ANOTHER CENTURY OF ECONOMIC SCIENCE*

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I. METHODOLOGICAL INNOVATION:

THE TRIUMPHS OF TWENTIETH-CENTURY ECONOMICS

There is a widespread consensus that during the past century, economic science has come of age. It has developed powerful statistical tools to analyse economic data, to make forecasts, and to test alternative hypotheses, and it has employed sophisticated mathematical techniques to articulate its theories and to prove basic theorems characterising the economy. Samuelson (1947) encapsulated the mid-century enthusiasm for the scientific method in his classic *Foundations of Economic Analysis*, arguing that economics should be based on observable behaviour and testable hypotheses, and, with his theory of revealed preference, showing how this could be done with the theory of consumers' behaviour.¹

II. METHODOLOGY OVER SUBSTANCE? OR IDEOLOGY OVER SCIENCE?

Yet, in spite of these methodological triumphs, the subject does not bear all the hallmarks of some of the other sciences. Most strikingly, while economists of many persuasions may agree about the tools to be employed, there is no agreement about the basic economic model for describing the economy: while in many circles, the competitive model, with perfectly informed agents, rational consumers and value maximising firms, is believed to provide the foundations for understanding both the aggregative behaviour of the economy and its components, in other circles, that model is viewed with some circumspection. Evidently, the tools are not strong enough to discriminate among fundamentally different hypotheses, or at least not strong enough to overcome differences in prior beliefs, beliefs which are often influenced by ideological concerns.

III. TWENTY-FIRST CENTURY ECONOMICS: CLOSER TO A TRUE SCIENCE?

My hope – and belief – is that the next century will be marked by a greater confluence of ideas, a greater degree of agreement on the underlying descriptions of the economy, not just on the tools used to analyse it. To be sure, economists will continue to differ in the detailed interpretation of events, and in the appropriateness of a particular model to a particular situation.

* Financial support from the National Science Foundation and the Hoover Institution are gratefully acknowledged.

¹ The irony that the logical foundations of Logical Positivism had already been severely attacked by the time that Samuelson tried to import it into economics need not detain us here.

IV. A GENERAL ECONOMIC THEORY, SYNTHESISING MACROECONOMICS AND MICROECONOMICS

The disparity in how different economists view the economy is perhaps most apparent if we reflect on the two great achievements of the past century: the development of the neoclassical–Walrasian paradigm, including the proofs of the fundamental theorems of welfare economics, the formal articulation of Adam Smith's invisible hand conjecture on the efficiency of market economies; and the development of Keynesian economics, with its argument that capitalist economies may be characterised by unemployment equilibria. The view of capitalism reflected in these two achievements seem diametrically opposed, and not even Samuelson's assertion of the neoclassical synthesis – that the economy, once the problems of unemployment were corrected, was well described by the neoclassical model – could smooth over the obvious schizophrenia in the profession. As I have written elsewhere (Greenwald and Stiglitz, 1987), there were two obvious remedies: making macroeconomics like neoclassical microeconomics (the new classical and real business cycle theories); and trying to develop a microeconomics which yielded aggregative implications which were more consistent with the observed behaviour. As the memories of the Great Depression and the multitude of other, earlier episodes of mass unemployment receded, the former school enjoyed a brief moment in the sun – maybe the Great Depression was not that bad, after all! But events have an uncanny way of interfering with such euphoria about the market economy, and the extended periods of high unemployment in Europe have placed new emphasis on research attempting to provide the micro-foundations of unemployment and business fluctuations. During the next century, I am confident that we will construct a unified theory, based on the recognition of the importance of information costs and other imperfections in labour, capital, and product markets.²

V. THE DEMISE OF EARLY TWENTIETH-CENTURY NEOCLASSICAL ECONOMICS³

One of the great achievements of the neoclassical economics of the past half century is the formulation of testable hypotheses. Some of the most interesting – such as the Modigliani–Miller theorem, showing that firm financial structure did not matter, and its correlate implication, that investment and other aspects

² As, for example, sketched out in my 1987 paper with Bruce Greenwald which discusses both information problems in the labour market (efficiency wage theories) and in the capital market, and in subsequent work. Other strands of ongoing research, such as those stressing the role of increasing returns and complementarities, will undoubtedly play a part in the unified theory which will ultimately be developed.

³ There seems no agreed-upon definition of neoclassical economics; by some, the term neoclassical is synonymous with 'good'; thus recent work in transactions costs and information economics is embraced as part of neoclassical economics. I am using the term here (with the qualifier 'early twentieth century,' lest there be any confusion) to represent the perfect competition, perfect market model in all of its representations, including Samuelson's *Foundations*, Arrow–Debreu, and Modigliani–Miller.

of firm behaviour did not depend on firm balance sheet and cash flow variables – have been tested and rejected, both on the basis of casual empiricism and detailed econometric studies. While debates remain about which of the assumptions underlying the analysis is *most* faulty, e.g. the absence of bankruptcy, the assumption of perfect information, the hypothesis that firm income in each state of nature is fixed (unaffected by firm behaviour, which in turn might be affected by financial structure), the absence of transactions costs, it is clear that dropping any one of these assumptions leads to markedly different results.

The economists of the twentieth century, by pushing the neoclassical model to its logical conclusions, and thereby illuminating the absurdities of the world which they had created, have made an invaluable contribution to the economics of the coming century: they have set the agenda, work on which has already begun. We have already seen how the information theoretic paradigm can explain behaviour in the capital market (such as credit rationing and red-lining), product market (such as price dispersion and a variety of arrangements, such as non-linear pricing, intended to price discriminate in an environment in which informational imperfections limit perfect price discrimination), and labour market (with wages set at levels above market clearing).

VI. THE NEW INSTITUTIONAL ECONOMICS

Not only does this strand of literature offer the hope of providing the micro-foundations required to explain observed aggregative behaviour of the economy, but it also provides an explanation of many of the central institutional features of the economy. In the earlier part of the century there were major conflicts between institutional economists, who saw the particular arrangements by which particular economies conducted their economic affairs as essential, and neoclassical economists, who sought to see through these inessential details to the underlying fundamental forces – the forces of demand and supply. By the middle of the century, the triumph of neoclassical economics was – almost – complete, certainly in America and England, and by 1980, even in Germany. Yet, before the death-knell had been sounded, a New Institutional Economics had arisen, attempting to use the new insights to explain the institutions and to examine their consequences. For instance, Cheung (1969) argued that transactions costs could explain the institution of sharecropping, while Stiglitz (1974) developed a theory of sharecropping based on the costs of monitoring workers' effort. While both theories provided explanations of the persistence and pervasiveness of this institution, and even provided suggestions of the conditions under which one might expect it to become relatively less important, they had markedly different implications for, for instance, the consequences of a land reform. I expect that during the next century, this New Institutional economics will flourish, providing insights into more and more of the detailed arrangements through which economic affairs are conducted, and in some cases, provide bases for altering those arrangements in ways which will enhance economic efficiency.

VII. ORGANISATIONAL ECONOMICS AND COMPARATIVE ECONOMIC SYSTEMS

The major *economic* event of the latter part of the twentieth century has undoubtedly been the demise of socialism. While the desirability of socialism was a central subject of discussion in debates (e.g. between Hayek and Von Mises, on the one hand, and Lange-Lerner-Taylor on the other) during the first half of the century, it was not until the development of information economics during the past fifteen years that the nature of the information problem stressed by Hayek in his classic (1945) paper has been better understood.⁴ We have begun to understand, for instance, *which* of the many information problems facing an economy prices really adequately address. As the former socialist economies think about the kind of economic system they would like to adopt – is there, for instance, a ‘third way’? – their discussions have focused attention on such central issues as the role of property and the role of competition. The exploration of these questions, and an enhanced understanding of the merits of alternative economic systems, is likely to be another major achievement of economics of the next century.^{5,6}

One aspect of this analysis will focus on the economics of organisations. Most economic activity occurs within organisations, within which only limited use is made of the price system. It has frequently been observed that General Motors is larger than many economies. The success of the economy depends not just on how well markets work, but how well these organisations work. We have only just begun the exploration of how organisations function, what are the consequences of alternative ways of organising decision making, and what are the interactions between organisational design and incentives. (See, for example, Sah and Stiglitz (1985).) We know, for instance, that rent-seeking behaviour may be important within private organisations, just as it is in the public sphere.⁷ While I predict major advances in this area, which until recently has remained on the periphery of mainstream economics, I see one major obstacle that will limit the success of this research endeavour: the interactions within organisations, particularly small organisations, are governed not just by the narrow ‘rational’ concerns upon which economics has traditionally focused. (See Simon and March (1955).)

VIII. BROADENING THE ‘RATIONAL ECONOMIC MODEL’

The deficiencies in the ‘rational actor’ model have long been recognised, but economists have defended their pursuit of the rational actor model on the grounds that it was the best game in town: it gave well-defined (refutable, and,

⁴ For a fuller discussion of these issues, see Stiglitz (1990).

⁵ Whether the recent literature on mechanism design will contribute significantly to these discussions remains a moot question. So far, it has not.

⁶ Already, these discussions have made clear the central importance of certain ‘institutional’ features (including laws regulating competition and bankruptcy) which were ignored in traditional neoclassical economics, though focused upon in the imperfect information-imperfect markets paradigm.

⁷ See Hannaway (1989) or Milgrom and Roberts (1990).

unfortunately, refuted) predictions, while the alternative was a Pandora's box – there was an infinity of possible irrational behaviours.

Just as one of the great contributions of twentieth-century neoclassical economics was to make clear why considerations which they had excluded from their analyses – such as information and transactions costs – simply *had* to be brought into the analysis, so too one of the central contributions of game theory has been to make it clear that the 'rational' actor model is not only descriptively inaccurate (as earlier economists had charged), but internally incomplete and/or inconsistent (see Binmore (1987, 1988) and Reny (1985)). The hope of game theory that some simple version of rationality could lead to well-defined, let alone reasonable, predictions of behaviour has been dashed. Game theorists have increasingly relied in their analyses on 'small' degrees of irrationality, while at the same time showing that the exact nature of the equilibrium depends precisely on the nature of these small irrationalities (see Fudenberg and Maskin (1990)). This research makes it clear (if it was not already so) that economists must study how individuals actually behave, whether that conforms to some economists' preconception of rationality or not.

Fortunately, advances in sociology and psychology (see, for example, the work of Tversky) have shown that there may be systematic patterns to individual behaviour, even when they are irrational. Economic science is concerned with exploring predictable behaviour; the fact that behaviour is not rational, in some sense, does not mean that it is not predictable. Akerlof, in a variety of papers, has shown that these concerns can be incorporated into economic models. I anticipate that over the next century major advances in this direction will occur. At the same time, different people behave differently in different situations. It is not clear that there will emerge out of this work a general theory – a theory of the generality of the 'rational actor' model.

IX. THE ROLE OF GOVERNMENT

Deciding on the appropriate role for government has long been one of the central concerns of economics. This, too, is one of the questions which the former socialist economies are now asking themselves. As the century draws to a close, many of the achievements of twentieth-century economics in this regard are being called into question.

First, the Fundamental Theorems of Welfare Economics provided not only the formal articulation of Adam Smith's invisible hand conjecture, they also provided the framework for the market failures approach to the role of government. Yet, more recently, Greenwald and Stiglitz (1986, 1988) have shown that whenever markets are incomplete and information is imperfect – that is, essentially, always – the economy is almost never constrained Pareto efficient; there are, in principle, government interventions, consistent with the limitations on markets and information, which can make some individuals better off without making anyone else worse off.

The market failures approach itself was attacked by the Public Choice economists, who emphasised that one had to analyse the behaviour of the government in terms of rational behaviour of voters, bureaucrats, and special

interest groups. But while these theories share with stock market analysts the ability to provide ready interpretations of whatever occurs, their success in predicting these political forces is much more limited. How do we explain why alleged rent-seeking behaviour interferes with economic efficiency in Pakistan, but has much more limited deleterious effects in Korea; why corruption in Korea was a problem in some periods and not in others; why agriculture is subsidised in those countries where it is small and taxed where it is large; why the same pattern does not exist for other commodities; why was there 'regulatory capture' in some states, in some industries and not in others? If economists really believed these models, why devote so much effort to changing the Common Agriculture policy? Surely the words of a few economists cannot change the economic realities?

Yet the centrality of the issues will ensure that these issues will continue to be a primary focus of research in the coming century. I envisaged considerable advances in defining the ways in which government is different from other economic institutions,⁸ understanding the circumstances under which markets are not constrained Pareto efficient and devising institutional arrangements which will enable the government to effect Pareto improvements, and in enhancing our understanding of public failures, both the circumstances in which they occur and the reasons for them. These advances, I suspect, will make use of the insights into organisations and the broader perspectives on economic behaviour described in the preceding two subsections.

X. DYNAMICS

The formal achievements in dynamic economics of the past century are indeed impressive; these include developments in linear and nonlinear business cycles, chaos, turnpike theory, and neoclassical growth theory. We have come to recognise the central role of expectations, and the kind of dichotomy common in the earlier part of the century, where ad hoc dynamics are adjoined to sophisticated equilibrium behaviour, is now eschewed by the profession at large. Yet, when all is said and done, while our mathematical tools for analysing dynamics are greatly improved, I am not sure that we have learned a great deal about either the short- or long-run dynamics of the economy. Short-run dynamic models have ignored the central role that credit constraints, partly based on information asymmetries, play.

In the long run, technological change is central, as Schumpeter emphasised and as the neoclassical growth models of the 1960s helped to quantify. We now have a *better* understanding of the microeconomics of technological change, and work has begun on the construction of macroeconomic models based on those microeconomic foundations, models which reflect some of Schumpeter's views concerning the role of credit constraints and imperfect competition. These models incorporate both learning by doing and R & D expenditures.

Though over the years, economists have played a certain amount of lip-service to evolutionary processes and the role of natural selection, there has

⁸ I have attempted a beginning of this line of enquiry in Stiglitz (1989).

been relatively little formal modelling of this evolutionary process,⁹ of the role that bankruptcy laws and competition policy play in that process, and indeed, no evaluation of the efficiency with which that process works, and upon what that depends.

These failures have increasingly been recognised, and the work that has recently begun trying to remedy these deficiencies at least holds out the hope that over the coming decades, significant progress will be made.

XI. THE FAILURES OF TWENTY-FIRST-CENTURY ECONOMICS

I have dwelt extensively on what I see as the most important achievements of economics over the next century: the development of a general economic theory, unifying macroeconomics and microeconomics, able both to explain its aggregative behaviour and the details of some of its more important institutions.

I have forecast partial success in three other dimensions: an understanding of the economic behaviour of organisations, within which so much economic activity occurs; a development which in turn will be based on an incorporation into economics of systematic findings of other social sciences, notably psychology and sociology; and an enhanced understanding of the economic role of the government, and the development of a theory of public failure to parallel our analysis of market failure. Within the more developed countries, these enhanced understandings will, I am confident, lead to better public economic policies and greater economic efficiency, both within the public and private sectors, contributing a modicum to an enhanced standard of living.

There is one important area in which I am less sanguine about the future success of our profession. I began the study of economics with the (admittedly naive) hope that the study of economics would somehow enable something to be done about the plight of the three-quarters of mankind living in desperate poverty, particularly within the Third World. In the ensuing quarter of a century, we have seen remarkable growth in several countries, some of which have moved out of the ranks of the less developed. In each case, we can, with the vision of hindsight, tell stories about what led to success. But we have no prescription, no formula with which to go to those who remain among the poor, which gives them even a reasonable hope of success. Indeed, we can only imperfectly guess which among the LDCs will be successful, or even which of the more developed countries will fail to grow. Who in the middle of the nineteenth century, could have forecast the fortunes of England and Argentina? In the ensuing century, several of the less developed countries will undoubtedly join the ranks of the middle and upper income countries. If we are lucky, our studies of developing countries will enable a few more countries to escape the mire of poverty within which they have lived for centuries. We can only try.

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⁹ There are, of course, exceptions; see, for example, Nelson and Winter (1982).

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