

9. Popper, the Rationality Principle and Economic Explanation

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INTRODUCTION

In earlier work (1985a, 1990) I have argued that an inconsistency exists between Karl Popper's falsificationist philosophy of natural science and the type of methodological advice he gives to social scientists in his writings on situational analysis. To emphasize this point I introduced (1985a: 90) the appellation Popper_n for the well-known views of Popper the falsificationist philosopher of natural science, and Popper_s for the lesser known views expressed in Popper's writings on social science and situational analysis.¹ While I have not been alone in calling attention to this potential contradiction, there does not seem to be any consensus in the literature regarding either the significance of this tension or how it might be alleviated.² Mark Blaug for instance, in his response to my paper, recognized the 'clash' between the two positions, but resolved the conflict by simply dismissing Popper's writings on social science: 'The fact is that Popper knew little about social science and even less about economics' (1985: 287).

In this paper I should like to continue the examination of Popper_n, Popper_s and the implications for social science and economics. I will argue that the tension in Popper is in fact an essential tension and that it stems from Popper's unfailing commitment to the covering-law model of scientific explanation; Popper's meta-commitment to this model forced him to characterize social science explanations in a way that clashed with his support of the 'unity of science' thesis and certain other aspects of his philosophical programme. This essential tension has important implications for the overall success of Popper's philosophical project as well as for the social science that Popper used as his general model: neoclassical microeconomics. In order to make my case for the essential tension it will be necessary to review Popper's perspective on scientific explanation, the unity of science and situational analysis. This review will be contained in the first two sections below. Possible philosophical 'solutions', some Popperian and some non-Popperian, as well as the implications for economic methodology will be examined in the last two sections.

POPPER ON SCIENTIFIC EXPLANATION AND THE UNITY OF SCIENCE

Although philosophical discussion on the nature of scientific explanation goes back to at least Aristotle, it is Hempel and Oppenheim (1948) who are usually given credit for the first definitive statement of what now constitutes the received view on scientific

explanation: the deductive-nomological (D-N) or covering-law model.³ According to the D-N model the key to scientific explanation is the deductive subsumption of the explanandum (the thing to be explained) under general laws; the explanandum should be deductively 'expected', given the initial conditions and the relevant general laws.

For example, suppose we wish to explain why a particular piece of wire expanded. A D-N explanation of this phenomenon might take the following form:

- (a) The wire was made of metal.
- (b) The wire was heated.
- (c) *All metals expand when heated.*
- (d) The wire expanded.

The *explanandum*, a statement describing the phenomenon to be explained, is given by sentence (d). It is *logically deduced* from the *explanans*, the explanation of the phenomenon, given by sentences (a) to (c). Notice that the explanans is composed of two different types of sentence – sentences (a) and (b) which state the *initial* (or *antecedent*) *conditions* and sentence (c) which states a *general scientific law*. Any D-N explanation must contain at least one general law and, according to the original Hempel and Oppenheim characterization, the general laws as well as the initial conditions must all be *true*. In general then, a legitimate D-N explanation must take the following deductive form:

$$\begin{array}{l} C_1, C_2, \dots, C_n \quad \text{explanans (or explicans)} \\ \hline L_1, L_2, \dots, L_m \quad \text{explanandum (or explicandum)} \\ \hline E \end{array}$$

where each C_i represents a sentence describing one of the initial conditions and each L_i represents a general law.

Now, while it is standard to credit Hempel and Oppenheim with first delineating D-N schema, Karl Popper may in fact have a legitimate claim to priority. In the *Logic of Scientific Discovery* (1968), a work originally published in 1935, Popper clearly sets out the covering-law model of scientific explanation:⁴

To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements, the *initial conditions*. (1966: 59)

We have thus two different kinds of statement, both of which are necessary ingredients of a complete causal explanation. They are (1) *universal statements*, i.e. hypotheses of the character of natural laws, and (2) *singular statements*, which apply to the specific event in question and which I shall call 'initial conditions'. It is from universal statements in conjunction with initial conditions that we *deduce* the singular statement. . . . (Ibid.: 60)

The question of whether Popper was actually the first to characterize the D-N model is not really the point; the point is that Popper clearly *accepts* the D-N framework as the proper way of characterizing scientific explanations.⁵ In fact, not only does Popper accept the covering-law view, he is actually a much stronger advocate of the model than Hempel (despite the fact that it is often called the 'Hempel' theory). There are at least two reasons to believe that Popper is a stronger advocate. One is that for Popper this is the *only* legitimate form of scientific explanation;⁶ for Hempel other types of scientific

explanation are possible. In particular, Hempel accepts both deductive-statistical (D-S) explanations and inductive statistical (I-S) explanations.⁷ Secondly, for Popper such explanations provide the *aim* of science: 'I suggest that it is the aim of science to find *satisfactory explanations* of whatever strikes us as being in need of explanation' (Popper 1972: 191).⁸ Thus for Popper covering law explanations are uniquely important; they provide the only legitimate form of scientific explanation and explanation is the aim of science.

Now, as mentioned above, in the Hempelian characterization a true scientific explanation is only obtained when both the laws and the initial conditions in the explanans are true. But Popper could not possibly endorse such a requirement. According to Popper's fallibilist theory of knowledge we never know when a scientific law (or even an initial condition) is true – all we have is a way for (we hope) eliminating error. All we can do is to test the theory empirically, and provisionally accept those theories that have passed severe tests.

The *explicans* in order to be satisfactory (satisfactoriness may be a matter of degree), must satisfy a number of conditions. First it must logically entail the *explicandum*. Secondly, the *explicans* ought to be true although it will not, in general, be known to be true; in any case, it must not be known to be false – not even after the most critical examination. If it is not known to be true (as will usually be the case) there must be *independent* evidence in its favour. In other words, it must be *independently* testable; and it will be the more satisfactory the greater the severity of the independent tests it has survived. (Popper 1983: 132–3).

In this way Popper's acceptance of the D-N model of explanation connects up cleanly with his general falsificationist methodology. The aim of science is explanation, explanations require general laws, and the general laws must be scientific theories which have survived severe, independent, empirical tests. In terms of the language of Popperian falsificationism, the 'laws' in the explicans must be corroborated scientific theories. So what about social science? How are explanations and the theories involved in them different in social sciences than in natural science? Or are they?

The short answer is that for Popper there is no difference. Popper advocates the 'unity of science' or methodological monism: the position that the method employed in social science is fundamentally the same method as that employed in natural science. Social science might be a little more difficult, the data might not be as clean or as available, but the basic methodology is the same for both social and natural science.

I am going to propose a doctrine of the unity of method; that is to say, the view that all theoretical or generalizing sciences make use of the same method, whether they are natural sciences or social sciences. . . . The methods always consist in offering deductive causal explanations, and in testing them (by way of predictions). (Popper 1961: 130–1)

Actually the unity-of-science thesis is such a fundamental part of the Popperian philosophical position that to abandon it could potentially undermine the entire programme. As Popper makes very clear in his autobiography (1976b) and elsewhere, one of his early motivations for the development of his philosophy of science, and of his demarcation criterion in particular, was to define 'science' in such a way that certain social sciences – specifically Marxism and Freudian psychology – would fall outside the domain of science.⁹ For Popper successfully to *criticize* Marx and Freud as being 'unscientific' necessarily presupposes methodological monism for it presupposes that social science *can* and *should* be judged by the same methodological standards as the natural sciences. If

social theorizing is fundamentally different from theorizing in the natural sciences then Popper's criticism of these two research programmes is simply a category mistake.

In summary then, the Popperian position seems quite clear. The aim of theoretical science, either natural or social, is to provide causal explanations. These causal explanations must conform to the D-N model and the universal statements (the laws) involved must live up to falsificationist methodological standards. In particular, these theories need to be 'scientific' – they must be potentially falsifiable by empirical basic statements – and they must have survived severe independent testing. This all seems relatively straightforward; now let us consider Popper's writings on situational analysis.

SITUATIONAL ANALYSIS AND THE RATIONALITY PRINCIPLE

Only a small fraction of Popper's written work is concerned explicitly with social science.¹⁰ His approach to social science is called 'situational analysis' and it was developed as 'an attempt to generalize the method of economic theory (marginal utility theory) so as to become applicable to the other theoretical social sciences' (Popper 1976b: 117–18). It is not entirely clear whether Popper considers situational analysis to be merely one approach to human science or whether he considers it to be the 'only' method available to social science.¹¹

The basic method of situational analysis is to specify the 'situation' the agent is in – this includes the individual's beliefs and desires as well as objective constraints and information – and then deduce the behaviour of the agent on the basis of the *rationality principle* (RP) that all individuals act 'appropriately' (rationally), given their situation. Since the situational description, no matter how detailed, is simply a description, the rationality principle is necessary to 'animate' that description into action. In general, 'there is only one animating law involved – the principle of acting appropriately to the situation', it 'is known in the literature under the name "*rationality principle*"' (1985: 359).

In schematic form, then, if we want to use situational analysis (SA) to explain why a particular agent A did action X our explanation would take the following form:¹²

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

Comparing this SA explanation with the D-N explanatory schema presented above we have the first two parts – the description and the analysis – serve as the initial conditions and the RP serve as the general law. Popper generally, though he is not entirely consistent on this, calls the first two parts the 'model' of the situation; thus 'the premises of the explanation usually consist of a situational model and of the so-called rationality principle' (Popper 1976a: 100 note). Given Popper's use of the term 'model' the two explanatory schemata can be compared in the following way:¹³

D-N explanation

Explanans: Initial conditions theory (or law)

Explanandum: Event

SA explanation

Explanans: Model (or theory)

RP

Explanandum: Action

So far so good; nothing seems to be inconsistent with what was said about Popper in the previous section. Popper requires D-N explanations for all sciences – he advocates SA for social science – but SA just seems to be a special form of the D-N model. So why should there be a conflict between Popper_n (the advocate of the left-hand D-N schema) and Popper_s (the advocate of the right hand SA schema)?

The key to this question *lies in the nature of the rationality principle*. Since the RP serves as the general law in a D-N explanation then it needs to be a corroborated scientific theory and ‘it must not be known to be false’ (Popper 1983: 132), but this requirement presents severe difficulties for the RP. First of all, Popper is never clear whether the RP requires the agent to act appropriately in some *objective* sense, or whether it only requires the agent to act appropriately in the *subjective* sense of acting appropriately ‘as they see it’.¹⁴ For this reason Nadeau (1990: 10) considers (to) separate rationality principles: RP_o, the objective rationality principle that agents act appropriately to the situation in some objective (ideal observer) sense, and RP_s, the subjective rationality principle that all agents act appropriately as they see the situation (however distorted their viewpoint may be to any other observer). I shall argue that the RP runs into problems with Popper’s demarcation criterion, and thus Popper_n is in conflict with Popper_s, on *either one* of these characteristics.

First consider the RP_s. The problem with the subjective interpretation of RP is that it is empirically *unfalsifiable*. What possible observation could we make of an individual’s behaviour that would falsify the claim that the individual was acting appropriately to the situation ‘as they saw it’? The only way to falsify such a claim would be to have independent (and objective) information about the individual’s beliefs: if we knew his beliefs and the (observable) action he took, we could then evaluate whether he did in fact act appropriately given his beliefs.¹⁵ But we do not read minds. The only way we might possibly find out about a person’s beliefs would be to ask him, but this entails a vicious regress. A speech act is no less an intentional action as any other human action. How someone will respond to a question about his beliefs will in fact depend on his beliefs; in particular how he responds will be what is appropriate for him to say given the conversational situation as he sees it.¹⁶ Any way we might test RP_s necessarily presupposes it and thus the rationality principle in its subjective interpretation is unfalsifiable.

Now consider RP_o. The problem with the objective interpretation of RP is that it is empirically *false*, people do not always act in a way that is objectively appropriate given their situation. Popper (1985: 361) clearly admits the falsity of the RP_o:

I think one can see very easily that this is not so. One has only to observe a flustered driver, desperately trying to park his car when there is no parking space to be found, in order to see that we do not always act in accordance with the rationality principle. . . . But a principle that is not universally true is false. Thus the rationality principle is false. I think there is no way out of this.¹⁷

Thus we clearly have a tension between Popper_n and Popper_s. The best social science explanations, those involving the rationality principle and situational analysis, employ a

general law that is either *unfalsifiable* (on the RP_s interpretation) or *false* (on the RP_o interpretation). In either case situational analysis does not provide explanations that are bona fide scientific explanations. If scientific explanations are the aim of science and the best social science cannot provide such explanations, then it must be the case that all of social science falls on the non-science side of the demarcational fence. Thus economics, Popper's model for situational analysis, is indicted, and Popper's entire argument against Marx and Freud is defused. This ultimately must undermine the unity of science thesis and the internal integrity of the entire Popperian philosophical programme.

SOME WAYS OUT

I should like to discuss a number of ways in which one might philosophically get around the negative conclusion of the previous section. These 'solutions' will be divided into two separate groups. The first group constitutes solutions which could be called 'Popperian' in spirit; that is, they are solutions that are generally consistent with the fundamental tenets of the Popperian programme. The second group of possible solutions are non-Popperian in that they require the sacrifice of at least one important part of the Popperian ensemble.

The first of these Popperian solutions is what I would call the *verisimilitude solution* and there seems to be reasonably good evidence that this was in fact Popper's own solution to the problem.¹⁸ During the 1960s Popper developed the concept of 'verisimilitude' or 'truthlikeness'.¹⁹ The basic idea was, first, to formalize the notion of being 'closer' to the truth or approximately true and, secondly, to demonstrate that scientific theories consistent with Popper's falsificationist methodology exhibited this property. It is easy to see how the concept of verisimilitude might provide a solution to Popper's problem. Even though the RP is false, if it could be shown to be closer to the truth than other 'laws' which might be used in social science explanations, then situational analysis would be vindicated. In the verisimilitude scenario the unity of science continues to hold; the only difference between Popper_s and Popper_n is that the former is more approximate, has less verisimilitude, than the latter. As Popper himself (1976a: 103) says:

The explanations of situational logic described here are rational, theoretical reconstructions. They are oversimplified and overschematized and consequently in general *false*. Nevertheless, they can possess a considerable truth content and they can, in the strictly logical sense, be good approximations to the truth, and better than certain other testable explanations. In this sense, the logical concept of approximation to the truth is indispensable for a social science using the method of situational analysis.

There are at least two reasons why the verisimilitude solution fails to be successful. One of these is simply that Popper's general programme in verisimilitude was a failure.²⁰ Not only did Popper's formal concept of a 'measure' of verisimilitude run into insurmountable difficulties, but even in a less formal guise the concept was never linked up with the rest of the Popperian programme; in particular it was never demonstrated that increasing the degree of corroboration increased the verisimilitude of a theory. Secondly, even if Popper's concept of verisimilitude had been a complete success there are still problems with defending SA in this way. Suppose Popper had clearly established a link between passing severe tests (being corroborated) and being approximately true (having verisimilitude). For this to help SA it would be necessary to actually corroborate the RP

– it would be fine for the RP to be false in a number of specific cases – but it would continually need to be challenged with severe tests and pass them. This is *not* what Popper recommends for the RP. In fact Popper argues that the RP should not itself be tested; the arrow of *modus tollens* should be aimed at the ‘model’ and not at the RP.

Now if a theory is tested, and found faulty, then we have always to decide which of its various constituent parts we shall make accountable for its failure. My thesis is that it is sound methodological policy to decide not to make the rationality principle accountable but the rest of the theory; that is, the model. (Popper 1985: 362)

Since Popper does not recommend testing the RP, it cannot be corroborated and thus it cannot be shown to have increasing verisimilitude even if the concept of verisimilitude were unproblematic.

The second Popperian solution could be called the *Lakatosian solution*. Suppose we treat the RP as a hard-core proposition in the situational analysis research programme in social science. Since according to Lakatos’s ‘Methodology of Scientific Research Programmes’ (1970), the hard-core propositions are never tested, Popper’s defensive strategy regarding the RP would be entirely appropriate.²¹ Given Lakatosian standards for ‘progressive problem shifts’ we could examine the overall progress of the general SA research programme or the progress of a particular specialized version of it, such as microeconomics. For example, if large portions of the neoclassical research programme could be reconstructed as progressive by Lakatos’s standards it would certainly diminish the impact of the conflict between Popper_s and Popper_n. I say diminish rather than eliminate for a number of reasons, the most important of these being that no one has ever argued convincingly that the theories extracted from progressive Lakatosian research programmes can legitimately serve as the general laws in D-N explanations. But such issues are probably just ‘in-house’ debates between those of a more old-fashioned Popperian persuasion and those in the Lakatosian wing of the programme. Ultimately whether the Lakatosian solution will be viable or not will depend on the overall success of the Lakatosian programme and that is a topic which is surely beyond the scope of the current chapter.²²

It should probably be noted that the Lakatosian solution is the solution endorsed by Mark Blaug.²³ Since Blaug has never actually admitted that the tension between Popper_n and Popper_s is an issue of major significance it might be better to say that because Blaug has endorsed Lakatos’s methodology as the proper way to apply Popper to economics for over a decade,²⁴ it seems reasonable to argue that he would endorse Lakatos’s work as the solution to any problems that one might uncover relating Popper’s philosophy to the social sciences.

The third and final Popperian solution I shall examine is the *critical rationalist solution*. It is the solution recently endorsed by Bruce Caldwell (1991). Critical rationalism is an alternative interpretation of Popperian philosophy due primarily to W. W. Bartley (1984).²⁵ It is an interpretation which Popper himself seemed to support in later years, especially in (1983). The basic argument of critical rationalism is that the ‘method of science’ consists of criticism and being open to criticism. ‘Scientific theories are distinguished from myths merely in being criticizable, and in being open to modifications in the light of criticism’ (Popper 1983: 7). On this view, empirical falsifiability is simply one of many different forms of criticism; it is the form most important in scientific discourse, but it is simply one form of criticism. According to critical rationalism the issue is not demarcation, where something either is or is not science, but rather criticizability which

is not yes or no but a continuum from very criticizable to not criticizable (or dogmatic). Since critical rationalism blurs the distinction between science and non-science it provides a way out of the tension between Popper_n and Popper_s. As Caldwell (1991: 48–9) argues:

Critical rationalism is a problem-solving approach which itself appears to resolve a problem within Popper's philosophy of science, the tension between situational analysis and falsificationism. Critical rationalism states that sometimes it is appropriate to evaluate a theory using the strict empirical criteria of falsificationism. But at other times, especially within the social sciences, one is better able to criticize a theory by applying the canons of situational logic. And there are still other circumstances, particularly when metaphysical theories are considered, when other routes to criticism are preferable. Which methods of criticism are most appropriate cannot be specified in advance: that will depend on the subject matter and the problem to be solved.

Caldwell is entirely correct that *if* one accepts critical rationalism then the tension between the two Poppers seems to dissolve. The real question is not about whether it solves the problem, but about critical rationalism itself. In earlier work I have expressed some scepticism about this approach²⁶ and I still tend to think of it as an epistemically retreatist position (as some of Popper's generals advising him to fall back to a region of such philosophically unstable terrain that his critics will be unwilling to continue their pursuit), but none the less I must also admit that critical rationalism is a position on which the jury is still out. The argument is relatively new and both its general philosophical adequacy as well as its Popperian fidelity are still open questions.²⁷

Turning now to philosophical solutions that cannot properly be called 'Popperian', I should like to examine two possibilities (there are of course many more that could legitimately be discussed). The first of these non-Popperian solutions could be called *social science instrumentalism*. Many of the criticisms raised against the RP and SA are general criticisms of any 'action' explanations that utilize beliefs, desires and other intentional notions in their explanans. Such explanations are called 'folk psychology' and a fairly extensive philosophical literature has developed which considers the explanations given by folk psychology to be something less than bona fide scientific explanations.²⁸ This literature has not as yet addressed itself explicitly to Popper's situational analysis but since many of the criticisms of folk psychology apply directly to Popper's SA, those things proposed to circumvent the problems of folk psychology could also serve as a solution to the tension between the two Poppers. Now, one of the most discussed responses to the problem of folk psychology is social science instrumentalism; it is a position usually associated with Daniel Dennett.²⁹ The argument is that while social sciences that utilize belief and desire in their explanans do not provide us with causal scientific explanations, they might none the less be instrumentally quite good when judged solely on the basis of their predictive adequacy. Maybe the involvement of intentional notions prevents scientific explanation, but such notions do not preclude the instrumental usefulness of the theories in which they are embedded. In addition, so the argument goes, maybe such instrumental usefulness is all that we can expect from any social science – at least for the immediate future. This instrumentalist view of social science is certainly non-Popperian, both because it violates the unity of science thesis and because it supports instrumentalism, a view that Popper has long criticized (in any science). Obviously, whether one is Popperian or not, the degree to which one finds this solution acceptable will ultimately depend on one's views about instrumentalism and one's commitment to the unity of science.

The second non-Popperian solution is what I shall call the *alternative views of*

explanation solution. The basic argument is that the tension between Popper_s and Popper_n is a tension created by Popper's too-strong adherence to the D-N model of explanation. The covering-law model has been under almost continuous siege for the last 40 years and while no new consensus has emerged to replace the received view of the D-N model, a few points of consensus have surfaced. One of the points of consensus, the one most relevant to the question at hand, is that our concept of a scientific explanation needs to be more pragmatic or context-dependent.³⁰ The D-N model was a purely formal, *a priori* framework for appraising scientific explanations; its replacement should be sensitive to the pragmatic context in which explanations are proposed. In the last few years a number of different approaches to scientific explanation have been suggested which try in various ways to accommodate this pragmatic component. These approaches include the explicitly pragmatic approach of van Fraassen (1980), the explanatory unification account of Kitcher (1989) and the causal explanation view of Miller (1987).³¹ While I do not intend to endorse any particular one of the alternative views, I would argue that with a less rigid, more pragmatic notion of explanation the criticisms of SA explanations may disappear. As I discussed above, if SA is to be forced into the D-N framework then the RP must satisfy the requirements for being a general scientific law; on a more pragmatic characterization this very strict requirement would no longer need to be met. For example, on Richard Miller's (1987: 91) causal view, explanations are judged in part by the much weaker criterion of whether they satisfy certain field-specific 'standard causal patterns'.

Now, of course, it is possible that SA will fail to live up to the standards of even a more context-dependent view of explanation. It is also possible that these pragmatic notions of explanation simply surrender too much, that they will accommodate 'explanations' of a clearly non-scientific type. Both these questions will be a matter for further research. My point is simply that if the D-N model is now generally accepted as being too rigid even for natural science – if our best examples of successful scientific explanation fail to fit the D-N model – why should such standards continue to be used in the indictment of social science explanations, SA or otherwise?

IMPLICATIONS FOR ECONOMIC METHODOLOGY

While it is very unlikely that awareness of the tension between the two Poppers or its origin in Popper's strict adherence to the D-N model will have a profound impact on most practising economists, it should have an impact on the kinds of issues that concern economic methodologists. For those of the Popperian persuasion such as Blaug and Hutchison the issue is obvious; as Blaug himself (1985: 287) admits, 'Those like myself who claim that modern economists largely subscribe to Popperian falsificationism have a little difficulty here that they have not squarely faced up to.' But even for those who do not consider Popper to be a useful guide to the philosophical investigation of economics, the issue should also be of substantial concern. As the above discussion of folk psychology indicated, this is not simply a problem of an inconsistency or tension within the Popperian tradition; it is potentially a problem for any social science that appeals to intentional notions such as beliefs and desires in an attempt to provide covering-law explanations of human action. And given that most standard microeconomic explanations are considered to be just this kind of intentional yet nomic explanation, the real issue is the general question of the explanatory adequacy of microeconomics.³² While Alexander Rosenberg (1980, 1983, 1988) seems to be the only author who has applied this critique of folk

psychology directly to economics, the issue is substantive and independent of one's commitment to Popperian philosophy.

Now, one could argue that even though these issues are substantive, their impact on economic methodology will be small since most of the 'solutions' are already standard positions within the methodological literature. Lakatos's methodology of scientific research programmes provides a possible solution and has certainly received a lot of attention from economists – no doubt for reasons other than those discussed above. On the other hand, Caldwell's support of critical rationalism seems to be motivated by precisely these concerns. Instrumentalism offers a solution and it certainly has a wide following within the economics profession. While some of instrumentalism's support may come from blind adherence to Friedman's famous essay (1953), far more of it comes from instrumentalism's ability to provide a solution to the problems examined here. This is not to say that practising economists have adopted an instrumentalist position because they care about the essential tension within Popperian philosophy – they most certainly do not. The point is simply that practising economists adopt instrumentalism because they know that the basic assumptions of their models (including the RP) are false and yet they find these models to be very useful in predicting economic behaviour.³³ Finally, the new literature on pragmatic explanation has not explicitly been applied to economics, but none the less the economics profession has a long tradition of resisting attempts to force economic explanation into the procrustean bed of the covering-law model.

Despite the fact that many of these solutions are already in the literature, the essential tension within the Popperian tradition and the implied critique of microeconomic explanations constitutes more than a new way of interpreting the existing work in economic methodology. These issues also raise fundamentally new methodological questions that require our attention in future research. I should like to close this chapter by examining one of these questions.

The question is: Are microeconomic explanations really SA explanations? Certainly Popper thought so since he considered microeconomics to be the model for his characterization of SA. And certainly those like Rosenberg who apply the critique of folk psychology to microeconomics must believe that economic explanations are of this general intentional form. In fact the entire discussion of 'solutions' was implicitly based on the argument that real social science explanations, particularly those in microeconomics, were of this type. But are they?

I frankly do not have a firm answer to this question, but I do think it is a topic that deserves serious investigation. Consider the issues. The explanandum of an SA explanation is the action of an individual; in terms of the schema in the third section above, 'A did X'. Now it is possible that microeconomics could be used to explain why an individual consumer purchased a particular good or why an individual worker offered to work at a particular wage, but this is certainly not the only, or even the most common, type of microeconomic explanation. More commonly, microeconomics is used to explain phenomena that emerge from the actions of a large number of individuals – things like market prices and quantities. Even in the realm of purely *microeconomics*, it is aggregated (market) variables that are usually the topic of investigation, not the action of an individual economic agent. The event to be explained is often an *unintended* consequence of the actions of the individual agents and since, according to SA, the actions of the individual agents are intentional, the event to be explained must necessarily be subject to laws that emerge at a higher level than that of the individual agent. Now Popper clearly pays lip service to the unintended consequences of individual action. He says (1965: 342),

in fact, that the main task of theoretical social science is 'to trace the unintended social repercussions of intentional human actions'³⁴ and he even uses supply and demand as an example (1965: 124), but this is not really what SA explains. SA, like any folk psychology explanation in terms of the beliefs and desires of agents, explains only the actions of individuals. If unintended consequences or other emergent properties are to be explained it seems that 'laws' other than the RP and 'causes' other than individual beliefs must be involved. It is possible that such an argument can be used to extricate social sciences like microeconomics from the general critique of SA and intentional psychology.³⁵ If this is the case, the tension is simply a problem for the internal integrity of the Popperian programme and not necessarily a general problem about the way economists explain. Such an argument would of course lend credence to Blaug's (1985b: 287) original response that 'Popper knew little about social science and even less about economics'.

NOTES

1. The Popper_n position is best represented by *The Logic of Scientific Discovery* (1968) and the essays in *Conjectures and Refutations* (1965); Popper_s is best represented by Popper (1976a) and (1985).
2. Some of the contributors to this literature include: Blaug (1985b), Caldwell (1991), Farr (1983), Koertge (1974, 1975, 1979, 1985), Lagueux (1990), Latsis (1976, 1983), Nadeau (1990), Pepin (1990), Schmid (1988) and Watkins (1970).
3. Hempel and Oppenheim (1948) is reprinted in Hempel (1965). Salmon (1989) provides a detailed history of the D-N model and the philosophical literature that has surrounded it. The D-N model is also summarized in Blaug (1980a) and Caldwell (1982).
4. Hempel does in fact recognize Popper's early contribution to the literature although he does not actually go so far as to give Popper priority (1965: 251 note 7, and 337 note 2). Popper himself, though, seems to be quite clear on the matter: 'To my knowledge, the theory of causality sketched here in the text was first presented in my book, *Logik der Forschung* (1935) – now translated as *The Logic of Scientific Discovery*' (1966: 362).
5. In case someone might attribute Popper's support for the D-N model to mere youthful exuberance, it should be noted that almost 50 years later in the 'Postscript to the Logic of Scientific Discovery' his view is unchanged (see Popper 1983: 132).
6. Because of this position Salmon (1988: 95) finds Popper guilty of 'explanatory deductive chauvinism'.
7. See Hempel (1965: 376–412); the topic is also given a detailed discussion in the Salmon (1989) survey on explanation.
8. Also see Popper (1968: 61 note, and 1983: 132).
9. See Popper (1976b: 36–7, 41–2; and 1965: 33–5).
10. In addition to Popper (1976a and 1985) listed in note 1, the other places where social science is given more than a passing comment are (1961, 1966, 1976b).
11. Certainly in (1985: 358) he says, 'My thesis is that only in this way can we explain and understand what happens in society. Similar comments are on 361.
12. This schematic form is based on Koertge (1975: 87; 1979: 440). The scheme goes back at least to Hempel (1962: 12), a paper where Popper is not cited but which some have called 'Hempel's attempt to codify the main features of Popper's "logic of the situation"' (Leach 1968: 260).
13. This way of comparing the two explanatory schemata is given in Nadeau (1990: 4).
14. Popper is *really* unclear on this; Latsis (1983: 133) claims, I think rightly, that Popper is either 'confused or deliberately elusive' on this issue. For example, in 1985 Popper clearly takes the subjective interpretation saying that rationality is only 'as they see it' and thus SA can be applied to the behaviour of a 'madman' (1985: 363). On the other hand, he calls SA 'a purely objective method' which 'can be developed independently of all subjective or psychological ideas' (1976a: 102), and in 1966 he says the RP is the 'general law that *sane* persons as a rule act more or less rationally' (1966: 265).
15. 'The only way to falsify such a statement would be to prove that an agent did not act in accordance with his own beliefs. Such a proof would only be possible if the observer could have access to the beliefs of an agent *other than by observing his behavior*; but this is clearly impossible' (Nadeau 1990: 14).
16. This argument is presented in greater detail in Rosenberg (1988: 30–6).
17. Similar remark in Popper (1976a: 103).
18. Although here, as elsewhere in Popper's discussion of SA, the issue is hardly clear cut.
19. Popper's primary discussion of verisimilitude is contained in chapters 2 and 9 of (1972) and chapter 10 of (1965). Verisimilitude is discussed in much greater detail in Hands (1991 a and b).

20. Popper himself discusses this in (1983: xxxv–xxxvi); the problem is also surveyed in Watkins (1984: 279–88).
21. To the best of my knowledge this solution was first applied to microeconomics in Latsis (1972) and SA in general in Koertge (1975).
22. The literature surrounding Lakatos's programme is far too extensive to cite here but two important volumes of essays are: Cohen, Feyerabend and Wartofsky (1976) and Gavroglu, Gouderoulis and Nicolacopoulos (1989). The literature on Lakatos and economics is also extensive but the two most important volumes are probably Latsis (1976) and Blaug and de Marchi (1991).
23. This is how Blaug is interpreted in Caldwell (1991: 40).
24. Blaug (1976, 1980, 1985, 1987, 1990).
25. Caldwell provides a very careful discussion of critical rationalism in (1991: 40–52).
26. Hands (1990: 30–1); also see Nola (1987).
27. A fourth Popperian solution which I did not discuss is that offered by Watkins (1984). Watkins is not really concerned with the tension between Popper_s and Popper_n, but he is concerned with formulating a neo-Popperian programme that avoids some of the pitfalls of the Popperian original. At least two of the changes in the Popperian programme that Watkins proposes may actually help dissolve the tension discussed above. For one thing, Watkins proposes weaker adequacy requirements rather than a demarcation criterion, and secondly, Watkins weakens the concept of explanation, away from the D-N model and more toward explanatory unification.
28. This argument, a version of the view that reasons cannot be causes, is often traced to Wittgenstein; important recent works include Churchland, P. M. (1984), Churchland, P. S. (1986) and Stich (1983). Chapter 2 of Rosenberg (1988) summarizes the argument nicely.
29. See Dennett (1978); criticisms of this view include Baker (1989) and Stich (1983: 242–6).
30. Salmon (1989: 181) lists three other such points of consensus.
31. This is just a small sample of the literature on alternative theories of explanation. Many of the influential papers are reprinted in Pitt (1988), and Salmon (1989) provides a detailed survey.
32. I shall simply avoid the quite interesting topic of macroeconomics and how it related to all of these issues.
33. This argument for the popularity of instrumentalism is presented in Caldwell (1990).
34. For similar remarks see Popper (1966: 95, 323; 1972: 160; 1976b: 185).
35. Such an argument is presented in Nelson, A. (1990), but the author warns us (correctly) that his position does not automatically endorse the social sciences. It only says that the fate of the social sciences is *independent* of the fate of intentional psychology; the social sciences may still have problems of their own.