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Scientific Thinking without Scientific Method: Two Views of Popper

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> Popper almost alone, and alone in our century, has claimed that criticism belongs not to the *hors d'oeuvre*, but to the main dish.
>
> Agassi 1968, p. 317

> The importance lent to the falsifiability criterion and the demarcation problem by Popper and others distorts his thought.
>
> Bartley 1968, p. 43

> The idea that science can and should be run according to some fixed rules, and that its rationality consists in agreement with such rules, is both unrealistic and vicious. It is unrealistic, since it takes too simple a view of the talents of men and of the circumstances which encourage, or cause, their development. And it is vicious, since the attempt to enforce the rules will undoubtedly erect barriers to what men might have been, and will reduce our humanity by increasing our professional qualifications.
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> Feyerabend 1970, p. 91

There are two views of scientific thinking attributed to Karl Popper. The more popular among economic methodologists is not very challenging and to be useful requires only a minor adjustment to commonly held views. The less well known view considers Popper's theory of science to be revolutionary and extremely challenging and requiring a major change in attitude toward science and scientific thinking. In this paper I will explain the nature of these two views and their implications for the study of economic methodology. Also I will examine why there are two different views and why one is more popular than the other. Above all, I will try to explain why I think the less popular is the more important.

THE POPULAR POPPER

In economics, the popular view of Popper's philosophy of science is due primarily to Mark Blaug. It is the view usually known as 'falsificationism'. According to this view, scientific thinking is distinguishable from non-scientific thinking by merely noting that scientific theories are falsifiable and non-scientific theories are not. Popper's view is explicitly distinguished from a competing earlier view where scientific theories were distinguished from metaphysics using a criterion of empirical verifiability. Where scientific theories were claimed to be empirically verifiable and thus meaningful, metaphysics was alleged to be non-verifiable and thus not meaningful. In the 1930s Popper explains the earlier view by claiming that the old distinction was designed to solve a problem of demarcating science from metaphysics. Popper then argues that the earlier view's solution is inadequate. Since scientific theories are explanations, Popper argues that for reasons of quantificational logic, if science is to be characterized as empirical knowledge, verifiability cannot be used to identify scientific theories. Specifically, every explanation involves assumptions of a strictly universal nature (e.g., 'all swans are white') and strictly universal statements can never be verified with empirical observations. However, such statements can be refuted by observation (e.g., 'Today, I saw a black swan in the zoo'). Popper offers his alternative solution, namely, that the extent to which any science is empirical, its distinguishing characteristic must be its empirical falsifiability. Based on Popper's view, the history of science can be seen not as an accumulation of verified theories (since they are impossible) but as the evolution and vicissitudes resulting from the empirical overthrow of false theories. Scientific knowledge is then considered merely a residue of failed attempts to refute, or

more specifically, a collection of falsifiable but as yet unrefuted conjectural theories.

The practice of falsificationism

Based on his many observations about empirical falsifiability, many writers, both critics and friends, have saddled Popper with a 'Popperian methodology' that he is presumed to be prescribing for practicing scientists. Usually, it says that scientists should (1) consider only falsifiable explanations, (2) limit scientific activity to trying to falsify existing explanations, and (3) accept those explanations that have been tested but have so far not been falsified. Some argue that this so-called methodology is not necessarily a prescription, it is better considered a hopeful description of scientific practice.

Armed with the criterion of falsifiability, Popperian methodologists are thought to be engaged in an ongoing process of appraising past and present economic thinking using this Popperian methodology as the standard. For example, a minimum condition for any theory to be considered a possible contribution to scientific knowledge is that it be empirically falsifiable. Thus, this type of Popperian methodologist is always on guard to root out and prosecute anyone who does not display a concern for falsifiability. Of particular concern are both ad hoc adjustments that attempt to overcome refutations and 'immunizing stratagems' designed to protect favourite theories from premature refutation.

Surely, anyone who thinks methodology must be prescriptive will not be satisfied with the nihilism and negativity that is being attributed to Popper. From the perspective of economic methodology, falsifiability by itself is no more challenging than Paul Samuelson's version of operationalism (recall that for an economic proposition to be meaningful Samuelson requires only that it be 'refutable in principle'). Furthermore, according to some observers, if methodologists can tell the scientist what not to do, should they not also be able to give some positive advice? Surely, it is easy to think that those who actively engage in refuting one theory are doing so only because they have an alternative theory in mind. All that we need are criteria to allow us to make a rational choice between competing theories.

According to the popular view of Popper, falsifiability is nothing more than one of the many criteria used to choose the best among competing explanations. Perhaps, as some say, it is the best criterion. In this sense, it would appear that Popper is offering advice to choose the most falsifiable, try to test it, reject it if it fails the test and then move on to test the next most testable theory. In this way one could see the history of science or economics as a sequence of conjectured theories offered as explanations of observed phenomena but which when empirically rejected are replaced by another conjecture. The only question here is whether the popular Popperian view of scientific method captures the widely believed view of the history of science, namely that since the time of Issac Newton there has been a stable and continuous accumulation of scientific knowledge.

Falsificationism and the history of science

It was not obvious that Popper's so-called falsificationism could ever provide an adequate explanation for the apparent stability of science until Imre Lakatos came to the rescue. Lakatos presented a version of falsificationism that substitutes what he called scientific research programmes for singular theories. Lakatos explained why there can be continuity and stability while at the same time recognizing that the business of science involves conjectures, testing and refutations. History according to this view is a sequence of theories and models designed to carry out the research programme. While the research programme may change very slowly, there can be numerous conjectures and refutations of specific models and theories. The task for any historian of thought would seem to be the identification of those aspects of a programme that do not change and those that do. Apart from indentifying which programmes rely on immunizing stratagems and which do not, falsification does not seem to play a big role in the explanation of any research programme. While historians of science and economic thought have found the notion of a research programme useful – since it may give them something to do while analyzing

and modelling various research programmes – there does not seem to be much for a so-called falsificationist methodologist to do.

THE SOCRATIC POPPER

There is a very different view of Popper's theory of science that is not well known in economics. In this alternative view, falsifiability plays a very minor role. Moreover, this view does not take for granted that the history of science is one of stability and progressive accumulation. Popper's theory of science emphasizes that science is embodied in a process which is not at all choice or endorsement but rather criticism or rejection. Theories are rejected because they do not meet available criticism – for example, the criticism may include empirical data that is thought to conflict with the theories. Where many traditional philosophers prior to Popper equated science with rationality and rational choice, Popper emphasizes the critical role of rationality. Briefly stated, science for Popper is a special case of Socratic dialogue, namely, one where we learn with the elimination of error in response to empirical criticism. Rationality is critical debate – with the emphasis on debate. Popper sometimes calls this Critical Rationalism. Given its emphasis on Socratic dialectics, I will call this view the Socratic Popper.

Problem orientation and situational analysis

In his early work Popper openly employs a problem orientation as is evident in his promotion of what he called the Problems of Demarcation and Induction. Popper both offers and recommends a problem orientation to facilitate the emphasis on criticism. It is important to recognize that the problems he identifies are tools which he has manufactured to explain past events or theories. One must be careful when reading Popper not to confuse the message with the medium.

Socratic Popperians stress the centrality of problems. Specifically, to understand any economist we have to know his or her problems. Consider for example one of the favourite topics of historians of economic thought, namely the question of whether some particular idea is novel. It is not enough to indicate that the idea is or was new but according to the Socratic Popper, one would want to show that it is a solution to some problem. But the new idea may not have been introduced to solve the conjectured problem literally. That is, the problem orientation is a heuristic. Every invention of an idea can be seen *post hoc* to solve a problem or answer a question. Said another way, there may not be an answer for every question but there is a question for every answer and similarly, there may not be a solution for every problem but there is a problem for every solution.

While problem orientation is central to Popper's view of science it is also important to recognize how it is based on his view of rationality. When examining the contribution of an economic thinker, problem orientation always involves presuming that the thinker was implicitly or explicitly trying to solve a problem: achieving his or her aims by overcoming or dealing with all relevant obstacles. This orientation, sometimes called situational analysis, is second nature to every neoclassical economist. Consider the textbook consumer. A neoclassical economist sees the logic of the situation for a consumer to be one where the aim is utility maximization but the consumer faces the constraint of a limited budget as defined by available income and existing prices. The only difference for Socratic Popperians is that they would say that the economist sees the consumer attempting to solve a choice problem. But it is important to keep in mind that problem orientation is always retrospective. The consumer has already made a choice and the economist *post hoc* tries to explain how the choice was made. For example, the consumer is usually thought to be facing a limited budget and psychologically given preferences. The budget defines what can be afforded and preferences enable the consumer to compare any two alternative decisions, and specifically to determine which is better. When the consumer is deciding how much of two goods, say A and B, to buy with that budget, he or she is thought to consider every possible bundle of quantities (where a bundle consists of a pair of quantities, one for each good). If the consumer chooses to spend the entire budget, then certain trade-offs must be made. The consumer is thought to have tentatively picked one affordable bundle and then considered a

second bundle which has one less unit of A and comparing how much more B could be purchased and whether the additional amount of B leaves the consumer better off or not. If the second bundle is better, the consumer is presumed to switch to that second bundle and then to use it as a basis of the next comparison – one might see this as a trial and elimination process. The consumer is thus thought to have solved the choice problem by determining which of many possible affordable bundles is better than any other affordable bundle. The economist's explanation thus explains why the consumer chose the bundle in question and why all other bundles would not have been chosen (i.e., all other bundles are either inferior according to the preferences or not affordable according to the limited budget, or both). Presented this way, it should be easy for everyone to understand Popper's problem orientation.

Practicing Socratic Popper

As a few writers in economics have recently noted, the essence of Popper's view of science is a matter of embracing a 'critical attitude'. While this is true, it somewhat misses the main point. The main point is that, as Socratic dialogue and critical debate, science is based on non-justificationist rationalism. Some writers think Popper is saying merely that it is impossible to justify one's beliefs. If their view were true, it would be saying that Popper is merely offering us his form of skepticism. The reason usually given for this interpretation of Popper is that he says that he has an unambiguously negative view of what can be called the problem of justification (i.e., the problem of providing a justification for any knowledge claim). What Popper is most negative about is the *necessity* to solve this problem. (Unfortunately, Popper insists on declaring some of his rejections of the necessity of solving specific problems to be 'solutions' of those problems – e.g., the problem of induction).

The practice of a Popperian methodologist who follows the notion that science is Socratic debate will differ considerably from the activities of those methodologists who see themselves as Popperian falsificationists. Methodologists who follow the Socratic Popper will devote most of their time to fostering and encouraging criticism. Problem orientation is the most popular approach. Using situational analysis, they will provide explanations of existing criticism and critiques, usually by indentifying a problem for which existing solutions are inadequate or are in dispute. If there is any appraisal activity, it will be limited to the effectiveness of existing lines of criticism. For example, one might attempt to determine the most effective way to criticize Friedman's instrumentalism or try to explain why criticizing the maximization hypothesis might not be given a sympathetic hearing by neoclassical economists.

Learning and Socratic dialectics

The Socratic aspects of Popper's view are most evident in his claim that people learn from their errors. In Popper's terms, this is not only a process of trial and error, but a process motivated by rational criticism and not by the pursuit of a rational justification. Non-justificational rationalism says that the rationality of a debate or an argument does not guarantee its truth status. More important, the combination of trial-and-error with the absence of guarantees means that science is inherently unstable.

To say that science is Socratic dialectics begs an explanation of the nature of Socratic dialectics, at least with reference to learning. My view is that Plato's early dialogue 'Euthyphro' is a perfect case study. Recall that in this dialogue the situation is that Socrates is on his way to his famous trial for impiety and he encounters Euthyphro who is on his way to a trial where he is prosecuting his own father for impiety. As I see this dialogue, Socrates is attempting to deal with a problem: he does not understand why he is being tried for impiety since by his understanding of piety he has committed no crime – Socrates' understanding may be erroneous but Socrates cannot find the error. Now, Euthyphro is obviously an expert on matters involving piety and impiety – if for no other reason, only an expert would prosecute his own father. So, in this dialogue, Socrates is the student trying to learn from Euthyphro the expert. The dialogue proceeds by Socrates presenting his understanding of piety and impiety and inviting Euthyphro to point out where

Socrates is in error – after all, if Socrates' understanding were correct he would not be seen to be guilty. Socrates wishes to learn where he is in error and thus lays out his understanding, step by step. Unfortunately, at each step Euthyphro agrees with Socrates – consequently, if there is an error in Socrates' understanding, Euthyphro failed to find it. At the end, Socrates invites Euthyphro to restart at the beginning but Euthyphro declines. Thus, while there was the perfect opportunity to learn – discovering one's error – Socrates failed to learn anything. For my purposes, Plato's 'Euthyphro' illustrates all of the major ingredients of Popper's theory of learning. Trying to learn from discovering error, inviting criticism in order to learn, putting one's own knowledge at the maximum risk in doing so, and demonstrating the absence of guarantees. Of course, it is important to emphasize that the person who wishes to learn asks the questions.

My interpretation of this dialogue is not universally accepted. I have been publicly criticized for not realizing that Socrates is the teacher and Euthyphro is the student and thus this dialogue cannot illustrate what I claim is Popper's theory of learning – discovering the errors in one's knowledge. My critics say that it is obvious that Socrates is trying to show Euthyphro the shortcomings of Euthyphro's assumed knowledge of what is pious and impious. My critics say that Socrates leads Euthyphro into a circular argument to convince Euthyphro that his understanding of piety and impiety is inadequate. But Socrates fails and thus Euthyphro does not learn. That there is a failure in learning here we all agree. But my critics claim that the evidence that learning did not take place is that Euthyphro did not see that his knowledge was in error. But as can be plainly seen, my critics invoke Popper's theory of learning in order to claim that Euthyphro did not learn! So either way one interprets this dialogue, it would appear that it does illustrate that one learns from discovering one's errors and fails to learn when errors are not uncovered. And either way, it illustrates the absence of a guaranteed outcome. What my interpretation captures but my critics' does not is why Socrates would go to the trouble of asking questions of Euthyphro in the first place. The motivation is that Socrates recognizes that his problem is one that Euthyphro might be able to solve. In other words, Socrates wishes to learn and that is why he asks the questions. By either interpretation, Plato's 'Euthyphro' provides a good metaphor to help understand Popper's view of the process of science; namely, science is critical theory without a method that can guarantee a desired outcome.

Science in flux

Apart from the recognition that even though Socrates follows his usual method of learning, success was not assured, the Euthyphro dialogue may not be the best way to bring out the revolutionary aspects of Popper's view of science. Another way to appreciate why Popper's view is revolutionary would be to consider the difference between how the relationship between rationality and science was viewed before and after Albert Einstein.

Looking as far back as the 18th-century one can find people who commonly believed that if science is rational then it is stable. Rationality provides universality and universality provides stability. The key point here is that a minimum requirement for an argument to be rational is that *everyone* who accepts the truth of its premises must by both the force and definition of logic accept the truth of all validly inferred conclusions from those premises. Universality is provided by the fact that this is true for *everyone* who accepts the assumptions. When we also realize that people once thought that rational proof included infallible inductive proof, that is, proof based only on undisputed observations, there would be very little room for disagreement and hence for instability. Today, the task of the philosopher or historian of science is more often thought to involve explaining the success of science and thus there is even less room to see instability in science.

Throughout the 19th century, the most obvious evidence in favour of this equation between a rational science and a stable science was Newton's mechanics. But at the beginning of the 20th century, Einstein's theories challenged the adequacy of Newton's theories and of inductivist scientific method and openly demonstrated that science is fallible. That is, the success of science is not necessarily the result of an infallible scientific method. Moreover, recognition of a fallible science meant that a rational science cannot assure a stable intellectual foundation on which so

much of Western culture depends. In this regard, then, Popper's view is revolutionary since it is probably the first to deal with the post-Einsteinian reality of science. According to the Socratic-Popper view, science should be seen to be a process which is potentially in a state of constant flux rather than one which establishes incorrigible stable truths. There are no infallible methods, no authorities and no unquestionable facts. Science is scientific thinking without scientific method.

POPPER'S SEMINAR AND THE HIJACKER

During the 1950s Popper generated a group of self-declared disciples by means of his 'Tuesday Afternoon Seminar'. Popperian style seminars are notorious. There is much criticism, tension and above all constant interruptions. Nothing is to be protected from criticism. The rule seems to be, as noted by J.O. Wisdom, 'thou shall not speak while I am interrupting!'. Students and participants who can handle all the tension, as well as the shameless disregard for the traditional rules, will usually find such seminars very stimulating and productive.

Since Popperian seminars are almost exclusively concerned with learning and criticism, participants are warned at the outset to 'leave their toes outside the door'. That is, participants should not take criticism personally because if they do they limit their own opportunities to learn. Even when this warning is heeded, Popperian seminars often run into difficulties. Students unfamiliar with the medium will often start looking for the rules and methods required to conduct a successful seminar and tension begins as soon as it is pointed out that there are no such rules or methods other than 'everything is open to question'. Interestingly, such difficulties are virtually the same ones which Popper has faced in his struggles with the entire philosophy profession – which for most of the 19th century had been built on the presumption of a reliable method that would guarantee success.

Some of the early disciples of Popper and his seminar were Wisdom and John Watkins. Joseph Agassi joined the group at the beginning of 1953 when Paul Feyerabend was about to be Popper's assistant. When Feyerabend left for Vienna, Agassi became Popper's assistant. Assistants often were put in charge of constructing indexes for Popper's books. Ian Jarvie attended the seminar as an undergraduate. William Bartley joined Agassi in the seminar and somewhat later Agassi brought Imre Lakatos. With the exception of Lakatos, all of them were Popper's devoted disciples, particularly with regard to Popper's constant complaints that he had not received the recognition he was due in the philosophy profession.

The disciples were united in their appreciation for what I am calling the Socratic version of Popper's philosophy of science. Criticism and problem orientation are essential to learning and understanding. Some of the disciples thought they understood this well enough to put Popper's views into practice – they even ventured criticism of Popper's views. Their efforts have led to much acrimony, sometimes at the level of soap-opera.

The all-consuming situation in the early 1960s was that while there was a rapidly growing interest in the philosophy and history of science, the name most often mentioned was not Popper's but that of Thomas Kuhn. Everyone in almost every discipline seemed to be discussing Kuhn's 'paradigms'. Some of the disciples claim that Lakatos took advantage of the situation and, in effect, hijacked Popper's seminar. Supposedly, Lakatos convinced Popper that the desired recognition could be obtained by recasting Popper's views in a form closer to Kuhn's. Thus Lakatos and Popper made much more of the growth of knowledge implications of Popper's view and much, much less of the Socratic dialectical aspects which the disciples advocated.

POPPER'S DISCIPLES VS POPPER AND THE HIJACKER

While some may wish to argue about which version of Popper's philosophy of science is the 'true Popper', I think it is more important to recognize that there is more than one view. But why are there two views? What are the sources of the arguments or disagreements? Is Popper's at fault or his followers?

Admittedly, Popper's recommended method of criticism can itself be the source of disputes. When criticizing a writer's views, Popper insists on a problem orientation whereby the critic must

present the writer's problem and solution but only after making every effort to present the writer's views in the most sympathetic light. That is, the critic must make all unchallengeable improvements that can be made before launching the criticism. One would not wish to distract the debate into irrelevant side issues. In effect, the criticism must be conducted in terms that the writer can accept. This sympathetic problem orientation very often leads Popper to lean backwards to grant as much as possible to the criticized writer and this in turn leads readers to miss the rhetoric and thus to misunderstand Popper's own views.

Popper's Tuesday Afternoon Seminar itself is probably the major source of disagreements. In the early 1960s some of its participants, such as Agassi and Bartley, began publishing criticisms of Popper. The complaints from Agassi and Bartley seem to be based on apparent inconsistencies between what they thought Popper preached or practiced in this seminar and what he said in his writings. Those of us who never attended the famous seminar are left only with the views Popper expressed in his writings. And if one is not aware of his sympathetic problem orientation, it is all too easy to see inconsistencies where they do not exist.

Popper's writings do not seem to stress the importance of criticism nearly as much as his disciples claim his seminar did. The participants in the seminar equate Popper's view of science with what I have called the Socratic Popper. It is not surprising then that when Lakatos developed what he called the 'methodology of scientific research programmes' as his version of Popper's view of science, the other members of the Seminar were very critical. Bartley claimed that Lakatos added nothing of importance to the philosophy of science other than a few catchy phrases. Agassi claims that Lakatos did not know enough about the philosophy of science to make his pronouncements worthwhile. While almost everyone says that Lakatos made significant contributions to the philosophy of mathematics, the disciples routinely claim that Lakatos did not understand Popper and the 'methodology of scientific research programmes' of Lakatos does not represent the views of Popper. Morever, they say, Lakatos misled Popper into a pursuit of fame at the expense of integrity, that is, at the expense of throwing the Socratic baby out with the inconveniently unmarketable dirty bath water.

THE POPULAR POPPER VS THE IMPORTANT POPPER

The major question to consider is why so much is known about the Lakatos version of Popper's philosophy of science and so little about the Socratic Popper promoted by Popper's disciples. An obvious reason is that the popularly accepted version of Popper's view allows one to see Popper as a philosopher making only minor improvements in the ordinary view of science. The ordinary view is that science is a stable enterprise and its stability is based on the avoidance of irresolvable questions such as those concerned with the absolute truth of scientific theories. After all, scientific theories cannot be proven true but only false. But Popper warned that the ordinary view allows any refutation to be avoided by refusing to accept the refuting evidence. Popper's disciples call the ordinary view 'conventionalism' since according to conventionalism, theories are not to be considered true in an absolute sense but only in a sense where a theory is 'true' as defined by the conventional notions of truth. Typically, a probability calculus is substituted for an absolute notion of truth status. According to the ordinary view, it is rational to accept a theory with a high probability of being true (given currently available empirical data) and to reject any theory with a lower probability. The issue thus is not one of truth status but one of *rational* acceptance by a community of scientists.

Rationality and conventionalism

There is much more to the ordinary view than its foundation of conventionalism. While the notion of rationality underlying conventionalism presumes science is rational, the presumption of rationality implies that any belief in a scientific theory can be proven (i.e., justified) – at least to the point of demonstrating its logical consistency with conventional acceptance criteria. This is definitely not the non-justificationalist notion of rationality presumed in the Socratic version of

Popper's view. But there is even more to conventionalism. An essential notion is that in science one strives to be able to choose the best theory from competing theories. Moreover, it is presumed that the criteria used in science are the best criteria.

While it may be difficult for followers of the popular Popper to see why anyone might strongly object to the commonplace notions of conventionalism, there are obvious reasons for why the followers of the Socratic Popper strongly reject conventionalism. It would be difficult to see how Socratic dialectics could be seriously pursued whenever it is allowed that one can always defend one's position by claiming that one's theories are not to be considered absolutely true but only the best available. The conventionalist defense that relies on the substitution of 'best' for 'absolutely true' seems to beg many questions. The most obvious question is whether the criteria that define 'best' are themselves really the best – such a question leads to an infinite regress, of course. Given the inherent possibility of avoiding contradictions with facts by denying the intended truth status and the impossibility of avoiding an infinite regress whenever rational acceptance is considered a substitute for truth status, how could one ever engage in a Socratic dialogue?

Conventionalism and the stability of science

According to the ordinary view of science, the everyday business of a methodologist seems to be either confined to a linguistic analysis of what economists say in their explanations or limited to a historical description of how particular economists reached their conclusions. Of course, there has always been room for a methodologist to make grand claims. Today, however, it would seem that moderation in methodology is much more common. Moderation may be the consequence of a certain complacency which also exists today. In terms of the alleged stability of science, there is an obvious consistency between stability and the presumptions of the ordinary view. Specifically, if everyone practiced conventionalism, the chances of a 'science in flux' would be very unlikely. It is very difficult to push on something so soft and forgiving. According to the ordinary view, Einstein's views can easily be seen as mere adjustments, such that Newton's views are viewed as a special case. In economics, Keynes' view need not be considered revolutionary but merely a special case of general equilibrium analysis. Of course, in economics there continues to be a problem of providing the micro-foundations of Keynesian macro-economics which would prove that Keynes was not a revolutionary.

Interestingly, it is Kuhn's conception of a paradigm that seems to capture the essence of the ordinary view of science. But Kuhn goes further to say (in a verbal response to Lakatos at the 1970 Boston AAAS meetings) that what makes science scientific is that the scientific community is made up of scientists imbued with a scientific mentality! I am not sure Kuhn's elaborated psychologistic view, if widely known, would be widely accepted. Nevertheless, the ordinary view does see science and scientific knowledge as an entity on a historical continuum. Revolutions are rare and ordinary science is more a question of day-to-day puzzle solving. It is difficult to see how we could have the current textbook-based education system without Kuhn's view being correct. It is exactly the textbook-based education system that presents an overwhelming obstacle to the appreciation of the Socratic version of Popper's view of science that the disciples promote.

Understanding Socratic Popper

The followers of the ordinary view have considerable difficulty in understanding the disciples' alternative view of Popper. This difficulty needs to be explained and understood. The situation is very complex. As can be seen above, there are differences concerning theories of rationality, the history of science, the necessity of a scientific method, the nature of dialectics, and above all the presumption that all true knowledge can be justified.

The presumption taken for granted by all followers of the ordinary view says that we would have to justify our knowledge before we can claim to know anything. There is widespread fear that without a method which will assure that only true knowledge claims will be justified, we

would have to give knowledge claims of mysticism, fundamentalist religions, and similar 'unscientific' disciples an equal status with science. There is nothing in the Socratic version of Popper that would overcome this fear. But more important, the disciples claim that this fear could never be overcome. Proponents of the popular falsificationist Popper, however, think the requirement of falsifiability is a sufficient prophylactic. It may be sufficient but the disciples claim that it too often rules out potentially scientific notions that happen not to be at the time in a form that is falsifiable. And besides, some aspects of science such as metaphysics may not be falsifiable but they are essential. In one sense, every theory that is designed to explain observable events is an application of a particular metaphysics. After all, one cannot explain everything at once. Something must be assumed. For example, in neoclassical economics, every theory or model will assume that the decision-maker is an optimizer even though it is virtually impossible to refute this assumption. This is because the neoclassical decision-maker is presumed to maximize *something*. Since the 'something' does not always need to be specified, it is difficult to define what would constitute a refutation of the assumption of maximization.

For many centuries, rationality was viewed as a stable and reliable means to convince everyone that one's view was true, that is, to justify one's knowledge by means of irrefutable logical proof. Since the time of David Hume, the ability of rationality to deliver on this promise has been in doubt. Moreover, it is against this promise that some of Popper's disciples argue that rationality is better understood as a means of criticizing. Criticism is built upon discovering logical contradictions. After all, an empirical refutation is merely a contradiction between the theory and the available empirical data (i.e., it cannot be that both are true). Except for tautologies, rationality does not guarantee that one's knowledge is true but rationality can be a means of proving that one's knowledge is false. This asymmetry parallels Popper's distinction between verifiability and refutability. Every argument consists of (two or more) assumptions and at least one conclusion which is claimed to be true whenever all of the assumptions are true. In terms of rhetoric, it would be better to say the conclusion is true whenever one accepts the assumptions as true. In one sense it could be claimed that the conjunction of the assumptions forms a justification of truth of the conclusion statement. But, the justification is conditional on the actual truth of the assumptions. Thus, such a justification is always open to question. From a non-justification standpoint, the argument is a means of criticism. For example, if one accepts all the assumptions as true then one cannot at the same time accept statements which contradict any valid conclusion based on those assumptions. Specifically, if one had a consumer theory that said that the demand curve for a good is downward sloping when certain conditions are met, then if those conditions are met and the assumptions are all accepted as true, one could not at the same time claim to accept the existence of so-called Giffen goods. So, rationality may still retain its universality and ability to convince but the disciples argue that the ability may be limited to criticism and refutations.

The widespread presumption that rationality-based science is a successful stable enterprise is denied by Popper's disciples. Nevertheless, since the presumption is so widespread, they cannot completely ignore it. Some of the disciples claim that the history of science appears to be stable only to those who wish to ignore the impact of Einstein's overthrow of Newton's mechanics. In the 1950s when I was a high-school student, some science textbooks led one to think that there existed an infallible scientific method which if followed step by step would lead to the establishment of a scientific law. The first step was the collection of data. The second was the formation of an hypothesis to explain the collected data. The third step was the formation of an experiment to test the hypothesis. If the hypothesis passed the test, the hypothesis was declared a theory. If the theory passed the tests of all other scientists, then one's theory would become a law! While today's atmosphere of moderation would not be so optimistic, the old textbook writers were quite confident. The basis for their confidence was their belief that the success of Newton's physics was sufficient proof that such a method existed and it worked. What is most disturbing for Popper's disciples is the presumption that any success in science must be due to a practiced scientific methodology. Again, the disciples take the view that methodology has no more guarantees than a Socratic dialogue. Unfortunately, proponents of the ordinary view of science seem to want more.

The foundation of the belief in the stability and reliability of science has always been a belief in the universality and certainty of a scientific method. When it turned out that Newton's mechanics failed under certain conditions, believers in scientific method chose to switch rather than fight. Specifically, they held to a view that still claimed there was an infallible method but switched to say that it never was a method for proving the truth of scientific theories but only a method for choosing the best from existing competitors. So when Einstein or Popper claim that theories are either true or false, believers in the existence of an infallible scientific method are at a loss about what to do. They still wish to believe that scientific knowledge has been accumulating in a positive, progressive and stable way. Thus, it is easier to soften the goal of science so as to maintain a belief in Newton's positive contribution than have to admit that Newton's theory is somehow false. It could be argued that the softened version of scientific method not only lacks guarantees but it also lacks a purpose other than possibly to apologize for Newton's failure.

While the ordinary view sees a scientific method providing a stable and certain science, Socratic dialectics lacks guarantees, as illustrated with the Euthyphro dialogue. And while the softened version of scientific method also lacks guarantees, at least Socratic dialectics promotes a potential for learning. The potentiality is mostly due to Socratic dialectics maintaining that theories are true or false (rather than better or worse). But by promising only potentiality while requiring that theories be absolutely true, we face a dilemma. One the one hand, since the softened version of scientific method promises very little, success is easily achieved. On the other hand, while profound learning is possible with Socratic dialectics, it may take a long time. It is always possible that by engaging in a Socratic dialogue one might discover monumental truths, but more often the dialogue is one like Euthyphro. Perhaps, only one of a hundred dialogues are productive. For methodologists in a hurry, dialectics do not seem to be a promising endeavour.

THE FUTURE OF POPPERIAN ECONOMIC METHODOLOGY

Kuhn's view of science presents a very comfortable (albeit, dull) picture of a science of hard working and level-headed scientists who rarely if ever stage a revolution. The Lakatos view appears less dull but that may be due merely to its spicy language of 'hard cores' and 'protective belts'. Both views seem to provide a clear picture of a stable science. If instead of following Kuhn or Lakatos we were to follow the disciples' version of Popper, then the picture would be much less clear. What is clear is the disciples' rejection of the substitution of a probability calculus for truth status. According to the disciples' Socratic version of Popper, theories are either true or false. With such a severe stance regarding the truth status of theories it would seem that science would always be in a state of rapid flux, possibly even chaos. So how do the disciples deal with the commonly accepted view that science seems to be rather stable?

Explaining stability away

In economics, the obvious example of a well-developed and stable research programme is neoclassical theory which in terms of its basic ideas (i.e., the principles of economics) has not changed much in the last hundred years. With this programme in mind, the Socratic-Popper view of science would seem to be of limited use. Either the Lakatos-Popper view with its emphasis on a well-protected core or the Kuhnian textbook-based view would seem to be more appropriate. But their comparative advantage may be illusionary.

Why might an ordinary methodologist think the Socratic-Popper view of science implies a science in a constant state of rapid flux and chaos? The source of this supposition would have to be the ordinary view's notion that scientists are actively *choosing* among competing theories. According to the ordinary view, should any theory be refuted (i.e., proven false) there would then be an immediate switch to the next best theory. Such alternating refutation and theory switching would almost definitely see science in a state of flux. But the disciples say that while all theories are open to testing, a state of rapid flux or chaos is not a necessary outcome. There is nothing that forces one to choose any theory. One *may* choose to accept a theory that has not been refuted

by the latest test, but there is no reason for why we *must* make a choice. The fact that there is no reason to make a choice leads to a certain type of stability but this type of stability cannot be seen to be caused by the existence of a reliable method. It is certainly not due to the acceptance of a rationality designed to justify the currently chosen theory.

While the Socratic version of Popper's view would seem to imply a science that is in constant flux and turmoil, expecting such a state of affairs presumes too much. Most obvious is the presumption that since science is fallible it is easy to overturn. For any discipline to be rapidly changing it would seem to require all science teachers to be on the frontiers of knowledge developments. Since significant changes would involve challenging strongly held views (i.e., the accepted paradigm), peer review processes are unlikely to grant funds to someone whose views seem far out. While we give lip service to the notion that a Ph.D. thesis is to be not only significant but also original, any thesis that was completely original would be difficult to access on the basis of the currently accepted paradigm. Advances in any discipline are usually marginal because marginal changes are easy to understand. This notion of marginalism parallels Popper's views of social change and social policy which he calls 'piecemeal engineering'.

There are many reasons for the apparent stability of science in general and of neoclassical economics in particular. Foremost is the recognition that science is a social institution involving such things as educational institutions, research funding institutions based on peer reviews, textbook publishers and overall the constraining influence of the sociology of any scientific community. And we must not overlook the necessity for any theory or research programme to be based on some metaphysical notions that are purposefully put beyond question or are at best very difficult to test. What some disciples argue is that the apparent stability of any science is an intended consequence of decisions made within the scientific community. The stability, apparent or otherwise, is a social artifact and not in any way a logical property of an inherent nature of scientific knowledge.

The practicing Popperian methodologist

In this chapter I have tried to expose the reader to a view of Popper's philosophy of science that does not seem to be widely understood. Briefly stated, according to the Socratic-Popper view of science, criticism is the main course and falsifiability, situational analysis, critical rationalism all belong to the *hors d'oeuvre*.

In the process of writing this chapter I have acquired a new perspective on my own efforts at practicing Popperian methodology. I have known for some time that I have considerable difficulty communicating with those falsificationist methodologists who see their role as that of appraising various aspects of economics. I have also known that the difficulty is due to their believing that Lakatos correctly portrayed Popper's philosophy of science as falsificationism. It is clear to me now that things are much worse. It would appear that the followers of Lakatos are totally unaware of the disciples' view of Popper.

Socratic dialectics is central to Popper's view of science. Accordingly, science is critical debate. As with any debate, there is no foolproof method, no guarantees. Problem orientation is Popper's medium for conducting debates but it is not the central message. Situational analysis is only a convenient vehicle for interpreting the rationality of the problem situation but nothing more. Critical rationalism is a means of differentiating and precluding a justificational interpretation of the rationality of the problem situation but nothing more. In all of this, falsifiability is merely a logical condition required by critical rationalism. And rationality is essential but still it is only one aspect of criticism.

Since I started working in the field of economic methodology at a time before Lakatos began promoting his version of Popper, I knew only the disciples' version of Popper. In my work falsifiability plays at most a minor role. Until my 1992 book, which is explicitly about methods of criticizing neoclassical economics, I took the criticism-based Socratic-dialectical view of Popper for granted. In the 1980s I began encountering methodologists who equated Popper with a 'falsificationist methodology'. For example, some writers such as Blaug saw a virtue in this equation and others such as Dan Hausman said the equation represented a vice. In both cases, it

was difficult to figure out who this 'Popper' was they were discussing. Fortunately, in the 1990s things have begun to change. Judging by recent activities of Bruce Caldwell and Wade Hands, there are indications that the disciples' version of Popper might finally be receiving the attention it deserves in the field of economic methodology.

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