

## Kuhn vs. Popper by way of Lakatos and the Cold War by Lawrence A. Boland, FRSC

The idea of a debate between the historian of science (and would-be philosopher of science) Thomas S. Kuhn and the philosopher of science Karl R. Popper is not likely to be of an immediate interest to economic methodologists or historians of economic thought. Too bad. Steve Fuller's little book<sup>1</sup> – which is based on such a debate – offers much more and, I think, can be of great interest to economists.

Before Mark Blaug [1975] demolished Kuhn's historiography in favour of that of Imre Lakatos, it was not uncommon to see historians of economic thought promoting Kuhn's view of the history of science (e.g., Burt [1972]). It was and still is even more common in the other social sciences.

Fuller is a sociologist of science who is interested in the social role of historians of science. His first interest is that Kuhn's view is most commonly seen to be the prevailing view of the history of science – namely, that everyday science is not revolutionary science but “normal science”. Normal science is characterized by two distinctive attributes: puzzle solving and a standard textbook that enshrines the current “paradigm”. According to Kuhn, while revolutions have occurred in the history of science, they are rare and conform to a certain social structure. That structure involves two important factors: participants who are willingly non-aggressive puzzle-solvers and an institutional structure that rewards such participation. Fundamental criticism is discouraged and refutations (which Kuhn calls “anomalies”) are socially and personally accommodated by putting them on the shelf for later consideration. Only when the shelf is getting full – *and* there exists an alternative paradigm that can replace the embattled one – will the “revolution” take place. In other words, revolutions are more the exception than the rule since accumulation of many “anomalies” and the development of an acceptable alternative takes time.

Kuhn's rather conservative normal science was first presented in his famous 1962 book, *The Structure of Scientific Revolutions*. Given the word “revolutions” in its title, many saw it as a radical view of science that could be used to support ongoing efforts to revolutionize various social sciences. To followers of Popper this interpretation and its popularity seemed at best unfair. After all, Popper had been apparently successful in claiming that the business of science is constantly trying to overturn the current theories by aggressively criticizing, testing and hopefully refuting them. In Popper's words, science was a community devoted to “conjectures and refutations”. Moreover, many of his followers starting with Lakatos credit Popper with promoting the idea that what distinguished science from other intellectual efforts was that scientific efforts are falsifiable – the view that analytical philosopher's continue to call “falsificationism”.

Economists were probably the first to recognize Popper this way – starting with Terrence Hutchison's [1938] use of falsifiability to fend off the “barbarians at the gate” [Hutchison 1988] and later in the 1960s with the first edition of Richard Lipsey's [1963] famous textbook. But, unfortunately for Popper's followers, the invocation of the falsifiability test in economics is due more to Paul Samuelson's efforts to promote

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<sup>1</sup> Steve Fuller, *Kuhn vs. Popper: The Struggle for the Soul of Science*. Columbia University Press, 2004, 160 pp. Hardcover (0-231-13428-2)

mathematical model building without falling victim to claims that mathematical models are inherently tautological since tautologies are not falsifiable or even conceivably false [see Boland 1989, Epilogue].

Outside of economics, Popper seemed to have been completely ignored while at the same time other social scientists were extolling the virtues of Kuhn's radical view. Actually, Popper was more dismissed than ignored as he was seen to be a critic of the Marxist social theory that was so popular with many social scientists in the 1960s and 70s. They considered Popper to be a conservative authoritarian rather than the radical philosopher who was revolutionizing philosophy of science.

Fuller wishes to reverse this image. Now that the Cold War is over, perhaps social scientists should reconsider their understanding of Kuhn and maybe even Popper. Fuller's main concern is the mistaken image of Kuhn's view of science. It is not a radical or revolutionary view; it is a reactionary, "corporatist" view which is the view evident in the 1940s' and 50s' economics establishment that is detailed in Philip Mirowski's [2002] *Machine Dreams*. In economics, this view involves the growth of mathematical economics and operations research in particular. In the philosophy of science, it involves the role of science and scientists in society. Of particular concern to Fuller is that Kuhn's view of science is anti-radical, anti-revolutionary – even anti-Socratic.

According to Fuller, Kuhn is in the service of corporatism while Popper is its enemy. Fuller traces corporatism back to Wilhelm von Humboldt in the early nineteenth century and even to the twelfth century – let me quote this in detail as it is probably the most important history that, according to the Canadian historian-philosopher John Ralston Saul [1995], economists should wake up to. Fuller [2004, pp. 73–4]:

Popper follows in the footsteps of the philosophers of the 18th century Enlightenment. Their common starting point may be summarised in the following principle: *The price of acquiring any knowledge at all is that it will be somehow distorted by the conditions of its acquisition; hence, criticism is the only universally reliable method.* Theology was the original Enlightenment target for this perspective, in which the findings of mechanics and the natural history of animals and humans functioned as critical instruments. In this context, [this] 'critical-historical method' was a thoroughly *moral* activity....

The two centuries that separated Popper's methodology from the theology of Spinoza and Pierre Bayle witnessed the migration of the critical-historical method from the freethinking churches and salons to the university, where in the hands of Wilhelm von Humboldt (1767 – 1835), famed first Rector of the University of Berlin, it became the touchstone for rediscovering that institution's original corporate autonomy.

Here it is worth recalling that until the 12th century, Roman law divided human interaction into two basic categories. In exceptional cases, legal protection was granted to limited social engagements (*socius*), such as business ventures and military expeditions, the point of which was to achieve goals set out by the people involved in them. Mission accomplished, the partners reverted to their default category of existence as members of particular families (*gens*), which were the means by which

status and wealth were reproduced across generations. What had been lacking was a third category that would enable both individuals to acquire social identities other than the ones they inherited and collectives to pursue goals that transcend the interests of their current members. This third category came to be known in Roman law as *universitas*, which is best rendered as ‘corporation’, but contained universities among its earliest exemplars – along with craft guilds, churches, religious orders and city-states.

The revolutionary feature of the *universitas* was the legal recognition it gave to activities inherently worth pursuing by granting their practitioners a perpetual right to decide what counts as its worthy pursuit and who is worthy to pursue it. At last, humanity’s sociology decisively broke with its biology, since the individuals delegated with transmitting the corporate activity over time were not necessarily, or even usually, members of the same family. This innovation was luminous in the context of Christendom, which attached great significance to the liberation of the human spirit from its material captivity. Thus, legally protected lineages based on common mental training rather than common physical ancestors became the *via regia* of institutionalised spirituality, which in secular garb (as ‘credentials’) has come to be the principal means by which social status is now recognised.

Saul only traces corporatism back to late nineteenth century with Emile Durkheim and Max Weber, otherwise his view is in agreement with Fuller’s. For Saul it is a matter of how we define individualism – particularly, how it is defined in economics – and how the individual relates to society or specifically, how the individual is obligated to the “corporate structure”:

The reality of obligation, as it is presented to us today, is ... one of loyalty – that is, of obedience – to the corporatist structures. The origin of this deformed idea of obligation is relatively simple to trace. It goes back to the birth of the corporatist movement around 1870, when religious leaders and established hierarchical interests were looking for a way in which to accept industrialization while denying individualism and democracy. Their solution was to combine and restructure the old concept of the faithful servant of God and the dutiful subject of social authority in order to create the obligated subject of rational corporatist structures. [Saul 1995, p. 164]

He goes on to say [p. 169]:

Now the very essence of corporatism is minding your own business. And the very essence of individualism is the refusal to mind your own business. This is not a particularly pleasant or easy style of life. It is not profitable, efficient, competitive or rewarded. It often consists of being persistently annoying to others as well as being stubborn and repetitive. The German voice of the Enlightenment, Friedrich Nicolai, put it clearly: "Criticism is the only helpmate we have which, while disclosing our inadequacies, can at the same time awake us to the desire for greater improvement."

Criticism is perhaps the citizen's primary weapon in the exercise of her legitimacy. That is why, in this corporatist society, conformism, loyalty and silence are so admired and rewarded; why criticism is so punished or marginalized. Who has not experienced this conflict?

Corporatism is the realm of the “company man”, the no-man’s land for the whistle blowers – it is the domain of the neoclassical economic man. More specifically, it is the domain where the individual aligns his or her interests with the corporation – that is, he or she wishes to advance in the corporation by conforming to the needs of the corporation rather than be concerned with the needs of society. Saul blames economics departments and business schools for encouraging and promoting such behaviour.

What is important for academics is that such behaviour leads to conformity and non-innovation – except innovation that can get immediate rewards. It leads in science faculties to acquiescence with boring laboratory work, to a willingness not to seek overthrowing the system. It leads to Kuhn’s normal science.

What concerns Fuller most is that during the Cold War the corporatism embraced by Kuhn led scientists to put aside any consideration of the political or social consequences for their science work even though such work may have been only of benefit to the “military-industrial complex”. Despite some Marxist social scientists in the 1960s and 70s that were using Kuhn’s view of science as a guide for their revolutionary programs, in the natural sciences according to Fuller it was all too easy for scientists to use the conservatism of Kuhn’s normal science to justify what are possibly anti-social consequences – and moreover to avoid speaking out, or worse, criticizing.

At root, the debate and the differences between Kuhn and Popper was a matter of whether criticism is a necessary attribute for a functioning science. As the Popperian philosopher of science, Joseph Agassi observes [2002, p. 399], according to Sir Francis Bacon criticism “conveys contempt ..., to accept criticism is to admit weakness”. And so,

Kuhn did not speak of scientific error. Following his mentor, James Bryant Conant, he declared it unrealistic to expect people to have no prejudice..., he declared it obligatory to endorse the dogmas of scientific leaders. He saw science as a profession that makes great demands on its affiliates, yet he did not include among these the demand that they should respect rivals. Kuhn declared that science recognizes no rivalry. As a historian of science, he opposed concealing controversy; as a philosopher of science, he advocated suppressing controversy. This latter is neither possible nor necessary. Rather, we should all learn to argue in dignity. All that is needed are suitable procedures and sensible, skillful moderators. [p. 400]

Popper’s view of science (and the community of scientists) is easy to distinguish from Kuhn’s. Popper’s view is compatible with what Socrates practiced and advocated in Plato’s *Apology*. Socrates critically examined himself as he did of others – and, as he explained in *Crito*, he thought a life doing otherwise was not worth living. Obviously Socrates would not survive in a corporatist world or in Kuhn’s normal science.

The role of Kuhn’s mentor’s role in the development of “Big Science” should be of interest to economists. Specifically, Conant is recognized as “one of the main players in importing and developing the discipline of ‘operations research’ into the United States in the Second World War” [Mirowski 2004, p. 91]. But it is the relationship between Kuhn

and Conant that Fuller thinks is essential to putting Kuhn's view of science into proper perspective. The main issue is the relationship between Kuhn's conservative view of science and Big Science:

Overall, the Conant-Kuhn relationship is best characterised as an exchange in which each used the other for his own ends. The looming normative question is whether each considered why the other would want to use him as he did. By his own account, Conant was largely responsible for introducing the industrial division of labour model of scientific research from German to American academia in the 1920s, as chairman of the Harvard chemistry department. Conant was also fully aware that many excellent students like Kuhn who underwent scientific training at the start of World War II to pursue philosophical questions by low-tech means would be disappointed by the scaled-up specialised work of 'Big Science' that awaited them at the end of the war. [Harvard's] General Education in Science programme was created with them specifically in mind. There they could impart to students a vision of science that focused on self-directed cognitive change, with science's political-economic entanglements playing a distinctly secondary role. Conant reasoned that the more future policy-makers could see the hand of Maxwell or Einstein in an expensive and risky research project, the more likely science's autonomy would be preserved in its increasing involvements in the Cold War's military-industrial complex.

Kuhn, of course, wanted to promote much the same vision as Conant, but mainly because it captured his original reason for pursuing science as natural philosophy by more exact means. Moreover, Conant and Kuhn overlapped not only in their overall vision of science but also in at least one means of realising that vision, namely, the manufacture of student course materials to bring out what is now often called the 'internal' history of science.... What neither Conant nor Kuhn anticipated, or approved, was that their shared non-instrumental vision of science would be appropriated by humanists and social scientists, in part to relativise the nature of science to whatever a community of inquirers happens to agree as their "paradigm". [Fuller 2004, pp115–16]

Of course, science was not always "Big Science". As Agassi explains,

Traditionally, empirical science was a loose network of amateurs. In the scientific revolution, the network became voluntary groups.... They became prestigious clubs. They called themselves "the republic of science," "the commonwealth of learning." Change followed the American and French Revolutions, the subsequent secularization of some universities, and the industrial revolution. Technical universities appeared in the mid-19th century. Interest in science grew. Academies still ignored research. Until World War I, the chemical industry employed only a few researchers, and research institutes employed fewer. The military stepped in significantly only during World War II, and more so in the cold war.... Almost all of today's vast science-based industry came during the cold war. Kuhn's familiarity

with the social history of science did not stop him from portraying research as a profession linked to political power.... He even declared this “necessarily permanent”).

To identify profession with competence is to overlook incompetent professionals and competent amateurs, not to mention outstanding amateurs.... Kuhn’s concern was with prospective leaders. They must work hard and imitate top physics professors. These oozed authority and boasted top reputations (as well as security clearance). A lively passage in Kuhn’s book on the quantum revolution ... pictures young, hardly known Einstein visiting a famous university, the professor showing him respect, and the students realizing that he counts. All this reflects the new mentality of the cold war. Harvard University president Conant made new conditions for academic jobs. He demanded professional authority and political conformity.... [Agassi 2002, pp. 403–4]

So, corporatism was the rule and according to both Fuller and Agassi, Kuhn provided the needed philosophical foundation.<sup>2</sup>

Orthodox Popperians will object to Fuller’s characterization of Popper’s theory of science as that of “falsificationism”.<sup>3</sup> But to be fair, Fuller is discussing how people who learned about Popper by reading Lakatos. Even so, by primarily discussing just the Lakatos version of Popper’s theory of science, the important element of criticism that characterized Popper’s Socratic view of science<sup>4</sup> misses the opportunity to contrast Kuhn’s corporatism with Popper’s anti-corporatism. And to be fair to Kuhn’s view of science, the extent to which science was hijacked by the military-industrial complex means that his “normal science” characterizes how scientists today rightly or wrongly view their own sciences. And thus Popper’s promotion of his Socratic view explains why it is not the prevailing view of science by the participants themselves. Moreover, as long as corporatism continues to dominate the science community, Popper’s Socratic view will never be welcome. This does not rule out the acceptance of the requirement of falsifiability commonly attributed to Popper so he will still be recognized as is evident

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<sup>2</sup> It is unfortunate that Fuller sets this all up with a rather insignificant debate between Kuhn and Popper. A far more interesting debate took place between Kuhn and Lakatos at the 1970 Philosophy of Science meetings in Boston. Lakatos challenged Kuhn to explain what distinguished the science community from any other community. Kuhn was cornered into admitting that he thought the scientific community was scientific only because it was made up of people with a scientific mentality. For those who are familiar with chapter 14 of Popper’s *Open Society*, this is merely the inferior concept of sociology he called “psychologism”. This is not a philosophy of science. It is a sociology of science and a rather poor one from the Popper’s perspective.

<sup>3</sup> It is interesting that only after Lakatos entered the scene at the London School of Economics and began characterizing Popper as a “falsificationist”, Popper’s students did not identify themselves as “Popperian”.

<sup>4</sup> Popper’s Socratic view is easily distinguished from the falsificationism that Lakatos promoted [see Boland 1994]. But more important is that focusing on Popper’s Socratic view would better illustrate the incompatibility between his view and Kuhn’s.

with the followers of Mark Blaug. Orthodox Popperians will continue to be unhappy if they think the corporatist community of scientists or economists are ever going to advocate Popper's Socratic view.

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