Elegance in Economics

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Stephen Leacock encapsulated his thoughts on economics in the following not very well known lines:

> Adam, Adam, Adam Smith
> Listen What I Charge You With!
> Didn't you say
> In the class one day
> That Selfishness was bound to pay?
> Of all doctrines that was the Pith.
> Wasn't it, wasn't it, wasn't it, Smith? (Leacock, 1936, p. 75)

The economics profession itself, however, has gone on to develop this notion in proportion to the analytical elegance to which it is conducive. Elegance in disputation in economics is more difficult to establish but is by no means without purpose and is focused on here in the spirit of Stephen Leacock, and in the spirit of encouraging initiates into the discipline to be generous in their understanding of elegance in economics.

Analytical rigour is, of course, in universal demand. Mathematics and inductive logic stand at the core of many a graduate programme. The increasing mathematical sophistication of many of the profession's top journals in fact places them beyond the reach of many practising economists, both within academia and without. Yet all is not lost for the less numerate "old guard", who continue to insist that the analytical potency or real world relevance of economics resides essentially in just six or eight simple, but insightful, cornerstones of economics, including the concepts of elasticity, opportunity cost, comparative advantage and optimization via the comparisons of marginal benefits and marginal costs of alternative policy options. Phelps-Brown (1972) has accordingly made clear that from the viewpoint of selecting good entrants to the UK public service, he would prefer to see stress placed not on theoretical elegance or mathematical rigour but on a knowledge of economic history and extant institutional structures. Those with a "feel" for such matters are most likely to produce good practical policy advice, if not elegance.

Wheelwright (1978) similarly disendorsed the significance of elegance in economics in the context of offering avuncular words of advice to new (Australian) students of the subject:

> Galbraith called these arrangements, whereby orthodoxy is preserved in the academy, "a new despotism" [as against "elegance"!], which consists in defining excellence as whatever is closest, in belief and method, to the scholarly tendency of the people who are already there … In short, conventional economic teaching is not telling "how it is", but it is serving an instrumental function, of hiding the realities of economic power in the modern world … [Galbraith] tells us that the teaching of neo-classical economics serves not the understanding

International Journal of Social Economics, Vol. 21 No. 9, 1994, pp. 3-10. 0306-8293

> or improvement of the economic system, but the goals of those who have power in that system…
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> Why then does neo-classical economics persist, if it is so biased and not an analysis of current reality? Galbraith's answer is that it is a formula for a quiet, non-controversial life: it owes a lot to tradition as it is a not implausible account of a society that once existed; it is the available doctrine, not yet replaced; students arrive, they must be taught – most teachers know only the orthodox doctrine; and the models lend themselves to endless refinement which gives a false impression of rigour and precision. …
>
> In short, economists come to consume and supply their own product… [and] tend to produce what the profession wants to hear… In 1962 Galbraith told us that "Professional economists, like members of city gangs …have the natural desire of all such groups to delineate and safeguard the boundary between those who belong and those who do not" (Wheelwright, 1978, pp. 3-5).

Similar sentiments are also expressed by Sen – often touted as a prospective Nobel Prize winner – in the striking metaphor:

> An economy can be [Pareto optimal] even when some people are rolling in luxury and others are near starvation as long as the starvers cannot be made better off without cutting into the pleasures of the rich. If preventing the burning of Rome would have made Emperor Nero feel worse off, then letting him burn Rome would have been Pareto-optimal. In short, a society or an economy can be Pareto-optimal and still be perfectly disgusting (Sen, 1979, p. 22).

On the whole, then, institutionalist economists sympathize with the sentiments that "What is accepted as economic theory, what passes for economic knowledge, is largely, albeit not entirely, a matter of belief" (Samuels, 1984, p. 64). Accordingly, the advice of institutionalist economists to new initiates is, in Galbraith's words, to concentrate on the mathematical minutiae if their goal is to advance in the profession. If their goal is to understand what is actually happening in the real world and why, however, they should instead focus on power structures and institutional realities. Because the first goal accords more closely with the utility maximization goals of most "rational, calculating individuals", the mathematical minutiae approach is more common in the profession. Indeed there is sociological evidence of this in respect of graduate student responses at Ivy League postgraduate schools. Colander and Klamer (1987), for example, report survey results to the effect that elegance, rigour and "truth" appear to be understood somewhat differently in different graduate schools.

Knowledge of the economy and economic literature does not make an economist successful, according to graduate students. Only 10 per cent believed a knowledge of economic literature was very important, while 68 per cent believed a thorough knowledge of the economy was unimportant. Some 57 per cent felt excellence in mathematics to be very important, while only 3 per cent felt the same about a thorough knowledge of the economy.

Moreover, Colander and Klamer observe that different graduate schools have quite distinctive characters. Whereas 47 per cent of Chicago graduate students agree strongly that economics is the most scientific social science, only 9 per cent of Harvard students hold that view. Likewise, while 60 per cent of graduate students at Yale agree strongly that fiscal policy is effective in stabilizing the economy, only 6 per cent think so at Chicago. Again, in terms of the importance of economic assumptions, 59 per cent at Chicago hold the assumption of rational expectations to be very important, while 0 per cent, 9 per cent and 14 per cent do so at MIT, Stanford and Harvard respectively.

As Colander and Klamer note, these results raise significant questions about the nature of graduate school, what is being taught, and the socialization process that occurs. Their survey revealed a significant degree of frustration and cynicism that went beyond the normal graduate school blues. What they found "was a strong sense that economics was a game and that hard work in devising relevant models that demonstrated a deep understanding of institutions would have a lower payoff than devising models that were analytically neat; the façade, not the depth of knowledge, was important" (Colander and Klamer, 1987, p. 100). Others, including Robinson (cited in Galbraith, 1970; Wheelwright, 1974), have likewise concluded that "elegant error" has come to be preferred to complex reality.

Another dimension to this dichotomy comes through in journal refereeing and citation matters. Gans and Shepherd (1994) have recounted recently something of the history of the rejection slip, as received even by the *glitterati* of the profession. In some cases articles first rejected by one or more journals went on to become recognized as celebrated and seminal contributions. Neither elegance nor empirical fit is sufficiently easy to define in economics to prevent such things from happening. Caustically enough, Hodgson (1993) similarly notes the contentious administrative tactic of funding UK economics departments in proportion to their success in gaining publications in a list of the most prestigious "top 20" core journals. Thus this practice combines the elegance of the mathematical contributions characterizing most of those journals – with the economy of wholly disregarding the many other less elegant (putatively less relevant and less worthy) journals, including such renowned heterodox journals as *The Journal of Economic Issues* and the *Cambridge Journal of Economics*. Unfortunately, for the supporters of the view that "elegance is in the superior journals" and "economics is what economists do", it was found that some heterodox journals – inelegant, non-quantitative, and interdisciplinary as they were – actually outscored some of the "Diamond List" journals on their *own* stipulated criteria of citation frequency and citation impact. Nonetheless the inelegant, unwelcome heterodox journals were known by some *other* insight to be unworthy and were vetted out of the "core journals" list. Moreover, some of the top ranked core journals were found to have abnormally high self-citation rates. In effect, Hodgson is suggesting that, if elegance is one requirement of professional acceptability, so too is political or ideological orientation a determinant of what is deemed elegant. Hence Robinson's quip that economics has always been part scientific and part ideological, and her conclusion that, "The purpose of studying economics is not to acquire a set of ready-made answers to economic questions, but to learn how to avoid being deceived by economists" (cited in Wheelwright, 1974). Hence also

Kaldor's observation that, "The powerful attraction of habits of thought engendered by 'equilibrium economics' has become a major obstacle to the development of economics as a *science*", (Kaldor, 1972, p. 1237) and Worswick's similar lament that "The danger is that university courses in economics will become increasingly mathematical and increasingly concerned with technique to the exclusion of the subject-matter itself" (Worswick, 1972, p. 84).

New Institutionalists such as Robert Lane (1991), in fact, tend to see free market theory *à la* Hayek less as an account of what actually happens in a capitalist economy than as a Cold War ideological description of an economy which ardent capitalists would like to see exist. Thus, its critics see that theory as having been charged with the ideological task of providing a model which can compete successfully with Marxism. In an attempt to bridge the gap from reality, New Institutionalists accordingly contest the relevance of three major mainstream assumptions which they consider gained currency not because they were realistic or socially useful, but because they were conducive to elegant modelling. In marked contradistinction New Institutionalists prefer to assume instead that:

- Work (or production) is itself not only a burden or disutility but also itself a source of utility and human fulfilment. New Institutionalists, thus, do not accept the orthodox assumption that economic life is essentially a trade-off between consumption as a source of utility and work as a source of disutility.
- With modern technology various industries are capable of generating increasing returns, as against decreasing returns; thus an original comparative advantage becomes self-sustaining and enlarging, as is perhaps the case with Swiss pharmaceuticals and Japanese electronics.
- Economic agents are error-prone and subject to trial and error – despite the difficulties such an assumption poses for rigorous, deterministic mathematical modelling. They happily leave the assumptions of rational economic man and rational expectations to the few zealots of orthodoxy who remain as believers.

Supercomputer technology now permits the development of institutionalist models incorporating such assumptions to produce booms and busts and general instability – and the implied need for State intervention – in the context of a free market. Accordingly, mainstream or orthodox models might now be seen to have not only a political or ideological dependence but a technological dependence as well. With newly available modelling technology, institutionalist or disequilibrating models now have more chance to present themselves as "scientific" alternatives via sophisticated mathematical elaboration.

Phelps-Brown (1972) adds further to this litany of complaint by objecting that the study of history has been so neglected in the training of economists that the basic problem with modern economics is that its assumptions about human behaviour are totally arbitrary, being literally "plucked from the air". Two years before winning the Nobel Prize for economics, Leontief likewise objected that:

> Continued preoccupation with imaginary, hypothetic, rather than with observable reality has gradually led to a distortion of the informal valuation scale used in our academic community to assess and to rank the scientific performance of its members. Empirical analysis, according to this scale, gets a lower rating than formal mathematical reasoning (Leontief, 1971, p. 3).

McCloskey (1983) extends this type of criticism of analytical elegance in economics by addressing the application of philosophy of science issues under the theme of the *rhetoric* of economics. As far as McCloskey is concerned, what really matters is not what economists *say* they do, but what they *actually* do. Whereas Blaug (1980) tells us falsification has won the battle in economics at least in principle, even if there are still some problems in practice, McCloskey insists that what economists really do is rely on much more than empirical falsification of contending hypotheses. According to McCloskey, what economists actually do in choosing between competing theoretical explanations is look, not only to empirical testing but also to such things as analytical elegance:

> the aptness of economic metaphors, the relevance of historical precedents, the persuasiveness of introspections, the power of authority, the charm of symmetry, the claims of morality. Nothing is gained from clinging to the Scientific Method, or to any methodology except honesty, clarity and tolerance (McCloskey, 1983, p. 482).

For McCloskey, economics is less of an empirical falsificationist science than is conventionally claimed, and more a rhetorical discourse dependent on persuasion by a variety of means. For Blaug, likewise:

> Economists [clearly] engage massively in empirical research … but, unfortunately, much of it is like playing tennis with the net down: instead of attempting to refute testable predictions, modern economists all too frequently are satisfied to demonstrate that the real world conforms to their predictions, thus replacing falsification, which is difficult, with verification, which is easy (Blaug, 1980, p. 256).

In this context it is easier to appreciate the concerns of social economists. For them the fact that it is difficult to model the most decisive philosophical assumptions indicates, to that extent, the irrelevance of elegant models rather than the insignificance of these assumptions. Mathematical rigour and computer models offer little help to those whose concern is with a separate and distinct class of implicit assumptions about political philosophy, the nature of man and questions of teleology. Those relatively few economists who recognize economics is necessarily about humankind and, therefore, incorporates an implicit and non-definitive political philosophy essentially object to the contentiousness of the philosophical a priori of elegant economics. Their concern is not with the pragmatics of increasing returns but with the political philosophy of Locke and Smith and their many rivals. In the wake of recognition of the greater modelling difficulties of institutionalist assumptions and theories, one can imagine readily how the still greater intractability of political philosophy to quantitative techniques leaves the really crucial determinants of the method and conclusions of modern economics out in the scientific cold.

In the absence of elegant models, social economists have sought to highlight the philosophical ethos silently reposed behind them. Thus:

> Machiavelli, Hobbes and Locke
> Thought teleology a bit of a block
> To the progress available
> Through freedom unassailable
> And the scientific method non-stop.
>
> Adam Smith added to this
> Individualism is the cause of bliss.
> From man's beginnings
> And base underpinnings
> Stems the welfare none wants to miss.
>
> With just one goal
> There's no need for the dole
> As technique is ever refined.
> But should we re-wind, and be not so blind,
> And accept a more cautionary role?
>
> With computers to excite us
> And technique to unite us
> Philosophizing fell from favour.
> Relativism seemed something to savour.
> Yet in a trice, though it's not very nice,
> Society found nihilism, not piety.

One of the old saws of economics is that, even if you laid all the world's economists end to end, they still would not reach a conclusion! There is indeed a lack of unanimity about both analytical method and policy efficacy. Nonetheless, surprisingly little work is published on the theme of why economists disagree. Gunnar Myrdal (1972) lamented loudly that, when reviewing his books, reviewers commonly focused on empirical issues in complete disregard of his provocative challenge to the profession on questions of methodology. He likewise objected that economists tend to look critically at all but themselves. Sharpe suitably builds on this theme by noting that Myrdal in fact shared the 1974 Nobel Prize with Friedrick von Hayek:

> I assume that the [Nobel] prize is proffered in recognition of notable advances in economic science. But since Myrdal and Hayek have been advancing in opposite directions for the past fifty years, this leaves the [Nobel] Committee with some explaining to do … The most generous view conceivable is that the Committee cannot tell the difference between "forward" and "backward" in economics (Sharpe, 1975, p. 3).

Despite their obvious professional differences, Myrdal and von Hayek nonetheless shared the views that methodological issues have been appreciated too little and that a narrowly defined putatively scientific conception of elegant economics is indeed a danger to the world. Hence von Hayek's insistence that:

> The physicist who is only a physicist can still be a first-class physicist and a most valuable member of society. But nobody can be a great economist who is only an economist – and I am even tempted to add that the economist who is only an economist is likely to become a nuisance if not a positive danger (von Hayek, 1956, p. 463).

Since replication is not possible in the historically malleable universe with which economics deals, there are real difficulties in deciding just what has been proved true or false. Despite recent advances in chaos theory and in many other areas of sophisticated modelling, unemployment, inflation, productivity growth, debt levels and consumer psychology continue to pose durable problems. Thurow and others of a "pessimistic" bent stress that quantitative techniques meant to elevate data above speculative theory ended up doing exactly the opposite, such that only occasionally can an economic theory be rejected because of the data (Thurow, 1983, p. 117). Stigler (1976, p. 350), nonetheless, allows us to conclude on the happier note that, whatever might be the problems with elegance and precision in economics, "[The] United States has perhaps the equivalent of 5,000 full-time research economists, costing – with ancillary services – perhaps $250 million per year. If the various special producer interest policies cost the nation $40 billion per year – a wild estimate but one that is not easily refuted – then we earn our keep if we reduce these exactions of consumers by about one-half of one percent."

A contemporary Stephen Leacock might, therefore, leave us with words somewhat like the following:

> Adam Smith, tell me this:
> Is elegance of relevance?
> Does mathematical rigour
> Make our knowledge bigger?
> Or, in matters economic,
> Involving assumptions astronomic,
> Are the elegant techniques they taught us
> The source merely of rigor mortis?

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