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Reviews

Sir Karl Popper 1902-1994: Falsification and Agricultural Economics

W.R. Stent*

Introduction

Sir Karl Popper died in London on September 18, 1994. He was 92 years old.

Popper was, without doubt, the most influential philosopher of science in the 20th Century. So, even though, since Kuhn, many may consider much of Popper's work *passé*, no serious discussion of the choice of scientific theories can take place without reference to his work. That this is so is testified to by the fact that the discussion of the scientific validity of neo-classical production theory recently published in the *Canadian Journal of Agricultural Economics* commenced with an article entitled 'Popper or Production'¹.

For eight years, Popper lived in New Zealand. He arrived there after fleeing Austria as a refugee from Nazism in March 1937, and taught Philosophy at Canterbury University College, Christchurch for eight years. He greatly enjoyed the 'wonderfully quiet and pleasant atmosphere for work' (Popper 1976, p.112) there and made a number of lifelong friends. So happy was he that, despite the hostility shown to his work by some of the university authorities, Popper was ready to stay there for good. However, in 1945 he received two invitations to teach elsewhere. The first was from the University of Sydney, but the xenophobia there at that time led him to reject it. No sooner had he done so than he received a cable from Professor Hayek of the London School of Economics inviting him to accept a Readership at the LSE. He accepted it and remained there until his formal retirement in 1969² (Popper 1976, p.120).

Popper's 'Unended Quest'

Popper's whole intellectual life was devoted to various aspects of the theory of knowledge. He was convinced that 'there is a real world, and that the

problem of knowledge is the problem of how to discover this world' (Popper 1976, p.75). He identified three main theories relating to the definition of Truth: correspondence theory, coherence theory and pragmatism (Popper 1972, p.308).

[Correspondence theory is] the theory that truth is correspondence with the facts, or to put it more precisely, that a statement is true if (and only if) it corresponds with the facts, or if it adequately describes the facts. ... [According to] coherence theory ... a statement is regarded as true if (and only if) it coheres with the rest of our knowledge. The third theory is that truth is pragmatic utility or pragmatic usefulness.

Popper rejected the coherence theory as inadequate. He pointed out that one of its versions, which states that a statement is true if it coheres with our *beliefs*, is clearly inadequate because beliefs may, even at the level of logic, be false. He rejected the second version of coherence theory, which states that a statement 'is to be accepted as true if (and only if) it coheres with statements we have previously accepted', because it is hopelessly and utterly conservative. To adopt such a standard would prevent 'entrenched' knowledge from ever being overthrown.

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¹ See Fox and Kivanda, Paris, and Clark and Coyle.

² Biographical details are from Popper (1976, p.112, p.120).

According to the theory of pragmatic utility, a theory is true if it can be shown in physical or other tests to 'work'. That is, if its implementation has some utility. Thus, for instance, according to this theory, the laws of Newtonian physics may be said to be 'true' because they can be successfully used for predicting the paths of celestial bodies. On this basis, theories would be deemed to be true even though subsequent developments in Physics have shown them to be false.

Both the coherence and pragmatic theories depend for their validation on subjective standards, - beliefs, traditions and utility. In Caldwell's apposite term, their truthfulness is determined by 'consensus' (p.233). This, of course, begs the question of 'whose consensus?'. In the end the formulation of a consensus involves some sort of 'vote'. That is 'truth' becomes subjective. There is real danger in this for, as Bertrand Russell wrote in the 1920s (Russell, p.77):

... when once the concept of objective truth is abandoned, it is clear that the question "what shall I believe?" is only to be settled, as I wrote in 1907, by "the appeal to force and the arbitrament of the big battalions", not by the methods of either theology or science.

Correspondence theory requires that a theory corresponds with the 'facts'. This, of course, raises the problem of just what are the facts, and indeed of whether they exist at all. For Popper, there was no doubt. He was a passionate believer in the concept of objective truth. That did not mean that he believed that he was its custodian. Far from it! From his earliest years, he was well aware of his (and our) 'infinite ignorance'³. Nevertheless he believed that the pursuit of objective truth was at the very heart of scientific progress and of civilisation itself.

In explaining why he believed that we could ever be assured of attaining objective truth, Popper wrote (1974, p.226):

The status of truth in the objective sense, as correspondence to the facts and its role as a regulatory principle, may be compared to that of a mountain peak usually wrapped in clouds. A climber may not merely have difficulties in getting there - he may not know when he gets there, because he may be unable to distinguish, in the clouds, between the main summit and a subsidiary peak. Yet this does not affect the objective existence of the summit; and if the

climber tells us 'I doubt whether I reached the actual summit', then he does, by implication, recognize the objective existence of the summit. The very idea of error, or of doubt (in its normal straight forward sense) implies the idea of an objective truth that we may fail to reach.

Indeterminate though objective truth may be, it was on his belief in its existence that Popper was able to develop his Demarcation Principle by which theories of empirical science could be distinguished from non-empirical theories. His principle requires that tests of scientific truth be made, not by seeking empirical support for a theory, but by the elimination of false theories by rejecting them when they are shown, by empirical testing, to be false. This is done by constantly maintaining a critical approach to all (scientific) theory. As he explains (1974, p.229):

... the rationality of science lies not in its habit of appealing to empirical evidence in support of dogmas - astrologers do so too - but solely in the *critical approach*: in an attitude which, of course, involves the critical use, among other arguments, of empirical evidence (especially in refutations). For us, therefore, science has nothing to do with the quest for certainty or probability or reliability. We are not interested in establishing scientific theories as secure, or certain, or probable. Conscious of our fallibility we are only interested in criticising them and testing them, hoping to find out where we are mistaken; of learning from our mistakes; and, if we are lucky, of proceeding to better theories.

And so, for Popper there was no disgrace in being mistaken. Indeed he argued that the progress of knowledge demanded that we make bold assertions which would subsequently, when examined against the evidence, be shown to be wrong.

³ Popper explains in his intellectual autobiography that he learned more about the theory of knowledge from the cabinet maker to whom he was apprenticed shortly after the First World War than from any other of his teachers. It was as a result of being constantly asked absurd questions about trivial facts by 'dear omniscient' Adalbert Psch that he became aware that 'any wisdom to which I might aspire could consist only in realizing more fully the infinity of my ignorance' (1976, p.7).

In many ways, for Popper, the pursuit of knowledge must be individualistic. He was not greatly enamoured with schools of thought and was especially distrustful of those who rushed to pursue the latest intellectual will-o-the-wisp (1972, pp.215-216):

There are fashions in science, and some scientists climb on the band wagon almost as readily as do some painters and musicians. But although fashions and band wagons may attract the weak, they should be resisted rather than encouraged.

Popper clearly distinguished between pure and applied science. Pure science was, for him, always seeking, through the critical testing of hypotheses, to expand the frontiers of knowledge. In contrast, he said, applied science is simply a search for power in which case, it does not matter whether the theories on which it operates are true or false. All that matters is that they work.

It was because he believed that Kuhn's 'normal' scientists⁴ acted uncritically that he showed disdain for them. He said had that they 'learned only how to apply some theoretical framework to the solving of problems which arise within this framework, and which are soluble in it (1972, p.182). Such 'scientists' were, in his view to be pitied (1970, p.53):

[he] has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking the reason why ... As a consequence, he has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist*.

'Popper or Production'

In their *Canadian Journal of Agricultural Economics* paper of this title, Fox and Kivanda sought to determine whether the apparent confidence which agricultural economists place in the standard neo-classical theory of production is 'scientifically' justified in a Popperian sense. They did this by reviewing 70 papers published over the previous 15 years in the principal English language agricultural economics to determine whether they employed Popper's falsification criteria in their discussion of production economics.

Fox and Kivanda identified homogeneity, monotonicity, curvature and symmetry as being the four categories of 'falsifiable hypotheses' incorporated within the theory of production. They then examined the 70 papers⁵ to determine 'which of the four relevant hypotheses were tested ... to see whether the tested hypotheses were refuted' (p.4).

They reported that the results of their analysis were 'disturbing' (p.5):

They show a distinct lack of seriousness among agricultural economists in following the falsificationist doctrine. ... About 54% of the articles (38 of 70 articles) reported one or more hypothesis test. However, only one of the 70 articles tested all four hypotheses, 11.4% tested three, 33% tested two and another 8.6% tested one.

Fox and Kivanda were even more concerned by the way in which refutable hypotheses employed in the papers were referred to as 'theoretical restrictions ... imposed in order to obtain efficient estimates'. They correctly point out that, by doing this, the 'writers give the impression that the validation of the theory has been established elsewhere'. This is thoroughly unjustifiable because, they assert, there is 'no basis for a Popperian economist to claim that "any sensible cost (or profit) function possesses the properties of ..."'. In other words, Fox and Kivanda say that there is no empirical justification for elevating the testable hypotheses employed in the papers that they examined to the level of axioms.

Fox and Kivanda suggest a number of reasons which agricultural economists put forward as a justification for their failing to test the correspondence of their theories to the real world:

- The theory of the firm is a small component of a larger (Lakatosian) Scientific Research Program⁶ which, taken overall, is 'progressive'.

⁴ See Kuhn), and, for a 'potted version' as applied to Australian agricultural economics, Stent.

⁵ Five were from the *Australian Journal of Agricultural Economics* and four from the *Review of Marketing and Agricultural Economics*.

⁶ For a brief outline of Lakatos's Methodology of Scientific Research Programmes see Caldwell (pp.85-89).

- The data are defective.
- The researchers never intended to test the theory, but merely to use it for some less ambitious purpose.
- The researchers were not relying on the hypotheses assumed by Fox and Kivanda to be implicit in the theory, but rather on those of the expected utility maximising model.
- "Everyone does it".
- There is no other model available.

• Fox and Kivanda reject all of these excuses and claim that main-line agricultural economics research, as currently practised and published in the English language journals, does not pass the test of Popper's Demarcation Principle. In passing, they note agricultural economics is not unique in this respect and point out that the same criticism can be levelled at neo-classical economics in general. For similar reasons, Caldwell concludes that 'the methodological approach which dominates the rhetoric of economic methodologists (falsificationism) cannot be applied successfully in economics' (p.231).

Agricultural Economics as a 'Science'

Fox and Kivanda assert that (p.9):

... agricultural economists generally want to be falsificationists. ... This creates an awkward dilemma. If we accept a Popperian view of methodology, then we must be much more circumspect in our role as advisers and explainers. If we reject the falsificationist protocol, what shall we use to replace it?

In posing their question in this way, Fox and Kivanda would seem to imply that there are no alternatives. There is Popperian science or *hocus pocus*. That is to overstate the case.

It may be that, in rigorous Popperian terms, agricultural economics cannot be termed 'scientific'? But does that matter? It is, of course, important that we don't delude ourselves by claiming to be something that we are not. But why insist on being Popperian scientists? There are other standards of 'science' which might better suit agricultural economics. For example, Kuhn's concept of 'normal' science might

be more appropriate for the profession of agricultural economics.

As Kuhnian 'normal' scientists, agricultural economists would claim simply to solve the puzzles which have been defined for them by the scientific paradigm within which they operate. (Whether that be neo-classical economics or some other theoretical framework is, from the point of view of the argument irrelevant). Agricultural economists would then not need to question the bounds of the paradigm within which they operate. Their professional task would simply be to fill in the missing pieces of its jigsaw⁷.

Popper felt sorry for people who acted in such a way and claimed that they reminded him of some engineering students of his acquaintance who (1970, p.53):

... merely wanted to 'know the facts'. Theories or hypotheses which were not 'generally accepted' but problematic, were unwanted: they made the students uneasy. These students wanted to know only those things, those facts, which they might apply with a good conscience, and without heart searching.

But it was not only for the people, as individuals, that Popper felt sorry. He believed that their attitude, if it became general, would be 'a danger to science and, indeed, to our civilisation' (1970, p.53).

One cannot but agree with him. But note that it is to the *uncritical attitude* of the engineering students that he takes most exception. He does not demand that all seekers after knowledge, at all times, subject every-

⁷ A referee takes a far more critical view of agricultural economics as a science (P Munz). He writes:

'Given a certain paradigm, both economics and psychology can be pursued as normal science. The question is whether any economic or psychological paradigm can be called "scientific". Probably not by Popperian standards. If not where does this leave normal economic science? If the paradigm is dicey, the normal science pursued under it is also dicey. I prefer to drop the science question altogether and think of both psychology and economics as politico-practical pursuits - governed by standards of decency, appropriateness, prudence, accommodation possibilities, etc; but not by standards of truth.'

I have some sympathy with this view which is, of course, far more radical than mine. Certainly, if the profession were to adopt it, it would lead to a far humbler and more tentative approach to policy matters than is currently the case.

thing they do to the fallibility test. What is far more important is that they should be aware that while they can never be the custodians of objective truth in its fullness, they should never give up seeking after it. They must always hold their knowledge tentatively, and always subject to critical test.

Popper insists that, to be 'scientific', a statement must be capable of being formulated so that, at least conceptually, it can be subject to the fallibility test, but he never claims that other sorts of statements are meaningless. Indeed he admits that theories (or statements) may be of great *instrumental* use, even if they are scientifically false. And so he says (1974, p.113):

For instrumental purposes of practical application a theory may continue to be used *even after its refutation*, within limits of its applicability: an astronomer who believes that Newton's theory has turned out to be false will not hesitate to apply its formalism within the limits of its applicability.

The trouble with such instrumentalism is that its use of theories is not scientific. Nor does it advance the 'search for knowledge' in which, according to Popper (1974, p.226):

... we are out to find true theories, or at least theories which are nearer than others to the truth - which correspond better to the facts, whereas in the search for powerful instruments we are, in many cases, quite well served by theories which are known to be false.

True science, in Popper's view, leads to real discoveries, 'the prediction of *new kinds of events* (which the physicist calls 'new effects') such as the prediction which led to the discovery of wireless wave' (1974, p.117) In contrast, instrumentalism leads only to a very limited range of discoveries - those associated with 'the prediction of *events of a kind which is known*, such as eclipses or thunderstorms'.

It is through real discovery that true science *extends* the bounds of knowledge. Instrumentalism cannot do this. It does the opposite. Through defining the bounds within which its theories operate it necessarily *restricts* them. This not only reduces the area of applicability of its theories, it also immunises them from being refuted. This is *degenerative* rather than *progressive* for, as Popper says, 'it is only in searching

for refutations that science can hope to learn and to advance' (1974, p.226)

Agricultural Economics and Instrumentalism

Agricultural economists are not, in their usual work, Popperian in their use of theory. They use theory in an instrumental way, predicting events of a known kind. In that sense they are, in Popper's terms, applied scientists, like engineers and not like physicists. That does not mean that they need warrant the criticism that Popper makes of Kuhnian normal scientists.

There is no reason why the profession as a whole, or agricultural economists as individuals, should not maintain a constantly critical approach to the theories they employ. As good instrumentalists they can constantly, through empiric testing, seek to determine the bounds within which their theories are valid. This will have two consequences. Firstly, through their being aware of the restricted range of validity of their theories they will not try to extrapolate beyond the bounds within which they have been proved to work. That is, they will not over-generalise their predictions. Secondly, through recognising that the theories are bounded in their applicability they will become open to the use of alternative theories. That is they will not be dogmatic, but like good Popperian pure scientists will maintain a critical; and questioning approach to their theories.

One side effect of adopting such a critical, some would say agnostic, view to theory would be that agricultural economists might lose much of whatever influence they now have with politicians. This is because, for politicians, tentativeness and uncertainty are anathema. If, however, through their newly developed humility and tentativeness, agricultural economists were able to influence policy makers also to take a critical approach to their knowledge, they will have achieved much. For, as Bertrand Russell pointed out more than 70 years ago (p.27):

In science, where alone something approaching genuine knowledge is to be found, men's attitude is tentative and full of doubt.

In religion and politics, on the contrary, though there is as yet nothing approaching scientific knowledge, everybody considers it *de rigueur* to have a dogmatic opinion, to be backed by inflicting starvation, prison and

war, and to be carefully guarded from argumentative competition with any different opinion. If only men could be brought into a tentative agnostic frame of mind, nine-tenths of the evils of the modern world would be cured.

Conclusion

Popper set standards by which science can be judged. He provided a methodology whereby errors can be eliminated. However, Popper's scientific method is not currently being applied within the profession of agricultural economics. Possibly it never can be. But that is not, necessarily, to condemn the profession. There are other tests by which a social science can be judged. Especially relevant is the effect of its policy recommendations on the nature of society itself. If the recommendations of agricultural economics can be shown, not in prospect but in outcome, to have led to the creation of a better society, however defined⁸, then it is irrelevant whether it is a 'science' or not.

Agricultural economists may not be justified in calling themselves Popperian scientists, but there is no reason for them turning their backs on his approach to seeking after knowledge⁹. If we follow it, we will have to take a far humbler view of our profession and our knowledge than we may have in the past. In particular, we will have to admit that all of our knowledge can be held only tentatively. Much of it might be wrong. And especially we must be prepared to admit that policy and other recommendations made on the base of our working hypotheses may be wrong. As we proceed in such a spirit, we can echo Popper's words (1974, p.129):

We must carry on a certain tradition. From the point of view of what we want as scientists - understanding, prediction, analysis, and so on - the world in which we live is extremely complex. I should be tempted to say that it is infinitely complex, if the phrase had any meaning. We do not know where or how to start our analysis of this world. There is no wisdom to tell us. Even the scientific tradition does not tell us. It only tells us where and how other people started and where they got to. It tells us that people have already constructed in this world a kind of theoretical framework - not perhaps a very good one, but one which works more or less; it serves as a kind of network, as a system of co-ordinates to which

we can refer the various complexities of this world. We use it by checking it over, and by criticising it. In this way we make progress.

⁸ Admittedly, there is always the problem of defining what is a 'better society'. Equalitarians believe that a more just and equitable society is better than a less just and equitable society. Others believe that a better society is one that produces more, irrespective of its internal distribution. In either case, the test may still be applied.

⁹ A reviewer (W.A. Stent) has written:

'Popper's scientific methodology was ahead of its time. I believe that it still is ahead of its time, and probably will be always. This is because it demands us to be comfortable (but not complacent) in our lack of understanding. It also demands us to be genuinely objective. This humility and almost mechanical objectivity is against human nature, and scientists (including agricultural economists) are humans.'

This is not to say that individual scientists cannot be true Popperian scientists: these individuals will always be on the fringe of scientific societies, but are fundamental to the progress of science.'

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