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THE NEXT HUNDRED YEARS

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I am pretty certain that the following prediction will prove to be correct: theorising of the 'pure' sort will become both less enjoyable and less and less possible. In the remainder of this note I shall give my reasons for making this prediction.

I do not share the view that pure theory is scholastic and so by implication bound to be irrelevant to the world. It is not my contention that it will wither under the scorn of practical men or women. The reasons for its demise are all 'internal' to pure theory itself. By this I mean that there will be an increasing realisation by theorists that rather radical changes in questions and methods are required if they are to deliver, not practical, but theoretically useful, results. This change in method will, I maintain, be a change which will deprive theorists of much pleasure.

Roughly speaking I mean by pure theory the activity of deducing implications from a small number of fundamental axioms. A theorist investigating special cases derived from special assumptions will ensure that the latter do not contradict these axioms. With surprising frequency this leads to beauty (Arrow's Theorem, The Core, etc.) and to surprise. The symmetry of the substitution term, the excess burden of indirect taxation and the two Welfare Theorems are examples. I have elsewhere (Hahn, 1984) argued at some length that it has also been crucial to our understanding of decentralised economies. Altogether then, an activity congenial to those who prefer proof to speculation and who derive satisfaction by subjecting their thinking to the discipline of the demands of consistency and coherence.

But a thriving subject will know at each stage of its development what the next crucial questions are. Pure theory is no exception. But it so happens that it is becoming ever more clear that almost none of them can be answered by the old procedures. Instead of theorems we shall need simulations, instead of simple transparent axioms there looms the likelihood of psychological, sociological and historical postulates. These new roads will find willing and happy travellers, but it is unlikely that those with the temperament and facilities of mid-twentieth-century theorists will find this a congenial road. There will be a change of personnel, and economics will become a 'softer' subject than it now is. That may, indeed surely will, be desirable for all sorts of reasons, but I am supposed to be predicting, not evaluating.

Consider the axiom of rationality. As increasingly complex questions began to be investigated it has become clearer than it was when Simon first raised his objections that it may be *theoretically* as well as empirically unsatisfactory. Certainly it was realised that the problem the rational agent was supposed to solve had to be computable. Not all are. If computable, there are strong

grounds for demanding 'computable in finite time' (or finite number of operations). But even so, computing is like working and so a subject for choice. On the other hand, until a computation has been performed its outcome is unknown and so therefore its potential benefit. The first reaction of theorists to such difficulties is to stick as closely as possible to the familiar. So the agent is modelled like a finite machine. But the subject of 'bounded rationality' is on the agenda, and I predict that it will soon escape the 'machine' into the realm of 'rules of thumb' and their simulation.

Even more serious is the realisation that the axiom has not yielded the hoped-for fruits. For instance, in game theory it has not sufficed to generate an agreed solution concept. Already there is talk of 'salient' solutions, which is not exactly a precise concept. Not only are solution concepts in dispute; for any one of them there are typically a number (sometimes very many) candidates. This led to a large literature on 'refinements', which illustrated that theorists were reluctant to follow more promising but uncongenial routes. These rather obviously demanded consideration of actual, say evolutionary, processes whose outcome might be one of the equilibria. Work here has started, but I conjecture that it will yield much activity on computers and few theorems.

What is true in game theory also applies to General Equilibrium analysis. We hailed Debreu's (1970) demonstration that almost always the number of equilibria is finite, but it still left us potentially with a very large number of them. Recently in models of incomplete markets (see below), even that result has had to be modified, and a large continuum of equilibria is possible. In a sense all of this shows that we have been on the right track: there are many possible worlds. But to explain any one of them we need to know how it came to be. History dependence stares us in the face (David, 1985; Arthur, 1989), but it is not the stuff of pure theory. Apart from anything else the analysis will quickly make analytical methods impossible. Some general insight will be gained from special simple examples, but economists will soon want to move on from these.

In this respect the signs are that the subject will return to its Marshallian affinities to biology. Evolutionary theories are beginning to flourish, and they are not the sort of theories we have had hitherto. In particular, biologists have always known that, say, the giraffe was not inevitable. There are many routes evolution could have taken even in stationary environments. But wildly complex systems need simulating. Interestingly enough, ideas from evolution are being applied to the learning (and behaviour) of the individual agent. There has been much interest in evolutionary algorithms which are designed for the computer. There are convergence theorems and there no doubt will be more and better ones. But while there will be work for the computer scientist, I very much doubt that economists will be able to establish general propositions in any but very special examples. Again I do not judge – simulation, especially when based on good data, is a perfectly respectable and probably fruitful activity.

I have just referred to learning by individual agents. For a while rational expectation theorists believed that they could skip this stage and concentrate their attention on situations where everything that could be learned had

already been learned. But the large multiplicity of such situations as well as the possibility that some of these allowed agents to live in an essentially fictitious world (sunspot equilibria) has convinced pure theorists (if not all macro-economists), that the learning stage cannot be skipped. At present some interesting models of this are being investigated (e.g. Woodford, 1988), but they are special cases and contain much which pure theorists are wont to call 'ad hockery'. That is inevitable in a situation where there is no agreement on what should be meant by 'rational learning'. (Bayesian learning is an obvious candidate but not free from objections.)

These are some of the 'grand' questions which most theorists know to be next on the agenda, and which I have argued will inevitably lead to deep changes in the manner of theorising. Paradoxically I am confirmed in this view by some of the more recent attempts to stem the tide. After all in other spheres, say religion, one often encounters increased orthodoxy amongst some just when religion is on the decline. Thus we have seen economists abandoning attempts to understand the central question of our subject, namely: how do decentralised choices interact and perhaps get coordinated in favour of a theory according to which an economy is to be understood as the outcome of the maximisation of a representative agent's utility over an infinite future? Apart from purely theoretical objections it is clear that this sort of thing heralds the decadence of endeavour just as clearly as Trajan's column heralded the decadence of Rome. It is the last twitch and gasp of a dying method. It rescues rational choice by ignoring every one of the questions pressing for attention. Moreover, those who pursue this line defend it on the grounds that it 'fits the data'. Nothing could illustrate better than this that the habits of proof and argument are gone.

There is another class of questions which have been brought to the forefront by recent theoretical work. One of these concerns the objectives of firms, the reason for their existence and the manner of their decision taking. Each of these questions will require modes of analysis quite different from those which have dominated this century.

The objective 'maximise profit' has been much debated with reference to its realism; to the theorist however its lack of meaning when 'markets are not complete' is more threatening. To reinterpret as 'maximise expected profit' requires the special *ad hoc* assumption of risk neutrality which is not deducible from the basic axioms. Equally serious difficulties arise here when markets are not perfect. If firms are, as in theory, owned by households, their motives require representation in a firm's moves, and since only in complete perfect market situations will shareholders be unanimous, no obvious traditional route of answering the question presents itself. There is here a clear invitation to add social and historical elements to an answer.

When we ask why firms exist we think of transaction costs and of increasing returns. Neither is well understood and both, except for trivial cases, resist incorporation into traditional modes of analysis. Moreover, there is unlikely to be any necessity for firms to exist. There could for instance be cooperatives or indeed no firms at all. Once again historical modes of analysis will eventually seem to be unavoidable.

As to a firm's organisation, we know that 'the entrepreneur' will not do and

that understanding will require not only organisation, information and team theory but almost surely social psychology and an account of historical development.

Evidently one could go on in this vein for a long time. My point has not been that twentieth-century theory sheds no light, nor indeed that its methods will not continue to provide some illumination. But it is my prediction that the latter will increasingly be found to be too faint in the search for answers to questions which have quite naturally arisen from twentieth-century theoretical developments. Not only will our successors have to be far less concerned with the general (leave alone the 'generic') than we have been, they will have to bring to the particular problems they will study particular histories and methods capable of dealing with the complexity of the particular, such as computer simulation. Not for them the grand unifying theory of particle physics which seems to beckon physicists. Not for them, or at least less frequently for them, the pleasures of theorems and proof. Instead the uncertain embrace of history and sociology and biology. Unfortunately, as recent work by biologists shows, these subjects are still in a state in which they can learn from our own past work rather than teach us new tricks. Our successors will be tempted by grand and woolly theories to escape the tedium of the computer. We must hope that they will on the whole resist it and patiently wait for a new dawn such as shone on those of us who came to economic theory after the last war.

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