

Popper and Lakatos in Economic Methodology¹

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Overview

The purpose of this chapter is to critically reappraise the methodological advice offered to economists by Popperian philosophy, in particular Popperian falsificationism and Lakatos's 'methodology of scientific research programmes'. These two philosophical positions and the difficulties they raise for economic methodology are carefully considered in the chapter. It is argued that while economists have benefited from the influence of Popperian philosophy in a number of ways, neither falsificationism nor Lakatos's methodology provide an appropriate guide to the acceptance or rejection of economic theories. The implications and caveats surrounding this argument are considered in the conclusion.

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Introduction

Popperian philosophy of science has been extremely influential in economic methodology. Popperian ‘falsificationism’, first introduced into economics by Hutchison (1938), remains one of the dominant approaches to economic methodology. In addition to this direct influence, Popperian philosophy has also affected economic methodology through the work of Imre Lakatos. A fairly extensive literature has developed around the question of the applicability of Lakatos’s ‘methodology of scientific research programmes’ (MSRP) to economics.²

The purpose of this chapter is to critically reappraise the methodological advice given by Popperian philosophy. In this reappraisal both Popperian falsificationism and Lakatos’s MSRP will be examined. Neo-institutionalist economics will not be explicitly discussed; instead the focus will be the general standards for economic theory choice which influence every economic theory (including neo-institutionalism). Throughout the discussion the philosophical positions will be appraised only with respect to *economic methodology*: ‘economic’ in that only economics and not other fields of enquiry will be discussed, and ‘methodology’ in that only questions of theory choice and theory appraisal (not more general philosophical considerations) will be examined. In particular, questions such as whether ‘economic methodology’ should be pursued at all (recently raised by McCloskey (1985)) will not be examined here.

Falsificationism

No doubt Karl Popper is best known for his falsificationist approach to the philosophy of science: a theory first presented in *Logik der Forschung* in 1934 (English translation, Popper 1959). Falsificationism represents Popper’s view of the growth of scientific knowledge as well as his solution to (or dissolution of) the problem of induction. It is for falsificationism that Popper claims responsibility for the death of logical positivism (Popper 1976b: 88).

Popperian falsificationism is actually composed of two separate theses: one on demarcation (demarcating science from non-science) and one on methodology (how science should be practised). The demarcation thesis is that for a theory to be ‘scientific’ it must be at least potentially falsifiable by empirical observation, that is, there must exist at least one empirical basic statement which is in conflict with the theory.³ This potential falsifiability

is a logical relationship between the theory and a basic statement; in particular, the demarcation criterion only requires that it be logically possible to falsify the theory, not that such a falsification has ever been attempted.⁴ While Popper's demarcation criterion has been the subject of an extensive debate in the philosophical literature, demarcation is seldom the issue in economics. For economists the more important issue is methodology (choosing between/among theories not merely labelling them scientific or unscientific) and Popperian *methodology* requires the practical (not just logical) falsifiability of scientific theories.

In a nutshell, falsificationist scientific practice proceeds as follows. The scientist starts with a scientific problem situation (something requiring a scientific explanation) and proposes a bold conjecture which might offer a solution to the problem. Next the conjecture is severely tested by comparing its least likely consequences with the relevant empirical data. Popper's argument for severe testing is that a test will be *more severe* the more *prima facie unlikely* the consequence that is being tested; the theory should be forced to 'stick its neck out', to 'offer the enemy, namely nature, the most exposed and extended surface' (Gellner 1974: 171). The final step in the falsificationist game depends on how the theory has performed during the testing stage. If the implications of the theory are not consistent with the evidence, then the conjecture is falsified and it should be replaced by a new conjecture which is *not ad hoc* relative to the original, that is, the new conjecture should not be contrived solely to avoid this empirical anomaly.⁵ If the theory is not falsified by the evidence then it is considered corroborated and it is accepted provisionally. Given Popper's fallibilism this acceptance is provisional forever; the method does not necessarily result in true theories, only ones that have faced a tough empirical opponent and won.

Now while there are a number of reasons why economists have felt that Popperian falsificationism would be a desirable methodology, the fact is that *falsificationism is seldom if ever practised 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 book which consistently advocates falsificationism as a normative ideal. The disagreement between critics and defenders of falsificationism is *not whether it has been practised*, basically it has not, but rather *whether it should be practised*. The real questions are whether the profession should 'try harder' to practise falsificationism though it has failed to do so in the past, and the related question of whether the discipline of economics would be substantially improved by a conscientious falsificationist practice.

One approach to the question of the appropriateness of falsificationism in economics would be to directly address the question of the adequacy of Popper's falsificationist methodology as a *general* approach to the growth of scientific knowledge; this is not the approach that will be followed here. Rather than delving into this general question, the following discussion will simply survey some of the criticisms which falsificationism has received explicitly as an economic methodology. This list of criticisms is not exhaustive, but it does capture the major concerns which have been raised regarding the falsificationism in economics. The list is not necessarily in order of importance.⁶

1. For a number of reasons, the so-called Duhemian problem (or Duhem–Quine problem) presents a great difficulty in economics.⁷ First, the complexity of human behaviour requires the use of numerous initial conditions and strong simplifying assumptions. Some of these restrictions may actually be false (such as the infinite divisibility of commodities), some of these assumptions may be logically unfalsifiable (such as the assumptions of eventually diminishing returns), while still others may be logically falsifiable but practically unfalsifiable (such as the completeness assumption in consumer choice theory). Even where assumptions and restrictions can be tested, such testing is very difficult because of the absence of a suitably controlled laboratory environment.⁸ In the presence of such a variety of restrictions it is virtually impossible to ‘aim the arrow of *modus tollens*’ at one particular problematic element of the set auxiliary hypotheses when contrary evidence is found. Second, there are many questions and disagreements about the empirical basis in economics. It is always possible to argue that what was observed was ‘not really’ involuntary unemployment or ‘not really’ economic profit, etc. Although it is fundamental to Popperian philosophy that the empirical basis need not be incorrigible, it is necessary that there be a generally accepted convention regarding the empirical basis,⁹ and in economics even such conventions are often not available. Third, even if these first two problems have somehow been eliminated it is still possible for the social sciences to 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).

2. Related to, but actually separate from the Duhemian problem, is the problem that the qualitative comparative statics technique used in economics makes severe testing very difficult and cheap corroborational success 'too easy'. In economics it is very often the case that the strongest available prediction is a qualitative comparative statics result which only specifies that the variable in question increases, decreases, or remains the same. Since having the correct sign is much easier than having both the correct sign and magnitude, an emphasis on such qualitative prediction generates theories which are low in empirical content, have few potential falsifiers, and are difficult if not impossible to test severely. The result is often economic theories which are confirmed by the evidence but provide very little information.¹⁰
3. Popper's 'admitted failure' (1983: xxxv) to develop an adequate theory of verisimilitude¹¹ presents a fundamental difficulty for a falsificationist methodology in economics. Popper's theory of verisimilitude developed as an attempt to reconcile his falsificationist methodology with scientific realism. For a realist science aims at '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 fulfil the realist aims of science it should be demonstrated that more corroborated theories are closer to the truth. Such a demonstration was precisely the goal of Popper's theory of verisimilitude. Actually a satisfactory theory of verisimilitude would serve Popperian philosophy in at least two different ways. One way, as already mentioned, would be to provide an epistemic justification for playing the game of science by falsificationist rules. Such a justification is very important for Popperian philosophy since without a theory of verisimilitude it can be argued that 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. Verisimilitude would provide rules for choosing the 'best' theory in troublesome cases: like the situation where both available theories have been falsified. A theory of verisimilitude would help in such cases because it would provide a rule for determining which of the two theories in question actually has more verisimilitude: which is closer to the truth. A similar argument could be made for cases involving a choice between a falsified but bold theory and a corroborated but modest theory; having a way to determine which has more verisimilitude would allow us to choose a theory which is more consistent with the aims of science, which is

- closer to the truth. This second, more practical, function of the theory of verisimilitude is very important in economic methodology. The reason is that economists are almost always faced with choosing between two falsified theories, or choosing 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 to play in economic theory choice. Without such a link between severe testing and truth-likeness, 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 seldom appropriate 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'.¹² This issue will be examined more carefully in the Lakatos section which follows, but for now it should be noted that while Popperian progress may sometimes be of interest to economists, often progress in economics is (and should be) very different to what Popper prescribes. Economists are often concerned with finding new explanations for well-known (non novel) facts, or alternatively, with explaining known phenomena by means of fewer theoretical restrictions. What constitutes '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 different, and possibly much more liberal, set of standards than those offered by strict Popperian falsificationism.

All of these criticisms add up to a negative appraisal of falsificationist economic methodology. Despite the fact that preaching falsificationist methodology has been very popular among economists, the method fails to provide a reasonably 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 behaviour than those currently available. How this result should be interpreted will be discussed in the conclusion, for now let us turn to Lakatos's MSRP.

Lakatos's Methodology of Scientific Research Programmes

Lakatos's work in the philosophy of science first appeared in the early 1970s (Lakatos 1970, 1971) and it was endorsed almost immediately by a number of economists. Numerous papers on Lakatos have appeared in the economics literature, many as a result of the Nafplion Colloquium on Research Programmes in Physics and Economics in 1974 (Latsis 1976a). This literature on 'Lakatos and economics' has basically been of two types. The first type is historical, it attempts to 'reconstruct' some particular episode in the history of economic thought along Lakatosian lines. The second type is more philosophical, it attempts to appraise Lakatos's methodology of scientific research programmes as an economic methodology and/or compare it to other philosophies such as Kuhn or Popperian falsificationism.

Lakatos's MSRP is clearly part of the Popperian tradition in the philosophy of science but it was also motivated by philosophically minded historians of science such as Kuhn (1970). For Lakatos the primary unit of appraisal in science is the 'research programme' rather than the scientific theory. A research programme is an ensemble consisting of a hard core, the positive and negative heuristics, and a protective belt.¹³ The hard core is composed of the fundamental metaphysical presuppositions of the programme; it defines the programme, and its elements are treated as irrefutable by the programme's practitioners. To participate in the programme is to accept and be guided by the programme's hard core. For example, in Weintraub's Lakatosian reconstruction of the Neo-Walrasian research programme in economics, the hard core consists of propositions such as: agents have preferences over outcomes and agents act independently and optimize subject to constraints. The positive and negative heuristics provide instructions about what should and should not be pursued in the development of the programme. The positive heuristic guides the researcher toward the right questions and the best tools to use in answering those questions; the negative heuristic indicates what questions should not be pursued and what tools are inappropriate. Again using Weintraub's analysis of the Neo-Walrasian programme as an example, the positive heuristic contains injunctions such as: construct theories where the agents optimize, while the negative heuristic implores researchers to avoid theories involving disequilibrium. Finally, the protective belt consists of the programme's actual theories, auxiliary hypotheses, empirical conventions and the (evolving) 'body' of the research programme. The major activity of the programme occurs in the protective belt, it occurs as a result of the interaction of the hard core, the heuristics, and the programme's

empirical record. For Weintraub's Neo-Walrasian programme the protective belt includes almost all of applied microeconomics.

A research programme is appraised on the basis of the theoretical and empirical activity in the protective belt. There is *theoretical progress* if each change in the protective belt is empirical content increasing; that is if it predicts *novel facts*.¹⁴ The research programme exhibits *empirical progress* if this excess empirical content actually gets corroborated (Lakatos 1970: 118). Lakatos also requires a third type of progress, heuristic progress (non-ad hoc₃ness), which specifies that the changes be consistent with the hard core of the programme. Lakatos's definitions of theoretical and empirical progress presuppose that the changes in question are consistent with heuristic progress.

One obvious example of the link between Lakatos and Popper is the way in which Lakatos characterizes empirical content and novel facts. Lakatos, like Popper, defines the empirical content of a theory to be 'the set of its potential falsifiers: the set of those observational propositions which may disprove it' (Lakatos 1970: 98, n. 2). Thus, even though Lakatos considers empirical progress to come through empirical corroboration rather than falsification, his characterization of the relationship between theory and fact is still basically falsificationist. There are many other signs of Lakatos's Popperian lineage but his definition of empirical content and novel facts are the most important in the appraisal of Lakatosian economic methodology.

On the other hand, there are many aspects of the MSRP which are fundamentally at odds with Popperian falsificationism. The most significant of these is the immunity of the hard core to empirical criticism; immunizing any part of scientific theory would be in conflict with Popper's falsificationist method of bold conjecture and severe test. Popper clearly recognized that science has experienced periods of Kuhnian 'normal science' where the critical spirit seems to be temporarily arrested, but for Popper these episodes are something to lament not praise (Popper 1970). Another point of disagreement is the question of corroboration versus falsification. While Lakatos defines empirical content in a thoroughly Popperian way, he has no respect for the role of falsification in science. For Lakatos all theories are 'born refuted' (1970: 120–1) and the task of philosophy of science should be to develop a methodology which *starts* from this fact. For Lakatos progress comes from the corroboration not falsification of novel facts. Finally, Lakatos clearly embraces a historical meta-methodology whereby the actual history of science is used to appraise various methodological proposals.¹⁵ This is very different from Popper where methodology is purely a normative affair

and where there is no pathway open for the actual history of science to help evaluate methodologies.

These places where Lakatos differs from Popper are exactly the places where Lakatos is likely to win the favour of economists since these happen to be areas where there is substantial tension between falsificationism and the actual practice of economics. Certainly economics is replete with metaphysical 'hard cores'; there is not much consensus on what these hard core propositions should be, but there seems to be a consensus that such hard core presuppositions exist and that they often define alternative research programmes in economics. A philosophical programme such as Popperian falsificationism which requires practitioners to be willing to give up almost any part of their research programme at any time will not provide as adequate a guide for economists as Lakatos's methodology which allows for such pervasive hard cores. This economic preference for Lakatos over Popper also extends to the issue of corroboration versus falsification. It is clear that falsificationism has not been practised in economics and there is good reason to believe that enforcement of such strict standards would all but eliminate the discipline as it currently exists. 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.

Finally, economists would prefer Lakatos to Popper on the question of the role of the history of science in supporting particular methodological proposals. The general question of the relationship between the history of science and the philosophy of science is an unsettled question which continues to be debated in the literature, but economists have recently been very sympathetic to methodological proposals that are sensitive to the actual history of their discipline. Economists have produced an extensive literature using the Lakatosian categories to reconstruct various parts of the history of economic thought. Most of this literature focuses on a particular research programme in economic theory (past or present) and tries to isolate the hard core, the positive and negative heuristics, and the type of theoretical activity occurring in the protective belt. Such work usually results in a positive or negative Lakatosian appraisal of the 'progressivity' of the particular economic research programme. Examples of these reconstructions range widely over various topics in the history of economic thought.

An overall assessment of this Lakatosian historical literature is very difficult because many of the economists writing in the field have taken very little care in the way they use the Lakatosian terminology. This lack of fidelity to Lakatosian terminology has resulted in 'hard cores', 'heuristics' and (particularly) 'novel facts' which bear little resemblance to their Lakatosian analogues

or how these terms have been used in reconstructions in the physical sciences. Much of this literature has provided valuable and independently interesting history of economic thought, but it sheds little light on the methodological adequacy of the MSRP. The only general conclusion that can be reached from this historical literature 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.* There have been some well-researched cases where novel facts actually seem to have been uncovered;¹⁶ but these cases correspond to only a very small portion of what the economics profession would consider its major theoretical 'advances'. Lakatos's criterion for 'theoretical progress', the prediction of novel facts, may have been sufficient for what economists have considered to be theoretical progress in certain special cases, but it does not seem to be generally 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 dismal on a strictly Lakatosian view.

The argument that empirical and theoretical advances in economics occur (and should occur) in ways other than Lakatos specified in the MSRP, reflects very poorly (again) on Popper. The reason is that *where economics is most likely to part ways with Lakatos is precisely where Lakatos borrowed most heavily from Popper.* In certain respects, Lakatos's work is much better suited to economics than Popper's; it seems that looking for the types of things which Lakatos suggests one should look for in the history of economics has helped guide a number of important historical studies. Certainly this historical research has drawn attention to the metaphysical hard core of certain economic research programmes and it has motivated enquiry into the important methodological question of the relationship between empirical and theoretical work in economics, that is, between econometrics and economic theory. What the MSRP does not provide is an appropriate model for the acceptance or rejection of economic theories. Lakatos's MSRP may constitute methodological progress over falsificationism, but it still fails to provide economists with an acceptable criterion for theory choice (or progressive problem shifts). This is particularly telling for Popper since the Lakatosian fit seems to be poorest where older Popperian parts were used with the least modification.

Conclusion

In the final evaluation it seems that 'Popperian' economic methodology must be given low marks. Falsificationism, Popper's fundamental programme for the growth of scientific knowledge, is particularly ill-suited to economics

and while the interest in Lakatos has produced some valuable historical studies,¹⁷ the overall fit between economics and the MSRP is not good: and not good precisely where Lakatos is the most Popperian.

This evaluation should not be too harshly interpreted though. It has been argued that Popperian methodology, both in its falsificationist and MSRP forms, does not provide a very good standard for judging the adequacy of economic theories; this does not mean that Popperian philosophy has not provided any insight at all into economic theorizing. In particular, the above argument does *not* say that testing should be unimportant in economics, that Lakatosian reconstructions in the history of economic thought have not provided valuable contributions to the historical literature, or that economists would have gained more by listening to some other particular school of philosophy.

In addition to the above disclaimers it should also be noted that the discussion has entirely neglected Popper's writings on the philosophy of *social science*: his so-called 'situational analysis' approach to social science.¹⁸ This method, the method of explaining the behaviour of a social agent on the basis of the logic of the agent's situation and the 'rationality principle', was proposed by Popper as a result of 'the logical investigation of economics' and it provides a method 'which can be applied to all social science' (Popper 1976a: 102). It is often argued that the rationality principle is in conflict with Popper's falsificationist standards,¹⁹ but regardless of how one views this controversy, the point here is simply to note that none of the above criticisms automatically transfer to Popper's work on situational analysis.

The task of this chapter was narrowly defined: to evaluate falsificationism and the MSRP as a methodology – as a tool for choosing between/among economic theories/research programmes. It has been argued that Popperian philosophy should be negatively appraised in this respect, it does not say that economists have nothing to learn from the Popperian tradition.

Notes

1. Helpful comments on an earlier draft were received from a number of people; in particular I would like to mention Bruce Caldwell, Christian Knudsen, Uskali Maki, and Jorma Sappinen. Partial support for the research was provided by University of Puget Sound Martin Nelson Award MNSA-4489 and portions of the argument also appear in Hands (1992). The recent article by Caldwell (1991) also provides an excellent discussion of these issues.
2. Blaug (1976, 1991), Cross (1982), de Marchi (1976), Diamond (1988), Fisher (1986), Fulton (1984), Glass and Johnson (1988), Hands (1985b), Latsis (1972, 1976b), Maddock (1984), and Weintraub (1985a,b, 1988), is a partial list of the

work on Lakatosian economics. A more complete list is contained in Hands (1985b) and (1992).

3. The expression 'basic statement' has a rather narrow interpretation in Popperian philosophy. The concept was introduced in chapter V of Popper (1959) and it is nicely summarized in Watkins (1984: 247–54).
4. 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).
5. There are a number of different types of *ad hocness* in Popperian philosophy; these are discussed in detail in Hands (1988). The type of *ad hocness* considered here, modification solely to avoid falsification, is called *ad hoc₁*. Popper developed his notion of independent testability to avoid this type of *ad hocness* (*ad hoc₁ness*). Another notion of *ad hocness* is *ad hoc₂*ness; a theoretical modification is non *ad hoc₂* if some of the independently testable implications actually get corroborated. A third type of *ad hocness* (*ad hoc₃ness*) was developed more fully by Lakatos. *Non-ad hoc₃ness* is equivalent to Lakatosian heuristic progress.
6. The main sources for this list of criticisms are Caldwell (1984), Hausman (1985, 1988), Latsis (1976b), and Salanti (1987).
7. 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 of 'not e ' implies 'not ($T \cdot A$)' rather than simply 'not T '; the test system is falsified, not necessarily the theory. The Duhemian problem is a standard issue in the philosophy of theory testing but it has only recently been recognized as an issue for economic methodology (see Cross 1982, for instance).
8. Experimental economics is still too young to tell whether it can substantially improve this situation. For a general discussion of the methodological implications of the literature on experimental economics see Roth (1986), and Smith (1982, 1985).
9. Popper (1965: 42, 267, 387–8; 1959: 43–4, 93–5, 97–111; 1983: 185–6).
10. This is one source of the 'innocuous falsification' mentioned by Blaug (1980: 128, 259) and Coddington (1975: 542–45). The problem of such qualitative (or generic) predictions is discussed in detail in Rosenberg (1989).
11. Popper's most important writings on verisimilitude are contained in Popper (1965) and (1972). Useful discussions of the topic are presented in Koertge (1979), Radnitzky (1982), and Watkins (1984). The question of the relationship between Popperian verisimilitude and economic methodology is examined in more detail in Hands (1991).
12. These concepts are discussed in detail with appropriate references to Popper's writings in Hands (1988). Other general discussions of these Popperian concepts include Dilworth (1986), Koertge (1979), Watkins (1978, 1984), and Worrall (1978).

13. Many summaries of the MSRP are available in the economics literature (Blaug (1980), Hands (1985a), Pheby (1988), and Weintraub (1985a, 1985b, 1988) for example) but the single best presentation of the argument remains Lakatos (1970). As with Popper's falsificationism, only a sketch of the main argument is provided here.
14. The definition of 'novel fact' has been much discussed in the Lakatosian (and Popperian) literature. See Carrier (1988), Gardner (1982), Hands (1985b) and Worrall (1978) on the different notions of novelty.
15. '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: 111).
16. Particularly Blaug (1991), Maddock (1984), and Weintraub (1988), though even here it depends on the exact definition of novelty one uses.
17. In addition to those mentioned in note 16, Cross (1982), de Marchi (1976) and Latsis (1972, 1976b) should be added to this list.
18. Popper's clearest writings on situational analysis are (1976a) and (1985); also see Hands (1985a, 1992) and Langlois and Csontos (this volume).
19. According to Popper's situational analysis view of social science, the action of an individual agent is explained by describing the 'situation' the agent is in (their preferences, beliefs, constraints, etc.) and the 'rationality principle' that all agents act appropriately (rationally) given their situation. The potential problem arises because the rationality principle serves as the universal law in such scientific 'explanations' and yet it is not clear that the rationality principle is (even potentially) falsifiable as Popper the falsificationist would require for the laws used in any valid scientific explanation. This is one of the reasons that Popper_n (Popper the falsificationist) was distinguished from Popper_s (Popper the philosopher of situational analysis) in Hands (1985a).

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