# The Rhetoric of Economics

*Paul O'Connell*

Economics is the art of conviction. Economists "...argue to render their judgements persuasive" (Klamer,1984:238). Exposition is an objective of ancillary importance.

This is the context in which the role of econometrics must be examined. Econometric methods in practice constitute a mechanism used to bolster the persuasive force of particular arguments, to secure adherence. Crucially, they are not as perceptible as other persuasive techniques, not because they are any less blatant, but because they are shrouded in false legitimacy by virtue of their apparent subscription to the credo of Scientific Method.

This essay explores this theme. Section one focuses on the dissonance between official and unofficial rhetoric. Section two investigates the reasons for this discord. Finally, section three looks at an alternative methodological prescription, countering the assertion that the anti-inductive, modernist emphasis of Popperian methodology is the only one suited to economics.

**The official and the unofficial rhetoric**

McCloskey (1983) distinguishes between the explicit and implicit methodology of economists. Officially, they subscribe to an amalgam of "logical positivism, behaviourism, operationalism, and the hypothetico-deductive model of science"(1983:484). Booth labels this amalgam as modernism, summarised in Kelvin's dictum: "When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind"(Kuhn,1977:178).

The origin and merit of such an approach can be traced to the methodological declarations of the modernist family from Descartes to Hume, Comte to Russell and Locke to Hempel. On the eve of the Cartesian Revolution, the French philosopher Ramus crystallized the medieval tendency to "...relegate rhetoric to mere eloquence" in an effort to achieve scientific objectivity (McCloskey,1983:483)[1]. In the three subsequent centuries, the direction of endeavour centred on the establishment of logical positivist foundations for knowledge. Probable argument languished.

The epistemological pronouncements of Popper and Lakatos influenced greatly the application of this Received View to economics.

> "Once put forward, none of our "anticipations" are dogmatically upheld. Our method of research is not to defend them, in order to prove how right we were. On the contrary, we try to overthrow them... using all the weapons of our logical, mathematical, and technical armoury"(Popper, 1968:279).

According to this official doctrine, theoretical disagreements can be settled in the eyes of any "objective" economist by systematically collating empirical evidence.

1 Throughout this paper, the adjective 'scientific' will be taken to mean objective and free from ideological bias.

Furthermore, on this view, falsification is a prime objective of the scientific economic theorist.

Contrast this official methodological prescription with unofficial, "workaday" practices:

> "If the results of the fitting to the data are reasonable, on grounds that are not themselves subject to examination, the article is sent off to a journal. If the results are unreasonable, the hypothesis is consigned to do a loop: the economic scientist returns to the hypotheses or specifications, altering them until a publishable article emerges"(McCloskey,1983: 494).

Thus, to use Richard's (1964) dichotomy, while economists purport to be scientific (in that arguments are advanced purely for the purposes of exposition), in practice they seek to sway and convince. To understand why such a glaring cleft divides official from workaday rhetoric, the assertion that economics is an art of persuasion must be returned to.

### The sociology of economics: *per fidem decipi*

For a variety of pragmatic reasons, economists must endeavour to have their theories accepted. Pratschke reminds us that "access to power...has been the conscious desire of economists from early Elizabethan times"(1985:145, quoted in Matthews, 1985). In the contemporary world of publication-counting-deans, motives are more pecuniary in nature, but equally distortionary. The fundamental sociological identity of an economist is also at stake, a point sketched in startling relief by Klamer:

> "Purely individual judgements usually do not count....A judgement only carries force when it is shared by a group of economists" (1984:253).

Yet if slanted pregnant discourse is demanded, it is not wholly licensed. Arguments must appear conformable to the epistemological standards of the day. For example, rigour and precision are appreciated, and neat and elegant models currently "fascinate". Given these constraints, what constitutes substantively rational behaviour on the part of the economist?

Consider the choice of language. Robinson writes: "...bigger is closer to better; equal to equitable; goods sound good; disequilibrium sounds uncomfortable; exploitat-ion wicked; and subnormal profits rather sad"(1962:19). Style can also be significant. Almost without exception, when assumptions are relaxed, the subjunctive mood is invoked, suggesting that the strictures placed on the context are realistic[2]. In addition, by appeal to Ricardian vice, results of a convincing nature can be derived from analysis of not-very-convincing hypothetical toy economies[3].

Examples of such devices proliferate on a little thought. Yet in the current context, the most important one is the empirical argument. Hendry and Mizon (1985) argue that the absence of serendipity in the creation of theories and models leads to the use of econometrics with false legitimacy. In the domain of unofficial rhetoric, econometric techniques constitute a psychological construct used as a mechanism (that has been ratified by the modernist tradition) to

---

2 Without a tin opener, we would not be able to open the can.

3 As a further interesting example, consider the equation MV = PT. As a useful but irrefutable equation, it is used in economics, and in physics as the equation of state of an ideal gas. However, it is often abused as a testable identity, leading to misleading conclusions.

McCloskey (1985) examined a sample of ten journal articles, each of which employed regression analysis[4]. Of the ten, only two do not admit experimenting with the regressions, sometimes with hundreds of different specifications. One uses "a sample of convenience so convenient, that it looks like a universe...about which sampling theory can tell nothing"(1985:204). Only three "...do not jump with abandon from statistical to substantive significance" (1985:204). None alter their level of significance and none mention the word "power".

An explanation for such practices is offered by Feige (1975). In an extraordinary paper, he argues that journal editorial policy puts an inordinate premium on the attainment of statistically significant results, which in turn contaminates published literature with Type I errors. He estimates the marginal net return to the production of statistically significant results for a researcher to be in the neighbourhood of $2,000 at 1975 prices. The consequence is an incentive for less than candid reporting of statistical tests and possible alternative model specifications (1975:1295).

**A New Methodological Departure**

It thus becomes apparent that, in contravention of their official rhetoric, economists engage in Procrustean practices that often succeed in no more than the corroboration of prejudice and whim[5]. While such a perspective is anathema to most econometricians, some have begun to adopt it (Leamer,1983; Hendry and Mizon,1985; Blaug,1980). Yet their prescriptions for a new departure must be rejected. The establishment of an even more stringent mandate to be met by theorists ignores the nature of both economics and *Homo economicus*. For the reasons cited above, pre-test bias is inevitable. Economists cannot afford to be "scientific", and hence they cannot be compelled to be so[6]. If additional strictures are imposed, economists would make pretence to be meeting these criteria, while researching and publishing exactly as before - the Goodhart's Law of econometrics. The only difference: prejudice would become even harder to detect.

A robust alternative methodological prescription can only be distilled if the sociological ingredient is included. A clue is provided by Perelman and Olbrechts-Tyteca (1971) in their formidable treatise on literary argumentation. They note that the adoption of the "Descartes concept" represents "...a perfectly unjustified and unwarranted limitation of the domain of action of our faculty of reasoning"(1971:3). Not all regression analyses are more persuasive than all moral judgements. The pursuance of dialectic, syllogism, induction and other forms of argumentation has much to offer in its own right. But crucially, such methods of proof do not shelter under the cloak of self-righteousness that protects operationalism.

Such an invitation to practitioners to de-emphasise modernism meets head-on with the aged and durable fear of sophistry. In classical times, Plato chastised rhetoric in his dialogue. Indeed, it was this fear which spurred the adoption of rigorous modernist methodology. Yet the sociology of economics dictates that, while ostensibly

4 McCloskey takes his sample from the fifty full-length articles that used regression analysis in the American Economic Review between 1981 and 1983.

5 Procrustes was a Greek giant of Eleusis, who had two beds of unequal length. He would force travellers into either one or the other by stretching their bodies or cutting off their legs.

6 According to Polanyi, econometricians could, in theory be compelled to be scientifically impartial. However, this would involve the imposition of 'quixotic standards...which if rigorously followed, would reduce us all to involuntary imbecility'(1962:88).

otherwise, operationalist techniques are equally specious. In this context, there is no good reason for wishing to make logical-positivist as against plausible statements. The opposite is the case, since the latter are not defended as irrefutable. A more pluralistic methodology would result in a discipline which is more "scientific" if less superficially similar to the statistical methods used in experimental sciences.

## Conclusion

In assessing the role of econometrics, this paper has isolated the dissonance between the official and the unofficial rhetoric of economists It was argued that sociological factors are at the basis of this dissemblance. Prospects for redress hinge on the readmittance of probable argument, which is not shrouded in the false legitimacy that tends to immunize empiricism from challenge.

Modernism is no longer the Received View. Rorty (1979) notes that philosophers have adjusted their metaphysical position, acknowledging the Duhem thesis and its indictment of falsification. Economists must therefore realign their allegiance. Nothing is gained by clinging to any methodology except clarity, tolerance and honesty. Emphasis on "conceivable falsification" and "some future test" is not a panacea.

---

## References

Blaug, M. (1980) *The Methodology of Economics: or How Economists Explain.* Cambridge: Cambridge U. Press.

Feige, E. (1975) "The Consequences of Journal Editorial Policies and a Suggestion for Revision," *Journal of Political Economy,* 83(6), 1291-1296.

Hendry, D. and Mizon, E. (1985) "Procrustean Econometrics or the Stretching and Squeezing of Data," Centre for Economic Policy Research, Discussion Paper Series, October 1985, 68.

Klamer, A. (1984) *The New Classical Macroeconomics.* Brighton: Harvester Wheatsheaf.

Kuhn, T. (1977) *The Essential Tradition: Selected Studies in Scientific Tradition and Change.* Chicago, IL.: U. of Chicago Press.

Leamer, E. (1983) "Let's Take the Con out of Econometrics," *American Economic Review,* 73 (Mar.), 31-43.

Matthews, A. (1985) "Economics and Ideology," The Crane Bag, 9(2).

McCloskey, D. (1983) "The Rhetoric of Economics," *Journal of Economic Literature,* 21 (Jun.), 481-517.

______. (1985) "The Loss Function has been Mislaid: The Rhetoric of Significance Tests," *American Economic Review,* 75 (May.), 201-205.

Perelman, C. and Olbrechts-Tyteca, L. (1967) *The New Rhetoric: A Treatise on Argumentation,* (Eng. Trans.). London: Notre Dame U. Press.

Polanyi, M. (1962) *Personal Knowledge: Towards a Post-Critical Philosophy.* Chicago, IL.: U. of Chicago Press.

Popper, K. (1968) *The Logic of Scientific Discovery.* London: Hutchinson.

Pratschke, J. (1985) "Economics and Ideology," Studies, (Summer).

Richards, I. (1979) *The Philosophy of Rhetoric.* Oxford: Oxford U. Press.

Robinson, J. (1962) *Economic Philosophy.* London: Watts.

Rorty, R. (1979) *Philosophy and the Mirror of Nature.* Princeton, NJ.: Princeton U. Press.