

The future of economics

In the long run, is the subject dead?

THE discipline to which this column is dedicated is in trouble. It nearly always has been. Only for a decade or two after 1945 did things look promising. That was when a revolution in methodology led many to believe that economics would at last become a proper science, not a literary pursuit for gentlemen-scholars. Mathematics, statistics and formal theoretical models were the new way forward; better forecasts and scientifically proven economic policies would be the result. Alas, the mathematics stayed but the optimism did not. Clarity and certainty, not to mention public esteem, still elude the discipline.

Where has economics gone wrong? A symposium in the current issue of *Journal of Economic Perspectives* looks at the future of the subject from different angles, and suggests some interesting answers.

But a first possibility (an obvious one, you might think, to practitioners of the dismal science) was somewhat neglected: maybe the discipline has come down with nothing more complicated than a bad case of diminishing returns. Perhaps as much light as will ever be shone on the inexhaustibly complex behaviour of economic actors has by now been cast, and it is futile to hope for more. Of course, it will always be possible to gather extra data, looking for finer and finer detail—hence the trend towards specialisation in the universities. But this will not, you might argue, render the big picture any more faithfully, or bring general economic laws any better into focus. Indeed it may make the big picture harder to see, until the search for general

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laws is called off altogether.

A depressing thought. It would be much more encouraging to think that economics has been making big mistakes. Mistakes can be corrected, and the onset of diminishing returns pushed back. Two of the most interesting papers in the symposium argue that the discipline has indeed been making more or less the same mistake—though, true to form, the authors propose very different ways of putting things right.

David Colander of Middlebury College, in an article that looks back on the present from an imagined 2050, blames current discontent on the orthodox general-equilibrium model that underlies most of today's economic theory. He favours a shift from the current approach, which has been called "loose-fitting positivism" (propose a model consistent with standard assumptions, then test it), to one based on "loose-fitting pragmatism" (forget about canonical principles, just search for patterns in the data).

Such an approach, he says, would be consistent with "the rise of complexity science within the scientific community generally". Researchers sitting at their computers, subjecting data to a withering barrage of statistical analysis, would still hope to come up with laws of a sort, or regularities at any rate. But these "laws" would be regarded as provisional and ever-shifting; indeed, the claim is that changeless underlying patterns do not exist. Complex systems expand and evolve; even at the most fundamental level, the patterns are temporary. But whether this approach could still be called "economics" is debatable.

The second approach is much easier to reconcile with traditional methods. Its most celebrated exponent is Richard Thaler of the University of Chicago, who has also written a paper for the symposium. Mr Thaler agrees that the canonical principles of orthodox theory have led economics astray, but he believes these mistakes can be put right. He seeks, in other words, a tighter-fitting positivism. You improve the fit above

all, he would argue, by putting a more realistic account of human cognition at the centre of the theory.

Orthodox theory famously assumes that people are rational. In reality, they are not. On the other hand, they are not crazy, or crassly incompetent—in other words, their behaviour is not random. If economics could try harder to recognise that people try to be rational, but in certain, often predictable, ways fail to be, the positivist approach would have a better foundation. In essence, what Mr Thaler calls for is a marriage, or at least much closer cohabitation, between economics and psychology.

In fact this liaison has already begun (see the article "Rethinking thinking" in our issue of December 18th). Lots of papers on "bounded rationality" or "nearly rational" behaviour are now appearing. One of the most fruitful insights from psychology is "prospect theory", which predicts that people are more hurt by losses than they are uplifted by gains of a corresponding size. This is a violation of "rationality", and yet as an alternative canonical principle it is theoretically tractable: settled models of economic behaviour can be built up from this starting-point.

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In his paper, Mr Thaler looks further ahead and suggests new directions for this kind of research. Bounded rationality is already being examined, but much less attention has been devoted, for instance, to bounded memory. Consider what absent-mindedness implies for the theory of how principals (such as shareholders) deal with agents (such as managers). A forgetful principal finds it difficult to distinguish between success and failure on the part of his agent, because bad outcomes are often regarded, after the event, as having been foreseeable ("hindsight bias" is a well-documented psychological phenomenon). Emotion is yet another tract of relatively uncharted economic territory: people often do things out of love, or out of spite, and not just in pursuit of material self-interest.

If models are to reveal anything, they must be simpler than reality: the challenge is to simplify usefully. Mr Thaler's direction offers real hope of models that are truthful without being uselessly unwieldy. To go the other way, abandoning orthodox scientific method altogether, may conceivably produce better economic forecasts—but grinding out statistical patterns sounds a lot less interesting than thinking about what people do and why. And economics owes it to the world to be interesting.

