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Towards A Critique of Economics

by

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TOWARD A CRITIQUE OF ECONOMICS

BY PAUL M. SWEETZ

Orthodox economics takes the existing social system for granted, much as though it were part of the natural order of things. Within this framework it searches for harmonies of interest among individuals, groups, classes, and nations; it investigates tendencies toward equilibrium; and it assumes that change is gradual and non-disruptive. I don't think I need to illustrate or support these propositions beyond reminding you that the foundation of all orthodox economics is general and/or partial equilibrium (the two, far from being incompatible, really imply each other). And as for the point about gradualism, I need only recall that printed on the title page of Alfred Marshall's *magnum opus*, the *Principles of Economics*, is the motto *natura non facit saltum*—nature makes no leaps.

It might perhaps be plausibly argued that equilibrium and gradualism provided a workable axiomatic base for a real social science at a certain time and place—the time being roughly the half century before the First World War, and the place Britain and a few other countries of advanced capitalism. For my part, I do not believe this was true even then. I think economics by the time of what may be called the “marginalist revolution” of the 1870s had already practically ceased to be a science and had become mainly an apologetic ideology. Putting harmony, equilibrium, and gradualism at the center of the stage was dictated not by the scientific requirement of fidelity to reality, but by the bourgeois need to prettify and justify a system which was anything but harmonious, equilibrated, and gradualistic.

This is the slightly revised text of a talk given at the New England regional meeting of the Union for Radical Political Economics (URPE) held at MIT, November 1-2, 1969.

It was almost at the same time as the marginalist revolution, when economics (as distinct from classical political economy) was being born as an apologetic ideology, that Karl Marx put forward a radically different and opposed mode of analyzing the dominant economic systems. In place of harmony he put conflict. In place of forces tending toward equilibrium he stressed forces tending to disrupt and transform the status quo. In place of gradualism he found qualitative discontinuity. *Natura facit salta* could well have been imprinted on the title page of *Das Kapital*.

It seems to me that from a scientific point of view the question of choosing between these two approaches—the orthodox or the Marxian—can be answered quite simply. Which more accurately reflects the fundamental characteristics of the social reality which is under analysis? I have already indicated my own view that orthodox economics does not reflect that reality but rather serves as an apologetic rationalization for it. Similarly it seems to me that Marxism *does* reflect capitalist reality. Or, to put the matter in other terms, the world we live in is not one of harmonies of interest, tendencies to equilibrium, and gradual change. Rather, it is a world dominated by conflicts of interest, tendencies to disequilibrium, and recurring breaks in the continuity of development. A would-be science which starts with a false or irrelevant conception of reality cannot yield very significant results, no matter how refined and sophisticated its methods may be. The answers a scientist gets depend, first and foremost, not on the methods he uses but on the questions he asks.

This is of course not to denigrate the importance of methods and techniques of investigation. In the development of science they have probably played as important a role as basic theory. The two are in fact intimately interrelated: theory poses questions, methods are devised to answer them, the answers or lack of answers make more theory necessary, and so on *ad infinitum*.

But the scientific endeavor is really not quite so simple and straightforward as this would suggest. Some of you may be familiar with the little book by Thomas S. Kuhn, entitled

The Structure of Scientific Revolutions,* which I think is very helpful in this connection. Kuhn argues that every scientific theory rests on what he calls a paradigm, which I think is very close to what I have been referring to as a conception of reality (or of some aspect of reality). Ptolemaic astronomy, for example, rested on a geocentric conception of the cosmos. The questions any science asks are fundamentally limited and conditioned by its underlying paradigm, which in time thus tends to become a hindrance rather than a stimulus to further advance. When this happens, the science in question enters into a period of crisis. The previously existing consensus among its practitioners crumbles. What is now needed is a new paradigm or, in my terminology, a new conception of reality which will once again form the basis for advance. This is often provided, as Kuhn shows in a most interesting way, by outsiders, i.e., men coming to the science from some other field where they have never learned to accept and venerate the conventional wisdom of the science with whose problems they are now concerning themselves. Moreover, as a rule the older scientists are unable to free themselves from their training and preconceptions, while the younger ones find it much easier to accept the new approaches. Gradually a new paradigm emerges and once again provides the basis for theoretical advance and for the unity of the science. In the new phase what Kuhn calls "normal science" becomes the order of the day, normal science being the posing and answering of the questions which are explicitly or implicitly allowed by the new paradigm or conception of reality.

In Kuhn's view, then, scientific advance takes place not in a straight-line, cumulative manner, starting from small beginnings and building up step-by-step and brick-by-brick to the imposing scientific edifices of today. This, incidentally, is the false idea which not only the lay public but the scientists themselves have of the process of scientific advance, a fact which Kuhn attributes in large part to the role of textbooks in the training of scientists. There are also other reasons, of course, among which I would rate as very important the tendency of scientists, in

* Paperback edition, University of Chicago Press, 1962.

common with other bourgeois thinkers, to view all of history, and not only the history of science, in an undialectical way. The pattern of scientific advance, in Kuhn's view, is rather through the exhaustion and breakdown of paradigms, leading in sequence to crisis, revolution via the construction of a new paradigm, and advance through normal science until a new period of breakdown and crisis is reached.

It would be interesting, and very likely fruitful, to try to apply this schema to the interpretation of the history of the social sciences. But certain obvious complications come to mind. For one thing it is clear that in the social sciences a paradigm can break down not only for what may be called internal reasons—i.e., the exhaustion of the questions it permits to be asked—but also because the social reality which the paradigm reflects undergoes fundamental changes. The crisis of Ptolemaic astronomy did not arise from any change in the functioning of the heavenly bodies, but rather because the geocentric paradigm became increasingly unsatisfactory as a basis for explaining observed phenomena. In the case of social science a new dimension is added: not only the observation of phenomena but the phenomena themselves are subject to change.

Another complicating factor is that the social world involves the *interests* of individuals, groups, classes, nations, in a way that is obviously not the case with the natural world. The resistance to the abandonment of old paradigms and the adoption of new ones is therefore much more complicated and is likely to be much more stubborn in the social sciences than in the natural sciences. I believe it could be shown that one consequence of this is that revolutions in the social sciences are always associated in one way or another with political and social revolutions.

Let us now turn to a consideration of the case of orthodox economics. Here it seems to me that the underlying paradigm, along with the normal science to which it gives rise, can and should be subjected to critical attack on several grounds. As I have already suggested, this paradigm takes the existing social order for granted, which means that it assumes, implicitly if not explicitly, that the capitalist system is permanent. Further,

it assumes that within this system (a) the interests of individuals, groups, and classes are harmonious (or, if not harmonious, at least reconcilable; (b) tendencies to equilibrium exist and assert themselves in the long run; and (c) change is and will continue to be gradual and adaptive.

One line of attack would be that this paradigm is about a century old and that most of the basic questions it allows to be asked have long since been posed and explored by the great economists of the first and second generations—men like Menger, Wieser, Böhm-Bawerk, and Wicksell in one tradition; Walras, Pareto, and the early mathematical economists in another tradition; and Marshall, Pigou, and Keynes in still another. (The list is of course intended to be illustrative rather than exhaustive.) More recent orthodox economics, remaining within the same fundamental limits, has therefore tended, so to speak, to yield diminishing returns. It has concerned itself with smaller and decreasingly significant questions, even judging magnitude and significance by its own standards. To compensate for this trivialization of content, it has paid increasing attention to elaborating and refining its techniques. The consequence is that today we often find a truly stupefying gap between the questions posed and the techniques employed to answer them. Let me cite, only partly for your amusement, one of the more extreme examples of this disparity that I happen to have run across:

Given a set of economic agents and a set of coalitions, a non-empty family of subsets of the first set closed under the formation of countable unions and complements, an allocation is a countably additive function from the set of coalitions to the closed positive orthant of the commodity space. To describe preferences in this context, one can either introduce a positive, finite real measure defined on the set of coalitions and specify, for each agent, a relation of preference-or-indifference on the closed positive orthant of the commodity space, or specify, for each coalition, a relation of preference-or-indifference on the set of allocations. This article studies the extent to which these two approaches are equivalent.*

* Gerard Debreu, "Preference Functions on Measure Spaces of Economic Agents," Center for Research in Management Science, University of California, Berkeley, January 1966 (mimeographed).

You will doubtless be glad to know that in his search for an answer to this momentous question the author enjoyed the support of the National Science Foundation and the Office of Naval Research.

But a much more fundamental line of attack on orthodox economics proceeds from the proposition that, whatever relative validity its underlying paradigm may have had a hundred years ago has largely disappeared as a result of intervening changes in the global structure and functioning of the capitalist system. Conflicts of interest, disruptive forces, abrupt and often violent change—these are clearly the *dominant* characteristics of capitalism on a world-wide scale today. But they are outside the self-imposed limits of orthodox economics, which is therefore condemned to increasing irrelevance and impotence.

Before I turn in conclusion to the state of Marxian economics, let me add that what I have been saying applies to economics considered as a social science, as the modern successor to classical political economy, whose task is to comprehend the *modus operandi* of the socio-economic system. I quite realize that a great deal of what is actually taught in economics departments today and is *called* economics is something entirely different. It seeks not to understand a certain aspect of reality but rather to devise ways and means of manipulating *given* institutions and variables to achieve results which for one reason or another are considered desirable. How should a corporation allocate its resources to obtain maximum profits? How should a government department weigh costs and benefits in making its decisions? How can a centrally planned society achieve a distribution of goods and services and a rate of growth in conformity with the directives of its political authorities?

Naturally, I have no objection to asking and trying to answer questions of this kind, and I suppose it is no great matter that the work is carried out in economics departments (as well as in business schools, departments of public administration, and the like). What I do object to is calling this sort of thing "science." It is no more social science than engineering is physical science. The analogy may not be perfect, but I do not think it is basically misleading either. I will only add that

I think a great deal of this social "engineering" is vitiated by taking its assumptions about how economic entities and institutions work from what I consider faulty social science. Here the analogy certainly does work: engineering isn't physics or chemistry, but its success depends on making use of the scientific laws of physics and chemistry. Social engineering is in the same state of dependence, and this explains why much of it is beside the point or worse. Try, for example, to prescribe a solution for a problem involving irreconcilable conflicts of interest on the assumption of underlying harmony. This, as it happens, is being done all the time in the United States today—with respect to such problems as the racial and urban crises, relations between the advanced and underdeveloped countries, and many others.

Now in conclusion a few words about Marxian economics. Here the underlying paradigm stressing conflict, disequilibrium, and discontinuity is also about a hundred years old. Since the knowledge which it yields is totally critical of the existing society, it was naturally unacceptable to the beneficiaries of that social order—in the first instance the propertied classes which are also the possessors of political power. Marxian economics was therefore banned from all the established institutions of society such as government, schools, colleges, and universities. As a result it became the social science of the individuals and classes in revolt against the existing social order. Three points need to be emphasized here.

(1) The class character of Marxian economics in no way calls into question or impugns its scientific validity. That depends entirely on its ability to explain reality. And in this respect it seems clear, to me at any rate, that the record of Marxian economics is far better than that of orthodox economics.

(2) But it also seems to me that the record is not anywhere near as good as it could have been. There are probably several reasons for this, only one of which will be mentioned here. This is that the practice of "normal science" within the framework of the Marxian paradigm has from the beginning been extremely difficult. Excluded from universities and research institutes,

Marxian economists have generally lacked the facilities, the time, and the congenial environment available to other scientists. Most of them have had to make their living at other jobs, often in the nerve-racking and fatiguing area of political activism. In these circumstances what is perhaps remarkable is that so much rather than so little has been accomplished.

(3) But why, it may be asked, have not the revolutions of the twentieth century, mostly espousing Marxism as their official ideology, not resulted in a flowering of Marxian economics (and other social sciences)? Here, I think, we meet a paradox which, however, can be explained by a Marxian analysis. Revolutionary regimes so far this century have come to power in relatively backward countries and have been largely preoccupied with retaining power against internal and external enemies. In these circumstances, their attitudes toward Marxism as a social science have been ambivalent for the simple reason that it is, or is always likely to become, critical of the new social order. It follows that under revolutionary regimes, as under the previous capitalist regimes, for Marxists the practice of normal science has been difficult and often practically impossible.

I do not want to end these remarks on such a negative note. Despite all the hindrances and difficulties, I think Marxian economics has indeed made notable progress and produced important contributions to our understanding of today's world. Let me cite just one area in which I think its superiority over orthodox economics is obvious and overwhelming—in explaining what has often been called the most important problem of the twentieth century, the growing gap between a handful of advanced capitalist countries and the so-called Third World.

Orthodox economics has nothing useful to say on this subject—largely, I would argue, because it is ruled out by the underlying paradigm. And the prescriptions of orthodox economics for overcoming the gap have been proving their impotence for many years now.

For Marxian economics, on the other hand, the explanation, if not simple, is at least perfectly clear in its main outlines. This explanation can be put schematically as follows:

(a) From the beginning, the development of today's advanced capitalist countries has been based on subjugation and exploitation of Third World countries. The latter's pre-existing societies were largely destroyed, and they were then reorganized to serve the purposes of the conquerors. The wealth transferred to the advanced countries was one of their chief sources of capital accumulation.

(b) The relations established between the two groups of countries—trade, investment, and more recently so-called aid—have been such as to promote development in the one and underdevelopment in the other.

(c) There is therefore nothing at all mysterious about either the gap or its widening. Both are the inevitable consequence of the global structure of the capitalist system.

(d) It follows that the situation can be changed and real development can take place in the Third World only if the existing pattern of relations is decisively broken. The countries of the Third World must secure control over their own economic surplus and use it not for the enrichment of others but for their own development and their own people. This means thoroughgoing revolution to overthrow imperialism and its local allies and agents.

Marxian economists still have a tremendous amount of work to do to explain and elucidate the many complex facets of this global process. But I suggest that in the work of such outstanding Marxists as the late Paul Baran and Andre Gunder Frank great strides have been made in recent years, and that large numbers of dedicated young social scientists, not least in the Third World itself, are not only following in their footsteps but pushing on to new frontiers.

Can anything remotely comparable be said of the contribution of orthodox economists? I think the answer is obvious. And the thought I would leave you with is that the fault lies not in any lack of talent or dedication on the part of the practitioners of orthodox economics but rather in the fundamental falsity of the conception of reality which underlies all their theoretical and empirical work.

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