Symposium on Microeconomics
1 Reflections on the State of Economics: 1988*

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Let me first express my mixed feelings about being invited to share with you some reflections on the state of economics. Traditionally, it is the grey beards within the profession who are called upon to deliver addresses such as this. Though I have come to deal with the unpleasant fact that my beard is greying (a fact which intrudes upon my consciousness each time I look in the mirror or have my beard trimmed, and look upon the trimmings on the barber’s floor), I still prefer to think of myself as a promising young man. It is bad enough being called a middle-brow theorist; I am not sure I can cope with being thought of as a middle-aged economist.

Some two decades ago, there were several occasions on which several of the more distinguished grey beards of an earlier generation were called upon to share their reflections. Galbraith (1973) and Leontief (1971) all expressed a strong sense of disappointment with the directions in which they saw the profession was moving, a profession to which they had, in their youth, contributed so much.

To a large extent, their concerns were, I think, well justified. At the time, the competitive paradigm reigned supreme. At MIT, we were taught the competitive model just as those at Chicago were: the only difference was that at Chicago, they believed the model, while our teachers conveyed certain doubts. At the very least, we were taught that we should not have the sense of conviction that those who went to Chicago had. But we were not taught an alternative approach; there was not an alternative paradigm. For this, we were left on our good sense, on judgements which were outside the scientific paradigm.

There was, in much of economic theory, a certain sterility: clever students demonstrated their high IQ’s by Ptolemaic exercises showing how one could reconcile evidence that seemed inconsistent with the competitive paradigm with that model. Theories in which the cost of shoe leather could explain why individuals would accept much lower rates of interest on certain bank accounts (compared to what they could receive on alternative financial instruments) were taken seriously. Kudos went to those who could show perverse results, such as how seat belts could increase accident rates, as a result of individuals taking less care, given that the danger if an accident occurred was reduced.

Let me say, at the onset, that today I am very bullish on economics. There has been a paradigm shift. There has been a tremendous expansion both in the range of questions addressed and the range of tools, concepts, and theories used to provide answers. We have come to understand some of the major deficiencies of the competitive paradigm, and we have had considerable success in constructing alternative approaches. There is, I think, throughout the profession, a sense of excitement about these developments. Of course, the new views are not universally accepted. There are bastions of resistance. But even this contributes to the liveliness of the discipline, as advocates of the new approaches engage in active competition in the market place of ideas with the defenders of the Old Wisdom.

I want to divide my remarks into two parts. In the first, I shall identify three specific areas in which the profession has made great strides during the past two decades. In focusing on these, I do not mean to denigrate the importance of other areas: these are simply some of the areas with which I

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am most familiar. In the second, I shall identify some areas in which, were I to grade the profession, its marks would be less than perfect.

Some Recent Successes

The three areas of success on which I wish to focus are, to some extent, related. The first concerns the recognition of the importance and consequences of imperfect and costly information; the second the recognition of the importance and consequences of imperfect competition. And the third is concerned with the enhanced understanding of the behaviour of firms.

Imperfect Information

Traditional competitive theory (represented by, for instance, the Arrow-Debreu model), while it recognized the possibility of imperfect information, assumed that individuals' information (beliefs) were fixed. Observations concerning prices and quantities, for instance, did not alter their beliefs, nor did these theories contemplate the possibility of individuals expending resources to obtain better information.

Earlier work in this area showed how all of the traditional results in competitive equilibrium theorem were not robust, that introducing even small imperfections in information could drastically alter the conclusions. The traditional theory came to be viewed as a limiting case, a special example of particular historical interest, but (except for its ability to handle large numbers of superscripts and subscripts) hardly a general theory.

Among the fundamental results which were called into question were those concerning the existence of equilibrium (Rothschild-Stiglitz, 1976; Wilson, 1977) and the characterization of equilibrium (the repeal of the Law of Supply and Demand [Stiglitz, 1987], the repeal of the Law of the Single Price [Salop and Stiglitz, 1977; Stiglitz, 1985], the existence of equilibrium with no trade among individuals with different preferences and endowments [Akerlof, 1970], equilibrium prices not fully revealing information, i.e. capital markets not being efficient [Grossman and Stiglitz, 1976, 1980], and equilibrium prices exceeding minimum average costs [Diamond, 1971]). More generally, the fundamental mathematical structure underlying so much of competitive analysis—the assumptions concerning

1 For a fuller discussion of these, see Stiglitz (1985).
For a survey of results concerning product markets, see Stiglitz (1989).

convexity of technology—have been shown not to apply in situations where information can be acquired at a cost (Stiglitz, 1989; Radner-Stiglitz, 1984).

A central tenet of classical economics is Adam Smith's invisible hand, the contention that competitive markets lead to Pareto efficient allocations. This idea has been embodied in the fundamental theorems of welfare economics. Both of the theorems have been shown not to be true when information is imperfect (or risk markets are incomplete).2 In a sense, Arrow and Debreu's great achievement was to find that singular case—that special example—where Smith's insight was valid.

While in its earlier developments, information economics focused on these criticisms of the standard competitive paradigm, more recent work has gone well beyond this. It has shown that it can provide an explanation of a wide range of phenomena which are either inconsistent with or not addressed by the standard paradigm. It has provided insights into how capital, labour, and product markets work. It has provided the basis of a new theory of the firms, and new foundations for macroeconomics. It is the power of the paradigm to provide explanations for phenomena which hitherto had remained unexplained which accounts, I think, for so much of the success of the new paradigm.

Imperfect Competition

A second area of success in recent work in

2 Greenwald and Stiglitz (1986, 1988) show that under these circumstances, market equilibrium is essentially never constrained Pareto efficient; that is, there exist interventions by the government, which respect the limitations on the existence of markets and the costs of information, which can make some individuals better off without making any one worse off.

There exist certain singular cases, e.g. stock markets in economies in which there exists only one commodity, or insurance markets with moral hazard with only one commodity, which are constrained Pareto efficient. But there are singular cases.

Armott and Stiglitz (1986) analyze the set of tax interventions which, in the presence of moral hazard, can improve the efficiency of the economy.

In a subsequent paper (Armott and Stiglitz, 1988), they attack the second Fundamental Theorem of Welfare Economics, that every Pareto efficient equilibrium can be attained by a decentralized market economy with lump-sum redistributions. They show not only is this not true, but it is not even the case that the government can attain certain Pareto efficient outcomes with a decentralized mechanism in which the government uses ad valorem and specific taxes and subsidies.
economic theory concerns imperfect competition. It is now widely recognized that most firms do not face horizontal demand curves for their products. In many, if not most industries, the competitive paradigm does not provide a good first approximation. This is not to say that there is no competition—there is—but competition takes on forms which are quite different from those envisaged by the traditional competitive market.3

We have made great strides in understanding both the origins of the imperfections of competition and its consequences. Imperfect information has been shown to give rise to imperfect competition, even when there are a large number of firms (Diamond, 1971; Stiglitz, 1987; Salop, 1976). Even small sunk costs may serve as an effective barrier to entry (Stiglitz, 1987).4 (R & D will, in general, be associated with non-convexities of the kind which give rise to imperfect competition, particularly since expenditures on technological change are, effectively, sunk costs.)

We now also understand better why the limiting models of perfect competition and perfect monopoly have to be treated with such caution: in these models, firms have no incentive to take actions which affect the degree of competition. In many markets with imperfect competition, however, much of firm's efforts are directed at effecting the degree of competition, of deterring, for instance entry, or undertaking actions which facilitate collusion.5

The models of imperfect competition have not only affected standard micro-theory, but have found widespread applications, e.g. in the theory of international trade. They have provided an alternative set of explanations (to Heckscher-Olin theory, based on factor endowments and tastes) for the nature and direction of trade, an explanation which is much more in accord with the facts concerning most trade (which occurs among countries of comparable endowments and tastes) than the earlier theory.6

The New Theory of the Firm

A third major development concerns our enhanced understanding of the nature of the firm. For decades, the managerial theory of the firm was dismissed as ad hoc and atheoretical, though it often seemed to provide a better description of firm behaviour than the neoclassical theory of the firm.

Transactions costs theory (Williamson, 1975) has provided us insights into why some economic activity occurs within firms, and some does not. Information economics has provided us with a rationale for why managers in fact have considerable discretion.

We now understand better why traditional control mechanisms (e.g. take-overs and voting) work only imperfectly.7 Managers' incentives may not perfectly coincide with that of their shareholders. There is what has come to be called a moral hazard or principle agent problem.8

The standard theory of the firm also assumed that the firm had unlimited access to the capital market; so long as it had a project which yielded may actually lower welfare, as firms are induced to engage in entry deterring activity (Stiglitz, 1981).

Other work has been directed at explaining a wide variety of practices, such as vertical restraints, including exclusive territories. (See Rey and Tirolo, 1986, and Rey and Stiglitz, 1988)

Much of the progress in this area has been the consequence of the application of insights from information economics (e.g. in the discussion of vertical restraints) and the techniques of modern game theory.

Among the important contributions to this literature are those of Krugman (1979), applying the Dixit-Stiglitz (1977) theory of monopolistic competition. These new trade theories have even questioned the basic propositions concerning the gains from trade. For a survey, see Krugman (1989).


8 This problem was first discussed by Stiglitz (1974) and Ross (1973). Jensen and Meckling (1976) refer to this as the agency problem.
a return greater than the market interest rate, the project would be undertaken. Problems of information asymmetry (both moral hazard and adverse selection) mean that firms may face serious problems in raising capital; funds outside the firm are not perfect substitutes for funds inside the firm. Firms may be finance constrained—they may face both credit and equity rationing. These capital market constraints have profound implications for firm behaviour.9

Finally, earlier theory had little to say about the structure of decision making within firms, about the relative merits of hierarchical versus decentralized decision making and the circumstances under which one might expect one organizational form over another. The recent work of Sah and Stiglitz (see, e.g. Sah and Stiglitz, 1985, 1986) provides a framework within which these questions can be addressed. It recognizes that limited information results inevitably in human fallibility, that individuals will make mistakes, say, in their evaluation of different projects, and it shows how different ways of organizing decision making (what they call the architecture of an economic system) affect how those mistakes are aggregated, e.g. the prevalence of good projects being rejected versus bad projects being accepted.

Four Complaints

I would be remiss in my responsibilities if I did not take this opportunity to express some complaints and concerns, some misgivings about some of the unfruitful directions in which the discipline has concentrated its energies, and some of the potentially important directions which in my judgement have received inadequate attention.

Decisions about how resources are allocated within our discipline (as in other academic areas) are made in a very decentralized way. We would be naive to assume that the free market for ideas necessarily results in an efficient allocation of resources; indeed, one of the results to which I referred earlier was that, with imperfect information, the free market equilibrium is essentially never Pareto efficient. But that does not imply that there is any obvious or simple remedy to market inefficiencies.

Resource allocations within the discipline are affected by the funding agencies (NSF), by the various departments, in their hiring and promotion decisions (and the incentives to which that gives rise) and in the influence which they exert on the impressionable graduate students, and by the journals (with acceptance policies affecting incentives both directly, and indirectly, through the effect of publications on promotions and hiring).

The coin of the realm is persuasion: It is my hope that these brief remarks at least draw attention to some of the potential deficiencies in the discipline’s resource allocation, and widen the debate within the profession (within our academic departments, our journals, and the funding agencies) about the direction of both our research and teaching.

The four problems which I note are certainly not the only problems, and they may not be even the most important ones. They represent areas in which I have been particularly involved, and therefore about which I feel quite strongly.

Microeconomic Foundations of Macroeconomics

For the decades immediately following Keynes, a standard complaint was the absence of adequate micro-foundations for macroeconomics. Economics had a schizophrenic attitude towards its two major subdisciplines. In microeconomics, students were taught the fundamental theorem of welfare economics, how perfectly markets functioned. In macroeconomics, they focused on the ills of the capitalistic economics. They learned, for instance, that the market could be characterized by unemployment.

The ruse of Samuelson—the neoclassical synthesis—which held that, except for unemployment, the market economy was ‘perfect’ has been recognized for what it is: a ruse, based not on any scientific propositions but rather a simple articulation of faith.

The attempts to reconcile macro- and microeconomics during the past decade are thus, in one sense, quite welcome. But these have taken two different courses. One has attempted to make micro like macro, the other macro like micro. The former has shown how the analysis of markets with imperfect information—labour, capital, and product markets—can provide a basis for understanding many of the central phenomena of macroeconomics.

The latter adopted the perfect information, perfect competition model just as that model was being discredited.

One aspect of that approach which is of particular concern is its use of representative agent
models. Economics is a social science. If the economy consisted of a single individual, Robinson Crusoe, there could not be any coordination problems. Issues of asymmetric information require there being at least two individuals. Thus, in my view, these models are likely to be of very limited use in understanding macroeconomic phenomena, although, to be sure, by illustrating the rich set of behavioural patterns that are consistent with a representative agent, they have enhanced our understanding of economic processes.

Rationality

Some economists have taken the assumption of rational behaviour as defining what economics is all about. An economic model without rationality is like life without love: it simply doesn’t make sense. It’s not economics.

I think this view is wrong. Economics is a behavioural science. It is concerned with explaining how individuals and societies make resource allocation decisions. Whether individuals act in a rational way is, to a large extent at least, an empirical question. Economists should have an open mind on this.

We have become increasingly aware of certain areas of individuals’ behaviour which impact upon economic decisions which can be reconciled with conventional notions of rationality only with the most tortured Ptolemaic reasoning. Psychologists have explored in detail how individuals form judgements under uncertainty, and imperfect information, and they have noted systematic biases in those judgements. Though it has become fashionable in some circles to insert dutiful footnotes, acknowledging the fact that the author is aware of the writings of Tversky, Kahneman, and their associates, there have been few attempts to integrate the insights into economic analysis; the footnotes do little more than demonstrate the authors’ breadth of vision, while the fact that the lessons to be drawn from these studies are studiously ignored is meant to convey the author’s soundness of judgement.

There are, of course, some important exceptions—Scitovsky (1976), Akerlof (1982), and Akerlof and Dickens (1982), come to mind—and the increasing attention which their work has attracted is a hopeful sign for the future of the profession.

Technological Change

The two complaints I have just discussed are, to a large extent, concerned with methodology, with how economists approach the questions in which they are interested. The next two carp are complaints about the choice of topics.

If one were to pick up a newspaper or business magazine, a central problem, certainly for the US, is international competitiveness and the decline in the rate of productivity growth. Has the US lost its technological lead? How can we maintain an ever increasing standard of living?

Addressing these questions requires an understanding of the economics of technological change, of the determinants, for instance, of R & D and learning-by-doing. Though these topics get far more attention today than they did a decade ago, the disparity between the importance attached to them by economists (at least as revealed by their behaviour) and popular concern is remarkable. For instance, the number of research proposals to the National Science Foundation on topics related to technological change are far fewer than the number proposing still one more wrinkle on a standard game theoretic model of oligopoly where technological change plays no role—this in spite of the fact that technological change provides the arena of competition for many oligopolies.

One of the reasons, of course, for the lack of proposals in this area is the difficulty of embedding problems of technological change in the standard competitive, perfect information paradigm. Research and learning can be viewed as special forms of information acquisition, and all of the difficulties with the old standard paradigm that were noted in our discussion above of information economics apply here. Several of the proposals I have seen represent serious attempts to explore alternative approaches (borrowing, for instance, what psychologists have learned about learning). Work in this area is at an early stage, and not surprisingly, there is no consensus about what will prove to be fruitful approaches.

The reviewers of these proposals were, in my view, unnecessarily unsympathetic to the difficulties facing these researchers: proposals were criticized, for instance, for the failure to use good Bayesian statistical inference theory, while the heart of the economics lies, at least in my judgement, elsewhere.

The difficulty of the topic—the absence of any consensus model—provides one of the explanations for the lack of research in this area; but it cannot be the full explanation.

10 For a recent discussion of some aspects of this research and a guide to further reading, see Machina (1988).
A second part of the explanation has to do with the basic graduate courses. These have increasingly focused on teaching tools. The standard competitive model provides a convenient context in which the new techniques can be used. The courses are not problem oriented. Many graduate courses, even in our finer universities, spend almost no time on technological change. It would be unusual if more than two weeks were devoted to the topic.

It is important for students to learn techniques of analysis; but it is no less important that they learn what are the important questions. What is required, in my judgement, is a greater balance. Students' sense of what is important is learned by observing the successful members of the profession. The system is self-perpetuating!

My complaint here is hardly original: Schumpeter11 complained forcefully and clearly—but with little success—about the courses in his day. He was rewarded for his efforts by being studiously ignored, at least in the standard graduate courses.

Development Economics

My remarks here will, perhaps, show my age:

When I was a youth, most of us professed a deep concern for the poverty which afflicted the vast majority of mankind, and particularly those who lived in the less-developed countries. It was the perhaps misguided hope that we might do something about these problems that provided one of the main attractions of economics. Development economics was becoming a coherent, perhaps even respected, sub-discipline. It attracted great minds like Little and Scitovsky, in one generation, Mirrlees and Sen in the next.

Progress has been admitted slowly—though perhaps not so much slower than in some other branches of economics. But today, few of the better students chose to specialize in development economics, or even to take development as a special field. They would rather take finance or industrial organization, learning how the rich got rich, rather than why the poor remain poor. There is more attention paid by high IQ economists to the minute by minute fluctuations in the price of stocks on the New York stock exchange than there is to the pervasive and persistent poverty of the several billion individuals living in the Third World.

The lack of attention is not because the problems of development have been solved. It has to do with values, role models and rewards. Again, I do not want to seem too harsh: there are some first-rate economists doing important work on development problems. They are to be commended. I hope, however, that among the next generation of economists, development economics will not only be restored to the stature that it held some quarter century ago, but that it attain the even greater prominence which it rightfully deserves.

Concluding Remarks

I always feel queasy suggesting to others how they should allocate their time, how they should direct their research efforts. In the long run, I suspect our most successful attempts at persuasion lie not in exhortation but in example: showing the forcefulness of a new paradigm, its ability to explain phenomena which hitherto seemed inexplicable, and the inconsistencies of an old paradigm demonstrating that interesting and insightful models can be constructed to address new problems or unresolved old ones.

Still, it is useful on occasion to step back from one's own research program, to take stock. Let me, in conclusion, thank the organizers of this conference for the opportunity they have afforded to share with you some of my reflections on the state of economics today.

REFERENCES


11 In Capitalism, Socialism, and Democracy.


