the same may be said about economists' definitions and understandings of the role of method, worldview and values in science and economics. Economists differed as to whether science (economics) could be/should be conducted from an objective, i.e., extraparadigmatic, point of reference. Consequently, they disagreed as to whether the restrictions paradigms placed upon science/economics were beneficial or deleterious, necessary or unnecessary to the cause of science and/or economics. In sum, economists' heterogeneous perspectives on economics and science coupled with the heterogeneity of Kuhn's own specification of the paradigm concept gave rise to economists' multiple interpretations of the definition and function of paradigm. Similarly, prior to the introduction of Kuhn's normal science concept into economics, economists were already divided in their understanding of what a science is/should be. These differing conceptions help explain why economists chose different elements from Kuhn's multifaceted depiction of normal science. Finally, prior to the introduction of Kuhn's scientific revolution concept, economists were divided regarding the nature and extent of change science in general and economics in particular undergoes in its

development. These understandings colored their understanding of the extent of change implied by a scientific revolution as well as their assessment of the applicability of that notion to science in general and economics in particular.

Not only were Kuhn's notions diverse, not only were economists' perspectives on Kuhn's notions and economics divergent, economics itself is a heterogeneous field -- in large part a product of economists' multiple and selective perceptions of their discipline. A vast array of theories, worldviews, value systems and methodologies inhabit the economics discipline at a multitude of levels. Viewing this expansive and multi-layered landscape from varying perspectives and having different understandings of Kuhn's broadly specified paradigm notion, economists have diverged in the economics paradigms they identify, their descriptions of those paradigms and the levels and areas of the discipline in which they locate them. Applying their selective interpretations of Kuhn's multifaceted normal science concept to their selective perceptions of a multifaceted economics, economists have diverged in their specifications of normal economic science. Identifying different elements from economics' diverse past as fundamental, economists have diverged in their understandings as to what comprised economics' paradigm prior to and following putative revolutions in economics history. These differing understandings lead economists to look in different directions for the changes a revolution has effected and to lend differing importance to the changes they find. Coupled with economists' disagreements as to the nature and extent of change implied by a Kuhnian scientific revolution, these differences have led economists to divergent conclusions as to whether the marginal (utility), the Keynesian or other putative revolutions in economics constitute a scientific revolution.

Differences among economists' Kuhnian portrayals of economics arise not only from their disagreements as to what economics *is*, but also from their divergent understandings as to what economics *should be*. Heterodox economists define

economics' mainstream paradigm in largely negative terms, i.e., in terms of those aspects of the mainstream which they find most objectionable. In their specifications of the mainstream paradigm, heterodox economists highlight those elements present in the mainstream which they feel should be jettisoned and/or those elements missing from the mainstream which should be incorporated into it. Heterodox economists, however, vary widely in their conception of an ideal economics. Consequently, they have forwarded a broad range of characterizations of economics' mainstream paradigm. Those describing a paradigm to which they ascribe define that paradigm in terms of those elements they feel constitute an ideal paradigm. While socialization into a given economics paradigm breeds a certain measure of homogeneity among its adherents, fundamental disagreements persist within most schools of economics regarding the proper scope, definition and method of economics. As a result, economists offer divergent characterizations of the "same" paradigm in economics.

Similarly, many of those describing normal economic science are also engaged in criticizing economic orthodoxy. In characterizing normal economic science, they highlight those aspects of orthodox economic practice they find objectionable. Finding different elements of the economic orthodoxy problematical, these critics highlight different elements of normal economic science, i.e., those elements which they find objectionable. Ideological considerations also impact upon economists' portrayals of putative revolutions in economics' history. However, it is difficult to forge a link between economists' ideological positions and their characterizations of these revolutions. Some critics of the mainstream find no marginal (utility) and/or Keynesian scientific revolution in economics history and bemoan what mainstream economics lost by short-circuiting a prospective scientific revolution. On the other hand, other critics of the mainstream do locate a scientific revolution(s), but express regrets over what economics gave up as a result of that (those) revolution(s).

Ideological and methodological considerations help to explain why most economists applying Kuhn's normal science concept to economics are highly critical of normal economic science. The majority of those employing Kuhn's concepts were methodologists, historians of thought and heterodox economists. Methodologists and historians of thought, however, tend to be pluralistic in their outlook on economics. Given the strong associations between normal science and a monistic outlook, it is not surprising that these economists censure the practice of normal science. Along similar lines, given that heterodox economists link normal economic science with the orthodoxy, about which they harbor serious reservations, it is not surprising that they are so critical of normal economic science. That so few mainstream economists apply Kuhn's normal science concept to economics, let alone forward a normative assessment of normal economic science is not surprising, in part, because most mainstream practitioners have little interest in methodology or philosophy of science. Further, given that Kuhn's normal science concept calls into serious question those models of science which legitimate orthodox economic practice (positivism, falsificationism), it is not surprising that orthodox economists are loathe to apply Kuhn's apparatus. Ideological considerations, however, most likely play a role here as well.

As remarked earlier, application of Kuhn's concepts to economics did little to lessen disagreements among economists regarding their discipline. While Kuhn undoubtedly provided economists with a new analytical apparatus and new sets of questions to ask, he also provided a new vocabulary for old debates in economics. Indeed, given the plasticity of Kuhn's notions (especially "paradigm"), application of Kuhn's model of science eliminated few, if any, of the fundamental disagreements extant in economics prior to the introduction of his notions. If anything, the introduction of Kuhn's model of science and scientific change added to the contentiousness. In addition to the issues about which economists disagreed prior to Kuhn, the introduction of Kuhn's model added to the list of economists' disagreements.



In addition to disagreements regarding the (fundamental) definition of economics, the introduction of Kuhn's model added the controversy as to whether that which fundamentally defined economics was a paradigm. In addition to the question as to whether economics is/should be a science, was added the question whether economics was/should be a *normal* science. To the disagreements as to whether and, if so, when economics underwent revolutions were added disagreements as to whether and, if so, when had economics undergone a *scientific* revolution.

This investigation provides some indication that economics finds itself in the midst of a Kuhnian crisis. As with Kuhn's portrayal of a crisis, economists broadly and fundamentally disagree as to what does/should comprise economics' paradigm. Numerous methodologists and heterodox economists have levied serious charges against normal economic science. Further, innumerable paradigms are being advanced as replacements for what is seen by many to be a defective orthodox paradigm. Surrounding and underlying these disagreements are fundamental methodological debates concerning the scope, method and definition of economics. In many respects, the current situation in economics resembles a Kuhnian crisis -- although as we have seen, that concept is heterogeneous and ambiguous.

Does this imply that economics is on the verge of a scientific revolution? Here, the answer is considerably less straightforward. First, even if economics developed according to Kuhn's model of science, it is unclear whether a crisis under Kuhn's schema need conduce to a scientific revolution. Further, it is open to question whether economics, a social science, evolves according to Kuhn's theory, which is premised upon the evolution of the natural sciences. Finally, in the light of economics' past, it seems unlikely that the current crisis in economics will lead to a scientific revolution. In many ways, economics has been in "crisis" (exhibited crisis-like characteristics, e.g., heterogeneous composition, disagreements as to what economics is/should be) throughout its history. Although the foci of the debates have shifted over time,

fundamental disagreement among economists has been the rule rather than the exception. Thus, it is likely that, as with past crises, the current crisis will lead not to the installation of a new paradigm around which economists will be unified, but rather will spawn new controversies and give birth to new non-systemic crises.

The present investigation suggests further inquiries concerning Kuhn's theory of science and scientific change and its application to the discipline of economics. Areas left to be explored include: (1) an examination of Kuhn's theory of *pre*-paradigmatic science, including consideration of that theory's applicability to economics and economists' (though rare) applications of that theory; (2) a broader examination of economists' use of Kuhn's theories and notions as tools of rhetoric; (3) consideration of the significance of Kuhn's theory of the sociology of science and economists' applications of that theory as both an analytical and rhetorical tool; (4) examination of Kuhn's understanding of the means and criteria employed in theory choice along with economists' interpretations and applications -- both analytical and rhetorical-- of his understanding; (5) analysis of Kuhn's notions with respect to others who, like Kuhn, underline the central importance of the interpretive framework within which investigators work (e.g., Michel Foucault, Hans-Georg Gadamer, Jürg Habermas and Claude Levi-Strauss); (6) analysis of Kuhn's notions with respect to others who, like Kuhn, question foundationalist conceptions of knowledge and science (e.g., Richard Rorty and Paul Feyerabend); and (7) examination of the implications of Kuhn's theory of science and scientific change and economists' multiple interpretations of his theory and of economics for various epistemological positions (e.g., philosophical realism, relativism and nihilism).

## NOTES

1. This line of reasoning also applies to those who would assert that group consensus brings us closer to the "Truth." This line of reasoning, likewise, confronts the problem of the hermeneutic circle: "On what grounds may one assert that consensus brings us closer to the Truth?" "On what grounds do we justify those grounds?" . . . "What means do we employ to determine how 'close' to Truth we have gotten?" "On what grounds do we justify the 'accuracy' of those means?" . . .
2. An economist's attempt to "change" economics, of course, presupposes a given conception of the present state of economics, as well as a set of normative criteria against which to assess the status quo and the desired alternative.
3. Along with the relativity of the importance of truth as opposed to other objectives such as practical utility, elegance or simplicity.

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