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2. The Problem of Excess Content: Economics, Novelty and a Long Popperian Tale

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INTRODUCTION

I have previously argued against using the Popperian/Lakatosian notions of excess content and novel facts as the sole criteria for theory appraisal in economics (Hands, 1985a, 1988, 1989a, 1990). It is not my intention to repeat my earlier arguments here. My current task is related but more historical. What I will do is trace the sequence of events which brought Popperian philosophy (including Lakatos) to its current position on the issues of excess content, novelty and scientific progress. My general approach will be to analyse Popper's and Lakatos's positions on these issues as an appropriate response to the particular philosophical problem situations in which they found themselves. In particular, I will argue that Popper's concept of verisimilitude played a fundamental role in the evolving problem situations of these two authors. Finally, after reconstructing this Popperian tale, I will return to economics and economic methodology. Some of the problems in contemporary economic methodology can be much better understood in the light of this reconstructed history of the Popperian and Lakatosian view.

In the first section below I will reconstruct Popper's problem situation as it pertains to the issues of content, novelty and truth. Particular attention will be given to verisimilitude and its role in Popper's position. The second section will discuss Lakatos's view and analyse his methodology of scientific research programs (MSRP) in the context of his, inherited, problem situation. The third section will consider contemporary problems in Popperian philosophy, particularly problems with the concept of verisimilitude, and relate these problems to the earlier discussion of Popper's problem situation. The final section will discuss economics and the relationship between these events in Popperian philosophy and recent work on economic methodology.

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1 A LONG POPPERIAN TALE ABOUT CONTENT, NOVELTY AND TRUTH[1]

Popper's 1934 position in *Logik der Forschung* was purely methodological without being epistemological.[2] His falsificationist methodology of bold conjecture and severe test provided a set of rules for the game of science (rules for demarcating science from non-science, rules for correctly playing the scientific game, and a criterion for successful play or progress) without providing an ultimate aim or purpose for playing the game. Earlier philosophies of science had been explicit about the aim of science and had subordinated their rules to their stated aim. 'In Popper's philosophy this link seems to be severed. The rules of the game, the methodology, stand on their own feet; but these feet dangle in the air without philosophical support' (Lakatos, 1978, p. 154). Popper clearly recognized this lacuna in his early view and sought to fill it in his later work. 'Since publishing the *Logik der Forschung* (that is, since 1934) I have developed a more systematic treatment of the problem of scientific method: I have tried to start with some suggestions about the aims of scientific activity, and to derive most of what I have to say about the methods of science – including many comments about its history – from this suggestion' (Popper, 1983, p. 131).

Popper's main suggestion, introduced in 1959 or 1960, is that the aim of science is the 'search for truth'.[3] 'We should seek to see or discover the most urgent problems, and we should try to solve them by proposing true theories . . .' (Popper, 1973, p. 44). Popper had always preferred scientific realism and had wanted to characterize science as the search for truth, but in the early 1930s, when he was writing *LSD*, the correspondence theory of truth was in such disrepute that Popper strategically chose to 'avoid the topic' (1965, p. 223). It was not until Popper became aware of Alfred Tarski's theory that he lost his 'uneasiness concerning the notion of truth' (1973, p. 320) and formally endorsed truth as the aim of science.[4]

Actually Popper went far beyond Tarski and the Tarskian notion of truth by introducing his own concept of truthlikeness or 'verisimilitude'. Popper argued that, if science is to aim at truth, it is necessary to have a notion of approximate truth or coming nearer to the truth. His goal in introducing the concept of verisimilitude was to be able to say 'that some theory T_1 is superseded by some new theory, say T_2, because T_2 is more like the truth than T_1' (1973, p. 47). Such a concept 'allows us to say that the aim of science is truth in the sense of better approximation to truth, or greater verisimilitude' (1973, p. 57). Popper intended verisimilitude to apply to strictly false theories; it should allow us to make sense out of the notion that one theory is closer to the truth than another even if both theories are false.[5]

While an elaborate discussion of Popper's concept of verisimilitude is not required here, a brief sketch of the idea will be useful.[6] Popper's definition of verisimilitude relies fundamentally on the notion of the *content* of a statement (or conjecture or theory). The *content* of a particular statement a, is the class of all non-tautological statements which are logically entailed by a. This content class (call it A) is subdivided into the truth content (A_T): the set of all true statements which follow from A, and the falsity content (A_F): the set of all false statements which follow from a. Actually we need to be a little more careful in specifying A_F since true statements can be deduced from false ones, but these definitions of A_T and A_F capture the basic ideas of truth and falsity content.[7]

Given these notions of truth and falsity content it is rather simple to define verisimilitude or truthlikeness. A theory T_2 has more *verisimilitude* than a theory T_1 if and only if (a) their contents are comparable, and *either* (b) the truth content but not the falsity content of T_2 is greater than T_1, *or* (c) the falsity content but not the truth content of T_1 is greater than T_2. In other words, of two comparable theories, the one with more true implications (and no more false implications) or fewer false implications (and no fewer true implications) has the greater verisimilitude. This type of verisimilitude or nearness to truth became the Popperian aim of science: 'To say that the aim of science is verisimilitude has considerable advantage over the perhaps simpler formulation that the aim of science is truth' (Popper, 1973, p. 57).

So around 1960 Popper introduced the notions of truth and truthlikeness, and specified truthlikeness or verisimilitude as the aim of science. It is easy to see this move as an appropriate response to his philosophical problem situation. Popper wanted an approach which was basically realist ('the only sensible hypothesis', Popper, 1973, p. 42) while still avoiding essentialism; he perceived Tarski's notion of truth and his own concept of verisimilitude to be a solution to his problem.

> I wish to be able to say that science aims at truth in the sense of correspondence to the facts or to reality; and I also wish to say (with Einstein and other scientists) that relativity theory is – or so we conjecture – a better approximation to truth than is Newton's theory, just as the latter is a better approximation to the truth than is Kepler's theory. And I wish to be able to say these things without fearing that the concept of nearness to truth or verisimilitude is logically misconceived, or 'meaningless'. (Popper, 1973, p. 59)

Now, however satisfying verisimilitude might be as the aim of science, it only becomes an appropriate solution to Popper's problem situation if it does not require him to abandon the falsificationist methodology of *LSD*; not surprisingly it does not. Popper argues that verisimilitude as an aim is perfectly consistent with the falsificationist methodology of bold conjecture and severe test which he has advocated in all of his methodological writings.

For a new theory to have more verisimilitude than its comparable predecessor the new theory either needs to have more truth content or less falsity content than the theory it replaces. First consider the case where the falsity content of the two theories is the same and focus on the truth content. In this case the bolder theory, the one with the most total content, the one which says more, will have more truth content and thus more verisimilitude. In this way Popper's traditional preference for bold theories, theories which take greater risks, seems to be entirely consistent with verisimilitude as the aim of science. Now consider falsity content. For more verisimilitude we want less falsity content. Popper's methodology has always been a method of severe test and attempted refutation; it is a falsificationist method. This falsificationist method of seriously attempting to refute theories and eliminating those which fail these severe tests seems entirely consistent with the aim of finding theories with low falsity content. Popper summarizes these arguments in the following way.

> A theory is the bolder the greater its content. It is also the riskier: it is the more probable to start with that it will be false. We try to find its weak points, to refute it. If we fail to refute it, or if the refutations we find are at the same time also refutations of the weaker theory which was its predecessor, thus we have reason to suspect, or to conjecture, that the stronger theory has no greater falsity content than its weaker predecessor, and, therefore, that it has the greater degree of verisimilitude. (1973, p. 53)

So Popper argues that verisimilitude as an aim is consistent with falsificationism as a method: but this is not the end of his argument. It seems that verisimilitude as a 'solution' opens up another problem. The search for truth or truthlikeness is not enough; we need 'interesting truth' (Popper, 1965, p. 229). 'In other words, we are not simply looking for truth, we are after interesting and enlightening truth, after theories which offer solutions to interesting *problems*. If at all possible, we are after deep theories' (Popper, 1973, p. 55, emphasis in original). The search for ever deeper theories requires *more than* simple falsificationist practice: more than bold conjecture, severe test, and avoiding ad hoc defensive stratagems. The method of *LSD* is consistent with the search for truth but it is not enough. Popper says: 'I have been asked, "What more do you want?" My answer is that there are many more things I want; or rather, which I think are required by the logic of the general problem situation in which the scientist finds himself; by the task of getting nearer to the truth. I shall confine myself here to the discussion of three such requirements' (1965, p. 241).

The first of Popper's famous three requirements is that the 'new theory should proceed from some *simple, new, and powerful unifying idea*' (1965, p. 241, emphasis in original); this is the so-called 'simplicity requirement'. The second requirement is the requirement of 'independent testability'. 'That is to say, apart from explaining all the *explicanda* which the new theory was designed to explain, it must have new and testable consequences (preferably consequences of a *new kind*); it must lead to the prediction of phenomena which have not so far been observed' (1965, p. 241, emphasis in original). This second requirement is designed to eliminate ad hoc modification of a falsified theory parading as a new theory.[8]

Finally, Popper's 'third requirement', is that 'the theory should pass some new, and severe, tests' (1965, p. 292). This third requirement is the requirement of 'empirical success' and it simply says that some of the independently testable implications required by the second requirement actually gets corroborated by the empirical evidence. Popper says,

> I contend that further progress in science would become impossible if we did not reasonably often manage to meet the third requirement; thus if the progress of science is to continue, and its rationality not to decline, we need not only successful refutations, but also positive successes. We must, that is, manage reasonably often to produce theories that entail new predictions, especially predictions of new effects, new testable consequences, suggested by the new theory and never thought of before. (1965, p. 243)

Popper argues that this third requirement may 'sound strange' (1965, p. 247) because it makes the evidence which counts in the progress of science a 'partly historical idea' (1965, p. 248). Nevertheless this third requirement is indispensable to a science which seeks theories of ever-increasing verisimilitude.

Let us now recap this rather long Popperian tale. Though Popper has always been sympathetic to scientific realism, in *LSD* he advocated a falsificationist methodology without connecting it to truth.[9] Later Popper not only accepted truth as the aim of science but proposed his own theory of verisimilitude to better characterize that aim. Since verisimilitude was consistent with his falsificationist methodology, it allowed Popper to solve his earlier problem of a methodology independent of truth. But this solution raised new problems; once verisimilitude was the stated aim the methodology needed to be expanded. Since science progressed and approached truth by means of deeper and deeper theories, certain kinds of corroborations were of special significance. Corroborations of predictions which had 'never been thought of before' (or what later came to be called 'novel facts') became necessary for progress towards theories of ever greater verisimilitude. This final position became part of the inherited problem situation of Imre Lakatos, to which I now turn.

2 LAKATOS WHIFFS THE PROBLEM

The philosophical problem situation of Imre Lakatos was in part based on the above Popperian view of scientific progress and in part based on more historical influences such as Thomas Kuhn. Lakatos's response to these influences generated the Lakatosian rebellion which attacked the Popperian conventional wisdom (presented above) at two separate levels: the methodological level and the epistemological level. The former, the methodological criticism, is the best known part of Lakatos's work and it resulted in his own methodology, the methodology of scientific research programs (MSRP).[10] The second part of the attack, the epistemological criticism, is probably less well known and it resulted in Lakatos's plea to Popper for a 'whiff of inductivism'.[11]

Regarding the *methodological* issues Lakatos saw a number of problems with the Popperian conventional wisdom. Probably most important was the fact (apparent from contemporary history of science) that Popper's falsificationist methodology was at odds with the actual practice of great science. Lakatos advocated a 'quasi-empirical approach' (1978, p. 153); he argued that a methodology 'must reconstruct the acknowledgedly best games and the most esteemed gambits as "scientific"; if it fails to do so, it has to be rejected' (1978, p. 145). Lakatos, among others, argued that Popper's methodology failed in this regard. In particular, Popperian falsificationism fails because it 'stubbornly overestimates the immediate striking force of purely negative criticism' (1978, p. 148). For Lakatos, actual great science has succeeded in a sea of inconsistencies and anomalies (refutations) and any adequate methodology must recognize (and rationalize) these facts. Many parts of Lakatos's MSRP, particularly the concepts of a research 'program', the hard core, and the protective belts, were attempts to accommodate the actual history of science and avoid these perceived problems in Popper's methodology.

One methodological issue where Lakatos did *not* disagree with Popper was on the importance of predicting novel facts;[12] for Lakatos's MSRP, as for the Popperian view which he inherited,[13] progress requires novelty. On the same page (1970, p. 118) where Lakatos presents his famous definitions of 'theoretical progress' ('predicts some novel, hitherto unexpected fact') and 'empirical progress' ('this excess empirical content is also corroborated') he provided the following footnote, accentuating the importance of novelty.

> If I already know P_1: 'Swan A is white', P_w: 'All swans are white' represents no progress, because it may only lead to the discovery of such further similar facts as P_2: 'Swan B is white.' So-called 'empirical generalizations' constitute no progress. A *new* fact must be improbable or even impossible in the light of previous knowledge. (1970, p. 118h)[14]

Thus, while Lakatos's MSRP was critical of much of the Popperian methodology, one aspect, the importance of novel facts, was not only accepted, but actually emphasized more than by Popper. For Popper novel facts are a bit of an add-on, since much of the empirical burden is still carried by falsification and refutation. For Lakatos, on the other hand, who wanted to decrease (to zero) the role of negative evidence and refutation, the entire burden of scientific progress is left on the shoulders of novel facts. Lakatos's solution to his perceived methodological problem situation left him with a methodological position where 'the only observational phenomena which have *any* bearing on the assessment of a research programme are those which are "novel" ' (Gardner, 1982, p. 1).

In the realm of *epistemology*, as opposed to the realm of methodology, Lakatos is more critical of his progenitor. His criticism is based on the fact that Popper, despite his claim that verisimilitude is the aim of science, remains deeply sceptical about the certainty or guaranteed reliability of our knowledge. Popper has argued consistently that, while we may (and should) search for truth, we never know when we find it; 'even if we hit upon a true theory, we shall as a rule be merely guessing, and it may well be impossible for us to know that it *is* true' (1965, p. 225). For Popper there are 'no general criteria by which we can

recognize truth' (1965, p. 226). Not only can we not recognize truth, this lack of epistemological confidence extends even to our ability to know that we are getting closer to the truth. 'My defence of the legitimacy of the idea of verisimilitude has sometimes been grossly misunderstood. In order to avoid these misunderstandings it is advisable to keep in mind my view that not only are all theories conjectural, but also all appraisals of theories, including comparisons of theories from the point of view of verisimilitude' (1973, p. 58). 'We cannot justify our theories, or the belief that they are true; nor can we justify the belief that they are near to the truth' (1983, p. 61).

Popper does believe that we have rational arguments for our theoretical preferences; we have rational reasons to suspect or conjecture that one theory has more verisimilitude, but we do not *know*. Recall the earlier quotation, where Popper was relating verisimilitude to boldness; he says, 'we have reason to suspect, or to conjecture, that the stronger theory has no greater falsity content than its weaker predecessor' (1973, p. 53): notice 'reason to suspect', not 'we know'. Later in the same paper Popper says, 'But if it passes all these tests then we may have good reason to conjecture that our theory, which as we know has greater truth content than its predecessor, may have no greater falsity content' (1973, p. 81). We 'know' about truth content because that is a logical relation, but when it comes to the question of less falsity content, required for verisimilitude, Popper says only that 'we may have good reason to conjecture that' it 'may have no greater falsity content'.

Lakatos considers this lack of epistemological bite to be a major difficulty for the Popperian position. Lakatos's argument is that merely defining the aim of science as verisimilitude is not enough; one needs to be able to identify this progress when it occurs. It is not enough to say that novel corroborations 'may be a good reason to conjecture' that we are getting closer to the truth: we need a firm connection between the two; the game of science needs a 'truly epistemological dimension' (Zahar, 1983, p. 167). Lakatos gives Popper credit for his Tarskian turn, for introducing verisimilitude and promoting it as the aim of science; it is just that Popper did not go far enough. Lakatos argues that, after Popper modified his views, it 'became possible for the first time to define progress even for a sequence of false theories: such a sequence constitutes progress, if its truth-content, or, as Popper proposed, its verisimilitude (truth-content minus falsity-content) increases. But this is not enough: we have to *recognize* progress' (Lakatos, 1978, p. 156). As Watkins characterizes the problem, 'it is one thing to have an aim for science and another to have a method by which we can, in favourable circumstances, actually pick out the hypothesis that best satisfies that aim' (1984, p. 279). For Lakatos, 'Popper has not fully exploited the possibilities opened up by his Tarskian turn' (1978, p. 159); he provided a solution in the form of his theory of verisimilitude, but then 'shrank back' (ibid.) from actually solving the epistemological problem associated with his methodology.

Lakatos's solution to this epistemological problem is a 'plea to Popper for a whiff of "inductivism" ' (ibid.). He recommends 'an inductive principle which connects realist metaphysics with methodological appraisals, verisimilitude with corroboration, which reinterprets the rules of the 'scientific game' as a-conjectural-theory about the *signs* of the *growth of knowledge*, that is, about the signs of *growing verisimilitude of our scientific theories*' (1978, p. 156, emphasis in original). Lakatos argues that, without such an inductive principle, any methodological proposals are mere conventions without epistemological bite:

> Without this principle Popper's 'corroborations' or 'refutations' and my 'progress' or 'degeneration', would remain mere honorific titles awarded in a pure game. With a *positive* solution of the problem of induction, however thin, methodological theories of demarcation can be turned from arbitrary conventions into rational metaphysics. (1978, p. 165)

In summary, Lakatos's solution was to modify Popper's methodology: play down falsification, emphasize novel facts and add both historical and heuristic dimensions to appraisal, but basically to take as given Popper's aim and his characterization of verisimilitude. On the epistemological side Lakatos was more radical: he pleaded for a 'whiff of inductivism', a positive solution to the problem of induction which would connect the purported methodological rules (his or Popper's) with verisimilitude as an aim. This brings the discussion to developments in post-verisimilitude Popperian philosophy of science.

3 THE TALE UNRAVELS

So we have heard a long Popperian tale with a Lakatosian twist on the end. Where does the story go from Lakatos? Unfortunately, beyond Lakatos, the story starts to unravel. I will not provide a complete discussion of these events, but let me at least review the previous discussion and comment on the evolution of the main parts of the story.

First, on the final question discussed above, Lakatos's plea for a whiff of inductivism, there is relatively little to say. Lakatos's proposal, however guarded his presentation, is basically a plea for a justification

of the method of science in terms of the truth likeness of the theories the method produces. However recurrent such pleas have been in the history of philosophy it is doubtful whether such a justification will soon be upon us. Some neo-Popperians, Watkins (1984) in particular, argue that Lakatos's concern is legitimate but that the problem is not to be solved in Lakatos's way, by connecting corroborated appraisals to verisimilitude appraisals with an inductive principle (1984, pp. 282–8). Rather, Watkins rejects verisimilitude as the aim of science and proposes his own adequacy requirements (1984, p. 124) for any such aim. Given these broader adequacy requirements, Watkins argues that corroborations are in fact sufficient for the aim of science (1984, p. 306); this provides a possible solution to Lakatos's problem without actually responding to his 'plea'.

Clearly the most important factor in the unravelling of the later Popperian position has been the 'admitted failure' (Popper, 1983, p. xxxv) of Popper's definition of verisimilitude. This definition, which formed the backbone of Popper's methodological proposals regarding progress and novelty, has encountered a number of serious difficulties. Hacking, in a discussion of Lakatos's work, refers to verisimilitude as 'Popper's hokum' (1979, p. 387), while for Agassi it is simply 'a booboo' (1988, p. 473) and, for advocates of alternative non-Popperian interpretations of verisimilitude such as Oddie, Popper's work on verisimilitude has simply produced 'embarrassing results' (1986, p. 164). The recognition of these difficulties was initiated in two papers, Miller (1974) and Tichy (1974), which demonstrated that no false theory ever has more verisimilitude than any other false theory. Since being able to make sense of statements like 'Newton's theory is closer to the truth of Kepler's theory' was one of the most important reasons for the verisimilitude concept (Popper, 1973, p. 59) these initial negative results represented a serious setback for the program. Following these initial papers the critical literature has continued to such an extent that Popper now fully admits the failure of the project:

> A new definition is of interest only if it strengthens a theory. I thought that I could do this with my theory of the aims of science: the theory that science aims at truth *and* the solving of problems of explanation, that is, at theories of greater explanatory power, greater content, and greater testability. The hope further to strengthen this theory of the aims of science by the definition of verisimilitude in terms of truth and of content was, unfortunately, vain. (1983, p. xxxvi)

Now, while almost everyone admits that Popper's definition of verisimilitude has severe problems, opinions differ regarding the *importance* of these difficulties. Some, such as Zahar, consider these results to be 'a severe setback for fallibilist realism as a whole' (1983, p. 167). Others, such as Watkins (1984), discussed briefly above, simply reject verisimilitude as the aim of science and build their own neo-Popperian program without it. Popper himself, of course, does not see the difficulties of his technical/quantitative definition of verisimilitude to be at all significant. He says:

> The widely held view that scrapping this definition weakens my theory is completely baseless. I may add that I accepted the criticism of my definition within minutes of its presentation, wondering why I had not seen the mistake before; but nobody has ever shown that my theory of knowledge, which I developed at least as early as 1933 and which has been growing lustily ever since and which is much used by working scientists, is shaken in the least by this unfortunate mistaken definition, or why the idea of verisimilitude (which is not an essential part of my theory) should not be used further within my theory as an undefined concept. (1983, pp. xxxvi–xxxvii)

Such a disclaimer is relatively easy for Popper in the 1980s, since his latest turn has been towards 'critical rationalism'. This is the view, often associated with W.W. Bartley III, that rationally accepted propositions are those which have been critically discussed: that rationality simply means openness to criticism.[15]

Despite these Popperian disclaimers about the importance of verisimilitude, there are at least two places in Popperian philosophy where the problems of verisimilitude clearly have important (and negative) ramifications. Both of these places, by the way, are important in the relationship between Popperian philosophy and economic methodology. The first place where the failure of verisimilitude matters is in Popperian philosophy of social science, his so-called 'situational analysis' view of social science explanations. The second place where verisimilitude matters is in Lakatosian methodology. Let us consider Popper's philosophy of social science first.

Popper's 'situational analysis' approach to social science requires a 'rationality principle' which serves as the law in situational analysis explanations.[16] This rationality principle is 'an integral part of every, or nearly every, testable social theory' (Popper, 1985, p. 361), and yet 'the rationality principle is false' (Popper, 1985, p. 361). Assuming that the aim of science is truth, and assuming (as Popper clearly does) that there are no fundamental methodological differences between social and physical science, the falsity of the rationality principle represents a real difficulty. But the solution to the problem is easy: verisimilitude saves the day; if verisimilitude is the aim of science and if one false theory

can have more verisimilitude than another false theory, then the notion of progress towards truth need not be lost when theories involve the rationality principle. 'Ultimately, the idea of verisimilitude is most important in cases where we know that we have to work with theories which are *at best* approximations – that is to say, theories of which we actually know that they cannot be true. (This is often the case in the social sciences.)' (Popper, 1965, p. 235.)

> 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. (1976a, p. 103)

Notice that Popper said that verisimilitude was 'indispensable' for social science using the rationality principle.

Now consider Lakatos and the MSRP. Recall Lakatos's problem situation and how he came to advocate novel facts as the sole criterion for progress in science. Popper, in attempting to improve his basic *LSD* methodology so that it was more in agreement with verisimilitude as the aim of science (attempting to capture the notion of ever deeper theories), proposed novel facts as a particularly significant form of corroboration. Lakatos, picking up Popper's suggestion, abandoned falsificationism entirely, and elevated novel facts to the position of sole criterion for scientific progress. If Popper had never taken the Tarskian turn, never advocated verisimilitude as the aim of science, the notions of novelty and independent testability would be given only a minor role in Popperian methodology. Lakatos would have needed either to present a more traditional Popperian (that is, falsificationist) methodology, or to move entirely away from Popper, take a big whiff of inductivism, and simply rate research programs on the basis of how well they were confirmed by the evidence. As it worked out Lakatos could advocate novel facts as the sole criterion for progress in science and stay attached (however insecurely) to the Popperian tradition. But this linkage, so important to Lakatos, is, in the light of the entire Popperian story, quite thin and frail. If what is known about verisimilitude now had been known to Lakatos there is a very real possibility that the MSRP would characterize scientific progress in an entirely different way.

4 ECONOMICS: FINALLY

This Popperian tale can give us a number of important insights into contemporary economic methodology. First consider Lakatos and the MSRP. Economics has been fertile ground for application of Lakatos's MSRP. Because of Lakatos's novel fact requirement for progress, many of these economic applications have amounted to novel fact hunts. And, in particular, since there are now many different definitions of novel facts in the literature, these papers can become mired in semantic debates which provide little insight into either economics or philosophy.[17] More useful Lakatosian notions like the metaphysical hard core, the programmatic nature of scientific research, and the positive and negative heuristics, often get neglected in the rush to find novel facts. In the light of the above Popperian story, novel facts have a minor role in the overall Popperian approach to the philosophy of science; they were introduced to help forge a link between methodology and verisimilitude which now seems either futile or unnecessary (depending on how one views the final evolution of the Popperian position). If Lakatos is going to continue to play a role in economic methodology, then, in the light of the above story, we should re-evaluate the various roles of the different parts of his position.

Popperian philosophy of social science and situational analysis is another area where the developments in Popperian philosophy (particularly verisimilitude) have an impact on economic methodology. Microeconomic explanations are a special case of situational analysis using the rationality principle. Situational analysis itself is a special case of so-called 'folk psychology': explanations of behaviour in terms of the beliefs, desires and intentions of the relevant agent. Recently folk psychology (and therefore, by implication, situational analysis and microeconomics) have come under attack by philosophers who argue that intentional explanations are not legitimate scientific explanations. While most of these critics (and defenders as well) are concerned with folk psychology in general, Rosenberg (1981, 1988) has specifically indicted economics in this regard. Now the success of Popper's verisimilitude program would have provided at least a partial realist defence of such explanations (and thereby microeconomics), but, as it is, with the failure of verisimilitude, Popperians are left with only a critical-rationalism-as-a-default sort of argument in favour of situational analysis. This issue, the question of how (whether) it is possible to regard intentional explanations as valid explanations, is an important issue in the philosophy of economics. One possible defence, the one which seemed to be envisioned by Popper, has apparently collapsed with verisimilitude.

Finally there are the important questions of instrumentalism and essentialism. Let me consider instrumentalism first. Many, perhaps most, practising economists think of their work exclusively in instrumentalist terms. There does not seem to be any one single explanation for this instrumentalist preference. Some of the things which might reasonably be suggested for it are: the nature of econometrics and the ease with which it allows prediction from past trends, the demand for economic predictions from government agencies and private firms, and (possibly) the influence of Friedman's instrumentalist methodology. Of course Popper has always been an outspoken critic of instrumentalism.[18] His major criticism is that instrumentalism does not distinguish between science and mere technology; instrumentalism makes scientific theories 'nothing but computational rules' (Popper, 1983, p. 113) and, according to the instrumentalist, 'scientific theories cannot be real discoveries: they are gadgets. Science is an activity of gadget-making-glorified plumbing' (Popper, 1983, p. 122). Despite arguments such as these and a sustained anti-instrumentalist rhetoric, Popper's fallibilist philosophy of science hovers very close to instrumentalism: the empirical basis of science is accepted by convention, the methodological rules themselves are merely conventions, and all knowledge is conjectural (among other things). Verisimilitude temporarily seemed to widen the gap between Popper and instrumentalism, but the failure of the verisimilitude program has again moved them closer. In Lakatos where falsification makes no contribution to science and progress occurs only through a special type of corroboration, the instrumentalist flavour seems to be even stronger.

The fact of instrumentalist practice in economics and the anti-instrumentalism of Popperian philosophy generates a tension in Popperian economic methodology. It is clearly the case that economic methodologists *need* to make sense of instrumentalism in economics, but the dominance of Popperian ideas and the negative evaluation of instrumentalism in Popperian philosophy have focused the attention of economic methodologists away from serious consideration of this instrumentalist practice.

On the other hand, it is also true that Popperian philosophy is fundamentally anti-essentialist. Not only did the Popperian influence in economic methodology contribute to the neglect of instrumentalist practice, it also seems to have biased the discussion against essentialism. While much of contemporary economic practice may appear to be instrumentalist, the founders of modern microeconomics, Menger in particular, were committed to essentialist realism.[19] Of course the same can also be said of the Marxian tradition in economics and, on a broader (non-Popperian) definition of essentialism, maybe much of modern economics as well. It is very likely that Popper's influence has simultaneously turned our attention away from *both* how the founders of the profession perceived the discipline *and* how modern economics is practised.

None of these problems might matter too much if Popper's program of conjectural realism were on firm foundations, but it is not, and it does not appear that it will find such foundations in the near future. The failure of verisimilitude is certainly an important part of these difficulties. Lakatos's MSRP, however pregnant it might be with interesting ideas, is also unable to provide the requisite forward thrust. Economic methodology has been dominated by Popperian ideas for quite a number of years and, in the light of the entire story, that domination may have lasted quite long enough. There are still many issues in economic methodology which could benefit from the Popperian light, but in using it we should keep two points in mind: (a) the history of how that particular light came to shine the way it does; (b) there are other lights.

NOTES

1. Even before I start this tale it should be noted that many 'Popperians' will find my argument to be too much of a Lakatosian version (or possibly adulteration) of the story. In fact my story is very close to Lakatos's version but the reason is not that I am unfamiliar with the alternatives; the reason is that I think the Lakatosian version is basically correct. The story which follows seems to rationalize the value-impregnated history of Popperian philosophy much better than any of the other stories available in the literature.
2. English translation, *The Logic of Scientific Discovery* (1968): hereafter abbreviated *LSD*.
3. Popper (1965; 1973, pp. 44–84, pp. 319–35; 1976b, p. 150; 1983, pp. 24–7) for instance.
4. Popper (1965, pp. 223–6; 1968, p. 274n; 1973, pp. 319–21; 1976b, pp. 98–9).
5. Popper (1965, p. 235; 1973, pp. 56–7) and Koertge (1979, p. 234).
6. Popper's primary discussion of verisimilitude is contained in (1973) ch. 2 (esp. pp. 44–60) and ch. 9 (esp. pp. 329–35) and (1965) ch. 10 (esp. pp. 231–5) and Addenda (esp. pp. 391–402). The topic is surveyed in a number of works by other authors: Koertge (1979, esp. pp. 234–8) and Watkins (1984, esp. pp. 279–88) for example.
7. The interested reader may consult Popper (1973, pp. 48–50) for a more detailed discussion.
8. Much of the above Popperian tale could be couched in 'ad hoc' terms, that is, in terms of finding theories which are non-ad hoc. In this discussion I will try to skirt the ad hocness issue as much as possible since I have discussed the topic in detail elsewhere (Hands, 1988).

9. In fact in *LSD* (p. 276) Popper said: 'I think that it would be far from "*useful*" to identify the concept of corroboration with that of truth.' (Emphasis in original.)
10. Lakatos (1970).
11. Lakatos (1978).
12. An extensive literature has developed around Lakatos's use of 'novel fact'. Did his concept differ from Popper's? Did he have more than one concept? Can his definition of novel fact be modified so that the MSRP will be able to rationalize a greater portion of the actual history of science? I will try to avoid these controversies by simply arguing that Lakatos considered the prediction of novel facts to be necessary for progress in science, and that Lakatos's notion of novel fact was 'something' like Popper's. The interested reader who would like to pursue the question of how the term 'novel fact' is properly defined should examine some of the extensive literature on the topic (for instance, Carrier, 1988; Gardner, 1982; Musgrave, 1974; Worrall, 1978).
13. As we will see in the next section this view is not necessarily the same as the view of the most recent Popper. As my references in the last few paragraphs of the previous section indicate, the best example of the view Lakatos inherited is 'Truth, Rationality, and the Growth of Knowledge' (ch. 10 of Popper, 1965). It is probably fair to say that this paper is the best presentation of the 'middle' (verisimilitude) Popper, as opposed to the 'early/*LSD*' (don't talk about truth) Popper, or the 'final' (critical rationalist) Popper discussed briefly in the next section.
14. Later (1970, pp. 155–7), in his discussion of 'budding research programmes', Lakatos states that factual novelty may not be 'immediately ascertainable', but he never alters his view of the fundamental role of novel facts in scientific progress.
15. This is not the place to become involved in this latest Popperian turn but I cannot resist a passing comment. I fundamentally agree with Lakatos on this topic: 'the basic weakness of this position is its emptiness. There is not much point in affirming the criticizability of any position we hold without concretely specifying the forms such criticism might take' (1978, p. 144n). Also see Nola (1987).
16. Popper's best statements of this view are (1976a) and (1985). For summaries by other authors see Hands (1985b, 1989a), Koertge (1974, 1975), or Watkins (1970).
17. I may have been partially responsible for some of these debates myself (Hands, 1985a).
18. See Popper (1965, ch. 3) and (1983, pp. 112–31) for instance.
19. See Mäki (1986) and (1989).

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COMMENT ON HANDS

Bert Hamminga

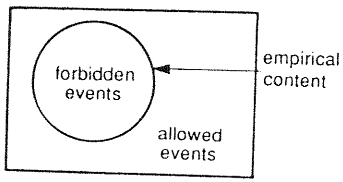
Wade Hands is discouraged in the hunt for novel facts when studying economic research programs. By way of consolation, he argues that 'novelty' is, after all, not so fundamental in the history of Popper–Lakatos methodology. But when he comes to listing the useful part of the Lakatosian notions, he not only omits 'novel facts', he also omits 'content', 'excess content' and 'corroborated excess content'. So Wade Hands' discouragement goes beyond the realm of 'novelty'.

I have no difficulty in understanding this. But I wish to make some attempt to apply the 'content'-related Lakatosian concepts to economic research programs. Let me try to do so in a more precise way than has, I suspect, hitherto been done.

1 The Meaning of Excess Content

Figure 1 shows what a theory does to events. Some events are forbidden; they are inside the circle. Some are allowed, they are outside the circle. The allowed events, if we are to believe Popper and Lakatos, are uninteresting, the forbidden events are the only ones that are interesting, because, if a forbidden event occurs, your theory is falsified. The empirical content is associated with the set of forbidden events.[1]

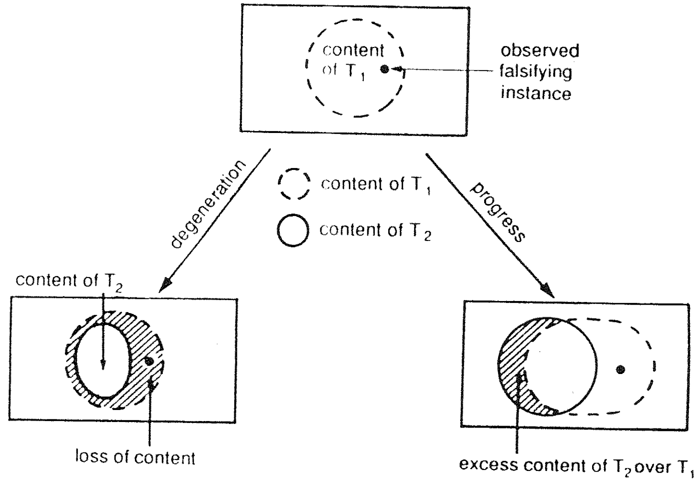
Figure 1 Theories and events



The path of degeneration is depicted by the path in Figure 2. If, after observing a falsifying event of T_1 you make a shift towards a T_2 that simply narrows down content so as to allow for the event that falsifies the old theory T_1, your research program will degenerate. This path

yields ever smaller circles and ends with empty content, with not forbidding any event, with not saying anything.

Figure 2 Progress and degeneration



Now, whatever T_2 you choose to shift towards, some loss of content is unavoidable: the new T_2 has to allow for the observed event that falsified T_1. But a problem shift to some T_2 is 'progressive' only if, by the same stroke, T_2 forbids some events that were allowed by the old theory T_1. These events are called the *excess content* of T_2 over T_1 (shaded in the box at lower right in Figure 2).

This excess content is *corroborated* if, despite severe attempts, we fail to observe some of the events it contains (forbids). And 'corroborated excess content' means the same as 'a (discovered) *novel fact*'. According to Lakatos (please note!) the discovery of a *novel fact* is not, as in ordinary English, the novel observation of an event, but the *failure, despite severe attempts, to observe a newly forbidden event*.[2]

These are the meanings of 'content', 'excess content' (or, what is the same, 'predicted novel fact') and 'corroborated excess content' (or, what is the same, 'the discovery of a novel fact'). Let us turn to their measurement.

2 The Measurement of Excess Content

Let T_1 be the theory that Samuelson presents as an 'Illustrative tax problem' on p. 14, *Foundations of Economic Analysis* (Samuelson, 1947). T_1 consists of (1) and (2):

$$\pi := xp(x) - C(x) - tx \tag{1}$$

where π is profit, $xp(x)$ is called 'total revenue in function of output', $C(x)$ is total production cost and t is a tax rate on output.

$$\frac{\partial \pi\,(x,t)}{\partial x} = 0\,, \quad \frac{\partial^2 \pi(x,t)}{\partial x^2} < 0 \tag{2}$$

Samuelson proves the following theorem:

$$T_1 \rightarrow \left(\frac{\partial x^o}{\partial t}\right) < 0 \tag{3}$$

Which events are forbidden by T_1, and which ones allowed? Before answering that question we have to determine the set of events *relevant* to the theory.[3] Profit is a function of x and t *only* (prices and costs are functions of x), so we have only two variables t and x, which can go up (+), remain unchanged (0), and go down (−). The list of logically possible, relevant events evidently reads as in Table 1.

The theory implies a downward effect of taxes on equilibrium output. Since there are no other variables, if taxes go up (events 1, 2, 3), equilibrium output should go down (event 3). Event 3 is allowed, events 1 and 2 are forbidden. They are in the 'content' of T_1, Lakatos and Popper would say. Similarly 4 and 6 are in the content, because t is, according to T_1, the *only* variable affecting equilibrium output. If t remains unchanged, x^o should remain unchanged (event 5 is allowed) and any change of x^o in this case (events 4 and 6) is forbidden.[4] Events 7, 8 and 9 now speak for themselves. The content of T_1, what is in the circle of Figure 1, so to speak, is the set of events 1, 2, 4, 6, 8, 9.

Kinship exists between these Popper–Lakatos ideas and Samuelson's concept of an 'operationally meaningful theorem'. Samuelson calls theorems like the right-hand side of (3) operationally meaningful

Table 1 T_1 *and 'events'*

	Set of events		
	tax t	*equilibrium output* x^o	*Content of* T_1
1	+	+	forbidden
2	+	0	forbidden
3	+	−	allowed
4	0	+	forbidden!
5	0	0	allowed
6	0	−	forbidden
7	−	+	allowed
8	−	0	forbidden
9	−	−	forbidden

because they 'could conceivably be refuted, if only under ideal conditions' (Samuelson, 1947, p. 4). That is what Popper and Lakatos mean by 'having non-empty empirical content'.

Now let us study, by way of exercise, a hypothetical falsification and subsequent shift to a new theory T_2. Suppose we observe event 4 (this is symbolized by the exclamation mark). Now, T_1 is falsified. Any new T_2 that does nothing but omit event 4 from the content would lead us into degeneration. T_2 should at least 'newly' forbid some event allowed by the old T_1! So a theory T_2, say, giving in by allowing event 4 and at the same stroke forbidding, say, event 3, that was allowed by T_1, would mean 'progress', Popper and Lakatos would say. But there is another way that is more typical and illustrative, and that is to introduce a theory T_2 that contains an additional variable that is used to *explain* why we have observed this falsifying event.

Let us consider a shift towards some theory T_2 that stops treating price as a function $p(x)$ of output, and turns p into an exogenously given variable. The additional variable p can of course go up, down and remain unchanged, and this triples our list of relevant events (Table 2).

To event 1 of Table 1 (t and x both rising) there correspond events 1, 2 and 3 of Table 2 (p going up, remaining unchanged, or going down) etc. We now have tripled the size of the boxes of Figures 1 and 2, so to speak. This does not affect the content of T_1, of course. To every one forbidden row in Table 1, there now correspond three forbidden rows in Table 2. But where do we place our exclamation mark, symbolizing

		t	x^o	p	content T_1	content T_2	
	1	+	+	+	f		loss
1	2	+	+	0	f	f	
	3	+	+	−	f	f	
	4	+	0	+	f		loss
2	5	+	0	0	f	f	
	6	+	0	−	f	f	
	7	+	−	+			
3	8	+	−	0			
	9	+	−	−			
	10	0	+	+	f!		loss
4	11	0	+	0	f	f	
	12	0	+	−	f	f	
	13	0	0	+		f	gain (excess content)
5	14	0	0	0			
	15	0	0	−		f	gain (excess content)
	16	0	−	+	f	f	
6	17	0	−	0	f	f	
	18	0	−	−	f		loss
	19	−	+	+			
7	20	−	+	0			
	21	−	+	−			
	22	−	0	+	f	f	
8	23	−	0	0	f	f	
	24	−	0	−	f		loss
	25	−	−	+	f	f	
9	26	−	−	0	f	f	
	27	−	−	−	f		loss

the falsifying event? Our old falsifying event 4 in Table 1 corresponds to events 10, 11 and 12 in Table 2. Now we have to go back to our falsifying event to see what happened to prices! Suppose it is observed that they went up. So our exclamation mark should be on row 10. T_2 may still forbid events 11 and 12; they were not yet observed. But it should allow for event 10.

$$T_2 \rightarrow \left(\frac{\partial x^o}{\partial t}\right) < 0 \text{ and} \left(\frac{\partial x^o}{\partial p}\right) > 0$$

That is, taxes keep having a downward effect on equilibrium output, and prices now have an upward effect. Event 1 of Table 2 is allowed by T_2, because tax and price changes work in opposite directions and T_2 does not say which effect will dominate. But events 2 and 3 of Table 2 are forbidden by T_2 as they were by T_1. Likewise we lose content by newly allowing events 4, 10, 18, 24 and 27. But we gain content too! Events 13, 14 and 15, all allowed by T_1 because they correspond to event 5 of Table 1, are not *all* allowed by T_2. With price + and tax change 0, equilibrium output should go up (event 10) and therefore event 13 is forbidden by T_2. Similarly event 15 is forbidden by T_2. These two events, 13 and 15, form the *excess content of T_2 over T_1*, symbolized by the shaded area in the box at bottom right in Figure 2.

Given a falsification of T_1 by an event of type 4, from the perspective of Table 1, that turned out to be event 10 from the new perspective of Table 2, T_2 does all that Lakatos requires of a theoretically progressive problem shift: it removes event 10 from the content, at the same stroke adding events 13 and 15.[5] The problem shift is also empirically progressive if this excess content is also 'corroborated'; that is, if, despite severe attempts, we fail to observe the events 13 and 15. 'Severe attempts' means, here, collecting observational reports on events with rising prices and no tax change (events 10, 13, 16), and *never* finding an event 13 (always finding 10, since 16 is also forbidden). *Corroboration of the excess content of T_2 over T_1 means failure to observe event 13* and, similarly, failure to observe event 15.[6]

3 Conclusion

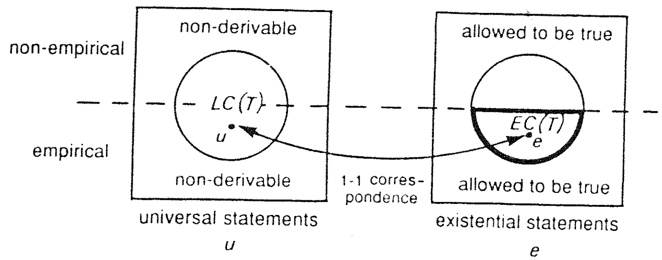
These exercises in economic Lakatosianism should suffice to make clear what blueprint of economics it implies. *Does* economics conform the blueprint? *Should* it do so? I do not know whether it *should*, so I am happy not to be in power. But, contrary to Wade Hands, I find these purely empirical questions interesting and not entirely useless: *do successive comparative static theories in economic research programs have 'excess content', in the specific logical meaning given to this notion by Popper and Lakatos? Is some of this excess content 'corroborated'?*

Note that *any two* comparative static theories can be compared in the above manner, *no matter what variables they contain!* Simply lump all variables together to construct your table of relevant events. Note that this procedure obeys nice mathematical laws, from the realm of combinatorial theory.

My present bet for the answer is that each shift (say in the history of demand theory or in the history of the theory of international trade, job search theory and so on) involves heavy losses in 'content', very small gains, some falsification and no 'corroboration'.[7] This does not mean raising your eyebrows about economics.[8] But any outcome whatever would mean some growth of knowledge about it. And it would throw some novel light upon Lakatos. Why not do some calculation!

Notes

1. Logical note: if you scrutinize the Popper–Lakatos texts on 'content', the definitions chosen turn out to be remarkably consistent and constant over time: theories T are universal statements. The 'Logical content of T' ($LC(T)$) is the consequence class of T, that is the set of universal statements u described by: $LC(T) := \{u|T \rightarrow u\}$, where '$\rightarrow$' means 'logically implies'. $LC(T)$ is the set symbolized by what is in the circle in the left-hand box below.



Out of the set of universal statements u, you can make the isomorphic set of existential statements e by taking, for every universal statement u in the left-hand box, a statement $e = \nvdash u$ ('$\nvdash$' means 'not'). The statement in the set $\{e|e = \nvdash u$ and $u \in LC(T)\}$ are those inconsistent with T. They are in the mirror-image circle of the right-hand box. But only part of these existential statements can be verified by observable events. This part is the 'empirical content of T' ($EC(T)$). $EC(T)$ is defined as a set of existential statements e: $EC(T) := \{e|e \rightarrow \nvdash T$ and $0(e)\}$, where the predicate '0' says that e is verifiable by observable events (though, if you believe T, you hope such events turn out never to occur, despite severe attempts to observe them; in other words, you hope that your T is, or will be, corroborated).

In the main text I only use the lower half (the empirical part) of the right-hand box, because that is where it all happens: the non-empirical upper half is methodologically irrelevant according to Popper and Lakatos, and the left-hand box is the exact mirror-image of the right-hand box, and hence superfluous. For this summary I used Popper (1934) (1963) (1972) and Lakatos (1970). The references are so numerous that I simply refer to the indexes of the books and I add that all passages use the concepts with complete consistency. My set-theoretical illustrations here are inspired by structuralism, especially Theo Kuipers's approach to verisimilitude (Kuipers, 1982, 1987).

2. Here is my evidence for this claim: for *corroboration* of T as the failure, despite severe attempts, to observe events forbidden by T, see Popper (1934), section 82 and *passim* in Popper (1934) (1963) (1972), as well as Lakatos (1970). Lakatos equates 'having excess empirical content' with the 'prediction of a novel fact' as follows: 'Let us say that . . . a series of theories is theoretically progressive (or "constitutes a theoretically progressive problemshift") if each new theory has some excess empirical content over its predecessor, that is [*sic!*], if it predicts some novel, hitherto unexpected fact' (Lakatos, 1970, p. 118). And Lakatos equates 'corroborated excess content' with 'the actual discovery of some new fact' as follows: 'Let us say that a theoretically progressive series of theories is also empirically progressive (or "constitutes an empirically progressive problemshift") if some of this excess content is also corroborated, that is [*sic!*], if each new theory leads us to the actual discovery of some new fact' (Lakatos, 1970, p. 118). These quotations are only examples. As long as we stick to the logic of the Popper–Lakatos definitions, the definition of novel fact as 'failure to observe' is crystal clear. Lakatos supplies an example of novel facts in research programs: Einstein's theory newly forbidding 'transmission of light along straight lines near large masses', and eclipse experiments actually failing to observe such linear transmission, thereby 'corroborating' Einstein's theory (Lakatos, 1970, p. 124). A large part of the 'economic applications' of Lakatos seems to have overlooked Lakatos's non-common use of 'novel fact'. This may partly have been caused by some ambiguities in Lakatos (especially 1971), where he sets out to show that his Methodology of Scientific Research Programs is itself 'progressive' in relation to older Popperian methodologies. He does so by pointing out that MSRP *allows* more types of behaviour by scientists, which, under his logical definitions, would count, not as a gain, but as *loss* of content. (See also M. Pera, 'Methodological Sophisticationism: A Degenerating Project', in Gavroglu, K. *et al.*, 1989.)
3. Structuralists call this the set of *partial potential models* (M_{pp}).
4. Some might object here that 4 and 6 may be allowed in case of violation of a *ceteris paribus* clause implicit in the theory. It is suggested that they read '*ceteris paribus* forbidden' instead of 'forbidden', and should of course do so too for events 1, 2, 8 and 9, because, if we assume that Samuelson's T_1 contains an implicit *ceteris paribus* clause *which is not claimed to hold*, we have no content whatsoever and every event is allowed.
5. Note that, if T_2, instead of T_1, was the *old* theory and if, say, event 13 had been observed, then T_1 would constitute a theoretically progressive problem shift over T_2 (T_1 has even more excess content over T_2, if we simply count events, than T_2 has over T_1)! This shows how essential 'history', that is, the observations made on specific points in time, are to MSRP.
6. If you wish to do an exercise in order to check whether you have mastered this subject, I calculated for Samuelson's famous analysis of the Keynesian system Case I (Samuelson, 1947, pp. 278–80) a set of 729 events, 454 of which are, after imposing all restrictions, in the content of the theory.
7. My bet is independent of what I wrote (1984), esp. pp. 303–14, where I dealt with some mathematical and formal econometric objections to considering 'content' as a concept relevant to economic research programs. And it is also independent of my observations (1983) on 'weakening-of-conditions related' strategies of theorizing (see, for Lakatos, esp. pp. 119–23).
8. My position here is that raising eyebrows is *never* the methodologist's job (see my (1983), pp. 1–2, 134–7 and my (1990)). Economists do it all the time, legitimately so, and methodologists should study when and why economists do it.

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COMMENT ON HANDS

Uskali Mäki*

There should be no doubt that, as Wade Hands points out, the Popperian dominance in the methodology of economics in recent years has had some undesirable effects (Hands, this volume; see also Mäki, 1990a). To these belong two omissions among the objects of methodological studies, namely prevailing instrumentalist and essentialist beliefs and practices within the economics profession, both denounced by Popperian canons. As Hands further argues, the Popperian project as a realist project has encountered difficulties because of Popper's failure to provide an adequate explication of the intuitive notion of closeness to the truth (truthlikeness, verisimilitude). Two ways out of this latter impasse can be chosen. One is to proceed with the Popperian project without the idea of increasing verisimilitude as the aim of science. This is the option chosen by Watkins (1984). The other is to rebuild a realist notion of truthlikeness without the burden of Popperian methodology. Such non-Popperian explications have been provided by Niiniluoto (1987) and Oddie (1986). In the brief remarks which follow I attempt to outline an idea of how an economist with essentialist intuitions might view scientific progress as increasing truthlikeness.

Popper's *Die Logik der Forschung* (1934) was metaphysically neutral or indifferent. He admitted that he had *realist inclinations*, but this was not incorporated into his methodological system. It would seem that Popper's early followers in economics, such as the M^2T group at the London School of Economics in the late 1950s and early 1960s (see de Marchi, 1988), are true to Popper in this respect, too. The revolution they proposed was essentially a methodological revolution. After all, realism would not have been a revolutionary idea at that time at the LSE: Lionel Robbins, emperor of the former reign, was no doubt a firm realist.

Popper introduced the notion of verisimilitude in 1960, but it was never adopted by Popperian methodologists of economics. Some of them, such as Mark Blaug, declare themselves to be advocates of realism, but they have not backed up this position with the Popperian notion of verisimilitude, or with any other well-developed doctrine of truth and truthlikeness. This implies that the reasons for the downfall of Popperianism in economic methodology have nothing to do with the failure of Popper's explication of the concept of truthlikeness.

* I wish to thank Ilkka Niiniluoto for helpful comments on an earlier draft.

It is true that there is, after all, an instrumentalist flavour involved in Popperian scepticism. This may also explain the fact that, even though many economists have declared themselves advocates of Popperian methodology, they nevertheless simultaneously feel comfortable with instrumentalist beliefs and practices, with no appreciable interest in matters of truth. It is the Popperian dictum of *testing theories by their empirical implications* that seems to have played the main role in the reception of Popperian ideas by economists. And, after all, was not this dictum precisely the one that was presented as the fundamental message of the Friedman–Machlup position in the methodological discussions by economists in the 1950s and 1960s? Needless to say, the dictum itself does not imply instrumentalism. The important thing is that the notion of truth does not have any operational significance in Popperian methodology; this may also be the case in much of economics.

Popper (1963, pp. 103–5) defines *essentialism* as a doctrine which subscribes to the following three tenets:

1. Scientific theories 'describe the "essence" or "essential natures" of things' lying behind the appearances. Let us call this the semantico-ontological component of essentialism.
2. By so doing, theories provide ultimate explanations in the sense that such theories are 'neither in need nor susceptible of further explanation'. Let us call this the ultimate explanation component.
3. The truth of such theories can be finally established 'beyond any reasonable doubt'. Let us call this the certitude component.

There should be no doubt, in my view, that many economists think of the task of theory formation in terms of something like the semantico-ontological component of essentialism. They do, indeed, think that good – or the best, or the desirable – economic theories provide true descriptions of what is essential in the economy, to the exclusion of the inessentials or the appearances. The other two, both epistemological, components seem to be much less popular among economists.

It follows that very few economists subscribe to the radical essentialism defined in terms of all three, both semantic and epistemological, components. Ludwig von Mises is a famous advocate of such a radical view. Many others, even Milton Friedman in a few passages of his 1953 essay, can be interpreted as espousing a weak version of essentialism consisting of the semantic component (see Mäki, 1990b). This means that, if we follow Popper by defining 'essentialism' in his narrow way, we manage to exclude weaker and probably rather popular forms of economic essentialism from serious consideration within the Popperian framework.

To this we immediately have to add that elsewhere Popper himself subscribes to something that at least comes close to the semantico-ontological component of essentialism. In his *Realism and the Aim of Science* (1983, p. 137), he writes as follows:

> . . . although I do not think that we can ever describe, by our universal laws, an *ultimate* essence of the world, I do not doubt that we may seek to probe deeper and deeper into the structure of our world or, as we might say, into properties of the world that are more and more essential, or of greater and greater depth.

Popper even calls this view 'modified essentialism'. Again, the problem with this is that the idea of science penetrating into deeper and deeper layers of the world has no adequate connection to Popper's methodological framework.

I shall now make a few speculative remarks on how weak or modified essentialism could be connected to the notion of progress, and whether and how it is being implicitly so connected in the research practices of the economics profession. I suggest, in rather intuitive terms, that the following forms of theoretical progress may occur in science:

1. Progress occurs when a theory is formed that gives a truthlike description of the essence or an essential layer of the object under study.
2. Progress occurs when a theory is formed that gives a more truthlike description than its predecessor of the essence or one of the essential layers of the object under study.
3. Progress occurs when a theory is formed that provides a truthlike description of a deeper essential layer of the object than its predecessor.
4. Progress occurs when a theory that gives a truthlike description of the essence of an object is expanded so as to give a truthlike account of the way(s) its essence or essential layer manifests itself.
5. Progress occurs when a theory that gives a truthlike description of the essence of an object is expanded so as to give a more truthlike account of the way(s) its essence or essential layer manifests itself.

It is difficult not to conflate these five forms of progress in Popper's framework, or rather the framework does not provide adequate tools for analysing the five forms. By using a bit of imagination, we can give them Lakatosian reformulations. The first step is to suggest that there are metaphysical or ontic correlates for the elements in the theories of Lakatosian research programs. Let us call the correlate of the theoretical hard core the 'ontic core' and the correlate of the protective belt the 'ontic periphery'. On this reformulation, the hard core of a research program is a condensed statement of the allegedly essential features of the subject-matter (the ontic core), while the protective belt describes some of the relatively less essential features (in the ontic periphery), which none the less have an impact on the manifest behaviour of the objects under study. The conjunction of the hard core and the protective belt implies statements about the appearances or manifestations of the objects of the theory.

Progress in senses (1)–(3) then consists of either formulating a successful hard core or improving it or replacing it with a better one. Progress in senses (4)–(5) takes place on the belt of auxiliary statements that describe the ways in which the essential features combine with other features so as to constitute the appearances of the object under study. A major part of progress in science is of kind (5). This is the case with 'mature' science in particular (see Nowak, 1980; Krajewski, 1977). For instance, it seems obvious that progress in senses (1) and (3) has been absent in mainstream economics for a long time. An essentialist interpretation of the situation might refer to neoclassicism as a mature science that has discovered and theoretically described the ontic core of the economy and now takes as its major task the refinement and application of the theory without questioning the statements of the hard core.

One difficulty with the Popperian notion of verisimilitude is that it is not adequate for discussing all of these five forms of scientific progress. The problem is that this notion is an attempt to explicate the idea that science approaches or should approach the true description of the whole of the actual universe. However such comprehensiveness is not required for progress to occur in senses (1)–(3) in particular. When trying to describe ontic cores or essences or essential layers, one deliberately omits most facts about the actual world. Such a description is an attempted theoretical isolation of the ontic core from peripheral factors. To speak about the truthlikeness of such descriptions, the Popperian concept will not do. As Popper (1963, p. 234) says, '[v]erisimilitude is so defined that maximum verisimilitude would be achieved only by a theory which is not only true, but completely comprehensively true: if it corresponds to all facts . . .'. This is, of course, a consequence of linking the notions of verisimilitude and 'content' together so as to vindicate his falsificationist methodology: a 'completely comprehensively true' theory is also maximal in regard to logical strength.

L. Jonathan Cohen (1980) makes a distinction between verisimilitude and what he calls 'legisimilitude'. He is critical of definitions of verisimilitude in terms of truth and falsity in regard to the actual world. Science, he argues, pursues truths about laws, understood as physically necessary truths. Such necessities are defined in terms of possible worlds. As Cohen (p. 500) points out, Popper (1959, pp. 432f) elsewhere accepts this notion of natural necessity, but 'he omits to consider its implications for the doctrine of verisimilitude'. Cohen suggests that, instead of verisimilitude, or truthlikeness, science is after legisimilitude, or lawlikeness.

Close to, but not identical with, Cohen's proposal is the distinction between 'descriptive truth' and 'theoretical truth', suggested by Theo Kuipers (1982). A statement is descriptively true if it is true in the actual world; a statement is theoretically true if it is true in all physically possible worlds (p. 347). Descriptive verisimilitude is then defined as closeness or likeness to descriptive truth and theoretical verisimilitude as closeness to theoretical truth (pp. 352–7). Kuipers criticizes other writers on the topic for conflating the two kinds of truth and verisimilitude and for ignoring the fact that theoretical scientists aim at theoretical truth.

What unites Kuipers's suggestion with that of Cohen is the idea of scientific theory having natural necessities as its object. It is not altogether inconceivable to pursue truth about natural necessities in economics, as the popular use of counter-factual reasoning and the theoretical endeavours of Austrian and Marxian traditions indicate. However the idea may be unnecessarily restrictive for our purposes. Necessity is, of course, one possible attribute of essence. Some economists, however, may use essentialist terminology: 'this is the essence of the matter', 'these seem to be the essential features of the situation' and so on, without committing themselves to the notion of natural necessity. Therefore perhaps we would need a more general notion for expressing the intuitive idea of likeness to the essential truth about the economy – that is, likeness or closeness to the truth about essences or ontic cores in the economy. Labouring over an analysis of this intuitive notion is a task for another occasion, but let us suggest a name here: perhaps it could be called 'essesimilitude'. Forms (1)–(3) of scientific progress would then imply increasing essesimilitude.

No analysis of essesimilitude is currently available. It is obvious that such an analysis cannot be provided in the Popperian framework. More powerful and flexible frameworks, such as Niiniluoto's (1987), are needed.

We have found that Popper's philosophy of science contains both what he calls 'modified essentialism' and the notion of natural necessity, but that these have not been adequately incorporated into his idea of verisimilitude. The methodology of economics might find some use for a notion of truthlikeness with these essentialist ingredients.

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REPLY TO HAMMINGA AND MÄKI

D. Wade Hands

1 Introduction

Responding to the comments of Mäki and Hamminga places me in a rather unusual position. On the face of it my chapter is quite critical of Popperian philosophy; I trace the development of the concepts of excess content, novelty and verisimilitude and demonstrate how these concepts have contributed to certain problems in economic methodology. Despite this critical stance, here and in other recent work, my philosophical preferences remain basically within the Popperian (or maybe neo-Popperian) tradition. My two commentators, on the other hand, are in general much less sympathetic to the Popperian program. Mäki basically agrees with my evaluation of the difficulties of Popperian economic methodology, but argues for an essentialist solution. Hamminga, contrary to my view, argues that the Popperian concept of excess content *is* a useful tool for understanding theory choice in economics, but his defence of excess content is based on a very narrow definition of the concept of a novel fact. Such a narrow interpretation of the Popperian terminology is frequently the analytical philosopher's response to Popper.

Thus Mäki and Hamminga leave me in the paradoxical position of responding to the comments on my anti-Popperian chapter by defending the Popperian tradition. With respect to Mäki, my argument will be that, despite the problems discussed in my chapter, Mäki's essentialism does not really offer a more viable solution to the current problems in economic methodology than simply continuing to go forward with the (evolving) Popperian tools. With respect to Hamminga, I appreciate his economic application, but his characterization of novelty does such an injustice to the history of this important concept that I feel compelled to provide a detailed (Popperian) defence. I will respond to Mäki first, since my comments are relatively brief. My response to Hamminga, though it focuses on only a small section of his comment, is more lengthy.

2 Response to Mäki

Mäki agrees that Popper's falsificationist methodology does not 'connect up' in any systematic way with his scientific realism and that Popper's theory of verisimilitude was an (unsuccessful) attempt to provide such a 'connection'. Mäki correctly notes that Watkins's (1984) replacement of verisimilitude by a set of adequacy requirements is one attempt to provide a Popperian solution to this problem – I would also add the 'structural realism' recently proposed by Worrall (1989), and Bartley's (1984, 1987) 'comprehensively critical rationalism'[1] to this list of purported Popperian solutions. Although Mäki mentions Watkins, his concern is not really with Popperian solutions; his solution is essentialism. Mäki does argue that Popper himself advocated a weak version of essentialism in his 'modified essentialism',[2] but this, like Popper's advocacy of scientific realism, has no systematic connection with his falsificationist methodology.

Mäki's solution seems to be a Lakatosian version of essentialism. The hard core of the scientific research program 'is a condensed statement of the allegedly essential features of the subject-matter (the ontic core), while the protective belt describes some of the relatively less essential features (in the ontic periphery), which none the less have an impact on the manifest behaviour of the objects under study' (p. 88). Unlike traditional essentialism, where essential natures are posited, Mäki's own modified essentialism only seeks 'essesimilitude' – likeness to the essential truth about the economy (p. 89) rather than necessity. Progress occurs by increasing essesimilitude and these increases can come about in a number of different ways during the development of the research program.

I have at least two difficulties with Mäki's suggested solution, but my difficulties are general problems I have with essentialism and essentialism in economics, rather than problems with Mäki's own Lakatosian version or his concept of essesimilitude. Mäki's own thesis is simply too briefly presented (as is appropriate in this context) to be evaluated at this time; I will simply withhold judgement until these ideas have been developed in more detail.

My first problem with essentialism as a solution to any problem in economic methodology (or the methodology of any other science) is that essentialism is methodologically mute. Essentialism is an ontological position; it implies no particular theory about how one comes to know these posited essential natures. In fact most defences of essentialism work in the opposite direction of most methodological discussions; they argue for an ontology of essential natures by showing that any ontology devoid of such essential natures would be in conflict with the inductive practice of natural science.[3] This is just the reverse of our normal concerns in the methodology of science; normally one is not concerned about which ontological posits are consistent with scientific practice, one is concerned with knowing which scientific

practice best exposes this posited necessity. It may be necessary to posit essential natures in order to proceed with empirical science (this is the source of Popper's modified essentialism) but, even if this is accepted, it only sets the stage: all of the methodological questions remain unanswered. This problem is demonstrated by Mäki's list of five ways of characterizing essentialist progress. This list, while perfectly reasonable, offers no procedures for determining when such progress has occurred; we are told where we are going, with no information about how to get there. Thus I find it hard to accept an essentialist solution to the problems in economic methodology. Essentialism does not solve the basic Popperian problem I discuss in my chapter, it just reverses it. Popper fails to connect his particular methodology with his desire for truth; essentialism fails to connect its desire for truth with any particular methodology.

Secondly, essentialism in economics must face a fundamental issue that must be faced by any type of scientific realism in economics. This is the so-called 'no miracles' argument or the 'ultimate argument' for realism. In the case of natural science the argument goes as follows:

> It would be a miracle, a coincidence on a near cosmic scale, if a theory made as many correct empirical predictions as, say, the general theory of relativity or the photon theory of light *without* what that theory says about the fundamental structure of the universe being correct or 'essentially' or 'basically' correct. But we shouldn't accept miracles, not at any rate if there is a nonmiraculous alternative. If what these theories say is going on 'behind' the phenomena is indeed true or 'approximately true' then it is no wonder that they get the phenomena right. So it is plausible to conclude that the presently accepted theories are indeed 'essentially' correct. (Worrall, 1989, p. 101)

The 'no miracles' argument is a standard argument in favour of a belief in scientific realism or, combined with essentialism, a belief that our best scientific theories are basically correct in isolating the real essential natures of the objects under investigation. The argument is simply that our best scientific theories must tell us the way things really are because there is no other way to explain their predictive success. Now such an argument may be quite compelling in the case of natural science, where the record of predictive success is beyond dispute, but can such an argument reasonably be applied to economics? Even at its best the predictive record of economics is nowhere near the predictive record of most established theories in natural science; economics is (literally) not rocket science. If the extraordinary predictive success of science is the best argument for scientific realism and that success is absent in economics, what is the argument for realism or essentialism in economics? I am not at all certain how one might answer this question, but it is an important question and I believe it is a question that needs to be answered by those arguing for essentialist realism in economics.

3 Response to Hamminga

Hamminga argues, contrary to the position I took in my paper, that continuing to search for Popperian/Lakatosian excess content and novel facts in economics is a worthwhile project. He supports this claim by defining novel facts (ostensibly in a straightforward Lakatosian way) and then providing a detailed application of his definition to Samuelson's *Foundations of Economic Analysis* (1947); this application constitutes the main body of his paper.

I would like to say right away that I do not have any real problems with Hamminga's particular application to Samuelson's *Foundations*. *Given Hamminga's definition of novelty*, what he says about Samuelson seems to be perfectly reasonable. My problem is not so much with the economic application, but rather with his definition of novelty. In fact, I do not even have a problem with his definition of novelty (it is a perfectly fine definition of novelty), *I have a problem with his saying that it is clearly Lakatos's definition of novelty*. There is no reason to start a debate among economists over what Lakatos 'really meant' by novel facts. I have spent a great amount of time with the Lakatosian literature and I have *no idea* what Lakatos 'really meant' by novel facts. Either Lakatos changed his mind between (and sometimes within) works, or he simply held more than one view simultaneously. The point is not to contest Hamminga's particular reading of Lakatos; the point is to contest anyone who has the audacity to argue that there is one 'crystal clear' (p. 83) definition of novel facts in Lakatos. There are, by my count, currently five separate definitions of novel facts in the literature, most proposed by philosophers who are active participants in the Popperian tradition. What Hamminga has proposed is H-novelty, Hamminga novelty, and it is a perfectly reasonable notion of novelty; it is also perfectly reasonable, and possibly interesting, to ask if such H-novelty can be found in Samuelson's *Foundations*. What is not perfectly reasonable is to assert that H-novelty is clearly what Lakatos meant by novelty.

Now if it were simply the issue that there is more than one reasonable way to interpret Lakatos on novel facts and Hamminga argued that his definition is the only one, then my response could be quite brief (in fact what I have already said with a few footnotes on the five kinds of novelty

would suffice). But the problem is much deeper than this. The main point of my chapter was that, if economists are going to use the concepts and nomenclature from the Popperian tradition, then they (we) need some sense of the history of these concepts, some sense of the filiation of the ideas, and some sense of the problem situations of the authors involved; this is why I told the 'long Popperian tale'. This history, this context, is precisely what Hamminga avoids: either because Popperian philosophy is trivially simple if one reads carefully (the analytical philosopher's response) or because the 'real' issue is economics and we want to spend a minimal amount of time on philosophy (the economic methodologist's response). Hamminga takes a few sentences where the term 'novel fact' appears in Lakatos (1970), finds it to be a relatively simple (though 'non-common') idea that can be demonstrated with a few Venn diagrams and then rushes out to apply it to economics. This is precisely the type of trivial misapplication of Popper and Lakatos that my chapter was written to protest. As I said above, Hamminga may have an independently interesting analysis of *Foundations*, but *as* an application of, or an exercise in, the philosophy of the Popperian tradition, it is entirely at odds with my central thesis. Therefore I do not feel that it is appropriate to simply end my response with 'there is more than one way to define novel facts'; I feel it is necessary to reply by ferreting out this notion of novelty, with its history and context squarely in mind, in order to demonstrate (reinforcing the theme of my chapter) how much is lost without it.

The concept of a novel fact arises in the Popperian tradition in response to the question: when does a particular fact *support* a particular theory? When does fact e support theory T? According to the *logical approach* to confirmation, evidence e supports or confirms theory T, if e is empirically accepted and if e is a logical consequence of T; to use the standard ornithological example, a particular black raven confirms the theory that all ravens are black. For Popper, though (and this is the seed of the entire discussion about novelty in the Popperian tradition) confirmation is not the issue, *corroboration* is the issue, and not all confirmations are corroborations. Confirmations only count as corroborations when they are the result of a severe test and a severe test is a test where the evidence was not probable (was not expected) on the basis of the background knowledge alone.[4] But notice that by introducing background knowledge we have moved away from a logical theory of corroboration and introduced a fundamentally historical (or temporal) element into the discussion of what counts as corroborating evidence.[5] If background knowledge is everything provisionally accepted by the scientific community, then any evidence e, known at the time the theory was proposed, is part of the background knowledge (has a probability of 1 on the basis of the background knowledge alone) and thus cannot count as a corroboration of the theory. Therefore the only evidence which *can* count as a corroboration of a theory is a fact which was *unknown* at the time the theory was proposed: 'a fact which was already known before the theory's proposal does not support it' (Worrall, 1978, p. 46).[6] This brings us to the first, strictly temporal, view of a novel fact:[7]

(1) Strictly Temporal Novelty: A fact e is novel with respect to theory T if e was unknown at time T was proposed.

Is (1) Lakatos's view of novel facts? Well, maybe. Gardner (1982, p. 2), Worrall (1978, p. 66) and Zahar (1973, pp. 101–3) clearly say that (1) is what Lakatos (1970) means by a novel fact, but Musgrave (1974, p. 8) claims that he has 'not been able to find where Lakatos explicitly adopts the strictly temporal view'.[8] Trying to garner support for (1) directly from Lakatos (1970) leads to ambiguity. For example, if one reads 'knowledge' in, 'A new fact must be improbable or even impossible in the light of previous knowledge' (p. 118) as 'background knowledge', then the sentence seems to support (1). On the other hand, with a different reading of the word 'knowledge', this same sentence can support other views (even Hamminga's). Similar ambiguous things can be said about almost every other quote (and there are many) involving 'novel facts' or related terms in Lakatos (1970). Probably the clearest support for (1) comes from where Lakatos says, 'Bohr's theory logically implied Balmer's formula for hydrogen lines as a consequence. Was this a novel fact? One might have been tempted to deny this, since after all, Balmer's formula was well-known' (1970, p. 156).

The next definition of novelty, what Musgrave (1974, p. 12) calls the 'heuristic view', is due to Worrall (1978) and Zahar (1973).

(2) Heuristic View of Novelty: A fact e is novel with respect to theory T if e was not used in the construction of T.

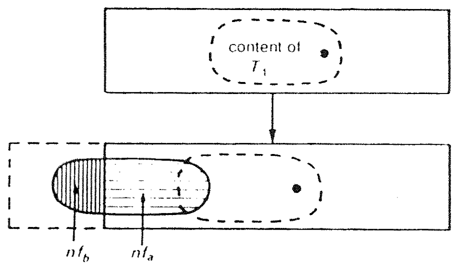
Definition (2) was a conscious attempt to weaken (1); if e was 'unknown' it certainly could not be used in the construction of the theory, however e could be known and still not be used in the construction of the theory. Thus any fact that satisfies (1) will satisfy (2), but the converse does not hold. If one wants to find a great number of novel facts, and thereby reconstruct large segments of the history of science as Lakatosian progressive, then (2) is more efficient than (1). I

used (2) as my standard for defining novel facts in my discussion of the Keynesian revolution (Hands, 1985, 1990) because I felt that it was the most popular definition in the Lakatosian literature and also because I wanted to be as lenient as possible on Keynesian economics: to give it the greatest chance of showing progress. There is some debate about whether (2) is the 'correct' definition of novel facts but there does not seem to be much of an exegetical debate since almost everyone agrees that (2) is a modification (or some would say improvement) of Lakatos's view of novel facts.

A third definition of novel facts is the 'background theory' view of Musgrave (1974, pp. 15–19).

(3) Background Theory View of Novelty (Musgrave): A fact e is novel with respect to theory T if e was not also predicted by the best existing predecessor to theory T.

Since empirical content in the Popperian tradition means potential falsifiers, (3) could be restated as 'the new theory should have potential falsifiers which are not also potential falsifiers of the old theory' (Musgrave, 1974, p. 16). The main difference between (3) and the earlier definitions of novelty, (1) and (2), is that (3) refers to novelty relative to a background *theory* rather than background *knowledge*. Under (3) novel facts can come from two separate sources: (a) predictions which conflict with the earlier theory (falsifiers which were not falsifiers of the earlier theory) and (b) predictions of phenomena about which the earlier theory says nothing at all. To demonstrate (3) we can use Hamminga's box diagrams; this was not possible for definitions (1) and (2) since Hamminga's diagrams are defined by what is forbidden or allowed by a particular theory, not by the (temporally variable) state of background knowledge. Applying these diagrams to definition (3) we have novel facts of type (a) from above given by nf_a and novel facts of type (b) from above given by nf_b in the box below.



There are a few things to notice that make my diagrams different from Hamminga's. First, I indicate one potential falsifier by a dot as in Hamminga's diagrams but I do not indicate that the dot is necessarily an observed falsifying instance. This is because novel facts need not have anything to do with falsification. One gets this idea from Popper's discussion since he is concerned with non-ad hoc adjustments in the face of falsifying evidence, but there is no inherent relationship between the two; it is quite reasonable to discuss the novel facts predicted by a new theory even though its predecessor was never falsified. Second, notice that the excess content indicated by nf_b is truly 'novel'; it consists of 'predictions concerning phenomena about which the background theory predicts nothing at all' (Musgrave, 1974, p. 16). Finally, notice the similarity between the (3) definition of novelty and Hamminga's definition, 'failure, despite severe attempts, to observe a newly forbidden event' (p. 77). Hamminga's definition simply amounts to the nf_a part of Musgrave's (3) definition.

Now what about the exegetical question: is (3) what Lakatos 'really meant' by novel facts? Well, again it is not at all clear. Musgrave (1974, p. 15) certainly believes that this *is* Lakatos's view, in fact he calls it Lakatos's view.[9] The problem is that the 'Lakatos' that Musgrave cites is Lakatos (1968), not Lakatos (1970), the author of the methodology of scientific research programs. As with (1), the Lakatosian fidelity of (3) seems to be an open question.

The fourth definition of novel facts is also a 'background theory'-based definition (like (3)) rather than a 'background knowledge'-based definition (like (1) and (2)). It is due to Watkins (1984, p. 295).

(4) Background Theory View of Novelty (Watkins): A fact e is novel with respect to theory T if e has no counterpart among the consequences of the existing predecessors to theory T.

In Watkins's words, the new theory 'breaks new ground here by making a predictive assertion in an area where its predecessor is silent' (1984, p. 295). Definition (4) differs from definition (3) in that (4) is a special case of (3): the case of (b). In the earlier (3) definition the new fact must not have been an event *allowed* by the old theory; this leaves the two possibilities: (a) it was forbidden by the old theory, or (b) the old theory said nothing at all about it. In Watkins's (4) definition the new fact must not be a consequent, either allowed or forbidden, by the old theory. This means that type (4) novel facts must break 'new ground', as indicated by the area nf_b in the above diagram. According to Watkins (1984, p. 297) a theory may receive a *strong* corroboration from such novel facts; moderate and weak corroborations may come

from other types of facts. Watkins does not attribute this (4) definition to Lakatos. He considers it to be his own definition and presents it as an improvement on both the 'Popperian' view (which for Watkins is (1)) and the Worrall–Zahar view (2).

The final definition of novelty (5) needs little discussion, since the author of this view, like Watkins, does not claim that his definition is 'really' what Lakatos meant but rather offers it as an improvement on the earlier definitions. This final definition is from Gardner (1982, p. 10), a definition he calls Novelty$_k$.

(5) Novelty$_k$: A fact e is novel with respect to theory T if e was unknown to the person who constructed theory T at the time theory T was constructed.

Gardner (1982) argues that this definition, though it may sound as if it makes novelty a person-relative affair, solves a number of the purported problems of earlier definitions, (1) and (2) in particular.[10]

So five different definitions of novelty have been presented, three Lakatosian and two 'improvements'. Hamminga's definition of novelty, H-novelty, is not exactly any of these five but it is most closely related to the (3) definition of Musgrave. In fact, in terms of the above diagram, if the Watkins definition (4) were removed from the Musgrave definition (3), the remainder would be H-novelty: the (3) definition is nf_a and nf_b, the (4) definition is nf_b alone, and H-novelty is nf_a alone. Can support for H-novelty be found in Lakatos? Of course, but then again support for (1) and (3) (and some would say (2) as well) can also be found in Lakatos. Is it interesting to see if H-novelty can be found in economics? Maybe, but not any more interesting than searching for any of the other types of novel facts (1)–(5). I have never suggested that such novel fact hunts are entirely without merit – I have certainly engaged in them myself. My point in what I have written in this volume, and to a lesser extent in Hands (1988), was simply that it is now time, if we are really interested in the Popperian tradition in economics, to take a more reflective stance. I argued that, rather than hunting for novel facts based on a particular definition or arguing over what the correct definition should be, economic methodologists should be concerned with understanding how novel facts came to play the role they did in Lakatos's methodology – similarly for the Popperian concepts of ad hocness, verisimilitude and the rationality principle. By understanding these concepts, their history and the role they have played in Popperian philosophy we can better understand their importance in economic methodology. We have tried (desperately) to 'apply' Popper and Lakatos to economics during the last 20 years and the results have not been a glorious success. For some this indicates that we should abandon the Popperian tradition entirely and seek guidance from other sources: essentialism, rhetoric, instrumentalism, the sociology of science, and so on. This is not my response. My response is not to give up, but rather to engage in a careful, reflective re-examination of the Popperian tradition. There is still room for economic 'applications' of Popperian concepts, but these applications should be careful to respect the integrity of the concepts being applied. Hamminga's 'Lakatosian' definition of novel facts does not.

Notes

1. It should be noted that, while Bartley's critical rationalism can be packaged as a solution to this Popperian problem, it is not a direct approach like that of Watkins (1984) or Worrall (1989). Rather it is a solution in the sense that one 'solution' to the problem of a poor tee-shot in golf is to play tennis.
2. Popper (1972, p. 195; 1983, p. 137). Worrall (1982, 1989) calls Popper's view 'conjectural realism', a more appropriate term for Popper's position.
3. See, for example, Fisk (1973, ch. IV).
4. Popper (1968, Appendix *ix; 1983, pp. 238–44).
5. The distinction between *logical* and *historical* (or temporal) theories confirmation is emphasized in Musgrave (1974), a paper which (rightly) remains one of the standard sources on these topics.
6. 'Facts "known to science" before a hypothesis is provided will not be able to confirm that hypothesis, since they will already be contained in background knowledge and cannot represent the results of severe tests' (Musgrave, 1974, p. 8).
7. This Popperian interest in severe tests is *only one* of many reasons why novel facts (however defined) might have a special status with respect to theory support. Another reason to favour novelty is simply the surprise power of unexpected facts. This 'anticipating nature' argument is traced through Descartes, Leibniz and Duhem by Musgrave (1974, pp. 1–3). Still another reason for their importance is that novel facts are less subject to the so-called 'paradoxes of confirmation'; temporal properties of evidence can prevent many of these paradoxes from occurring (see Lawson, 1985; Musgrave, 1974; Watkins, 1978, 1984). Finally, there seems to be 'diminishing returns' to background knowledge; each additional confirmation of the same 'kind' should count for less than a confirmation of a new (novel) kind (see Watkins, 1984, pp. 292–3); Popper, 1965, p. 240).
8. From here on I will only be concerned with Lakatos's view of novelty. Despite the fact that Hamminga claims that his definition is the view of both Popper and Lakatos, Popper is actually even less clear on the matter than Lakatos; Musgrave (1974, p. 19) finds at least three different views in Popper (1965) alone. In all fairness to Popper though, it should be noted that Popper's real concern was ad hocness (or non-ad hocness) and not novelty *per se*, thus his lack of consistency is not all that problematic. Of course the same cannot be said for Lakatos, the person responsible for what I have called 'a novel fact fetishism' (Hands, 1988, p. 135).
9. Lawson (1985, p. 405) concurs.
10. It should be noted that these five definitions are not exhaustive of the definitions in the literature; they are simply the definitions most discussed (others include Carrier, 1988; Nunan, 1984). There are also subtle differences in various presentations of the five definitions I have discussed; for example, what I present as the Worrall–Zahar definition (2) is slightly different in Zahar (1973) from what it is in Worrall (1978).

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B Testing