Some sociological explanations for the present condition of neoclassical economics

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In this chapter we will take a somewhat impressionistic look at the sociology of the field of economics. Given a discipline which, at the beginning of the twentieth century, has problems that it can not resolve, how will it develop, through the end of the twentieth and into the twenty-first century, with the claim that it is a science – indeed, 'the queen of the social sciences'?

MARSHALL'S AMBIVALENCE, AND HOW IT IS PERCEIVED

Since Marshall's time it has become ever more difficult to draw attention to the need for something besides technique to bridge the gap between the world that (presumably) exists outside our heads and the symbols through which we communicate about the world. Those economists who have attempted to lay stress upon this requirement have generally seen this part of their work marginalised under the name of 'institutionalism'. (They include such figures as Myrdal, Hirschman, Streeten, Scitovsky – to name a few.) The neglect of this part of the field may be traced to the neglect or depreciation of a part of Marshall's work; and that began with an essay by John Maynard Keynes.

In Chapter 5 it was noted that Marshall's persistence in bestriding the entire path of economics, even as it divided beneath him, led to a reputation for ambivalence, even inconsistency. Ambivalence, or multi-sidedness, can be understood and evaluated in a number of different ways. The interpretation that is most familiar today goes back to the way Marshall's memory was preserved by Keynes's leading essay in *Memorials to Alfred Marshall*. That essay retains its grip for good reasons: it is readable and astute, both in presenting a psychological portrait, and in outlining Marshall's most specific and solid achievements. It contains, however, some complicated twists.

Keynes who (in spite of his disclaimers of any normative tinge) may have been the most effective economist of all time as a giver of advice and meddler in practical affairs, chose, in discussing Marshall, to adopt a heavily positivist tone. Keynes leaves no doubt as to which side *he* regards as the 'higher', when he discusses the 'conflict', in Marshall, 'between an intellect, which was hard, dry, critical, as unsentimental as you could find, with emotions and aspirations, generally unspoken, of quite a different type. When his intellect chased diagrams and Foreign Trade and Money, there was an evangelical moralizer of an imp somewhere inside him, that was so ill-advised as to disapprove' (J.M. Keynes, 'Alfred Marshall, 1842–1924', in *Memorials*, p. 37). Earlier in the same essay, Keynes similarly referred to 'this double nature', saying that 'the piercing eyes and ranging wings of an eagle were often called back to earth to do the bidding of a moralizer' (ibid., p. 11).

The traditional view of Marshall which has grown directly out of Keynes's biographical and critical sketch attempts to downplay the multi-sidedness of the man and his work, as though it is assumed that his memory is best served by overlooking a slightly embarrassing tendency to be inconsistent, even fuzzy-headed. (This tendency was, by Keynes, implicitly interpreted as a result of Marshall's moralising tendency.) In anticipation of the prevailing wisdom of today, when it is assumed that s/he who does something more than economics is regarded as something less than an economist,[1] Keynes regarded some of Marshall's facets as detractions from, not additions to, his contributions as an economist. He was uncomfortable with Marshall's inclusiveness – his 'dual nature' – and tried to carve out of it for memorialisation the part that pointed in the direction that he, Keynes, thought economic science should develop.

It was not by accident, or inadvertently, that Marshall embraced a variety of apparently conflicting ideas. The subject matter of economics is itself full of ambiguities and contradictions: Marshall tried, in his theoretical writing, to be true to the fullness and complexity, even the contradictions, of human experience. That he continually strove to see all sides of a problem, and to avoid expressing himself in such a way that his work could be used in support of any singular or extreme position – this could be regarded as his greatest strength, not his greatest flaw.

Marshall's outstanding methodolgical characteristic was balance: balance between theory and facts, for example; or between tools (i.e., mathematics) and facts. But whereas in the first pair (theory and facts)

he saw an essential complementarity, in the second he saw a possible conflict; not one inherent either in the tools or in the facts, but arising out of the frailty of human nature. He foresaw the danger that researchers would lose interest in facts that were not amenable to their tools, or that they would effectively lose interest in facts, as the sophisticated development of some kinds of tools outran the quality of available data.

Marshall remarked in one place that to avoid altogether the danger of distortion of emphasis which accompanies the use of mathematical and other kinds of analysis 'would be to abandon the chief means of scientific progress'. At the same time, he was aware of 'a tendency towards assigning wrong proportions to economic forces; those elements being most emphasized which lend themselves most easily to analytical methods' (*Principles* p. 700). Above all he warned repeatedly against 'long chains of deductive reasoning'; even in the context of his most encouraging statement of the value of 'a training in mathematics' and 'experience in handling physical problems by mathematical methods' – or perhaps all the more so for being in such a context – Marshall is concerned with the danger which he could, it appears, already see in their employment:

> It is obvious that there is no room in economics for long trains of deductive reasoning; no economists, not even Ricardo, attempted them But a training in mathematics is helpful by giving command over a marvelously terse and exact language for expressing clearly some general relations and some short processes of economic reasoning; which can indeed be expressed in ordinary language, but not with equal sharpness of outline. And, what is of far greater importance, experience in handling physical problems by mathematical methods gives a grasp, that cannot be obtained equally well in any other way, of the mutual interaction of economic changes (*Principles*, p. 644).

A prime reason for Marshall's often-expressed distrust of mathematics was precisely that mathematics permits long chains of deductive reasoning. To modern economists, by contrast, this enabling is one of their most attractive features. Thus Marshall's desire to restrict the use of mathematics to the simpler situations of interest to economics is in radical opposition to much of modern 'scientific' thinking, which calls upon mathematical tools as our only hope of being able to master the growing complexity of human experience.

Looking over the successive editions of *Principles*, one may perceive a tendency in Marshall to suppress his more philosophical, reflective side, especially as it is applied to questions of methodology. A critical reflection was thus deleted from the fifth edition: there he had called for a 'general principle [which would] determine the point in the widening of the scope of economics, at which the growing loss of scientific precision would begin to outweigh the gain of increasing reality and philosophic completeness' (*Principles*, 2nd edn, deleted from the 5th edn; quoted in *Var.* II, p. 763). I do not have much confidence (though I do not entirely rule out the possibility) that it will be possible to enumerate such a 'general principle' in terms that are specific enough to carry much weight. However, the point remains that the field of economics is in need of renewed attention to the balance between 'scientific precision' on the one hand, and 'reality and philosophic completeness' on the other. In the absence of attention to this balance, the modern tendency has been to err in the direction of apparent scientific precision.

A view of what has been achieved since Marshall's time gives rise to the hypothesis that, **given the topics of intrinsic interest to economics, there was, from the beginning, a finite and relatively small subset upon which quantitative methods could effectively be brought to bear**. If Marshall was discontented with the disproportionate attention given in his time to *what could* versus *what couldn't be quantified*, he would be even more so now, as the remaining unexplored quantifiable ground has shrunk to insignificance by contrast to the vast tracts of nearly virgin territory awaiting the development of non-quantitative approaches.

A SOCIOLOGY OF ECONOMICS

In achieving even a summary understanding of how economics has developed from where it was at the turn of the century, under the dominance of Alfred Marshall, to its present condition, there are several trends in recent intellectual history that should be noted. One is the turn towards positivism which started near the beginning of this century and continued as a marked trend for several decades. It is interesting to note that Marshall is only one of a number of broad, turn-of-the-century social science thinkers whose heirs, unable to deal with the full complexity of their vision, whittled it down in a biased manner, retaining the side that lent itself to positivism and ignoring or (as Keynes did with Marshall) belittling the normative, humanistic,

subjective, intuitive, judgment-laden side. Others beside Marshall who have been treated in this way include John Dewey, Talcott Parsons and Henry James. Earlier writers whose works were similarly streamlined to fit the idea of 'science' of the early twentieth century include Charles Darwin and Adam Smith.

Over the course of this century the natural sciences have become progressively less certain of the infallibility or even the unique definability of 'the scientific method'; but there has continued to be a time lag between the methodological development of the natural and the social sciences. Economics, suffering particularly from 'physics envy' in its aspiration to the position of 'the queen of the social sciences', has sought to imitate a positivist mode now considered obsolete in physics itself.

The ambitions of economics, in the optimistic days of the 1950s and 1960s, included a boast of predictive powers. That expectation has been disappointed as the advice and explanations of economists have come into conflict with the events of the 1970s and 1980s (stagflation, fluctuations in the prices of basic commodities, the international debt crisis, etc.) which they had either failed to predict or could not explain, or on which the economics profession had apparently given poor advice. Economists needed, then, to bolster their reputation and image in the eyes of the public and of policy-makers.

The use of ever more sophisticated, difficult and, to the uninitiated, impenetrable, mathematics makes it harder for the public and the policy-makers to judge the conclusions of economics against what is known to make sense in the real world. At the same time, in the USA, the availability of funding through such agencies as the National Science Foundation has increasingly emphasised the desirability for economists of being able to speak the language of science.

I cannot cite a definite causal relationship between this history and the fact that, at this time, it is in academia that the direction of the field is controlled. However the latter fact does appear more firmly established now, at least in the USA, than it was during the 'optimistic times', when a greater number of influential economists could be found with weaker ties to academia than is now the case. Such professionalisation is, in any case, part of a more general trend in the natural and social sciences, as well as in the humanities.

Hence the growing importance of the four academic screening points where all the pressures of the field channel in the same direction. The qualities, skills and understandings

that get a prospective student admitted to a graduate department of economics;
- that help the student to get good grades and achieve an advanced economics degree;
- that assist in the writing of papers that will be accepted by the major journals; and
- that lead to academic promotion and tenure of an economist

all encourage abstract or mathematical modelling. As regards factual, historical, psychological, political or other contextual understandings or skills which have a usefulness in practical application of economics, the student develops these at peril of taking time away from studies on 'the cutting edge' of modern 'theory'; and the academic economist employs them with little or no encouragement or reward from within the profession.

Moreover, since the beginning of the twentieth century there has been a drift in the character of students going into economics, as undergraduates in mathematics, physics, and other mathematically oriented fields have increasingly found that they have a comparative advantage when they go on to graduate work in economics. This has continued to be a popular field for graduate studies, so that departments of economics have often been able to select, from among more applicants than they could accommodate, the students who they thought would most contribute to the enhancement of that department's reputation. It is difficult to test for the qualities of common sense, judgment, intuition, imagination, etc. which would make for a thoughtful, broad-minded economist; much easier to devise and to grade tests for mathematical ability. A simultaneous drift has occurred in respected economics journals, towards increased emphasis upon abstract or mathematical modelling.[2]

At the fourth of the academic screening points listed above we encounter the two modern academic imperatives: 'publish or perish' and 'up or out'. The tenure system as it now exists sets a competition which must be winnable by the young; the whole system tends to get skewed to allow older faculty to give high praise to the younger colleagues whom they would like to attract, and to allow young professionals to acquire the requisite publishing credentials. The virtues of elder members of the profession must stand aside in favour of what can be offered by recent graduates. The things that require many years to learn (roughly summarised as wisdom, and including judgment) have to be devalued relative to The Latest Techniques, for it is in those that recent graduates are likely to be ahead of their mentors.

The people who are most competent in the skills of youth are, then, the ones who gain the best jobs and set the continuing standards as to what shall be taught, what published, and how the academic screening points will continue to be managed. The likelihood that the skills of youth will occur in people who also possess, or will later develop, the qualities of common sense and judgment is, fortunately, not zero; there are some outstanding examples of individuals who have both. But there is little or no direct cultivation of judgment and wisdom in the field.

THE CHOICE OF LANGUAGE: WHERE WE ARE AND WHAT MIGHT BE DONE

The preceding section gave reasons why the lead in defining the major modern systems of economic theory (Marxian as well as neoclassical) has increasingly been taken by *those individuals capable of doing the most difficult mathematics*. The effect of this situation is thrown into relief by considering what would be the result of a different situation. If, for example, the leading edge of the field were defined as contained in the work of economists with another sort of skill – such as the sorts of analytical skill (clear, logical thinking, and imagination) required in the work of Harvey Leibenstein or Albert Hirschman – there would be less pressure for all the rest of the practitioners in the field to strive to show that they can operate on a mathematical frontier which may, in fact, be beyond their knowledge or understanding.

The common desire of the 'rank and file' in any field to emulate the leaders has both advantages and disadvantages. If the leaders are outstanding for their conscious reliance upon common sense and intuitive perception, inferior imitations may take the form of mushy thinking, even while the frequency of outright absurdities may be reduced. In the current situation in economics, occurrences of mushy thinking take a different form, being disguised by the apparent crispness of mathematics, and rendered invisible to their perpetrators by their own frequent inability to interpret their results in real-world terms. The requirement that *economic modelling should have a meaning in real-world terms* is, at the moment, given scant attention in the dominant academic arm of the field.

Whichever approach is dominant, the prevailing methodology and techniques of a field will not be used only by those who can do so with full competence. The reality of economic practice is not confined to the very best practitioners; an analysis of the value of techniques must include the ways in which they are, in fact, used. It is often said that nothing is gained by criticising a field through a criticism of its worst

practices. However if bad practices are widespread, and are repeated in journals and taught in classrooms, this tells us something not only about those practitioners, but also about the structure and sociology of the field which tolerates, encourages or even rewards them.

Users of mathematical language are too often tempted into wading in above their heads. It is hard enough for a first rate mathematician to make him/herself understood by the general public; that becomes downright impossible when a second-rate mathematician – or even, in fact, a first-rate one – has fallen into the trap of the Peterkin Principle[3] and has given in to the temptation of going one step beyond his/her own capacity to translate the final mathematical steps back into a verbal language. Not only does this remove the analysis from the reach and judgment of any but a few specialists, who do not have time to check the meaningfulness and realisticness of all their colleagues' work; it also deprives the individual thus stretching to his/her outer limits of the chance to check the results intuitively.

There are a variety of values to be weighed in the choice of language, in addition to the just-cited question of accessibility to a broad range of critics and to intuitive assessment. Another question is whether it is more desirable to permit, or to exclude, ambiguity. That is,

> Is ambiguity a proper and useful reflection of a complex reality; and is a language loaded with ambiguity the only possible way of making a bridge of translation between the complex world and our complex minds?
> Or, alternatively, is the gain in rigour more significant than the loss in reality when we force our experience into a set of singular, mutually consistent 'truths'?

One great strength of mathematics (when used correctly, which they often are not; in situations of extreme complexity mathematics are only used correctly when they are used brilliantly) is that they exclude ambiguity. One definition of a properly phrased mathematical statement holds that it can only have one meaning; a properly phrased mathematical question can only have one answer. By contrast, a great strength of most verbal languages is that they *permit* ambiguity.

Depending upon which value is stressed, the preference between abstract/quantitative and intuitive/verbal languages may shift. This chapter should not lead to the conclusion that, because of the dangers stressed so far in relation to non-verbal languages, they should never be used. What it should lead to is:

a recognition that there are trade-offs in the choice of language; and
a motive to further investigation as to what is the full set of trade-offs relevant to any particular situation.

Some additional institutional innovations may also be necessary in order to make it possible to develop a new way of developing and teaching an alternative system of economic theory. For example, in order to emphasise different qualities and skills than the 'skills of youth' now brought to the fore by the culture of 'publish or perish – up or out', there may need to be changes that would permit more individual development and intellectual (and other) maturation between the time a student finishes graduate school and the time that s/he must be considered for tenure. One way to accomplish this would be to say that, where social economics is taught, it would have different tenure rules than other areas: e.g., no social economist would be put up for tenure until s/he had completed something like three four-year stints at different locations; and some non-academic experience in that time would be regarded as desirable (rather than being a liability).

Given the conservative nature of bureaucracies, any change is difficult to achieve, and the particular suggestion just put forth may not, in any case, be the best one. It was suggested here mainly to emphasise two central points:

A new kind of economics will require new approaches to the education both of those who go on to practice it in the real world and those who go on to develop and teach it.
Changes, of some kind, within universities will be required to support such novel educational approaches.

WHY THE FIELD OF ECONOMICS NEEDS MORE THAN THE EXISTING SYSTEMS OF THEORY

The sociology of contemporary economic study and practice shows a strong tension between the two poles of *academic* versus *empirical/applied* economics. The definition which was proposed in Chapter 1 for the broad field of economics – starting from 'the questions asked of economists', and then modifying these with a recognition of economics' relation to adjacent fields of study, and of the cluster of goals which are especially attached to this particular field -- contained a definite bias towards application; social economics, in particular, is in large part designed to fill an important area, which might be labelled

policy economics, and which is inadequately illuminated by both the neoclassical and the Marxian systems of theory.

Ultimately, the content of the field of applied, or empirical, or policy economics is determined exogenously, by the questions that people outside the field (in government, business, policy-making in general) not only ask of economists, but are willing to pay them to answer. By contrast, the methodology of academic economics has developed quite separately from this exogenously-determined content, and is often poorly suited to it; and the content emphasised in academic economics is, I contend, to a large degree determined by its methodology.

One reason that this situation is of concern is that the education of all economists, wherever they will end up upon the spectrum between the applied/theoretical poles, is in the hands of the academic group. It is questionable whether those who will ultimately operate somewhere at the applied end are well prepared by a training programme designed near the opposite pole.

Attempting to respond to the challenges of the real world, individual economists find themselves using informal, seat-of-the-pants methods when they step outside the area illuminated by the formal theory. A major goal for social economics is to create a framework within which the best of such applied work can find a home -- a framework for generalisation about the useful real-world activities of economists – so that it will be possible to teach that kind of economics to those who wish to learn it. Such a framework will have to be found in another part of the field of economics than that now claimed by the currently dominant systems of theory.

A premise of this book is that it is unlikely that the dominant economic paradigms can or will expand to fill the conceptual space of the whole potential field of economics. I will suggest here only briefly my reasons for coming to this conclusion, and will, moreover, confine this reasoning mainly to a consideration of neoclassical theory.

The first reason to assume that neoclassical economics cannot expand to fill the rest of the space defined as the entire field of economics is that many excellent efforts to do just this have been expended without avail. Economists such as Leibenstein; Simon; Sen; Scitovsky; various game theorists; and many individuals working in the areas of labour and, above all, development economics – all have made valiant efforts to insert into neoclassical economics a more realistic understanding of human nature and more realistic models of economic behaviour, as it has seemed that these things were necessary to an expanded application of 'economics'. Each such endeavour has ended

up like a grain of sand inserted into an oyster: the irritation has produced accretions directly around the grain of sand, sometimes resulting in a little globe of interesting ideas; but the oyster – the system of neoclassical theory – has remained unchanged.

Possible reasons why this system of theory is so resistant to change include the following:

> Neoclassical economics has achieved a very tight (though not perfect) degree of internal consistency. It thus effectively excludes a large class of novel elements which, in changing some parts of the whole, elaborately interrelated system, would throw out of kilter their relationship to the rest.
>
> Neoclassical economics has developed its methodology in relation to its content in such a way that the two aspects are virtually inseparable; but the methodology has become the tail that wags the dog. This system of theory is inhospitable to any content which cannot well be handled by the elaborately developed methodologies now in use, because it would be unthinkable to reverse the direction in which the methodology is developing. Also,
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> The explanation behind reason 2 is as much sociological as intellectual. The reward systems and status orderings which have become attached to neoclassical economics (as, over time, some sort of reward and status systems inevitably become attached to any developed system of theory) are now closely related to a unidirectional type of progress which contains little possibility of doing anything other than extending the accepted methodologies.

Reference was made, earlier, to the Peterkin Principle. The name comes from a fictional family who always chose the most complicated available solution (as, for example, when they sought to reconstitute a cup of coffee chemically, after mistakenly putting salt into it instead of sugar). The Peterkin Principle states that:

> a social scientist is tempted by the reward systems of modern academia to use the most abstract level of mathematics of which s/he is capable; at which point of mental stretching the individual has gone beyond his/her ability to translate the mathematical analysis back into a verbal language – to check the results intuitively.

Contemplation of this principle and the story behind it may suggest two conclusions for the subject at hand.

The first is that economists (and all other social scientists) need to exercise discipline to resist showing off the most difficult techniques of which they are capable. Instead,

- in order *to press forward the frontiers of knowledge*, it is advisable, most of the time, *to operate well inside of the frontier of available techniques.*

The second conclusion is that,

- if the existing systems of economic theory do not suit all our needs, we may be wasting our time trying to reconstitute the paradigms from their present condition

in other words it is time to pour out a new cup.

The next three chapters of Part II will attempt to provide some tools that will be useful in the development of a new system of economic theory. In part, these are offered as tools which would be helpful in the early stages of the development of any new system of theory in the social sciences. More particularly, they are some elements of what I will suggest will be needed to fill the tool kit of the social economist.

Notes

We may contrast John Stuart Mill's comment, that 'a person is not likely to be a good economist, who is nothing else' (quoted by Marshall in *Principles*, p. 636).

As early as 1953 Pigou had commented upon this trend, saying that 'To anyone who has taken in the *Economic Journal* over a long period a notable change will have been apparent. At the end of the last century in general the articles and memoranda were written in ordinary language – *ordinary* language, not even the specialist jargon language which some people so much enjoy. Now they are predominantly mathematical in tone' (Pigou, 1953, p. 5).

The continuation of this drift has been widely noted. See, for example, 'On the Efficient Use of Mathematics in Economics; Some Theory, Facts, and Results of an Opinion Survey' by Herbert C. Grubel and Lawrence A. Boland, at Simon Fraser University, Vancouver, in manuscript. I am indebted to Vassily Leontief for bringing this paper to my attention.

See the end of this chapter for elaboration of this principle.