



Making sense of economists: from falsification to rhetoric and beyond

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To make sense of what economists do. Is there any other reason for studying economics as a science? It was the main reason for me, and it still is. Of course, other reasons have presented themselves, like being able to distinguish myself from fellow economists around me, becoming a confidant for the disgruntled among them, having the sheer pleasure of reading philosophy and an excuse for not doing econometrics, being in the company of interesting and like-minded people, getting published, being paid, slaying the dragon of scientism, and indulging my reflexive inclinations. And it seemed that I had something to contribute to the conversation among economists. At least so I thought at first. Whatever, making sense of the confusing scene that economists create with their science has always seemed the best reason to justify methodological discussions and my ongoing involvement in them.

When I try to make sense of developments and events in the field of methodology, the objective of this essay, I can't help but take my own involvement as vantage point, and apply the notions and insights that I have gained over the course of time. I ask the reader to forgive me this indulgence. How truthful this reconstruction is, is hard to tell. Being impressionistic and based on (deliberate and researched) reflection the narrative can hardly do justice to everything that has happened in the field. Yet I hope that in these personal ruminations a more general account emanates that illustrates the usefulness of the rhetorical approach and shows the future that I see for reflexive practices in the context of economics.

FRUSTRATING MODERNISM

When I went to the university in the early seventies I opted for the study of econometrics. I had a talent for mathematics, was interested in economic issues, wanted to work on the big economic problems such as inequality, poverty and suppression, and expected that econometrics would produce the best scientific solutions. I confess that this expectation may now seem naïve but arrogance was not a stranger for the young at that time. My hero was Jan Tinbergen, the Dutch econometrician, who dedicated his life to the cause of

justice and just had earned the first Nobel Prize. His fusion of idealism with hard science seemed ideal. The hardness of the science stood for certainty, I gather, and certainty must have been consoling for a novice to science. So I commenced the study of econometrics.

The first frustration came when I found out that econometrics was all linear algebra and mathematical statistics: Economic topics hardly came up in the discussions. My companions would rather get excited about problems with matches than about the problem of unemployment. The second frustration came when I found out that even econometricians did not really know what was going on in the world. They ran their regressions but did not settle any theoretical issues. The inconclusiveness of their research did not bother them as they were preoccupied with mathematical issues. It did bother me, though.

Frustrated I switched to economics and ran into other frustrations. Macroeconomics proved to be a battlefield on which Marxists and the post-Keynesians tried hard to dethrone the then dominant neo-Keynesians. (Monetarists were not taken very seriously at all at that time.) The disputes made economics look like a lively discipline; arguments appeared to matter. I flirted with Marxist theories and opted for the post-Keynesian position. Being so engaged was exhilarating alright but at the same time I realized I had lost an illusion along the way. For what was the truth? Why did the science of economics not settle the disputes? When a professor of mine confided that economics was all ideology, he only confirmed what I had begun to suspect: the hopelessness of the expectation that I could solve the 'real' problems by means of science. So what was I doing studying economics?

The subject of methodology offered some solace. Klant was the professor who introduced me to the discipline. He had just published *The Rules of the Game* (1984) (the Dutch version) in which he pursued the line of thought that T.W. Hutchison had set in. So I was to think of economics as an empirical science. The main question was whether economic theory generates hypotheses that could be falsified in empirical tests. And so I came across Karl Popper, the name we learned to utter in one breath with 'falsification'. The received view was over; verification was done with. As Alasdair MacIntyre expressed it later for me in a lecture, science had become a floating raft without anchors that can hook it to the hard bottom of reality; we scientists keep it afloat by changing pieces as we seem fit in a process of trial and error.

It occurred to me at that time that not the specialists but the practitioners dictated the methodological debates. Klant did not seem to leave much of a trace in the practice of economics. Neither did Mark Blaug, arguably the most influential among the specialists. Talk to any practitioner and in the rare case that they are aware of methodology at all, they will evoke the writings of Milton Friedman, Paul Samuelson, Charles Koopmans and, after 1983, possibly Deirdre McCloskey, the economic historian. The attention for the specialists is limited; specialists read the work of each other and criticize what

the practitioners have to say about the methodology of their discipline. And that is what we did during the seventies. Unfortunately, I had to be sympathetic at the time with Friedman's point that prediction is all that matters. This was unfortunate because his conservative ideology made him suspect to an economist with post-Keynesian leanings. Fortunately, we could declare his claim that the unrealism of assumptions does not matter at all a bad excuse for empty theorizing. The operationalist criterion of the more sympathetic Samuelson did not convince entirely because it was unclear what it meant for scientific practice. Friedman had the upper hand in this dispute. He certainly got most of the attention.

The analysis of Klant only reinforced the problems of economics as an empirical science to me. Using the formalization of Papandreou (who later would become prime minister of Greece – some of us at least go somewhere) he showed that empirical tests of economic models had to be inconclusive. In case of falsification one of the auxiliary hypotheses that are needed to allow for an empirical interpretation could be at fault. I appreciated the argument and only later realized that it was a neat elaboration of the Duhem-Quine thesis in the context of economics. Klant's argument left economists only with the desire of practicing an empirical science without the hope of realizing it. At least that was my conclusion at the time. I had already resigned myself to the impotence of the science of economics to be an objective guide to a better world. Jan Tinbergen the scientist had become for me Jan Tinbergen the idealist.

Whether economics is an empirical science or not has remained the major question driving the methodological inquiry. Just follow the methodological writings of Alexander Rosenberg, Mark Blaug, Alfred Eichner, and Daniel Hausman among many others. It is the search for some kind of standard that appears to drive much of the methodological literature, a standard of appraisal. If economics had any chance of being a science, it better demonstrates to develop and grow in accordance with such a standard. As Blaug put it: What methodology can do is provide criteria for the acceptance and rejection of research programmes, setting standards that will help us discriminate between wheat and chaff (1980: 264). Economists must have a non-ideological criterion by which they can establish the 'truth' of or, the second best option, just appraise their theories. If not, they cannot keep the barbarians out of the gate. If not, anything would go and that would be the end of economics as a science.

The obvious place to look is the connection between theory and the facts. If the facts do not verify, they should be able to falsify, shouldn't they? If not that, they may at least contribute to the plausibility of the theory (cf. Nooteboom 1986 and Hamminga 1982). These efforts seemed to be futile as they did not tell me how economists actually coped with the inconclusiveness of their empirical tests. Saving the empirical in economics has ceased to be interesting to me. When Mark Blaug's concluded in his *Methodology of*

Economics (1980) that econometricians are playing with the net down, I agreed but without the regret, if not indignation, that he expressed.

The fear of ‘anything goes’, of a discipline that falls prey to ideological interests, is apparently deeply seated in our profession. Klant was emphatic on this and so is Mark Blaug who had had a direct experience with barbarians within the gate. My current response is that even though economics lacks unambiguous standards, clearly not anything goes. Just try to reason that people buy more because God tells them to, or argue that economics is all rhetoric, and see what happens. If not constrained by some objective standard, economic discourse is social and hence is constrained socially. Still a graduate student I was unable to advance such an argument. I rather went along with the fascination that economic methodologists developed for Lakatos and his account of research programmes.

At that time – it was in the late seventies – I studied at Duke University with Neil deMarchi and E. Roy Weintraub. My luck was that several other graduate students were interested in the subject and we formed a graduate seminar. Almost all of those participating continued and published in economic methodology. I mention Bruce Caldwell, Janet Seiz, Rodney Maddock and Robert Fisher. The latter two applied Lakatos in their dissertations. The appeal of Lakatos was, I guess, that he was not Thomas Kuhn and that he opposed Feyerabend. Kuhn’s notions of paradigm and disciplinary matrix were too vague to make sense of economics. His perspective forced us in a sociological direction, making us think of invisible colleges, shared values and the like. Lakatos’s framework suggested the possibility of a rational reconstruction of a sequence of theories and with that it suggested a rationality in the development of economic knowledge. I tried it myself. After all, this could mean a rescue of the science in economics. But I ended up unconvinced. The operationalization of the ‘hard core’ and the ‘protective belt’ proved to be dubious and the determination of empirical progress speculative. There also was too much ‘as if’ reasoning here, that is, ‘as if economists take one rational step after another’. This did not correspond with my observations. Consequently, Lakatos’ framework did not help make sense of what economists do with their science.

THE LINGUISTIC TURN

It was about then that I made my linguistic turn. I had not the foggiest idea what made me do so. I must have sniffed something in the air. It must have something to do with the swearing off of modernism, as I realized only later (cf. Klammer 1993). Modernism, as I now take it, stands for the metanarrative of progress; in the context of science it stands for a belief of being able to capture the structure of reality by means of formal and reductionist reasoning. First Kuhn, then Feyerabend, Latour, Rorty, Foucault and so many others began to deconstruct the modernist buildings and showed a world that

was so much more pluralistic, multidimensional, linguistic and discursive than I had ever imagined when reading Popper, Lakatos, Klant or Blaug.

At any rate, I began making sense of the disputes between new classicals and neo/new-Keynesians in terms of arguments on various levels of discourse. The argument was that economists have to persuade each other and to that end they work on different arguments. The work on a model is part of a theoretical argument but presenting the results of an econometric test is another. The connection between the two arguments may be loose; they often follow their own logic, rules and conventions. I also distinguished philosophical arguments (about the implied world view and non-scientific values), common sense arguments ('any reasonable person will choose the best option, of course'), and epistemological arguments. The latter arguments are about the nature of science and the proper scientific strategy for economists. I judged them to be most important in disputes as between new classicals and neo-Keynesians. (I would stress the same point in the 1984 book *Conversations with Economists*, but did not do it persuasively as it was not picked up.) And then I came across a draft of McCloskey's article on the rhetorics of economics. That nailed it for me.

Since this linguistic turn the books of conventional methodology are gathering dust on my shelves and I had to gather an entire new selection of books for my library. Later disputes between McCloskey, myself and other methodologists have shown the importance of this aspect of scholarly work. We not only use different concepts, ask different questions, but we also read a different literature and relate to different texts. When someone writes some scathing comments about the silliness of the rhetorical approach, I look at their references and throw up my hands in despair. Have they not even seen what has happened in the philosophy and sociology of science, I then wonder? The critics turn out not to have read Aristotle, Perelman, Toulmin, Foucault, Booth, Rorty and so many others who have written about rhetorics and discursive practices. It's no wonder, therefore, that we have such a hard time communicating. We do not share the same texts anymore.

In my own perception the linguistic turn has forced me to look more closely at how economists argue. In this sense I consider the rhetorical approach more 'realistic' than conventional methodological analyses which usually take the form of rational reconstructions of what economists do. For example, to determine what sets various practices apart, a study of informal practices is necessary (e.g. Klammer and Colander 1991). Conducting conversations proved to be quite an effective method to find out about where economists clashed. When I pushed Hicks on the assumption of rationality I found out that it did not play a role in his thinking. He rather thought like an accountant about economic processes. The concepts of conflict and power evoked irritation. Emotional reactions point to values that are violated. In the end clashes are about values. For that reason I now consider the culture of economics an important topic of further research. It gets us to ask questions

about what drives economists, what values they propagate in their work, implicitly and explicitly. By bringing out the cultural dimension I expect to make even more sense of what moves economists.

Whereas McCloskey tends to play down differences between discursive practices, I accentuate them. The point of the rhetorical approach is for me the ability to bring out and to characterize differences. By considering the narratives and metaphors that Keynes employs we can quite easily show that he is not a Keynesian. Rhetorical differences matter. They also account for the limited role that economists play in public life: their non-dramatic rhetoric and formalistic reasoning does not play to an audience that thinks in terms of power struggles, heroism and victimization. We also can show that people who operate in markets employ a rhetoric that has virtually no overlap with the rhetoric that economics attribute to them.

Does all this matter? The question tends to be a killer. E. Roy Weintraub quite rightly points out that methodological work has had no influence on economic practice. He goes even so far to endorse Stanley Fish who claims that this does not matter at all. As far as he is concerned we do methodology because we want to do it, and we do economics because we want to. Although Weintraub may be perfectly content with this conclusion, I am not. In my experience reflection can matter for practice. Reflection on the practice of economists, therefore, may – or should it be should – influence economic theorizing. Right now it does not because of major differences in rhetorical practices. After all, how can economists who think in terms of formalized problems of choice, be receptive to the rhetoric of rhetoric? And how could they work theories of theory appraisal into a theory that does not even recognize the problem of knowledge? Contemporary mainstream economic practice is in no position to take in the lessons of methodology. But it could do so in due time.

One possibility that McCloskey and I are investigating concerns the rhetoric of markets. We suggest that thinking of markets in terms of conversations places markets in a new perspective. How an entire research programme can evolve from this starting point, we do not know. Fortunately so, for otherwise it would be no fun pursuing this line of inquiry.

To conclude, the methodology was a good refuge for someone frustrated with the science of economics. It provided the arrows to shoot at the defence systems that economists have constructed around their practices. Even if the dragon of scientism has been slayed in my eyes, many economists do not see it and continue as if the methodological discussion does not exist. The rhetorical approach made some splash but the effect was short lasting: economists continued to be as unselfconscious of their own practices as they always have been. The impact of the rhetorical approach on methodological practices has been limited, too. A careful dissection of rational reconstructions and a meticulous sorting out of concepts, as Uskali Mäki does so well, continues to be the preferred practice, at least that is my impression. I am content to show

that economists argue, that values inform their practices, in particular scientific values like rigor, completeness, parsimony and so on, and that practices differ rhetorically. The great challenge now is to show how these insights travel when used to view actual economic practices in markets and economic organisations.

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