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CHAPTER 1

FALSIFICATION, SITUATIONAL ANALYSIS AND SCIENTIFIC RESEARCH PROGRAMS: THE POPPERIAN TRADITION IN ECONOMIC METHODOLOGY

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INTRODUCTION

No other philosopher and his work have influenced economic methodology as much as Karl Popper; yet, in over fifty years of philosophical writing Popper explicitly considered economics in only a few rare cases. From the economic profession's introduction to falsificationist ideas in Hutchison (1938) to the recent spate of Lakatosian case studies in the history of economic thought,[1] no major issue in economic methodology has been discussed without a significant Popperian "voice." [2]

It is the purpose of this essay to critically re-examine the Popperian influence in economic methodology. The presentation will be in three parts, each corresponding to one of the three main points of contact between the Popperian tradition and the literature on economic methodology. The first section examines falsificationism: Popper's well-known approach to the philosophy of natural science. The second section discusses "situational analysis": Popper's less well-known approach to the social sciences. The final topic considered is the work of Imre Lakatos and how it has been applied to the history of economic thought. Lakatos' philosophical position is certainly different than Popper's, but his work is clearly enough within the general Popperian tradition to be included in this re-examination of Popper. In each of the three areas the focus will be on the literature that explicitly concerns economics; there will not be any effort to discuss general philosophical arguments or evaluations based on other scientific disciplines. Throughout the chapter survey material is provided to help familiarize the reader with the relevant literature, but the chapter does not provide an exhaustive survey of any of the three topics.

FALSIFICATIONISM

No doubt economists, philosophers, and members of the academic community in general, know Karl Popper best for his falsificationist approach to the philosophy of science. First presented in *Logik der Forschung* in 1934 (English translation, Popper (1968)), falsificationism represents Popper's approach to the growth of knowledge as well as his solution to (or dissolution of) the traditional problem of induction. It is for his falsificationism that Popper claims responsibility for the death of logical positivism.[3]

Actually, Popperian falsificationism is composed of two separate theses: one demarcational (concerned with demarcating science from nonscience) and one methodological (concerned with how science should be practiced). The demarcation thesis is that for a theory to be "scientific" it must be at least potentially falsifiable, that is, there must exist at least one empirical basic statement that is in conflict with the theory.[4] This potential falsifiability is a logical relationship between the theory and a basic statement; in particular, the demarcation criterion does not require that anyone has actually tried to falsify the theory, only that it would be logically possible to do so. Popper's demarcation criterion has been the subject of extensive debate in the philosophical literature, but it is seldom an issue in economics—possibly because most commentators feel that economic theories are potentially falsifiable.[5] For economists who advocate a falsificationist position, the more important issue is methodology rather than demarcation, and Popperian *methodology* requires the practical (rather than merely the logical) falsifiability of scientific theories.

Briefly, and neglecting a host of philosophical issues, Popper's falsificationist methodology requires the search for scientific knowledge to proceed in the following way. Start with a scientific problem situation: something requiring a scientific explanation.

Second, propose a bold conjecture that might offer a solution to the problem. Third, severely test the conjecture by comparing its least likely consequences with the relevant empirical data. The notion here is that a test is *more severe* the more prima facie *unlikely* the consequence tested; the theory should be forced to "stick its neck out," to "offer the enemy, namely nature, the most exposed and extended surface."[6] Finally, the last move in the game depends on how the theory performed during the third testing stage. If the implications of the theory were not supported by the evidence, the conjecture is falsified and it should be replaced by a new theory that is *not ad hoc* relative to the original.[7] If the theory was not falsified then it is considered corroborated by the test and it is accepted provisionally. It should be noted that given Popper's fallibilism, this acceptance is provisional forever; the method does not guarantee the surviving theory is true, only that is has faced a tough opponent and won.

There are a number of reasons why such a falsificationist methodology might be particularly appealing to economic methodologists. If the task of economic methodology is viewed (as it has been until quite recently) as "choosing" among various philosophies of natural science in order to "apply" one to economics, then Popperian falsificationism has some clear advantages over anything that might be borrowed from the positivist tradition. For one thing, falsificationism is eminently more straightforward and intuitive than the inductive logic of the later logical empiricists. Perhaps more important is the fact that Popper's falsificationism is truly a *methodology*. Unlike philosophers in the positivist tradition, Popper was not trying to provide an epistemic justification for the knowledge claims of science. Popper's goal was the more mundane task of characterizing a set of rules (a method) which would allow us to learn from experience. This distinction between methodology and justification is critical to understanding the Popperian preference of the economics profession. The reason is that by and large the positivist tradition was *not* a tradition of methodological rule making. Most logical empiricist philosophers were entirely convinced that science proceeded by induction; their philosophical task was to *justify* that procedure. Such a justificationist philosophy of science provides little or no guidance to an economics profession in search of scientific rules. The question for economic *methodology* is not to provide a philosophical justification for science, but rather to find a set of rules that can be followed so that economists can do whatever it is that scientists do.[8]

Another reason for the support of Popperian falsificationism among economists is that it seems to solve (or dissolve) the old induction versus deduction debate in economics; it provides a tidy way out of the *methodenstreit* without making either side the overall winner. Consistent with the apriorist/deductivist tradition in economics, the falsificationist method would allow hypotheses based on introspection and/or the supposition of rational action. Consistent with the historical/inductivist tradition in economics, falsificationism requires empirical testing and the discipline of the data. Popperian falsificationism seems to allow the profession to take advantage of what is best in each of these traditional approaches to economic research. It is permissible to leap to conjectures about economic behavior without the extensive accumulation of empirical data which would be required if only inductive generalizations were allowed, while at the same time (unlike the Misesian approach) the facts do matter and acceptable hypotheses must survive severe tests. This "best of both worlds" property makes falsificationism a natural philosophical companion to the Marshallian tradition in economics, a characteristic that provides for its support by many methodologists.

In addition to these philosophical issues there are possibly some forces of attraction that should be classified as "sociological" (and/or personal, and/or ideological). In particular, Popper's direct influence on a number of influential London School of Economics economists,[9] and his longstanding relationship with Hayek, may have contributed to Popper's popularity among economists. Certainly, citations originating from these two sources made Popper's name familiar to many economists who would not have otherwise been aware of his work. Finally, it is possible to find an ideological connection. Popper's own work in social and political philosophy[10] is decidedly antihistoricist and antiMarxist: views that are (at the very least) not inconsistent with those of most mainstream economists. These "sociological" and/or "ideological" factors do not directly support a falsificationist methodology for economics; rather, they explain why economists might consider Popper an "acceptable" philosopher and thereby (since falsificationism is Popper's most well-known view) lend indirect support to falsificationism in economics.[11]

Now despite all of these reasons why falsificationism might be a desirable methodology for economics, the fact is that *falsificationism is seldom if ever practiced in economics*. This seems to be the one point generally agreed upon by recent methodological commentators.

In fact, this (empirical) claim is supported at length by the case studies in Blaug (1980), a work that consistently advocates falsificationism as a normative doctrine. The disagreement between critics and defenders of falsificationism is *not whether it has been practiced*, basically it has not, but rather *whether it should be practiced*. The real questions are whether the profession should "try harder" to practice falsificationism, though it has failed to do so in the past, and would the discipline of economics be substantially improved by such falsificationist practice.[12]

One way to answer such queries about the appropriateness of falsificationism in economics would be to consider the appropriateness of Popper's falsificationist methodology as a *general* approach to the growth of scientific knowledge. Falsificationism may not be appropriate for economics, even if it is a good model for the growth of knowledge in natural science, but if it fails in natural science then its usefulness in economics is surely in doubt. Unfortunately, such an excursion into the vast philosophical literature criticizing Popperian falsificationism is far beyond the scope of the current essay.[13]

Rather than delving into the more general philosophical literature, the next section will simply list some of the criticisms of falsificationism that can and have been raised explicitly within the context of economics. These criticisms may of course overlap with more general concerns, but even so, only economics will be discussed. The list is not exhaustive, but it does capture many of the problems exhibited by a falsificationist methodology in economics. They are not listed in any particular order of importance.[14]

1. The Duhemian problem[15] and related issues pose insuperable problems for falsificationist practice in economics. There are a number of reasons why this is the case. First, the complexity of human behavior requires the use of numerous initial conditions and strong simplifying assumptions. Some of these restrictions may actually be false, such as the differentiability of production functions or the infinite divisibility of commodities. Some of these restrictions may be logically unfalsifiable, such as the assumptions of eventually diminishing returns or eventually decreasing returns to scale. Still others of these assumptions may be logically falsifiable but practically unfalsifiable, such as the completeness assumption in consumer choice theory. And finally, most of these restrictions are extremely difficult to test for because of the absence of a suitably controlled laboratory

environment. The presence of such a variety of restrictions makes it virtually impossible to "aim the arrow of *modus tollens*" at one particular problematic element of the set of auxiliary hypotheses when contrary evidence is found.

Secondly, in addition to these problems with auxiliary assumptions, there is no consensus regarding the empirical basis in economics. It is always possible to argue that what observed was "not really" involuntary unemployment or "not really" economic profit, etc. Now, it is a fundamental part of the Popperian program that the empirical basis need not be incorrigible, but Popper does require a generally accepted convention on the empirical basis.[16] In economics, such a conventionally accepted empirical basis often does not exist.

Finally, it should be noted that social sciences can have feedback effects that do not exist in the physical sciences. The test of an economic theory may itself alter the initial conditions for the test. Conducting a test of the relationship between the money supply and the price level may alter expectations in such a way that the initial conditions (which were true "initially") are not true after the test (or if the "same" test were conducted again).[17]

2. The qualitative comparative statics technique used in economics makes severe testing very difficult and cheap corroborational success "too easy." Even with the auxiliary assumptions discussed in (1.) still, it is very often the case that the strongest available prediction is a qualitative comparative statics result that only specifies that the variable in question increases, decreases, or remains the same. Since being of the correct sign is much easier than being of the correct sign and magnitude, this qualitative technique generates theories that are low in empirical content, have few potential falsifers, and are difficult, if not impossible, to test severely. The result is often economic theories that are corroborated, but trivial.[18]

3. Popper's "admitted failure" (1983, xxxv) to develop an adequate theory of verisimilitude[19] presents problems for a falsificationist methodology in economics. The problem of verisimilitude developed in an attempt to reconcile Popper's falsificationist methodology with his scientific realism. For a realist the aim of science is "true" theories; according to falsificationism, scientific theories should be chosen if they have been corroborated by passing severe tests. If the falsificationist method is to fulfill the realist aim of science, it should be demonstrated that more corroborated theories are closer to the truth; this is the goal of Popper's theory of verisimilitude.

Actually, a satisfactory theory of verisimilitude would serve Popperian philosophy in two separate ways. The first, mentioned above, would be to provide an epistemic justification for playing the game of science by falsificationist rules. This issue is extremely important for Popperian philosophy since it means that without a theory of verisimilitude there are philosophically "no good reasons" (Popper, 1972, 22) for choosing theories as Popper recommends. The second function of a theory of verisimilitude is more practical; it would provide some rules for choosing the "best" theory in troublesome cases. This is because a theory of verisimilitude would provide rules for discovering which of two theories has more verisimilitude, which is a better approximation to the truth. Thus, if we had two theories and both had been falsified, we could choose the one with more verisimilitude. Notice that falsificationism without a theory of verisimilitude is of no help in such cases; since both are false, both are *out* (similarly for cases involving a choice between a falsified but bold theory and a corroborated but modest theory).[20] Again having a way to determine which is closer to the truth might allow us to choose a theory more consistent with the aims of science than simple falsificationist rules.

This second, more practical, function of the theory of verisimilitude is extremely important for economic methodology. For all the reasons discussed above, and perhaps others as well, economists are almost always faced with choosing between two falsified theories or between a bold falsified theory and a more modest corroborated one. If Popper's theory of verisimilitude had been a success, and it could be added to the norms of simple falsificationism (both to justify the norms and to help in making the practical decisions of theory choice) then falsificationism might have an important role in economics. Without the link between severe testing and truthlikeness, the method is of limited value in pursuing the realist aim of science.

4. Popper's rules for progressive theory development (non *ad hocness*) are often inappropriate in economics. Popper argues that if one theory is to constitute "progress" over a predecessor, the new theory must be "independently testable"; it must have "excess empirical content," predict "novel facts."[21] This issue will be examined in more detail in the Lakatos section below, but for now let it be said that while progress of this Popperian type may sometimes be of interest to economists, often progress in economics is (and should be) much different. Often economists are concerned with finding new explanations for well-known stylized (non novel) facts, or alternatively, economists are concerned with explaining the same phenomena with fewer theoretical restrictions. What consititutes "progress" in economic theory (or what should constitute progress) is a complex and ongoing question, but it is apparent that any suitable answer will require a much more liberal set of standards than those offered by strict Popperian falsificationism.

All of these criticisms do not bode well for a falsificationist economic methodology. Despite all of the reasons why a falsificationist methodology might be attractive, it fails to provide an adequate set of rules for doing economics. Strict adherence to falsificationist norms would virtually *destroy all existing economic theory* and leave economists with a rule book for a game unlike anything the profession has played in the past. This high cost would be paid without any guarantee that obeying the new rules would result in theories any closer to the truth about economic behavior than those currently available.

Now, of course, denying that falsificationism provides the proper methodological rules for conducting economics does *not* mean that "the facts" should not matter in economic theory choice or that empirical testing is not important. This type of argument is a quite common red herring in methodological discussion involving falsificationism in economics. Popperian falsificationism is not generic empiricism; it is a *very* specific set of rules about how scientific inquiry should be conducted. Abandoning Popperian falsificationism as a methodology does not mean abandoning learning from experience.

SITUATIONAL ANALYSIS

While economic methodologists have long been concerned with Popperian falsificationism, Popper's views on situational analysis have only recently become an explicit part of the literature on economic method. The most likely reason for this neglect is the relative (to falsificationism) inaccessibility of Popper's work on the topic. The staunchest supporters of situational analysis have been social science oriented philosophers, such as I. C. Jarvie, who became familiar with the argument through Popper's lectures,[22] and while the central thesis was presented in Popper's work on social and political philosophy (1961, 1966), the clearest presentation of the argument is Popper (1967), a paper only recently translated from the original French (Popper 1985). Other presentations of the topic are scattered about in works such as Popper (1976a); a paper written as part of a debate with the Frankfurt School of sociology.

Situational analysis is Popper's method for the social sciences.[23] In fact, he argues that situational analysis is the *only* method appropriate for the social sciences.[24] Now, since economics is surely a *social* science, there is a paradox in the fact that economic methodologists have focused almost exclusively on falsificationism, Popper's philosophy of natural science, and neglected situational analysis. This paradox, though explicable in terms of the relative inaccessibility of Popper's writings on situational analysis, is even more pronounced since situational analysis *is the method of economic analysis*.[25]

According to Popper's situational analysis, explanations of human behavior should proceed as follows. Suppose the problem is to explain why agent A engaged in some particular type of behavior, say X. The first step in explaining this behavior is to describe the "situation" of the agent at the time the behavior in question took place. This description of the agent's situation will normally include both subjective components (the agent's goals, beliefs, desires, etc.) as well as objective components (physical and social constraints the agent faces). The second step in the explanation is to provide an analysis of the situation; to specify what type of behavior would be appropriate (i.e. rational) given the agent's situation. The third part of the explanation is to add, and this is the key, the *rationality principle* (RP), which asserts that *all* individuals *actually act in a way that is appropriate* to their situation (that is they act rationally). This RP allows us to deduce the act of the agent from the description of his/her "situation" and our analysis of what constitutes appropriate behavior. The RP is a bridge-principle that connects the "situation" with an "action;" it "stands in for the 'law' that 'animates' the otherwise inert collection of situational features" (Latsis 1983, 133).

Schematically then, the situational analysis explaining why agent A did X has the following form.[26]

I. Description of the Situation:	Agent A was in situation S.
II. Analysis of the Situation:	In situation S the appropriate (rational) thing to do is X.
III. The RP:	Agents always act appropriately (rationally) given their situations.
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IV. Explanandum:	Therefore: A did X.

It is easy to see that situational analysis is the method of microeconomics (and any macroeconomics based on micro foundations). Economists specify the situation of the agent (individual or firm) usually in terms of the preferences and/or technology and the relevant constraints (prices, income, factor constraints, etc.). Included in the description of the situation is some "motivating" consideration (maximizing utility, maximizing profit, etc.). The second step is to deduce the appropriate behavior of the agent given the situation specified (buy more, buy less, increase production, decrease production, etc.). This second step is what constitutes most of economic *theory*, the formal deduction (usually mathematical) of the "appropriate" behavior in a particular "situation." Finally, if the economist's task is to explain an observed action, the RP is activated to connect the analysis of the situation with the action to be explained. If the task is "pure theory," then this latter step is neglected and the "theoretical result" is technically deducing step II from a hypothetical situation in step I. Comparative statics results are simply performing the deduction from I to II twice, with a slight change in an element of the situation I between the deductions. Aggregative

phenomena, such as equilibrium prices, are explained by adding two additional steps to the above scheme: V and VI. Step V adds additional analysis about the aggregate impact of a number of agents (A_1, A_2, ... A_n) each doing the appropriate thing, $X = (X_1, X_2, \dots X_n)$. The analysis in step V takes the following form: if all A_i's do X_i; then the aggregate result will be Y. Step VI is then an aggregative explanandum: therefore Y.

Notice that such an explanatory scheme really captures all of (at least micro) economics, not just the textbook versions.[27] For example, the great debates over whether firms maximize profits, or satisfice, or mark-up prime costs, are not debates that alter the above scheme as the basic method of explanation. These are only debates about what constitutes an empirically interesting specification of the situation the agent (in this case the firm) faces. What is rigid about traditional textbook microeconomics is not that it requires adherence to the above scheme, but rather that *only certain things* are permitted in the description of the agent's situation. For instance, the conventions of the profession traditionally allow only preferences and technology as the private parts of the agent's situation, and only prices as the acceptable objective (public) constraints.

In summary then, Popper proposes situational analysis (hereafter SA) as the only general approach for providing explanations in social science and microeconomic explanations satisfy this criterion; microeconomic explanations are special cases of SA explanations. This relationship between SA and economics raises a number of issues; some of these issues involve Popper's SA approach itself (and therefore economics), while others involve the particular form that economic explanations take *within* the general SA framework.

The most important question for SA itself is that it produces "scientific" explanations that do not satisfy Popper's own (falsificationist) criteria for such explanations.[28] According to Popper the falsificationist, the universal generalizations used in a scientific explanation should be scientific theories. This means, as discussed above, that such generalizations should (1) be falsifiable, and (2) have actually passed severe tests (be corroborated). Now consider the RP. It serves as the universal generalization in such a SA explanation, it is the "law" in the explanation, and yet its nomic status is unclear.

Some claim that the RP is simply unfalsifiable; there exists no observation that would require us to give up (would logically conflict with) the claim that the agent is acting appropriately to the situation.

It can always be argued that there is something in (the subjective part of) the situation, unknown to us, that renders the action appropriate. Others (most philosophical commentators on the issue) argue that the RP *is* falsifiable, but that it should never be abandoned; that when faced with a potentially falsifying observation we should "*cling to the rationality principle* and revise or hypothesis about his aims and beliefs" (Watkins 1970, 173).[29] In either case though, whether the RP is unfalsifiable or whether we simply choose by methodological fiat to ignore its falsification, it is not the kind of universal generalization that Popper, the falsificationist, would allow in a scientific explanation. Thus, if we insist on Popper's demarcation criterion, social science explanations relying on the RP "are not *bona fide* scientific explanations" (Koertge, 1974, 201).[30] Or, even more strongly, since philosophers of science have traditionally considered the provision of scientific explanations to be an (possibly *the*) important aim of science, social science, including economics, is not *science* after all. This is certainly relevant to economics, but it is also relevant to the entire Popperian program in philosophy of science since Popper explicitly developed his demarcation criteria to demarcate scientific theories from what he considered pseudo-science: Marx and Freud (Popper, 1976b, 41-44). These social theories can hardly be criticized for not doing what the very best social sciences (in Popper's view) do not do.[31]

Before turning to the second type of questions raised by SA explanations, it should be noted that the above questions are fundamental philosophical issues. Some claim that to give up such explanations in social science would amount to abandoning free will to complete determinism.[32] On the other hand, explanations involving RP, unlike explanations in the physical sciences, are not causal; there is no mechanism connecting the situation with the act. It has been argued that it is precisely this causality avoidance, and thus freedom preserving characteristic, that makes SA explanations so attractive to Popper; "Popper wishes to escape the ugly consequences of what he considers to be Hume's dilemma by developing an account of behavior which is neither random nor determined but somewhere in between" (Latsis, 1983, 137).

Returning now to the more practical concerns of economic methodology, what can be said about microeconomic explanations *as* SA explanations? In other words, what if we disregard the above general criticism of SA explanations and focus on the particular form which SA explanations take in economics? What is the lesson here for economic methodology?

The lesson is that not much can be learned from Popper's writings on SA or related work by other philosophers. If we accept SA explanations as a fact of life in social science, then all of the "action" in economics must occur in the description of the agent's situation, and economists are left with all of the traditional questions regarding theory choice in their discipline. Since the RP (stage III) is in *every* explanation, it is the same from one "theory" to the next. The analysis step (step II) is of course different for each different posited explanation, but since this second step is mostly deduction from the specifications of the agent's situation (step I), it is relatively mechanical (though it may be technically quite complex). It seems the really creative part of economics and the place where different "theories" compete for attention is in the description of the agent's situation (step I). Economists must make decisions about how to specify the (subjective and objective) situation of the agent so that economic behavior may be predicted or explained. Recognizing that explanations in economics are really all Popperian SA explanations doesn't "help" with the fundamental issues of theory choice in economics. Economists must still make decisions about how the facts will influence their choices, how to modify the specification of the agent's situation when a prediction fails, what nonempirical considerations should influence theory choice, etc.

Popper and other philosophers writing on SA have focused on basically two issues. The first, discussed above, has been the question of the nomic status of the RP. The second has been to demonstrate (by example) the success of SA in various social sciences. Now the former question is an important philosophical issue that has an impact on the ultimate epistemic standing of economics, but it seems to have little to do directly with day to day matters of theory choice.[33] The latter issue, no matter how psychologically satisfying the results might be, has no effect on the economics profession since economics is already the source of the most successful applications of SA. Thus while economic explanations are SA explanations, and while such explanations raise important philosophical issues, the Popperian literature on SA has little to offer (at least at this time) economic methodologists concerned with the hard questions of why economists should choose one theory over another.

LAKATOS' METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMS

Lakatos's work in the philosophy of science first appeared in the early 1970s (Lakatos, 1970, 1971) and it was adopted almost immediately by a number of economic methodologists. Numerous papers on Lakatos appeared in the economics literature, many as a result of the Nafplion Colloquium on Research Programmes in Physics and Economics in 1974.[34] The literature on "Lakatos and economics" has taken basically two (non-mutually exclusive) forms. The first is principally historical, attempting to "reconstruct" some particular episode in the history of economic thought along Lakatosian lines; the second is more in the spirit of traditional work in economic methodology, attempting to appraise Lakatos's methodology of scientific research programs as an economic methodology and/or compare it to other philosophies such as Popper or Kuhn.[35]

In many respects Lakatos's methodology of scientific research programs (MSRP) is an extension of the evolution of the Popperian tradition in the philosophy of science, but in other respects it is very different, addressing issues raised by other philosophers such as Kuhn (1970) and those in the historical tradition. For Lakatos, the primary unit of appraisal in science is the "research program" rather than the scientific theory. A research program is a rather loose amalgam consisting of a hard core, positive and negative heuristics, and a protective belt.[36] The hard core contains the fundamental metaphysical presuppositions of the program; it defines the program, and its elements are treated as irrefutable by the program's practitioners. To participate in the program is to accept and be guided by the program's hard core. For example, in Weintraub's Lakatosian reconstruction of the NeoWalrasian research program in economics, the hard core consists of propositions such as: agents have preferences over outcomes, agents act independently and optimize subject to constraints, etc.[37] The positive and negative heuristics, respectively, are instructions about what should and should not be pursued in the development of the program. The positive heuristic guides researchers toward the right questions to ask and the best tools to use in answering them; the negative heuristic advises on what questions should not be pursued and what tools are inappropriate. Again, using Weintraub's analysis of the NeoWalrasian program as an example, the positive heuristic contains injunctions such as: construct theories where the agents optimize, while the negative heuristic implores researchers to avoid things like theories involving irrational behavior. Finally, the protective belt consists of the program's theories, auxiliary hypotheses, empirical conventions, and the (evolving) "body" of the research program. All of the activity of the program occurs in the protective belt as a result of the interaction of the hard core, the heuristics, and the program's empirical record. For the NeoWalrasian program it is argued that the protective belt includes almost all of applied microeconomics.

A research program is appraised on the basis of the theoretical activity in the protective belt. There is *theoretical progress* if each change in the protective belt is empirical content increasing, if it predicts novel facts.[38] The research program exhibits *empirical progress* if this excess empirical content is confirmed (Lakatos, 1970, 118). Lakatos considers a third type of progress, heuristic progress, that requires the changes to be consistent with the hard core of the program. His definitions of theoretical and empirical progress presuppose that conditions for this latter type of progress have been satisfied.

Lakatos's Popperian lineage is evident in a number of ways. One of these ways is in his characterization of empirical content and novel facts. For Lakatos, like Popper, "The empirical basis of a theory is the set of its potential falsifiers: the set of those observational propositions which may disprove it" (Lakatos, 1970, 98, n. 2). Thus, while Lakatos clearly considers progress to be achieved through empirical confirmation, rather than falsification, his characterization of the tension between theory and fact is fundamentally falsificationist. Also with respect to empirical content, Lakatos is clearly Popperian in his "conventionalism" about the empirical basis.[39] Finally, the Popperian spirit is evident in the way Lakatos defines "metaphysical" and his recognition of the importance of metaphysics in science.[40]

On the other hand, there are some very unPopperian things about the MSRP. Most important is the complete immunity of the hard core to empirical criticism; the idea that it is appropriate to completely immunize any part of scientific theory is in direct conflict with Popper's "nothing is sacred" falsificationist philosophy of science. Certainly, Popper recognizes that science has experienced periods of Kuhnian "normal science" where the critical spirit was temporarily arrested, but for Popper this is something to lament not praise (Popper, 1970). Another point of disagreement is obviously confirmation versus falsification. Other than the way Lakatos defines empirical content, he has little regard for falsification. For Lakatos all theories are "born refuted" (1970, 120-21), and the real task of philosophy of science is to develop a method of theory appraisal that *starts* from this fact. Finally, Lakatos embraces an historical metamethodology whereby the actual history of science can be used to appraise various methodologies of science.[41] For Popper, methodology is purely a normative affair and there is no sense in which the actual history of science can be used to "test" methodologies.

These places where Lakatos splits with Popper are the places where Lakatos is most likely to win the favor of economists since they happen to be areas of substantial tension between falsificationism and economics. Certainly, economics is replete with "hard cores." Not only is the rationality principle protected from refutation, but individual economic theories harbor hard core propositions as well: Weintraub's hard core elements of the NeoWalrasian program being an excellent case in point. While not all economists would agree on exactly what these hard core propositions should be in any particular domain of inquiry, there seems to be a consensus that such hard core presuppositions exist and that they necessarily define alternative research programs in economics. A philosophical program such as Popperian falsificationism, which requires practitioners to be willing to give up any part of their research program at any instant, can hardly provide a guide for doing economics as appropriate as one (such as Lakatos's) that allows for these pervasive hard cores. So too for the issue of confirmation versus falsification. It is clear that falsificationism has not been practiced in economics and to arbitrarily enforce it would essentially eliminate the discipline. On the other hand, there *is* a great amount of empirical activity in economics. The facts do matter, but they matter in a much more subtle and complex way than falsificationism allows. As Weintraub states, "the idea that facts can falsify theories, and that the role of applied work is to produce facts that falsify the theories that the theorists create, is simultaneously to misunderstand facts, theories, tests, and falsification" (1988, 222). Surely, Lakatos' notion of empirical progress is more like what the best empirical work in economics does and should do than Popperian falsificationism.

Finally, Lakatos (unlike Popper) has emphasized the role of the history of science in supporting particular methodological proposals. Of course, this question of the proper relationships between the

history of science and the philosophy of science is a very complex issue that continues to be debated in philosophy, but it is clearly the case that economists have recently been sympathetic to methodological proposals that are sensitive to the actual history of their discipline. Economists have produced quite a vast literature that uses the Lakatosian categories to reconstruct various parts of the history of economic thought. Standard practice in such literature is to choose a particular part of economic theory (past or present) and then try to isolate and identify the hard core, the positive and negative heuristics, and the type of theoretical activity occurring in the protective belt. The bottom line in such work is usually a positive or negative appraisal of the "progressivity" of this particular part of economics.[42] Examples of such reconstructions range in topic from Jevons, Menger and Walras (in Fisher, 1986), to rational expectations macro (in Maddock, 1984), to Henry George (in Petrella, 1988).

An overall assessment of this Lakatosian reconstruction literature is very difficult because of its vastness and diversity and also for a more fundamental reason. This second reason is that many economists writing in the field have taken very little care in the way the Lakatosian terminology is used. This lack of fidelity to Lakatos's concepts results in "hard cores," "heuristics," and (particularly) "novel facts" that bear little resemblance to their Lakatosian analogs or how these terms have been used in reconstructions in the physical sciences.[43] It is very difficult to evaluate such literature. Some of it is interesting (possibly creative) history of economic thought, but it is unclear what it says about the MSRP in the particular theories examined. What can be said is that *in the case studies where the relevant language is consistent with Lakatos*, "progress" and the prediction of novel facts it necessarily implies has been a *rare occurrence*. Now there have been some well-researched cases where the prediction of novel facts has actually been uncovered,[44] but such cases correspond to a miniscule portion of the theoretical "advances" of the profession. Lakatos's criterion for "theoretical progress," the prediction of novel facts, may be sufficient for what the profession considers to be theoretical progress but it is surely not necessary. Just as "the development of economic analysis would look a dismal affair through falsificationist spectacles" (Latsis, 1976b, 8), it seems that economics would look almost as bad on a strict Lakatosian view. This argument of course assumes that we actually define such things as "progress" and "novel fact" in the Lakatosian way. If these terms are defined with sufficient vagueness (as some economists have done), then one can produce any Panglossian historical record desired.

Now this claim, that the MSRP has much that is relevant for economics, but that empirical and theoretical advance in economics occur (and should occur) in many other ways than Lakatos specified in the MSRP, reflects very poorly (again) on Popper. The reason is that by and large *where economics is most likely to part ways with Lakatos is precisely where Lakatos borrowed most heavily from Popper*. Lakatos seems to have much to say about economics, and looking for the types of things that Lakatos suggests one should look for in the history of science has produced some excellent historical studies. This work has drawn attention to the discipline's metaphysical hard core and it has reopened the important methodological question of the exact relationship between applied economics/econometrics and pure economic theory. What Lakatos has not produced (and we have good reason to believe will never be produced—but that's another story) is a mechanical model for the growth of scientific knowledge that perfectly fits the development of economics. In Lakatos's case the fit seems to be poorest where older Popperian parts were used without much modification.

CONCLUSION

It appears that in the final evaluation "Popperian" economic methodology must be given low marks. Falsificationism, Popper's fundamental program for the growth of scientific knowledge, seems extremely ill-suited to economics. Popper's situational analysis view of social science is precisely what economists do, but the discussion of the topic in the Popperian literature does not help economics with any real methodological questions. The interest in Lakatos has produced some valuable historical studies but the overall fit of economics into the MSRP is not good; and not good precisely where Lakatos is the most Popperian.

Now despite, and even granting all of the above arguments, there is still a way to save the Popperian tradition from this negative evaluation. The defense is based on the claim that while all of the above may be true, it really doesn't say anything about Popperian philosophy. The argument is that Popper's *really* important work is something quite different from what has been discussed above, that his *real* contribution to philosophy is *critical rationalism*, not

falsificationism, and once this is recognized Popper does have something valuable to contribute to economic methodology.

Critical rationalism is Popper's general view of the philosophical method. It is the general method of rational discussion and the critical examination of proposed solutions.[45] Its overarching mandate is *criticize*, not falsify, though falsificationism is a special case of this more general method. Falsificationism is simply critical rationalism applied to the limited case of *empirical* criticism. Outside of this narrow empirical domain, critical rationalism is a quite general approach; metaphysical theories, philosophical theories, natural sciences that do not seem to fit falsificationism (evolutionary biology), and social sciences employing the rationality principle can all be examined, discussed and ultimately appraised through critical rationalism. Applying critical rationalism to economics simply means that we should criticize economic theories and we should be willing to learn from this critical discussion. Strategies that block or evade criticism should be shunned while those that open themselves to criticism should be welcomed. If this is Popper's real position, if this is the heart of Popperian philosophy, then the above criticisms seem to be of little importance, and it does appear that the economics profession has something to learn from Popperian philosophy.[46]

It is certainly difficult to argue against critical rationalism; for one thing it seems eminently reasonable and for another thing any rational argument against critical rationalism seems to presuppose it. One could argue against the exegetical claim that critical rationalism was *really* Popper's main contribution to philosophy but little would be gained by doing so; critical rationalism is actually in Popper (however minimally), and it may be appropriate even if the claim that it was Popper's main thesis is incorrect. The real problem for critical rationalism is not that one can say very much against it, but rather that one cannot say very much with it. Critical rationalism is a view that seems to be palatable by virtue of its blandness, the epistemological analog of the ethical mandate to "live the good life." Recent discussions of critical rationalism in the philosophical literature conclude that the notion is doomed to be a "contentless directive",[47] too amorphous to be of value in any interesting cases. This does not make it wrong or pernicious of course, just not very informative and devoid of the "bite" that is so attractive in Popperian philosophy. Thus, while the role of Popperian philosophy can be saved by turning to critical rationalism and away from falsification and demarcation, the victory is relatively hollow. If one listens carefully behind the roar of such a Popperian victory speech, one can hear Popper's old enemies, Hegel and Marx, chuckling in the dark.

NOTES

1. Blaug (1967, 1990), Brown (1981), Coats (1976), Cohen (1983), Cross (1982), de Marchi (1976), Diamond (1988), Fisher (1986), Fulton (1984), Hands (1985b, 1990), Latsis (1972, 1976b), Leijonhufvud (1976), Maddock (1984), Rizzo (1982), Schmidt (1982), and Weintraub (1985a, 1985b, 1988), is a partial listing of these Lakatosian reconstructions. See also de Marchi and Blaug (1991).

2. Influential books on economic method that fall broadly into the Popperian tradition include Blaug (1980), Boland (1982), Hutchison (1938), Klant (1984), Lipsey (1966), Weintraub (1985b), and (based on Caldwell (1991) Caldwell (1982).

3. Popper (1976b, 88).

4. The expression "basic statement" has a rather narrow meaning in Popperian philosophy. The concept was introduced in chapter V of Popper (1968); it is nicely summarized in Watkins (1984, 247-54).

5. Actually, as will be discussed below, scientific theories are not *by themselves* logically falsifiable. Rather, scientific theories along with (usually numerous) auxiliary hypotheses may form logically falsifiable *test systems* (see Hausman, 1988, 68-9).

6. Gellner (1974, 171).

7. See Hands (1988) for a general discussion of the Popperian notion of *ad hocness*.

8. An exception here is L. von Mises (1949, 1978); the Austrian "methodology" of von Mises seems to be justificationist in origin.

9. See de Marchi (1988b) for a discussion of the LSE connection.

10. Popper (1961) and (1966), for example.

11. An exception here seems to be Hutchison, who considers the link more direct. For him falsificationism is "the epistemological basis for a free, pluralist society" (1976, 203). A similar theme runs throughout Hutchison (1988).

12. This point is argued forcefully in Caldwell (1991).

13. Feyerabend (1975), Grunbaum (1976), Lakatos (1970), Maxwell (1972), and Putnam (1974) provide a small sample of such general criticism.

14. The main sources for this list of criticisms are Caldwell (1984, 1991), Hausman (1981, 1985, 1988), and Latsis (1976b).

15. The Duhemian problem (Duhem, 1954) arises because theories are never tested alone, rather they are tested in conjunction with certain auxiliary hypotheses (including those about the data). Thus if T is the theory, the prediction of evidence e is given by $T \cdot A \Rightarrow e$, where A is the set of auxiliary hypotheses. The conjunction $T \cdot A$ forms a test system and the observation "not e" implies "not $(T \cdot A)$" rather than simply "not T"; the test system is falsified, not necessarily the theory. In a scientific context there are a number of possible responses to "not e" (see Koertge, 1978, 255). One could challenge the reliability of the observation "not e." One could reject A or one of the elements of A. One could challenge the validity of the implication from $T \cdot A$ to e. And finally, we could follow Popper's advice and reject the theory T; discarding the theory is but one possibility. This problem is also called the Duhem-Quine problem, though the Duhemian appellation may be exegetically incorrect (see Ariew (1984)). It is a standard concern in the philosophy of science that has more recently been recognized as an issue for economic methodology (see Cross (1982) for instance). Popper clearly recognized the Duhemian problem (1965, 112, 239); and (1972, 353), for instance), but his methodological solution is itself subject to criticism (particularly 3.) below.

16. Popper (1965, 42, 267, 387-88); (1968, 43-44, 93-95, 97-111); (1983, 185-86).

17. This problem, but in reverse, is demonstrated by the examples in

Faulhaber and Baumol (1988). The authors discuss a number of cases where results from microeconomic theory have been "applied" by government and the business community. Of course, the fact that the implications of the theory were "applied" after, because of the economic theory, means that the implications did not hold when the theory was proposed. The microeconomic theories in question can be corroborated today because falsificationism wasn't practiced earlier. It is difficult to imagine such cases in physics.

18. This is one source of the "innocuous falsification" mentioned by Blaug (1980, 128, 259) and Coddington (1975, 542-45). It should be noted that if the parameters in the auxiliary hypotheses are not sufficiently restricted, but allowed to vary freely, then an even more severe problem develops: the so-called "parameter paradox" (Klant, 1984, 153-57); (1988, 108, 110-11). This is because if parameters are truly "variables" then *even* qualitative comparative statics can not be obtained. This results in a theory (or theoretical test system) that is completely unfalsifiable: there exists no observation in conflict with it.

19. Popper's most important writings on verisimilitude are contained in Popper (1965) and (1972). Useful surveys of the topic are Koertge (1979a, 234-38) and Watkins (1984, chapter 8). The question of verisimilitude in Popper's philosophy is examined in more detail in Hands (1991a).

20. This problem is demonstrated nicely by the following anecdote from Koertge (1979b, 237).

> "If two children are told to pick all and only the good cherries off a tree, who has done better: Clara Caution, who picks a tiny thimbleful, nearly all of which are firm and ripe, or Bella Bold, who brings home an enormous tubful, many of which are green or rotten? Which are worse, sins of omission or sins of comission?"

21. These concepts are discussed in detail with appropriate references to Popper's writings in Hands (1988). Other general discussions of these Popperian concepts include Koertge (1978), Watkins (1978, 1984), and Worrall (1978).

22. The argument is "nowhere fully explained outside of lectures" (Jarvie (1972, 5)).

23. General discussions of situational analysis by philosophers other than Popper include Farr (1983), Jarvie (1972, 1982), Koertge (1974, 1975, 1979a, 1985), and Watkins (1970). The methodological discussion regarding economics is contained in Blaug (1985), Caldwell (1991), Hands (1985a), and Latsis (1983) with brief mention in Blaug (1980), Hutchison (1981), Klant (1984, 1988), and Latsis (1976b). Wong (1978) uses situational analysis to criticize the theoretical contribution of an individual economist rather than examining the general implications for economic methodology.

24. Popper (1985, 358); Koertge (1974, 199); Latsis (1983, 136); Watkins (1970, 167).

25. Popper (1966, 97); (1976a, 102-03); (1976b, 117-18).

26. Koertge (1975, 87); (1979a, 440).

27. This scheme is not even restricted to "orthodox" economics. Consider the traditional Marxist answer to the question of why nineteenth century capitalists hired women and children and worked them long hours: Well, the situation of the individual capitalist was that they needed to make their rate of profit as high as possible or they would be pushed out of business and thus become proletariat themselves. The rate of profit is given by $\pi = S/(C+V)$ where S is surplus value, the amount of labor time obtained by the capitalist in excess of V, the amount of labor time necessary to reproduce the workers. Now given C, π can be increased by increasing S or by decreasing V. Working laborers longer hours will increase S, and hiring women and children (since V is based on reproducing the household which sustains the worker) will reduce V. Therefore, since agents always act "appropriately" to their situation, capitalists hired women and children and worked them long hours.

28. This difference prompted the distinction between Popper_s (social science/SA) and Popper_n (natural science) in Hands (1985a) and Caldwell (1991).

29. Popper himself certainly argues that the RP should never be abandoned (Popper (1985), 360). It is unclear though whether this is because the RP is unfalsifiable or simply because of a methodological decision.

30. Actually, SA explanations are *not bona fide* scientific explanations on *any* covering law model of scientific explanations (not just Popper's).

31. This point is made by Koertge (1979a, 84, 93).

32. Latsis (1983), Koertge (1975).

33. This of course assumes that economics is "stuck with" SA. If, on the other hand, one takes seriously the failure of the RP as a causal law, one possibility would be to abandon SA explanations altogether. This would force economists to "start from scratch" and if they are to explain economic behavior at all, to do so on the basis of the type of causal universal laws required in scientific explanations. This is essentially the proposal of Rosenberg (1980).

34. This conference produced the seminal volume Latsis (1976a).

35. See note 1 for references to the former literature. The latter literature also includes some of these same references as well as others such as, Archibald (1979), Goodwin (1980), Hands (1979, 1984, 1988), Hutchison (1976, 1981), Remenyi (1979), Robbins (1979), and Rosenberg (1986). Lakatos is also discussed in surveys such as Blaug (1980), Caldwell (1982) and Pheby (1988).

36. Many summaries of the MSRP are available in the economics literature (Blaug (1980), Hands (1985a), and Weintraub (1985a, 1985b, 1988) for instance) but the single best presentation of the argument remains Lakatos (1970) himself. As with Popper's falsificationism, only a sketch of the main thesis is provided here.

37. As notes 1 and 35 clearly indicate, there has been a lot of work in "Lakatosian economics." In all of this work, none has been as serious or as careful as Weintraub's work on the NeoWalrasian program (1985b, 1988).

38. The term "novel fact" has a very specific meaning in the Lakatosian (and Popperian) program. See Gardner (1982), Hands (1985b, 1991b) and Worrall (1978) on this issue.

39. This point is emphasized in Hands (1979).

40. Popper, unlike philosophers in the positivist tradition, has always recognized that metaphysics has a role to play in the growth of scientific knowledge. In fact, Popper's lifework is often characterized as a long process of systematically expanding the role of metaphysics in science (a view corroborated by the discussion of metaphysics in Popper (1983)). Philosophers in the Popperian tradition have intermittently considered the question of appraising metaphysics (Koertge (1978), Watkins (1958), Wisdom (1963, 1987) for example) but the topic remains underdeveloped. The issue will be raised again in the conclusing section.

41. "A general definition of science, thus, must reconstruct the acknowledgedly best gambits as 'scientific': if it fails to do so, it has to be rejected" (Lakatos (1971), p. 111).

42. These case studies use Lakatos to appraise economics; the exception is Hands (1985b) where economics is used to appraise Lakatos.

43. Rather than singling out the worst perpetrators of this terminological infidelity, I will take the opposite approach. In the reconstruction literature, certain economists have been careful in the way the Lakatosian terminology is used and in the way the economic and empirical concepts are mapped into these Lakatosian notions; a list of such work would need to include Blaug (1987), de Marchi (1976), Latsis (1976b), Maddock (1984) and Weintraub (1985a, 1985b, 1988).

44. See the references in note 43.

45. Critical rationalism has been an underlying theme throughout Popper's life's work. It is more pronounced in later work than earlier (esp. (1972, 1983)) but not even *The Logic of Scientific Discovery* is without it.

"And yet I am quite ready to admit that there is a method which might be described as 'the one method of philosophy.' But it is not characteristic of philosophy alone; it is, rather, the one method of all *rational discussion*, and therefore of the natural sciences as well as philosophy. The method I have in mind is that of stating one's problem clearly and of examining its various proposed solutions *critically*" ((1968), 16).

46. According to Caldwell (1991), critical rationalism is how his "pluralism" in (1982, 1988) should be interpreted and it is also how Klant interprets his "plausibilism" in (1984) (see Klant, 1988, 108).

47. Nola (1987, 497).

REFERENCES

Archibald, G. C. (1979), "Method and Appraisal in Economics," *Philosophy of the Social Sciences*, 9, 304-15.

Ariew, R. (1984), "The Duhem Thesis," *British Journal for the Philosophy of Science*, 35, 313-325.

Blaug, M. (1976), "Kuhn versus Lakatos, or Paradigms versus Research Programmes in the History of Economics," in S. J. Latsis (1976a), 149-80.

__________________. (1980), *The Methodology of Economics*. Cambridge: Cambridge University Press.

__________________. (1985), "Comments on D. Wade Hands, 'Karl Popper and Economic Methodology: A New Look'," *Economics and Philosophy*, 1, 286-88.

__________________. (1990), "Second Thoughts on the Keynesian Revolution," in Blaug.

Boland, L. (1982), *The Foundations of Economic Method*. London: Allen & Unwin.

Brown, E. K. (1981), "The Neoclassical and Post-Keynesian Research Programs: The Methodological Issues," *Review of Social Economy*, 34, 111-32.

Caldwell, B. J. (1982), *Beyond Positivism: Economic Methodology in the Twentieth Century*. London: Allen & Unwin.

__________________. (1984), "Some Problems with Falsificationism in Economics," *Philosophy of the Social Sciences*, 14, 489-95.

__________________. (1988), "The Case for Pluralism," in de Marchi (1988a), 231-44.

__________________. (1991), "Clarifying Popper," *Journal of Economic Literature*, 29, 1-33.

Coats, A. W. (1976), "Economics and Psychology: The Death and Resurrection of a Research Programme," in S. J. Latsis (1976a), 43-64.

Coddington, A. (1975), "The Rationale of General Equilibrium Theory," *Economic Inquiry*, 13, 339-58.

Cohen, A. (1983), "The Laws of Return Under Competitive Conditions: Progress in Microeconomics Since Sraffa," *Eastern Economic Journal*, 9, 213-20.

Cross, R. (1982), "The Duhem-Quine Thesis, Lakatos, and the Appraisal of Theories in Macroeconomics," *Economic Journal*, 92, 320-40.

de Marchi, N. (1976), "Anomaly and the Development of Economics: The Case of the Leontief Paradox," in S. J. Latsis (1976a), 109-27.

__________________. (ed.) (1988a), *The Popperian Legacy in Economics*. Cambridge: Cambridge University Press.

__________________. (1988b), "Popper and the LSE Economists," in de Marchi (1988a), 139-66.

__________________ and Blaug, M. (eds.). (1991), *Appraising Economic Theories: Studies in the Methodology of Research Programs*. Aldershot: Edward Elgar.

Diamond, A. M. Jr. (1988), "The Empirical Progressiveness of the General Equilibrium Research Program," *History of Political Economy*, 20, 119-35.

Duhem, P. (1954), *The Aim and Structure of Physical Theory*. Translated by P. P. Wiener, Princeton, NJ: Princeton University Press.

Farr, J. (1983), "Popper's Hermeneutics," *Philosophy of the Social Sciences*, 13, 157-76.

Faulhaber, G. R. and Baumol, W. J. (1988), "Economists as Innovators: Practical Products of Theoretical Research," *Journal of Economic Literature*, 26, 577-600.

Feyerabend, P. K. (1970), "Consolations for the Specialist," in *Criticism and the Growth of Knowledge*. I. Lakatos and A. Musgrave (eds.). Cambridge University Press, 197-230.

__________________. (1975), *Against Method*. London: New Left Books.

Fisher, F. M. (1986), *The Logic of Economic Discovery*. Washington Square, NY: New York University Press.

Fulton, G. (1984), "Research Programmes in Economics," *History of Political Economy*, 16, 187-205.

Gardner, M. R. (1982), "Predicting Novel Facts," *British Journal for the Philosophy of Science*, 33, 1-15.

Gellner, E. (1974), *Legitimation of Belief*. Cambridge: Cambridge University Press.

Goodwin, C. (1980), "Towards a Theory of the History of Economics," *History of Political Economy*, 12, 610-19.

Grunbaum, A. (1976), "Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper Versus Inductivism," in *Essays in Memory of Imre Lakatos*. R. Cohen et. al. (eds.), Dordrecht, Holland: D. Reidel.

Hands, D. W. (1979), "The Methodology of Economic Research Programmes," *Philosophy of the Social Sciences*, 9, 293-303.

__________________. (1984), "Blaug's Economic Methodology," *Philosophy of the Social Sciences*, 14, 115-25.

__________________. (1985a), "Karl Popper and Economic Methodology," *Economics and Philosophy*, 1, 83-99.

__________________. (1985b), "Second Thoughts on Lakatos," *History of Political Economy*, 17, 1-16.

__________________. (1988), "Ad Hocness in Economics and the Popperian Tradition," in de Marchi (1988a), 121-37.

__________________. (1990), "Second Thoughts on 'Second Thoughts': Reconsidering the Lakatosian Progress of *The General Theory*," *Review of Political Economy*, 2, 67-81.

__________________. (1991a), "The Problem of Excess Content: Economics, Novelty and A Long Popperian Tale," in de Marchi and Blaug (1991), 58-75.

__________________. (1991b), "Reply to Hamminga and Mäki," in de Marchi and Blaug (1991), 91-102.

Hausman, D. M. (1981), *Capital, Profits and Prices: An Essay in the Philosophy of Economics*. New York: Columbia University Press.

__________________. (1985), "Is Falsificationism Unpracticed or Unpracticable?" *Philosophy of the Social Sciences*, 15

313-19.

________________. (1988), "An Appraisal of Popperian Economic Methodology," in de Marchi (1988a), 65-85.

Hutchison, T. W. (1938), *The Significance and Basic Postulates of Economic Theory*. London: Macmillan (reprint, New York: Augustus M. Kelly, 1960).

________________. (1976), "On the History and Philosophy of Science and Economics," in S. J. Latsis (1976a), 181-205.

________________. (1981), "On the Aims and Methods of Economic Theorizing," in *The Politics and Philosophy of Economics*. T. W. Hutchison (ed.) New York: New York University Press, 266-307.

________________. (1988), "The Case for Falsificationism," in de Marchi (1988a), 169-81.

Jarvie, I. C. (1972), *Concepts and Society*. London: Routeledge and Kegan Paul.

________________. (1982), "Popper on the Difference Between the Natural and Social Sciences," in *In Pursuit of Truth: Essays on the Philosophy of Karl Popper on the Occasion of His 80th Birthday*. B. Levinson (ed.), Atlantic Highlands, NJ: Humanities Press, 83-107.

________________. (1984), *The Rules of the Game*. Cambridge: Cambridge University Press.

Klant, J. J. (1988), "The Natural Order," in de Marchi (1988a), 87-117.

Koertge, N. (1974), "On Popper's Philosophy of Social Science," in *PSA 1972*. K. F. Schaffner and R. S. Cohen (eds.)., Dordrecht, Holland: D. Reidel, 195-207.

________________. (1975), "Popper's Metaphysical Research Program for the Human Sciences," *Inquiry*, 19, 437-62.

________________. (1978), "Toward a New Theory of Scientific Inquiry," in *Progress and Rationality in Science*. G. Radnitzky and G. Anderson (eds.), Dordrecht, Holland: D. Reidel, 253-78.

________________. (1979a), "The Methodological Status of Popper's Rationality Principle," *Theory and Decision*, 10, 83-95.

________________. (1979b), "The Problems of Appraising Scientific Theories," in *Current Research in Philosophy of Science*. P. D. Asquith and H. E. Kyburg, Jr. (eds.), East Lansing, MI: Philosophy of Science Association, 228-251.

________________. (1985), "On Explaining Beliefs," *Erkenntnis*, 22, 175-86.

Kuhn, T. S. (1970), *The Structure of Scientific Revolutions*. 2nd ed., Chicago: University of Chicago Press.

Lakatos, I. (1970), "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*. I. Lakatos and A. Musgrave (eds.), Cambridge: Cambridge University Press, 91-196.

________________. (1971), "History of Science and Its Rational Reconstructions," in *Boston Studies in the Philosophy of Science*, Vol. 8. R. C. Buck and R. S. Cohen (eds.), Dordrecht, Holland: D. Reidel, 91-136.

Latsis, S. J. (1972), "Situational Determination in Economics," *The British Journal for the Philosophy of Science*, 23, 207-45.

________________. (1976a), *Method and Appraisal in Economics*. Cambridge: Cambridge University Press.

________________. (1976b), "A Research Programme in Economics," in S. J. Latsis (1976a), 1-41.

________________. (1983), "The Role and Status of the Rationality Principle in the Social Sciences," in *Epistemology, Methodology* *and the Social Sciences*. R. S. Cohen and M. W. Wartefsky (eds.), Dordrecht, Holland: D. Reidel, 123-51.

Leijonhufvud, A. (1976), "Schools, 'Revolutions,' and Research Programmes in Economic Theory," in S. J. Latsis (1976a), 65-108.

Lipsey, R. G. (1966), *An Introduction to Positive Economics*, 2nd ed., London: Weidenfeld and Nicolson.

Maddock, R, (1984), "Rational Expectations Macrotheory: A Lakatosian Case Study in Program Adjustment," *History of Political Economy*, 16, 291-309.

Maxwell, N. (1972), "A Critique of Popper's Views on Scientific Method," *Philosophy of Science*, 39, 131-52.

Mises, L. von (1949), *Human Action: A Treatise on Economics*. New Haven: Yale University Press.

__________________. (1978), *The Ultimate Foundation of Economic Science*. 2nd ed., Kansas City: Sheed Andrews and McMeel.

Nola, R. (1987), "The Status of Popper's Theory of Scientific Method," *British Journal for the Philosophy of Science*, 38, 441-480.

Petrella, F. (1988), "Henry George and the Classical Scientific Research Program: The Economics of Republican Millennialism" *American Journal of Economics and Sociology*, 47, 239-56.

Pheby, J. (1988), *Methodology and Economics: A Critical Introduction*. London: Macmillan.

Popper, K. R. (1961), *The Poverty of Historicism*. 3rd ed., New York: Harper and Row.

__________________. (1965), *Conjectures and Refutations*. 2nd ed., New York: Harper and Row.

__________________. (1966), *The Open Society and Its Enemies*. Vol. I and II, 2nd ed., New York: Harper and Row.

__________________. (1967), "La Rationalité et le Statut du Principle de Rationalité," in *Les Fondements Philosophiques des Systemes Economiques*. E. M. Classen (ed.), Paris: Payot, 142-50.

__________________. (1968), *The Logic of Scientific Discovery*. 2nd ed., New York: Harper and Row.

__________________. (1970), "Normal Science and Its Dangers," in *Criticism and the Growth of Knowledge*. I. Lakatos and A. Musgrave (eds.), Cambridge: Cambridge University Press, 51-8.

__________________. (1972), *Objective Knowledge*. Oxford: Oxford University Press.

__________________. (1976a), "The Logic of the Social Sciences," in *The Positivist Dispute in German Sociology*. T. W. Adorno et al. (eds.), translated by G. Adey and D. Frisby, New York: Harper and Row, 87-104.

__________________. (1976b), *Unended Quest*. LaSalle, IL: Open Court.

__________________. (1983), *Realism and the Aim of Science*. W. W. Bartley III (ed.), Totowa, NJ: Rowman and Littlefield.

__________________. (1985), "The Rationality Principle," English translation of Popper (1967) in *Popper Selections*. D. Miller (ed.), Princeton: Princeton University Press.

Putnam, H. (1974), "The 'Corroboration' of Theories," in *The Philosophy of Karl Popper*. P. A. Schilpp (ed.), LaSalle, IL: Open Court.

Remenyi, J. V. (1979), "Core Demi-core Interaction: Towards a General Theory of Disciplinary and Subdisciplinary Growth," *History of Political Economy*, 11, 30-63.

Rizzo, M. J. (1982), "Mises and Lakatos: A Reformulation of Austrian Methodology," in *Method, Process and Austrian Economics*. I. M.

Kirzner (ed.), Lexington, MA: Lexington Books.

Robbins, L. (1979), "On Latsis's Method and Appraisal in Economics: A Review Essay," *Journal of Economic Literature*, 17, 996-1004.

Rosenberg, A. (1980), *Sociobiology and the Pre-emption of Social Science*. Baltimore: Johns Hopkins University Press.

__________________. (1986), "Lakatosian Consolations for Economists," *Economics and Philosophy*, 2, 127-39.

Schmidt, R. H. (1982), "Methodology and Finance," *Theory and Decision*, 14, 391-413.

Watkins, J. (1958), "Confirmable and Influential Metaphysics," *Mind*, 67, 344-65.

__________________. (1970), "Imperfect Rationality," in *Explanation in the Behavioral Sciences*. R. Borger and F. Cioffi (eds.), Cambridge: Cambridge University Press, 91-121.

__________________. (1978), "The Popperian Approach to Scientific Knowledge," in *Progress and Rationality in Science*. G. Radnitzky and G. Anderson (eds.), Dordrecht, Holland: D. Reidel, 23-43.

__________________. (1984), *Science and Skepticism*. Princeton, NJ: Princeton University Press.

Weintraub, E. R. (1985a), "Appraising General Equilibrium Analysis," *Economics and Philosophy*, 1, 23-37.

__________________. (1985b), *General Equilibrium Analysis: Studies in Appraisal*. Cambridge: Cambridge University Press.

__________________. (1988), "The NeoWalrasian Program is Empirically Progressive," in de Marchi (1988a).

Wisdom, J. O. (1963), "The Refutability of 'Irrefutable' Laws," *British Journal for the Philosophy of Science*, 13, 303-306.

__________________, J. O. (1987), *Challengeability in Modern Science*. Aldershot, England: Gower Publishing.

Wong, S. (1978), *The Foundation of Paul Samuelson's Revealed Preference Theory*. London: Routledge and Kegan Paul.

Worrall, J. (1978), "The Ways in Which the Methodology of Scientific Research Programmes Improves on Popper's Methodology," in *Progress and Rationality in Science*. G. Radnitzky and G. Anderson (eds.), Dordrecht, Holland: D. Reidel, 45-70.