The Best Way to do Economics: Moves and Countermoves in the History of Economic Methodology

Michael Weiss[1]
Duke University
Durham, NC
April 15, 2002

[1] Michael Weiss graduated Magna Cum Laude from Duke University in 2002 with Distinction honors in Economics. He now resides in New York City and will begin work in the Financial Sponsors Group at Merrill Lynch in July.

Acknowledgment

I would like to thank my advisor, Roy Weintraub, whose persistence and dedication made this paper possible.

Also, very special thanks to my parents and grandparents for their endless support of my educational and intellectual pursuits.

I. Introduction

This is a story about a community of scholars and the ideas it developed, transformed, deconstructed and reconstructed. It is a positive history of the growth of knowledge theories in economics and not a normative criticism of existing methodologies. The aim is not to change how economists think, but instead to offer a new perspective and a new way of looking at historical developments. Its ultimate purpose then is historiographic.

Intellectual movements are not spontaneously conceived. They are outgrowths and refined reactions to historical events. The present can be best understood by looking at the past. The present is also the best point to start analyzing what the future may look like. Historically, in order to see where we are going we must look backwards and think forwards. That is what this paper tries to do.

A Disclaimer

> "Even if I am right in claiming a large overlap in perspective between Lakatosian methodology and mainstream economists' preferred self-image, the possibility that there is a reality gap means that the question whether MSRP (Methodology of Science Research Programmes) truly applies to economics remains open" (de Marchi 6)

It is with this disclaimer that Neil de Marchi began "Rethinking Lakatos" and the promise of rational theories of the growth of knowledge in economics. In 1919 Karl Popper had developed his idea of falsification and opened the door for new examinations, methods, and theories of the growth of scientific knowledge. In the 1960's Thomas Kuhn offered an alternative to Popper's theory of "conjectures and refutations" with his theory of revolutionary "paradigm" shifts. A bit later that decade Imre Lakatos developed a new unit of scientific appraisal and vehicle for the growth of knowledge, the MSRP. Together, Popper's and Lakatos's ideas formed the foundation for rational theories of the growth of knowledge. Lakatosian nuances and modifications of Kuhn's and Popper's ideas extended the idea of research programs from the natural sciences to economics. This extension of Lakatosian theory to economics offered the possibility that a new language and vocabulary could be used by historians of economics to construct coherent narratives of progress. However, history itself has told a different story. The once promising rational theory of the growth of knowledge offered by Lakatos has so far failed to apply neatly to economics.

As Wade Hands points out,

> "if one wants the MSRP to serve demarcationist ends – to provide strict methodological rules for demarcating good/scientific economics from bad/nonscientific economics – then it fails in its task…[By] contrast, if one wants to use the MSRP for more doable jobs that are local in character, primarily historical, less arrogant, and perhaps more interesting, then it may still have something to offer" (Hands *Reflection* 296).

The issue has shifted from away "what is good science?" and "Should we consider economics to be a science?" The space vacated by Lakatos has been gradually filled by new conceptual schema. These movements, which Hands appropriately calls "turns", concern the interrelation of science studies and economics. The sociologicial turn, the naturalistic turn, the rhetorical turn, and the economic turn all represent movements that evolved from the criticism of MSRP. However, before we examine the evolution of these post-MSRP ideas we must first consider the sources of Lakatosian ideas.

II. Economic Methodology: A Primer

Since the seventeenth century, debates about Methodology have concerned scientific knowledge. The Scientific Revolution and the Enlightenment appeared to produce a set of rules to help appraise scientific procedures. The question, "How does good science proceed?" was answered by the set of rules which came to be known as the *scientific method*. As a result, in order for scientific results to have credibility, the scientific method must be used; the rules must be followed.

Turning to economics the issue becomes, "Is economics a legitimate empirical science?" This question is the departure point for our discussion of Economic Methodology and the rise of critical rationalism. As Hands notes,

> "The argument is that science *progresses* in a way that no other human activity progresses, and if economics is to partake in such (even potential) progress, then it had better follow the scientific method….The scientific gauntlet has been thrown down. Either economists must demonstrate that their theoretical concepts pass rigorous scientific muster, or to make a convincing case for some kind of partial special-exemption that allows economics to *be* scientific…" (Hands *Reflection* 4)

This idea of scientific knowledge came to be known (in retrospect) as the "Received View". From the mid-nineteenth to the mid-twentieth century work on Economic Methodology either claimed that economics lived up to the stringent standards of science or that it deserved a special exemption from those standards, but should still be considered scientific. In the latter category we find the work of John Stuart Mill (1806-1873).

The Millian Tradition

Mill was one of the leading thinkers on political economy in the 19th century. In his1874 essay, "On the Definition of Political Economy" Mill put forth what came to be the dominant methodological view of economics for almost 100 years. Mill argued that economics is a science, but it follows a slightly different method from the physical sciences: "The practical man thinks of economic laws as riddled with exceptions, when in fact they should think of economic laws as exceptionless, but inexact, statements about tendencies; such laws can provide insight into concrete cases, but, by necessity, must remain at a relatively abstract level" (Hands *Reflection* 24). For the empiricist Mill, economics produced knowledge, but it is inexact knowledge and not the exact knowledge of the physical sciences. Nonetheless, the weight of economic conclusions should still carry scientific force, thus making economics a potentially powerful political tool. Throughout the following decades the central tenets of the Millian tradition remained intact, but details were modified by several leading economic thinkers including Cairnes, Neville Keynes and Robbins.

Similar to Mill, John Cairnes argued that economics deserved scientific status and he further believed that its differences from the physical sciences actually strengthened the case of economics. In his 1875 work, *The Character and Logical Method of Political Economy*, Cairnes slightly altered the Millian tradition to respond to popular criticism. Cairnes asserted that the lack of experimentation in economics, as opposed to physics, is an advantage. He argued that economics starts with tangible knowledge of the "ultimate causes" of events (human attitudes towards wealth), while physics undertakes rigorous research to find the forces at work. Like Mill, Cairnes also addressed the role of verification in the creation of economic knowledge, asserting that the only role for empiricism was for final verification. Later, in a defense of Marshallian price theory, John Neville Keynes would underscore the importance of economics as a positive and not a normative science. In addition, Keynes discussed how science proceeded by finding general laws, and compared this to economics (Hands *Reflection* 32). A final variation of the Millian tradition was offered by Lionel Robbins in *An Essay on the Nature and Significance of Economic Science* (1932). Again, as with Cairnes and Keynes, Robbins largely aligned with Mill, but differed with him on the definition of economics. Mill had asserted that economics concerned the pursuit of wealth and the relative efficiencies of different ways of obtaining that wealth. Robbins countered that economics was about scarcity, claiming "Economics is the science which studies human behavior as a relationship between ends and scarce means which have alternative uses (Robbins quoted in Hands *Reflections* 36). While the difference in details among Mill and the others seems rather meaningless, in the progression of traditional Economic Methodology from Mill to Cairnes to Keynes to Robbins there is a significant, albeit subtle, shift. As Hands remarks, "The change is the movement from *characterizing the method of economics as it contrasts with the different methods of other sciences* in Mill, to specifying *rules for proper conduct of any science, and thus economics.* in Robbins" (Hands *Reflection* 37). To clarify, for Mill chemistry, mathematics, utilitarian ethics and economics were all sciences, they just proceeded with slightly different methods. The issue was not "What is good science?" However, for Robbins economic science differs from economic history, politics, and utilitarian ethics, which he considered unscientific pursuits. Consequently as the Millian tradition evolved for economic methodology "demarcation and rules became the order of the day" (Hands *Reflection* 37).

The Received View and Logical Positivism

Not unrelated to interest in demarcation issues in the philosophy of science was the development of logical positivism. Logical positivism as a philosophical movement was created in Vienna in the late 1920's and became the specific philosophy of the Vienna Circle. In general, logical positivism holds that "there are only two types of meaningful propositions, synthetic propositions that must satisfy the verificationist criterion of meaning, and analytical propositions which say nothing about the world, but are true by definition" (Hands *Reflection* 100). The demarcation criteria between science and non-science for logical positivists was the notion of *meaningfulness*. Since the only valid form of synthetic knowledge was empirical, the Vienna Circle followed the early Wittgenstein (of the Tractatus) in claiming that non-empirical propositions held no significance. Therefore, statements originating in fields like theology or religion were meaningless and they were not worth talking about: the Vienna Circle went to great lengths not to waste philosophical time discussing such ideas.

Logical positivism was the predecessor to logical empiricism, which was, in general, what one did if one was a philosopher of science in the 1950's. Logical empiricism is more amorphous than logical positivism, which had a specific historical place and time. Logical empiricism was a continuation of logical positivism with several innovative features, and it came to be later identified as the Received View. When logical positivism was developing it was seen as offering a new and intellectually stimulating perspective on scientific knowledge. However, it came to be considered a step in the wrong direction, an ideological wrong turn. This transformation occurred when logical positivism came under attack in the latter half of the 20th century:

> "Everybody knows nowadays that logical positivism is dead. But nobody seems to suspect that there may be a question to be asked here -- the question "Who is responsible?"…I fear I must admit responsibility. (Popper quoted in Hands *Reflection* 70).
>
> -Karl Popper

III. Meet Karl Popper: The Father of Critical Rationalism

Karl Raimund Popper was born in Vienna on July 28, 1902. His father's family came from Bohemia and his mother's family came from Hungary. Popper's parents, Simon Carl Siegmund (1856-1932) and Jenny Schiff (1864-1938), were married on April 3, 1892 and quickly assimilated into German culture. Their residential pattern indicates they made a "rapid social climb" (Hacohen 23). Simon Popper spent much of his time working at his legal practice, but he remained intellectually engaged. He kept an extensive library of "twelve to fourteen thousand volumes" including current Viennese publications on "politics, social reform and psychoanalysis" (Hacohen 29). In his youth, Popper was exposed to everything from Latin poetry to classical music.

Prior to 1848, a Jewish family would not have had the opportunity to offer their children such a distinguished middle class upbringing. However, the revolution of 1848 emancipated the Jews and there began a massive movement of immigrants to Vienna. In 1857 a Vienna census recorded just over 6,000 Jews in Vienna. By 1880 there were 72,000 and by 1910 over 175,000 Jews in Vienna (Hacohen 29). Furthermore, during Popper's childhood, the Jewish minority grew to social, economic and cultural prominence. Jews disproportionately dominated the liberal professions like law and medicine and also owned several Vienna papers (Hacohen 29). Religiously, the opportunity to join German society created a crisis of identity among Jews. The question, whether to be a German Jew or a Jew living in a German land, was answered in several different ways. Liberals favored assimilation into German culture (German Jews), others reaffirmed their traditional Jewish identity (Jews in Germany), and alternatively the secular Jewish nationalist movement (Zionism) was born.

Before the turn of the century Simon and Jenny Popper converted from Judaism to Lutheranism. Simon Popper was strongly anti-clerical and his son received a progressive German education (Hacohen 32). Simon Popper's conversion drew the ire of the liberal Jewish community, a disposition Karl Popper would not forget. Starting early in his life Karl Popper renounced religion and had an extreme loathing of ethno-nationalist movements like Zionism. While he remained part of the assimilated Jewish intelligentsia for his entire life, Popper was more concerned with science then with religion. His theories of knowledge were born out of a lifelong engagement with progressive philosophy.

Popperian Falsification and Conjectures and Refutations

Interestingly, while modern scholars have been critical of the application of rational growth of knowledge theories to economics, it was Popper's own dissatisfaction with non-natural science theories that led him to develop his idea of conjectures and refutations (Popper 34). In 1919 Popper first began to struggle with the problem of demarcation in the philosophy of science. When should a theory be considered scientific? Popper's inquiry began when he was bothered by the difference he noticed between physical theories like those of Newton and Einstein and non-physical theories like Marx's theory of history, Freud's psychoanalysis, and Alder's individual psychology theory. What was the difference between physical and non-physical theories? What was the difference between science and pseudo-science? The popularly accepted answer was that science used a largely induction[2] based empirical method which appealed to observation and research. However, this did not satisfy Popper who noted that astrology, like astronomy, uses repeated observations and empirical evidence in developing horoscopes (Popper 34). For Popper, the theories of Marx and Alder held more in common with astrology than astronomy. So what made those theories so appealing to the public? Apparently it

[2] The "problem of induction" will be addressed in more detail later.

was their great “explanatory power” (Popper 34). Within their own framework the theories were able to explain world events, and each explanation was considered a verification of the theory. For example, “A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history” (Popper 35). Popper dismissed this “explanatory power” as self-fulfilling prophecy and elucidated the line of demarcation between science and pseudo-science by contrasting theories. Popper showed how both Freudian and Alderian psychoanalysis resulted in different diagnosis of the same case, but Einstein’s predictions were not contradicted by even seemingly radical new ideas. The theories of Marx, Alder, and Freud were effectively irrefutable— verifiable but not falsifiable. For Popper, irrefutability of a theory was a vice not a virtue. If a theory was not “incompatible with certain possible results of observation” (Popper 36) then it was not falsifiable. Importantly Popper believed that “the criterion of the scientific status of a theory is its falsifiability, or refutability or testability” (Popper 37).

In addition to the problem of demarcation, which he solved with falsification, Popper also discussed another major issue in the philosophy of science, the problem of induction. Induction can be summarized in a brief example. If one has only seen white swans one may infer that all swans are white. However, as soon as one sees a single non-white swan the inference no longer holds. Hume concluded induction could not be logically justified saying, “…we have no reason to draw any inference concerning any object beyond those of which we have had experience” (Hume in Popper 42). While Hume claimed that the human predilection to infer developed from repetition and habit, Popper disagreed. Popper instead replaced the psychological view of induction with his own view that “…without waiting passively for repetitions…we actively try to impose regularities on the world…Without waiting for premises we jump to conclusions. These may have to be discarded later, should observation show that they are wrong. This was a theory of trial and error—of *conjectures and refutations*” (Popper 46). Thus, the Popperian view of the growth of scientific knowledge was that theories are developed through conjectures and refutations and they must be appraised as falsifiable to be scientific. The logical positivist criterion of verification was to be discarded. Conjectures and refutations offered a new perspective from which to examine the history of science.

Lakatos, Popper and Kuhn

Other views about science began to circulate in the 1960's, competing with Popper's position. For instance, Imre Lakatos was a dedicated communist who, following the 1957 Soviet crushing of the Hungarian revolution, left Hungary to study at LSE where he would be influenced by the ideas of Popper. Lakatos came to disagree with Popper significantly on some points. Nevertheless, his exposure to Popperian ideas somewhat shaped the formation of his methodological unit of analysis, as he was likewise influenced by Thomas Kuhn's 1962 book, *The Structure of Scientific Revolutions.* In that book, Kuhn had argued that science did not progress by Popper's method of conjectures and refutations. Instead, Kuhn attributed scientific progress to revolutionary "paradigm" shifts, which interrupted periods of "normal science." One implication of this argument was that appraising a scientific theory necessitated more than just the examination of surviving individual theories. Falsification of a theory did not constitute a Kuhnian "revolution". Consequently, appraisal of scientific theories was a complex historical task. While Popper's analysis remained largely ahistorical, Kuhn evaluated past science by creating new histories and merging them with philosophical criticism. And Lakatos would undertake a similar task and develop his own "specific theory on the history of science: his methodology of scientific research programmes" (Kadvany 153).

IV. Lakatosian Research Programs

A Lakatosian research program has three main parts. The first part is a hard core of propositions taken to be true and irrefutable by individuals working in the field of the research program. The second part is a positive heuristic which contains rules for constructing theories based on the hard core which are open to tests of falsifiability. The third part is the negative heuristic that functions to protect the hard core from critics.

Just as Lakatos combined elements of Popper and Kuhn in developing the MSRP, he also concatenated notions from inductivism and conventionalism. Like Popper, Lakatos analyzed the problem of induction concluding that the inductivist historian recognized only two types of scientific discoveries: "hard factual propositions and inductive generalizations" (Lakatos 104). The existence of hard factual propositions was introduced at the "hard core" of MSRP's. From conventionalism, Lakatos highlighted that "false assumptions may have true consequences; therefore false theories may have great predictive power" (Lakatos 106). Furthermore, Lakatos borrowed from conventionalism the ability rationally to accept 'factual statements' and spatio-temporally universal theories. Henceforth, while theories can be falsified and research programs can degenerate, the hard core propositions of a research program remained irrefutable. The amalgamation of elements from inductivism, conventionalism, Popper, and Kuhn all contributed to the development of the complex notion of research programs.

Until now we have only examined the theoretical development of rational growth of knowledge theories culminating in the Lakatosian research program. The question remains, how do Lakatosian research programs practically apply to economics? To answer this question let us examine the case of general equilibrium analysis, and what has been termed "the neo-Walrasian research program" (Weintraub 109).

An Example of Research Programs in Action: General Equilibrium Analysis

As discussed earlier a research program is composed of a hard core, a positive heuristic, and a negative heuristic or protective belt. One Lakatosian appraisal of general equilibrium analysis asserts the existence of a neo-Walrasian research program[3]. The example below uses general equilibrium analysis to show the practical application of research programs to economics and how MSRP's lead to the development of economic theories.

The hard core suppositions are:

HC1. There exist economic agents

HC2. Agents have preferences over outcomes

HC3. Agents independently optimize subject to constraints

HC4. Choices are made in interrelated markets

HC5. Agents have full relevant knowledge

HC6. Observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium theories.

And the positive and negative heuristics are:

PH1. Go forth and construct theories in which economic agents optimize.

PH2. Construct theories that make predictions about changes in equilibrium states.

NH1. Do not construct theories in which irrational behavior plays any role.
NH2. Do not construct theories in which equilibrium has no meaning.
NH3. Do not test the hard core propositions (Weintraub 109).

This practical application provides a look at how research programs interact with economics and elucidates the role of the components of the research program.

A Note on Potential

One may question what work applying SRP's to economics does? Certainly Popper, Kuhn, and Lakatos offer new language and terminology with which to interpret the history of science and understand its future. The philosophy of science is dependent upon the history of science for its application. As A.W. Coats noted in 1974, "…in the present imperfect state of our knowledge MSRP offers the best hope of success for the historian of economics who is seeking to understand the general development of his field" (Coats in Latsis 44). Therefore, the growth of knowledge theories in economics need to be applied to rational reconstructions of the history of economics. Interpreting how successful science occurred in the past may help to set the most progressive course for science in the future.

V. Rationalism Reconsidered: Appraising the Appraiser

It Looked Good

In the early 1970's many scholars were optimistic about this potential application of the MSRP to economics. A student of Lakatos', Spiro Latsis, asserted, "…the methodology of scientific research programmes (MSRP)…fares better than any of the hitherto available methodologies of economics for the description and appraisal of developments in economic analysis". (Latsis 2) Latsis was not alone in his praise of the MSRP; notably both Neil de Marchi and Mark Blaug joined him.

In his work De Marchi examined the case of the Leontief paradox[4] in light of Popper, Kuhn, and Lakatos. Mark Blaug further supported the applicability of the MSRP to economics. In his work, Blaug compares Kuhn to Lakatos, or paradigms to research programs, in their ability

---

[3] For a history of general equilibrium analysis and further discussion of the example see Weintraub (1979).
[4] The Leontief paradox will be discussed in a later section

to explain the history of economics. Using the “Keynesian revolution” as an example, Blaug concludes, “…it is perfectly obvious, however, that the age old paradigm of ‘economic equilibrium via the market mechanism’, which Keynes is supposed to have supplanted, is actually a network of inter-connected sub-paradigms; in short, it is best regarded as a Lakatosian SRP” (Blaug in Latsis 160). Blaug supported his conclusion by using the theory of the firm as a case study of an applied MSRP in economics.

Despite Blaug’s conclusions, he offered some prescient misgivings about the applicability of Lakatosian research programs to economics. When considering the question, “Do economists practise what they preach?” Blaug expressed tentative doubts “about the applicability of any philosophy of science grounded in the history of the physical sciences to a social science like economics” (Blaug in Latsis 171). At the time, Blaug’s doubts were a minority opinion in a field full of optimism about the applicability of the MSRP to economics. Within the next twenty years those positions were to be reversed.

15 Years Later…

“Method and Appraisal in Economics” by Spiro Latsis exhibited scholarly optimism concerning the MSRP. It was published in 1976 and included the above mentioned papers by de Marchi and Blaug. But in 1991 de Marchi and Blaug co-edited “Appraising Economic Theories: Studies in the Methodology of Research Programs”. The 15 year-old doubts of Mark Blaug resurfaced in the introduction by Neil de Marchi entitled, “Rethinking Lakatos”. While Blaug himself somewhat disagreed with de Marchi, his own conclusion to the book offered insight into the scholarly change of mind that had taken place. Blaug said, “…I was personally taken aback by what can only be described as a generally dismissive, if not hostile reaction to Lakatos’s MSRP…Of the 17 papers delivered at the conference not more than five were unambiguously positive about the value of MSRP” (Blaug in de Marchi and Blaug 500).

What caused this shift in scholarly opinion? What difficulty did scholars find with the applicability of MSRP to economics? Blaug identified two main problems, but treated them dismissively (even as the majority of scholars at the conference were not as forgiving). The first problem identified by Blaug was that there was no way of writing down precisely what constituted the hard core of any SRP in economics. Put briefly, Lakatos was too vague on how to begin a Lakatosian appraisal of an economic theory (Blaug 500). The second main criticism was that scholars found Lakatos' "insistence on the importance of 'novel facts", or "measuring scientific 'progress' with an empirical yardstick" to be inappropriate in a social science like economics (Blaug 500). Axel Leijonhufvud had identified a final problem in the application of the MSRP to economics in his earlier essay "Schools, "Revolutions," and Research Programmes". Leijonhufvud asserted that the irrefutable hard core suppositions of an MSRP are impossible without language conventions. These language conventions opened economics to the sorts of rhetorical issues examined by Deirdre McCloskey in "The Rhetoric of Economics" and later by several scholars in "The Consequences of the Rhetoric of Economics". The debate over critical rationalism had created space for many new and different things. The examination of these problems marked the start of the "rhetorical turn" in the history of critical rationalism. The examination of language conventions would be one of several new doors scholars would soon open.

Criticism #1: Difficulty Defining the MSRP in Economics

As A.W. Coats and many others have noted, "Imre Lakatos originally conceived his methodology of scientific research programmes as a procedure for analysing and appraising developments in the natural sciences" (Coats in Latsis 43). Lakatos later encouraged the exploration of the application of his MSRP to economics. In 1984 Rodney Maddock published his work documenting his attempt to apply the rational expectations literature to the research program model. Maddock had difficulty "forcing the literature into the particular methodological framework" (Maddock in de Marchi and Blaug 336). As a result Maddock promoted the modification or abandonment of the research program model. In his work he outlines problems he encountered and uses them as a basis to suggest modification. One of the first problems he discovered was the difficulty defining a research program in economics.

Identifying and applying the research program model can be quite complex. When examining a theory one must question whether it is a solitary research program, a series of interconnected sub-programs, or a part of a series of larger programs. Maddock noted, "The fundamental difficulty with the application of the Lakatosian methods would thus appear to start from the fact that research programs, such as this one, are also members of a series of ever-broader, containing, programs. 'New classical economics sits within 'classical economics', which sits within 'economics' and so on" (Maddock in de Marchi and Blaug 337). On one level

Lakatos supplied a method of delineating programs, arguing that if a new theory did not include all of the unrefuted content of an earlier theory then it could be described as a separate research program. However, this distinction was inadequate to address a large literature in which one program's hard core contents may be the protective belt or positive heuristic of another. Similarly, Mary Morgan in her paper, "The Stamping Out of Process Analysis in Econometrics" ran into problems defining research programs. Roger Backhouse expounded on the difficulties Morgan encountered when he analyzed the simultaneous equations research program (SERP) and the process analysis research program (PARP). Backhouse says,

> "Mary Morgan's argument that they had different presuppositions and ways of attacking problems is persuasive and it is hard to resist the temptation to analyze it in terms of different hard cores and heuristics. There are good reasons, not least those given above, why we should not push Lakatosian ideas too far, or apply them too rigidly, but it might be helpful to see a more formal attempt to define hard cores and heuristics of the two research programs" (Backhouse in de Marchi and Blaug 267).

Backhouse was not the only scholar to identify problems with defining the MSRP. In his mostly positive earlier appraisal of the MSRP in 1985 Roy Weintraub also foresaw potential difficulties in defining the hard core and the MSRP. Weintraub noted the footnote of Lakatos who said, "The actual hard core of a program does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error…" (Weintraub 112). As Weintraub noted, "Lakatos tells us little about this hardening process" (Weintraub 112).

A further problem of appraising economics with the MSRP is deciding the actual size of a program, an issue complicated by sub-programs and difficulties in identifying the hard core and heuristics of closely related programs. Nonetheless, at the time of Weintraub's work in 1985, most scholars still seemed able credit the MSRP with doing valuable work despite these potential problems. As it turned out the problems encountered by Morgan and Maddock were more difficult to overcome then had been foreseen. As a result, scholars in the early 1990's argued that the vagueness in defining a research program weakened the value of the MSRP. This being the case, Blaug still dismissed these concerns as a minor or resolvable problem in the application of the MSRP to economics. A more pressing issue for him was the Lakatosian emphasis on progressivity and empirical excess content.

Criticism #2: Novel Facts

For Lakatos, excess content and the search for novel facts played a critical role in the pursuit of progress. Hands had argued the Lakatosian notion of excess content and novel facts evolved straight from Popper. Several times he argued "against using the Popperian/Lakatosian notions of excess content and novel facts as the sole criteria for theory appraisal in economics[5]" (Hands in de Marchi and Blaug 58). This link between Popper and Lakatos exposed Lakatos to some of the perceived problems in Popperian thought. Lakatos and others believed that Popper overestimated the degree to which falsification actually applies to and explains scientific episodes in history. Recognizing this perceived deficiency, Lakatos developed the hard core and dual heuristic approach in research programs to accommodate the actual history of science. Thus for Lakatos, who wanted to decrease the role of negative evidence in scientific progress, "the entire burden of scientific progress is left on the shoulders of novel facts" (Hands in de Marchi and Blaug 64).

The Popperian approach to social sciences required the application of a rationality principle to "nearly every testable social theory" (Popper, 1985, p. 361) but the "rationality principle is false" (Popper, 1985, p. 361). This problem can be solved, however, through Popper's concept of verisimilitude. As Hands said, "…if verisimilitude is the aim of science and if one false theory can have more verisimilitude than another false theory, then the notion of progress towards truth need not be lost when theories involve the rationality principle" (Hands in de Marchi and Blaug 68-69). Thus, it seems the notion of progress is preserved despite the falsity of the rationality principle. However, criticism of verisimilitude has weakened its ability to support theories that rely on the rationality principle. This criticism was founded on two papers by Miller (1974) and Tichy (1974) "which demonstrated that no false theory ever has more verisimilitude than any other false theory" (Hands in de Marchi and Blaug 67). Watkins similarly rejected verisimilitude as the aim of science (Watkins 1984, p.124) and was joined by scholars like Hacking and Agassi who referred to verisimilitude as a "boo-boo" (Agassi 1988, p. 473). Eventually, Popper himself conceded the defeat of verisimilitude saying,

> "A new definition is of interest only if it strengthens a theory. I thought that I could do this with my theory of the aims of science: the theory that science aims at truth *and* the solving of problems of explanation, that is, at theories of greater explanatory power,

> greater content, and greater testability. The hope further to strengthen this theory of the aims of science by the definition of verisimilitude in terms of truth and of content was, unfortunately, vain. (Popper, 1983, p. xxxvi)

Popper's definition of verisimilitude formed the backbone of his methodological proposals regarding progress and novelty, which were re-emphasized by Lakatos in the MSRP. The 'admitted failure' of verisimilitude had serious consequences for the Lakatosian notion of progress borrowed from Popper.

The Lakatosian theory of progress depends on finding novel facts. Applied to economics Lakatosian theory often lead to "novel fact hunts" (Hands in de Marchi and Blaug 70). These "novel fact hunts" led to debates over the definition of novel facts in economics which have offered very little insight. Furthermore, useful elements of the MSRP like the hard core and heuristic predictions have often been lost in the race for novel facts. In light of the minimization of the role of novel facts in Popper, it follows that if the Lakatosian MSRP is to continue to be applied to economics "we should re-evaluate the various roles of the different parts of his position" (Hands in de Marchi and Blaug 70). The Popperian domination of economic methodology had seemingly ran its course. However its link with Lakatos held back the MSRP: as Hands summarized, "Lakatos's MSRP, however pregnant it might be with interesting ideas, is also unable to provide the requisite forward thrust" (Hands in de Marchi and Blaug 72).

The limits on Lakatosian progress were also noted by Vernon Smith, Kevin McCabe and Stephen Rassenti in their paper, "Lakatos and Experimental Economics." After examining the role of theory in practice in economics they concluded that the literature driven incentives of economic scholarship prevented the examination of observations and problems of the world as seen in the physical sciences. They suggest it remained all too common for economists to choose their own problems and that if theorists concentrated on the problems of the laboratory and the real world their predictions would be more useful. In the absence of this shift by theorists, they concluded, "…Lakatosian progress within economics is far more limited than is deserved by the size and prominence of professional economics" (Smith in de Marchi and Blaug 224).

Criticism #3 Language Conventions: The Introduction of the Rhetorical Turn

Lakatos believed that the time for philosophers of science to begin studying the evolution of the social sciences had arrived. He left it up to the economists to provide the case studies but he "…deliberately designed the Nafplion Colloquium as an opportunity to test its applicability to the wider history of economics…" (Coats in Latsis 43). At the behest of Imre Lakatos, Axel Leijonhufvud supplied a re-examination of the Keynsian revolution story in light of the rational growth of knowledge theories of Kuhn and Lakatos in his essay entitled, "Schools, "Revolutions, and Research Programmes in Economic Theory".

In this essay Leijonhufvud found that in the evolution of research programs pre-suppositions would become hard core propositions, and the maturation process of a research program necessarily involved accommodation. Consequently, he noted

> "The development of the language conventions, without which "strictly irrefutable" hard-core propositions are impossible, pose a problem in applying Lakatos's theory (to economics, at least) in that the process will resemble that of degeneration. It is fairly clear that it will so appear to someone unsympathetic to the emerging research programme. What will this someone witness? That his criticisms and objections are increasingly met with the assertion of 'tautologies." That anomalies are being 'accommodated" (through 'verbal legerdemain') to remove all possibility of falsification. And, in some instances, of course, the enterprise seen exhibiting these repugnant symptoms *is* going nowhere in particular." (Leijonhufvud 311)

Later, Deirdre McCloskey would argue that accommodation of anomalies with language is part and parcel of science and economics. Furthermore McCloskey, in her book *The Rhetoric of Economics*, argued that the representation of economic ideas occurs in language, and can thus be well examined with literary methods. McCloskey thus shifted the discussion of economics as a science with her claim that, "Economics is a collection of literary forms, some of them expressed in mathematics, not a Science. Indeed, science is a collection of literary forms, not a Science…The idea that science is a way of talking…does not imply that science is inconclusive or that literature is cold-blooded. The point is that science uses art for urgent practical purposes" (McCloskey 21). For support McCloskey cited many examples but as case studies she examined classic economic texts of the twentieth century, Paul Samuelson's 1947 *Foundations of*

Economic Analysis and Robert Solow's 1957 essay on the production function and productivity change. We consider her arguments about each in turn.

a) In *Foundations* Samuelson began with a general mathematical layout. When discussing mathematical results Samuelson used the phrase "we", thus demonstrating that mathematical results are impersonal and easily apparent as true to anyone with the requisite mathematical background (McCloskey 36). However, when discussing economic conclusions Samuelson used the phrase "I". This disparity shows the personal and arguable nature of economic conclusions. If economic conclusions must be argued then the author must persuade the audience. Samuelson persuaded not with explicit logical technique, but with plentiful literary and rhetorical methods. McCloskey summarized Samuelson's appeals to authority and appeals to analogy as literary and rhetorical and noted they are figures of speech similarly used by a poet. The importance of metaphor was underscored by its noted "pregnant qualities" and identifying it, as I.A. Richards had, as a "borrowing between and intercourse of thoughts, a transaction between contexts" which is mutually advantageous to economic authors. Additionally, McCloskey identified several "relaxations of assumptions" and appeals to "toy economies" in Samuelson's work (McCloskey 37-38). McCloskey concluded that in a Cartesian framework these "figures of speech" are not persuasive at all. Furthermore, "None prove by deduction or falsification. Yet *Foundations of Economic Analysis* used them all, with hundreds of others, in rich array" (McCloskey 38).

b) A second representative example of the rhetorical structur e of economics examined by McCloskey was Robert Solow's 1957 paper on the production function. Almost any economist knows the importance of Solow's paper and associated work, which is highlighted by the fact that nearly 25 years after is publication it still receives over 20 citations in a given year (McCloskey 48). What makes Solow's paper so persuasive? McCloskey argued that Solow used three of the "four master tropes" which are, as identified by Kenneth Burke, "metaphor, metonymy, synechdoche, and irony" (McCloskey 49). To demonstrate the application of these devices McCloskey examined the opening gambit of Solow's paper,

> "In this day of rationally designed econometric studies and super-input-output tables, it takes something more than the usual "willing suspension of disbelief" to talk seriously of the aggregate production function. The new wrinkle I want to describe is an elementary way of segregating variations in output per head due to technical change from those due to the availability of capital per

> head. Either this kind of aggregate economics appeals or it doesn't. Personally I belong to both schools. It is convenient to begin with the special case of neutral technical change. In that case the production function takes the special form Q = *A(t) f(K,L)* and the multiplicative factor A*(t)* measures the cumulated effect of shifts over time. (Solow 1957, reprinted in Zellner 1968, pp. 349-50 in McCloskey)

McCloskey pointed out that immediately the argument depends on metaphor of the "aggregate production function" which is supposed to depict so much that it requires a "willing suspension of disbelief". Furthermore, she showed how the K and L in the equation are metonymies as they let "another thing merely associated with the thing in question stand as a symbol for it, as the White House does for the presidency" (McCloskey 49). Finally, McCloskey explained how the identification of A*(t)* with "technical change" is a synecdoche or the taking of a part to represent the whole (McCloskey 49). Overall, McCloskey argued that while Solow persuaded with "the symmetry of the mathematics and the appeal to the authority of scientific traditions in economics" he did so "with the perspectival tropes: metaphor, metonymy, and synecdoche" (McCloskey 50).

The final trope, which McCloskey asserted is utilized by economists, is irony. Booth had argued that most sophisticated economists are in favor of the use of irony (Booth 1974b in McCloskey 51). Historian Hayden White commenting on the impact of irony noted that,

> "[Irony] presupposes that the reader or auditor already knows, or is capable of recognizing, the absurdity of the characterization of the thing designated in the Metaphor, Metonymy, or Synecdoche used to give form to it…Irony thus represents a stage of consciousness in which the problematical nature of language itself has become recognized. It points to the potential foolishness of all linguistic characterizations of reality as much to the absurdity of the belief in parodies" (White in McCloskey 51).

These literary devices can weaken economic arguments in a Cartesian framework. Nonetheless, economic metaphors and the other literary devices have remained instrumental for economists to construct persuasive arguments. McCloskey concluded "no economist could speak without metaphor and the other master tropes" (McCloskey 51). Despite their persuasive ability, the necessity of rhetorical devices in economics is yet another weakness that contributed to the lack of scholarly acceptance of the work done by Lakatos and the application of the MSRP to economics. Simultaneously, the rhetorical weaknesses identified in economics opened the door to a brand new field for historical inquiry.

VI. A Case Study: The Leontief Paradox

One of the original papers in the Latsis volume (1976) was by Neil de Marchi whose shift from optimism (1976) to doubtfulness (1991) about the application of the MSRP to economics was emblematic of a wider scholarly shift. Wassily Leontief's study of factors of production in USA trade was a simple test of the Hecksher-Ohlin trade theory. As the hopeful (early!) de Marchi noted, "The Leontief test, though not perfectly controlled, is probably about as clear an example of a 'crucial experiment' as one is likely to encounter in economics" (de Marchi in Latsis 113). It was commonly assumed the USA was a capital intensive country but Leontief's results violated the prediction of the Hecksher-Ohlin trade theory by finding the USA exports to be labor intensive and imports to be capital intensive. De Marchi examined how economists responded to Leontief's findings.

On the surface, Leontief's results appeared to be a falsification of the Hecksher-Ohlin theory. It could be argued this is only a "naïve falsification" and not really Popperian "sophisticated falsification", which requires numerous repeated falsifications. Nonetheless, Popperian falsification would imply that Hecksher-Ohlin theory is non-scientific and useful only like astrology. Thus, it seems the Popperian model is not the best fit for explaining the Leontief paradox because despite Leontief's findings economists still supported and believed Hecksher-Ohlin offered valuable insights. Additionally, Hecksher-Ohlin was *ex post facto* conventionally rationalized by many economists who were reluctant to give up the theory.

As he had argued concerning Popper, de Marchi found that Kuhn's theory was similarly capable of explaining the paradox by claiming that sometimes anomalies appeared in periods before the crisis. However, again de Marchi concluded that the action of economists, some of whom supported Hecksher-Ohlin with alternate explanations for Leontief's findings and some of which developed some new theories, did not constitute the revolutionary response predicted by Kuhn's theory. So the question remained: "Did the action of economists fit the prediction of a Lakatosian Hecksher-Ohlin research program?" De Marchi answered with a qualified "yes". He claimed that a research program did indeed offer a plausible explanation for the Leontief Paradox saying, "The description given by Lakatos fits that programme surprisingly well" (de Marchi

123). De Marchi concluded that the research program helped explain why scientists fail to abandon a theory in the face of contrary evidence. Recall that MSRP's can degenerate and be progressively overtaken without being abandoned. For de Marchi, "…The methodology of research programmes is one way—not necessarily the only one" to explain the action of economists dealing with the Leontief paradox. This work of de Marchi's shows the early scholarly optimism about the application of research programs to economics.

Years passed, and de Marchi would eventually express regrets (in his introduction to "Appraising Economic Theories") about his earlier work. In that more recent work de Marchi labeled the progress of the MSRP in economics as "dubious." Why? First, de Marchi noted that at the earlier time he was, "conscious…of not wanting to reduce the creative and exploratory struggle of the economists involved in developing modern trade theory…the intuition of individuals like Heckscher, Ohlin, Samuelson and Leontief seemed to warrant a different sort of treatment, though I really did not have anything entirely satisfactory at hand" (de Marchi 16). De Marchi declared, "…putting Lakatos to work seems to *require* clear enunciations of hard core and heuristics that can be tested against the historical record, just as Weintraub has taught us to do" (de Marchi 16). This insight demonstrates de Marchi's inquiry into the first general criticism of the MSRP in economics, the difficulty in defining the research program. He later recounted the arguments of Wade Hands and other criticisms surrounding the MSRP in economics.

The case of the Leontief paradox is representative of the general attitude of scholarship surrounding the applicability of the MSRP to economics. In the 1970's de Marchi's early writings on the Leontief paradox demonstrates the original scholarly optimism towards the MSRP. However, as time passed, and scholarship reexamined the case of the Leontief paradox, critics emerged. Blaug saw the potential difficulty in his "The Methodology of Economics" (1980) and by 1991 in "Appraising Economic Theories" both Blaug and de Marchi recognized the multi-faceted criticism surrounding the MSRP. Overall, the methodology of scientific research program remains the "best fit" unit of appraisal to apply to the history of economics. However, recent criticisms have tempered the scholarly optimism of the 1970's.

What is the current status of the MSRP in economics? The twenty-year experiment of the MSRP and economics started with a bang and seems to be ending with a whimper. The optimism of the 1970's has been tempered, but potential for the MSRP remains. Perhaps other scholars will be able to explain the history of economics with the MSRP, giving us a way to understand the history of economics and the future of “Life Among the Econ”. For example, as Mark Blaug wistfully concluded the 1989 conference volume saying,

> Despite the jaundiced reaction to Lakatos of many of the Capri participants, I remain convinced that Lakatos is still capable of inspiring fruitful work in methodology. Vernon Smith *et al.*, Roger Backhouse and particularly Neil de Marchi summarize what is valuable in Lakatos and I do not need therefore to labour the point. In retrospect, I wish that we had succeeded in obtaining Lakatosian appraisals of such SRP’s as behavioural economics, post-Keynesian economics and both the old and new Institutionalism, each of which might have taught us lessons of their own. But perhaps that is an agenda for a third Lakatos conference in another five or ten years (Blaug 511).

So what ideas fill the space vacated by the Lakatosian MSRP? We have already seen how rhetorical criticism developed from analyzing the MSRP – what other new movements, perspectives and vocabularies are being developed?

Back to Popper: Re-Examining Critical Rationalism and Falsification

The relationship between critical rationalism and falsification is complex. Those who believe Popper aimed to use falsification to identify scientific progress themselves became enamored of the world of the Lakatosian MSRP. But with the demise of the “Received View”, and with the scholarly criticism of the MSRP, another perhaps more contemporary interpretation of Popper’s theory on science has re-emerged—critical rationalism.

The central tenet of critical rationalism is that a critical environment plays a vital role in answering questions about scientific epistemology. Critical rationalists are “less concerned with demarcating science from nonscience and more concerned with characterizing the social context necessary for the growth of scientific knowledge” (Hands *Reflections* 297). A critical environment has replaced the need for the empirical foundation for knowledge. As Hands describes,

> “Critical rationalism is normative *without providing any strict rules* for the conduct of scientific inquiry. The program asserts that there are *rational reasons* for believing in one theory rather than another, but these reasons are based on systematic *criticism* – criticism that in turn depends on the proper critical environment rather than on following any particular narrow set of methodological rules” (Hands *Reflection* 297-8)

Seen in this light, falsification is a particular case; the philosophy of critical rationalism applied to the issues concerning the logical positivists. Falsification then is not *the rule,* but instead, one perspective developed out of a critical environment.

Paul Feyerabend had argued in his older book *Against Method* that if there is one rule to govern science it is that there are no rules. Critical rationalists would counter that there is "…only one generally applicable methodological rule, and that is the exhortation to be critical and always ready to subject one's hypothesis to critical scrutiny (Klappholz and Agassi 1959, p. 60 in Hands *Reflection*). Consequently, the overarching issue facing critical rationalists is how to design "*scientific and educational institutions in a way that maximizes productive criticism*" (Hands *Reflection* 298). This later theoretical understanding falls in line with the ideology of a young Popper, whose philosophy underscored his own desire for social engineering, to end poverty and create liberty (Hacohen 46).

The re-emergence of critical rationalism is just one of many contemporary turns that fills the space vacated by the MSRP. The consideration of the social context of knowledge, highlighted by Popperian critical rationalism, has shifted the debate to subjects like the sociology of scientific knowledge and naturalized epistemology.

VII. The Naturalistic Turn

> [K]nowledge and belief, reference, meaning, and truth, and reasoning, explaining and learning, are each the focus of eroded confidence in "the grand old paradigm," a framework derived mainly from Logical Empiricism, whose roots in turn reach back to Hume, Locke, and Descartes…it is not that there has been a decisive refutation of "the grand old paradigm." Paradigms rarely fall with decisive refutations; rather, the become enfeebled and slowly lose adherents….But many of us sense that working within "the grand old paradigm" is not very rewarding. By contrast, there is considerable promise in a naturalistic approach….Epistemology conceived in this spirit is what W.V. Quine has called *naturalized epistemology*. (Churchland in Hands *Reflection 129*).

In order to understand the "turn" that Quine called *naturalized epistemology* one must understand its ideological predecessor. Traditionally, the *a priori* method of philosophical reflection and epistemology seemed to be at the top of a hierarchy of ideas, towering over the philosophy of science and science. Prior to Quine's work, the philosophy of science was commonly understood as applied epistemology. Epistemology dictated how the philosophy of science should function, which in turn determined the rules for science known as the scientific method. However, as a result of the undermining of the received view and traditional "mainstream philosophy", this long preserved hierarchy was called into question (Hands *Reflection* 131).

Enter *naturalism*, as scholars began to look for other ways to relate science, philosophy of science, and epistemology. Before, philosophers would decide what was knowledge and use that information to help science. But Naturalism inverts the previous hierarchy of ideas. As Hands simply notes, "Naturalists start with science (*a posteriori*) and use it to assist with philosophy (previously *a priori*)" (Hands *Reflection 132)*. As a result, the determining factor in epistemology is science itself.

The umbrella of naturalism covers many different points of view, but all depart from the same rejection of *a priori* philosophy informing science. Some versions of naturalism significantly limit the scope of epistemology to a single scientific theory and allow epistemology a broader scope. Nonetheless, the general naturalistic view runs into some very real problems.

A primary issue used to identify naturalists is *prescription* as opposed to *description*. Science shows what state the natural world is in, it is descriptive. However, one cannot deduce what "should be" from what "is". This conundrum is referred to as "Hume's Guillotine" Thus, it is impossible for science to inform any normative theory and subsequently, prevents any normative epistemology. Hands indicates that the problem with naturalism is simply "the application of Hume's Guillotine to *naturalized epistemology*". Naturalists have absolutely no interest in normative epistemology.

Naturalism faces many other issues. There is a debate if science should be used to reform or to replace existing epistemology. In addition, there is some question as to "what to naturalize on?" (Hands *Reflection* 132-6) and which science should inform conclusions about the philosophy of science. Also, there is concern over circularity in analyzing science. The potential exists to use science to create a normative theory in philosophy of science and then to use this normative theory to analyze the positive science which informed the theory to being with[6].

[6] The goal here is to demonstrate the richness and complexity of naturalism. For a more complete discussion see Hands *Reflection*, pp. 131-136.

Despite these substantial challenges naturalism is an intriguing turn in the history of science. The intellectual history of this movement has been discussed, but Hands also presents a more practical approach to the development of naturalism saying, "One wants to stand on the firmest available ground, and currently the ground beneath our best scientific practice seems to be much less squishy than that which supports empiricist epistemology" (Hands *Reflection 132).* The "squishy ground" also contributed to a related "turn" in contemporary science theory, the sociological turn.

VIII. The Sociological Turn

The breakdown in the Received View stripped science of its infallibility, enshrined since the seventeenth century. As empiricism came into question, scholars began to examine the role of human agency and social forces in the growth of knowledge. The sociology of scientific knowledge (SSK) focused on social and cultural aspects of science. As Pickering noted,

> "The great achievement of SSK was to bring the human and social dimensions of science to the fore. SSK, one can say, thematized the role of *human agency* in science. It thus partially displaced the representational idiom by seeing the production, evaluation, and use of scientific knowledge as structured by he interests and constraints upon real agents. Scientific beliefs, according to SSK are to be sociologically accounted for in just those terms (Pickering in Hands *Reflection 175).*

Just as scientific results are described with literary devices in the rhetorical turn, scientific results are created by the sociological turn. In physics, “You can get *resistances* in the laboratory; but in order for these resistances to make sense, they have to be interpreted. The very moment you interpret them, you enter the realm of the social world” (Knorr Cetina in Hands *Reflection* 192). In economics, one can find the deadweight loss associated with price floors. However, as soon as this area is interpreted and further applied it becomes sociological scientific knowledge. The role of human agency introduces an entirely new element to previously objectively infallible science. The critical and unavoidable role of humanity in science is reminiscent of the Romantic and Humanistic response to 17th century scientific certainty. For SSK scholars, science is a world where humans and other agents interact and create scientific knowledge.

In his book *Changing Order* (1985), Harry Collins explored SSK in the several specific cases of testing the accuracy of the TEA-laser, the detection of gravity waves, and the parapsychological studies examining the emotional life of plants. In that book Collins considered the issue of replicability; if it played an important role in science then it should play a different role in pseduoscience. The similarities between Collins and Popper begin and end with this demarcational issue because Collins endeavors to make a separate point. In trying to determine whether gravity waves hit the earth in detectable fluxes Collins found, "…we won't know if we have built a good detector until we have tried it and obtained the correct outcome! But we won't know what the correct outcome is until…and so on *ad infinitum*" (Collins in Hands *Reflection* 194). This case shows how scientific knowledge is socially constructed and highly contingent on human agency. As Collin says, "It is not the regularity of the world that imposes itself on our senses but the regularity of our institutionalized beliefs that imposes itself on the world." (Collins in Hands *Reflection* 194). As Hands notes, Collins work renders science, "relatively impotent" (Hands *Reflection* 194).

XI. Conclusion

A history that began with an attempt to demarcate good science from bad science has evolved into a series of increasingly complicated intellectual controversies. The dichotomy of the "nature pole" against the "social pole" has fueled a conflict between naturalism and the sociology of scientific knowledge. Furthermore, competing ideas about agency have resulted in fascinating new middle grounds like Actor-Network Theory, in which natural and human actors all have agency in an increasingly complex web of scientific knowledge. The specifics of contemporary debates in science theory and economics are not important for the purposes of this paper. It is sufficient to note the controversies in such diverse subject areas as, the science wars, the chicken debate, the issue of reflexivity, rhetoric in economics the naturalistic view, SSK, ESK, positive vs. normative ("is" vs. "ought"), the role of social agency in economics, etc. These new debates and disciplines are the answer to Lawrence Boland's criticism of methodological "appraisers", "Do they accuse economists of being unscientific? Who cares?" (Hands *Reflection* 1). The point is that demarcation is no longer the issue. Economists will not change their methodologies because of new understandings in science studies. Nonetheless, re-examining the economist's self-image in a critical environment creates new perspectives on economic and scientific knowledge. Critical rationalism is not about getting economists to change their methods or alter their research. The story of critical rationalism is finally about historiography more than economic methodology.

Anyone interested in writing a new history, exploring unexplored stories and debating unresolved conflicts has a stake in these matters. The contemporary turns demonstrate intriguing new perspectives on the construction of knowledge and its effects on humanity and the evolution of society. New understanding of scientific or economic knowledge offers a new standpoint from which to examine history from both inside and outside the world of the economist.

The question of whether economics is a science began an ideological evolution, which taught us a great deal about economics, a great deal about science and even more about the role of human interaction in both. Critical rationalism argues that the undermining of scientific certainty over the last century should not be examined from just one perspective. As Kadvany finds in his biography of Lakatos, questioning the foundations of knowledge can be more important then creating them:

> Not only are there no foundations for knowledge, we are constantly subject to the danger of creating foundations for the antiethics of Stalinism, the antiscience of Lysenko genetics, and the antihistories of Muscovite historians….Hungarian Stalinism shows that historical writing may be stood on its head, then heroically turned over again, and that historical reason is a body of strategies waiting to be seized. And almost anybody can seize them. “The analogy between political ideologies and scientific theories,” Lakatos writes, “is then more far-reaching than is commonly realized” (Kadvany 316).

Our own story is a history of ideas about science, economics, agency and human society, which were constructed, deconstructed and reconstructed. As long as the critical environment, developed by Popper and fostered by contemporary critical rationalism, persists, the story will remain unfinished. What might emerge is a historiography with the vital, yet daunting task, of looking backwards and thinking forwards.

References

De Marchi, Neil and Mark Blaug, eds. *Appraising Economic Theories: Studies in the Methodology of Research Programs.* Aldershot Hants, England: Edward Elgar, 1991.

Feyerabend, P. K. *Against Method*, London: NLB, 1975.

Hands, Wade D., *Reflection Without Rules: Economic Methodology and Contemporary Science Theory*. New York: Cambridge University Press, 2001.

Kadvany, John. *Imre Lakatos and the Guises of Reason*. Durham: Duke University Press, 2001.

Klamer, Arjo, D. McCloskey, and Robert Solow, eds. *The Consequences of Economic Rhetoric.* Cambridge: Cambridge University Press, 1988.

Hacohen, Malachi Haim. *Karl Popper: The Formative Years 1902-1945.* Cambridge University Press, 2000.

Kuhn, Thomas .S., *The Structure of Scientific Revolutions*, University of Chicago Press, 1962, 2nd edition, enlarged, 1970.

Lakatos, Imre. *History of Science and Its Rational Reconstructions, Boston Studies in the Philosophy of Science*, Vol.8, 1971.

Latsis, Spiro J. *Method and Appraisal in Economics*. New York: Cambridge University Press, 1976.

Leijonhufvud, Axel. "Schools, `Revolutions,' and Research Programmes in Economic Theory" [1976], reprinted in Leijonfufvud, *Information and Coordination*. Oxford: Oxford University Press, 1981.

McCloskey, D. *The Rhetoric of Economics.* Madison: The University of Wisconsin Press, 1985.

Popper, Karl R. ***Conjectures and Refutations:*** *The Growth of Scientific Knowledge.* New York: Harper & Row, 1963.

Weintraub, E. Roy. *General Equilibrium Analysis: Studies in Appraisal*, Cambridge University Press, 1985

Weintraub, E. Roy. *Microfoundations*, Cambridge University Press, 1979.