

## Reflections on the Present State of Economics

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In my lifetime I witnessed two major changes in outlook in economics: first the Keynesian revolution, and then the counterrevolution, the return of the Bourbons.

I was raised in the traditions of the Austrian School which represented an attempt to transplant British liberalism in general and Manchester liberalism in particular in space and time to an industrial latecomer. The transplant did not take too well, and after the First World War the school became anachronistic. Ludwig von Mises, then its chief representative, as secretary of the Chamber of Trade resided in the middle of a building dedicated mainly to the administration of quotas, clearing agreements, tariffs, etc. He was a splendid pamphleteer and speaker. In his view economics consisted of tautologies. This did not worry him, however, it only made it irrefutable. Ironically enough, he founded the *Austrian Institut für Konjunkturforschung* which was devoted mainly to empirical research. It was the focus of a circle of Austrian economists (Hayek and Morgenstern as successive directors, Haberler, Machlup, Strigl, Tintner as collaborators) and a stream of visitors from abroad. Owing to its skilful set-up (everybody who counted in economic life was represented on the board) it became, as reconstituted after the war, a rather important institution in Austrian economics.<sup>1</sup>

My teacher was Richard Strigl, a kind man and good teacher who taught me everything I soon came to disbelieve. He taught me, however, what economics is about, which is perhaps more than some students are learning today.

My existence as an economist is based not on one but on three strokes of luck: I got a job at the above-mentioned institute after graduation in 1935. After loosing this job as a consequence of the Nazi

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<sup>1</sup> Under the name *Österreichisches Institute für Wirtschaftsforschung*.

take-over in 1938 I obtained a grant as a research lecturer at Balliol College, Oxford, which enabled me to emigrate to England where I passed my formative years. And I worked next to Michal Kalecki at the Oxford Institute of Statistics from 1940 to 1944. Without these rather improbable events I would have been a wash out. This probably contributed to my later interest in random processes...

I should mention here that the leading members of the Austrian School, Mises, Haberler, Hayek and others, operating from abroad, showed great solidarity in organizing jobs in other countries for those economists whose career in Austria had been terminated by the political upheaval of 1938. This was a necessary condition also for my emigration. The sufficient condition was provided, in my case, by the open mindedness of the Master and Fellows of Balliol College, in the more general case, of British scholars and Universities who assisted refugees at a time when such action was not exactly popular, and when young British academics did not always find it easy to get a job.

My acquaintance with the work of Keynes was mediated by Gerhard Tintner who organized a seminar in the *Konjunkturinstitut* to discuss the *General Theory* as soon as it was published. The resulting process of re-orientation was painful, slow and difficult, for me as for most economists. It seemed to be another instance of the change of paradigm as described by Thomas Kuhn, which takes place from time to time in science.

What distinguished Kuhn from Popper and what shocked his readers was simply that he described scientific evolution as a social and historical process, which looks irrational from the point of view of a naive concept of scientific evolution as a process of selection of the fittest theory, i.e. that which can best survive falsification attempts. This naive concept of the development of science has a close parallel in the concept of pure economics, a rational scheme devoid of any social and historical elements. Such ideas seem to rest on a primitive misunderstanding: if you insist that the object of your analysis (the economy, the development of science) is rational or rationally organised this does not make your analysis rational. In fact, in these cases, it achieves the opposite.

One may feel tempted to apply the term paradigm to the changes in economic doctrines and especially to the great revolution brought about

by Keynes and Kalecki. If one considers the great bulk of the economic profession, however, the term is misplaced. Keynes has never been accepted by more than a minority. Kalecki's importance has hardly been known outside a small circle. The dominant doctrine was the neoclassical synthesis, a hotchpotch of ideas. I do not think I am excessively frivolous if instead of paradigms, I speak of fashions in economics ("Wearing the Lucas look," for example, rather than "paradigm of rational expectations"). But who creates fashions? Certainly not the designer alone. The miniskirt was the expression of a change in society, in attitudes, in the position of women. The designer interprets and realises the broad hints which he gets from society. This is what happens in economics. Kalecki and Keynes responded to the need for an economics which was not wholly contemplative. In the subsequent period the orientation coming from society was mainly in terms of negation: no more planning, welfare state, budget spending, inflation, etc. *Laissez-faire* in itself is a negative orientation ('don't' rather than 'do'). Correspondingly there was not a new paradigm to crowd out the old. There was only a big void, empty of new ideas. Nothing was left to fill it but a revival of the old neo-classical doctrine, which had already held the field between 1870 and Keynes. The first neo-classical period had been a reaction against the criticism of the capitalist system by Marx and others. The mere fact that Marx based himself on the classics made a new orientation necessary. Neo-classicism lacks the candid innocence of the classics who maintained that poverty was necessary to make people work. Conscious of guilt and always on the defensive it is purely apologetic. Its basic strategy is to eliminate history and society from the subject and reduce it to a mathematical exercise – an optimisation problem. In this way capitalism is made to appear everlasting and unchanging. The laws of economics are as strict as those of science, but while the latter facilitate engineering the former forbid it.

The present state of economics has developed gradually since the last war. In that time economics has prospered and grown out of bounds. The number of economists, teachers and students, of journals, the scope and influence of economic advisers in public and private service, have all

increased tremendously. Yet if you size it up in terms of relevance and usefulness, the results are out of proportion to these massive resources.

At the same time modern economics is dominated by a number of remarkable tendencies. A good deal of it is pure economics, isolated from other social sciences and history. In the extreme case it is pure formalism which does not even claim any connection with the subject of economics. The influence of mathematics in all this is undeniable. There is a misguided idea that since proficiency in mathematics can be judged more objectively than creative ideas in economics, selection of economists is best based on the former. Typically modern economics displays great subtlety and accomplishment in the formal apparatus together with a shocking disregard for relevance to our society and its problems. Excessive space has also been devoted to polemics between different schools, at the expense, I feel, of constructive work. While the general economists have shown little interest in economic realities this is by no means true of the specialists (in currency, trade, labour, technology, etc.) who do deal extensively with the economic policy problems of today. The trouble is only that their specialisation tends to narrow their horizon and to isolate them from each other and from general economics. Harvey Leibenstein, by playing out common sense against optimisation and the production function, just manages to carve out a field for himself, but does he pull away the carpet from under the artful edifice of neo-classical theory? Nobody takes much notice in these lofty regions.

The division into special fields such as banking etc. is a natural consequence of the growth of the subject, but the fragmentation of the discipline has gone on in other respects too. Econometrics has been split off and alienated, it has become a separate subject. The division into macro- and micro-economics has not been good for the students. In one course they are taught the trees and in another the wood, but what has one to do with the other? Marxism and Economics of Power are established as separate subjects as a counterweight to pure economics. And there is a fairly strong split between the empirical or policy and the theoretical field. In sum: instead of evolving towards multidisciplinary combinations of various fields in the social sciences, so urgently needed by the nature of

our problems, economics has gone the diametrically opposite way and has split up into parts which are becoming alienated from each other.

But let me return to the fashions. How is it possible that neo-classicism and monetarism established so quickly and so fully their dominance over the subject? How is economics really working? There are a number of successful fashion designers who have a flair for the political climate; they are not averse to becoming economic advisors of some sort. These fashion designers dictate what economists have to read, on what they have to work and publish. If the average economist thinks that a certain topic is bunkum he is nevertheless forced to waste his time on it. He has to study it in order to be able to teach it, to argue and publish about it, otherwise his peers will cease to recognise him. This is actually how the transition to the 'new paradigm' worked: masses of economists found themselves pressed as by invisible hands to study the new wisdom.

The expansion of modern economics to an industry appears now in a new light. Since the subject has become so large it has become an uphill race to keep up with the literature which the leading designers and their followers are fabricating on a large scale. The economist who is forced into this *corvée* has no time left for thinking. Spontaneity is killed. This explains the dearth of original work, except in special subjects. Specialisation indeed offers a certain way out, but as already argued, it leads to fragmentation with all its drawbacks.

Fashions in economics are nowadays made in the U.S. The diffusion is the more easy there since it is a predominantly conformist society in which everybody tries to adjust to the pattern of behaviour of his neighbour, as sociologists tell us. For other countries it is natural to accept the ideology of the leading economic power.

My years at Oxford were mainly spent at the Oxford Institute of Statistics, which was largely a congregation of European emigrants (Thomas Balogh, Fritz Burchardt, Michal Kalecki, Kurt Mandelbaum-Martin, E.F. Schumacher) with a minority of British scholars (J.L. Nicholson, G.D.N. Worswick and the director, A.L. Bowley). The inspiration of the Institute and my Guru was Kalecki. He had, independently of Keynes and before him, created economics anew,

unburdened by the traditions of the subject and inspired by the department scheme of Marx, unaware until the publication of the *General Theory* in 1936 that the same kind of revolution was taking place in Cambridge. Kalecki had a penetrating mind and a passionate interest in what was going on in the world. He continuously absorbed, analysed and discussed the daily flow of events in the economic and political sphere and his judgement almost always proved right.

It was due to the availability of Kaleckian solutions that we of the Oxford Institute felt very confident: we thought we had the answers to economic policy problems, at least in principle, although we were all, to a greater or lesser extent, aware of the political difficulties which would render the right methods unworkable.

In the post-war years (and in the absence of Kalecki) this positivistic attitude gradually weakened (it ultimately ended in the general feeling of desperate helplessness of the present). It could last only as long as the war maintained a unity of purpose which overlaid all the conflicts of interest which were released once the war came to an end. This internal truce in face of a common danger seems to be a basis for (relatively) constructive economic policy solutions.

On one occasion I talked with Kalecki about the crisis of capitalism. We both, as well as most socialists, took it for granted that capitalism was threatened by a crisis of existence, and we regarded the stagnation of the 1930s as a symptom of such a major crisis. But Kalecki found the reasons, given by Marx, why such a crisis should develop, unconvincing; at the same time, he did not have an explanation of his own. I still do not know, he said, why there should be a crisis of capitalism, and he added: could it have anything to do with monopoly? He subsequently suggested to me and to the Institute, before he left England, that I should work on this problem. It was a very Marxian problem, but my methods of dealing with it were Kaleckian. He did not see my book until it was published. I lost by not having his advice, which was motivated by the distance of New York, but also by a secret wish on my part to escape to some extent the overwhelming dominance of a so much superior mind and personality. He remains my inspiration and my reference system till today.

The return to Vienna in 1950 was a considerable break in my life. I was now occupied by work of a more practical nature in the Institute of Economic Research and I missed the independence and the academic freedom which was so important for my work in Oxford. I learned something, however, and gained new orientations. I realised the importance of technology and of innovation, the role of research and development and of education. I occupied myself with these things, but at a certain period I also had great optimism with regard to the possibilities of mathematics. Kalecki warned me of that, and he also warned me of the computer: he suggested that both were ideally suited as a scientific cloak to cover the lack of economic substance. Whatever the potentialities of mathematics, with regard to the use which was actually made of it he was dead right. The role of mathematics in economics has been a most unfortunate one. Instead of being a tool of the economist it has developed a life of its own. Rather than looking for methods to suit his economic problems, the student asks his teacher to set him problems which suit the formal methods he has learned.

It is not irrelevant to consider here the development of modern mathematics itself. There are two forces in its evolution, the one coming from outside, from the application to science, the other from the inner logic of the subject which demands, for example, clarification, rigor, generalization, axiomatization etc. Some mathematicians<sup>2</sup> maintain that the development of mathematics for the last hundred years has been increasingly dominated by the second of the above-mentioned forces at the expense of the first.

In the 18th and 19th centuries mathematics developed in close association with science, drawing its inspiration from the problems which science presented. Since then it has withdrawn into itself, occupied mainly with rigor, refinement, axiomatics etc. Morris Kline, who deplors this tendency to isolation and fears that it will ultimately lead to degeneration, can quote the opinions of a number of very great mathematicians in support of his view. Kronecker, Felix Klein, Poincaré,

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<sup>2</sup> KLINE M. (1980), *Mathematics: The Loss of Certainty*, New York: Oxford University Press, Chapter XIII, pp. 278-306.

Courant, Birkhoff and von Neumann all warned of the ultimate sterility of a mathematics detached from science and concentrated on its own problems (Kline, 1980, pp. 287-291). There is a certain parallel here, although with qualifications – *toutes les proportions gardées* – to the development of economics since the war. General equilibrium economics certainly exemplifies the dangers of atrophy resulting from an isolation against outside stimulus and irritation, an economics withdrawn into itself and contemplating its own navel.

The parallel to mathematics gives us to think, because it suggests that there must be also reasons other than the special sensitivity of economics to the political scenery. Kline suggests that the compulsion to publish combined with the fact that applied mathematics is more difficult (in so far as it requires a knowledge of science) may have favoured the existing trend. Perhaps the university organisation also plays an important role – the mere fact that mathematics has become a subject of its own. In a way it has been a misfortune that it has ever been separated from science. As soon as a subject gets a department of its own it is surrounded by ditches and difficulties of communication. This may indeed have been a major factor in economics, where the growth led to splitting into so many departments.

The trend in mathematics is relevant to the modern tendencies in economics also in a more direct way: it directly affects the attitudes of econometricians and economists who take it as a model for their own style of work. Thus the use of set theory often is no more than affectation.

The mathematical economists no doubt were motivated by the desire to emulate the success of science by imitating its methods. But they committed a gross error in imitating only the formal side – the mathematical language. As a result they produced a travesty of science. The substance escaped them. The substance, incidentally, is akin not to mechanics, as some economists seemed to think, but much more to biology. Demography which is basic in economics, is founded on biology; human behaviour and psychology is linked to it, and society is a phenomenon of the living world. Economists have tried to throw out all relevant material from the field of economics as if it were stones and to leave nothing in but the principle of rationality. But this principle is



empty as long as you do not know what people expect, nor how their manifold wishes, fears and doubts combine to produce a one-valued decision. To reduce them to a maximum of utility or profit is just begging the question, an infertile kind of a-priorism.

On the other hand I am not more reassured by the prospect of behaviouristic methods. The firm is not a black box and it will not do to draw conclusions from an observation of inputs and outputs, stimulus and response, of this black box. Rather is the large concern a state whose internal policies are relevant and interesting.

While on the one side abstract mathematical models without social and institutional substance were built, on the other side an empirical approach was pursued which tried to extract too much from very meagre empirical evidence. Econometric studies frequently depend on neo-classical prejudice. When they are or appear to be unprejudiced they often claim to be able to decide on the basis of regression methods which relations and influences are relevant and which not, even though Herman Wold had shown a long time ago that this unprejudiced statistical investigation is quite illusory.<sup>3</sup> Econometric studies work mostly with time series, which generally are strongly auto-correlated and are available only for a very limited number of years. Even for very limited periods, however, it is very doubtful whether the relations under investigation remain unchanged. From a minute amount of information formidable results as to the systematic responses of business or of consumers to certain interventions or events are derived. The procedures bear no relation to anything practised in science. Frustration and loss of confidence have become widespread among all concerned.

On the other hand I feel that not sufficient attempts have been made to gather and make intelligent use of panel data (relating to behaviour of a constant number of firms or households over a number of years), although these data should contain much more information than either time series or cross-sectional data. Nor have the possibilities of simulation by computer been very much used as an aid to business cycle

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<sup>3</sup> WOLD H. (1953), *Demand Analysis: A Study in Econometrics*, New York: Wiley.

theory etc. This is however not to be expected in the present climate of economics which does not encourage the posing of relevant questions.

My own work on random processes was wide off the beaten track. It has been motivated, *inter alia*, by the wish to establish a relation between the macro and the micro world in economics. The behaviour of the consumer, for example, can reasonably be described only in terms of probability. The aggregate of consumers, given a large population and a certain independence between them, will react in a way which is more predictable and can be strictly derived from the probabilities relating to the individual action. Again, I felt that random processes, if the stress is put on process, have a good chance of making our theory more dynamic. Their basic concept is the transition probability, that is, the chance of moving from a given state one day to a certain different state the next day. The empirical material suitable for estimating transition probabilities are the panel data mentioned above. A steady state can be derived from certain conditions and can be compared with the observed data, such as for example the distribution of wealth. The methods are, however, exceedingly difficult mathematically and work in this field is therefore a long-term investment. It is not likely to help us directly in solving the burning questions of the day, although it may throw light on the traditional interpretation of statistical results (for example, regression lines).

Notwithstanding the remarkable conformism of present day economists (especially in America) there have been counter-movements. Marxism and radical economics, grown mainly from the ranks of the student movement since about 1968, have come to oppose the mainstream. I am not sure whether this offers much solace. In the first place, this movement has not penetrated economics, rather it has established a ghetto. It has carved out a place for itself at the universities and has left the mainstream intact, and that is mainly due to the attitude of the Marxists themselves, especially their lack of interest in current economic policy. The renaissance of Marx has concentrated not on emulation but on the exegesis of his work. No doubt very much has been achieved here and we can be grateful for it. But Marx has lived very

much in his time and the Marxist economists do not seem to follow his example.

I find it regrettable that most of the renaissance of Marx – as far as economics is concerned – concentrated on the theory of value and the conundrums connected with it. This complex of questions is ultimately derived from Ricardo in so far as it relates to a competitive economy with equalization of profit rates and essentially without money. But there is a different side of Marx, a dynamic approach to economics (such as in the chapter on accumulation in *Capital*, Vol. I) which results from his aim to explain the development of capitalism, an approach to history with the tools of economic analysis. Whatever one may think of his specific answers, the questions which he put and his approach are unique or at least rare among economists. The essential condition for this achievement was his multi-disciplinary talent and inclination.

The juxtaposition, within the Marxian opus, of a dynamic approach to the accumulation process and a seemingly more static analysis of distribution in a competitive system with equalization of profit rates as well as of wages in different industries may have an importance also outside Marx. There are two approaches, one may almost feel two kinds of economics distinguished by the very aim as well as by the method: the one tries to explain what happens in time, for example the trade cycle or long run developments, using a lot of speculation, it is true, but always relating to concrete observation and experience. The other is not directly concerned with the explanation of a process but with the functioning of a system which, in view of the high level of abstraction, is imagined or rather constructed. The focus of interest, for example, may be a rational (whatever that means) allocation of resources. Alternatively it may be the reproduction (simple or extended) of a system and the conditions which ensure it. The system is therefore a construction and it is supposed to perform certain functions. I should not for a moment maintain that there are no connections whatever between the two economics; but surely their manifest aims and methods are very different. The meaning of the constructed system may become clearer if we look at one of its possible

applications. Oskar Lange in a paper long ago<sup>4</sup> tried to show how a rational allocation of resources could be obtained in a system of socialist planning. Lange answered this question, as he later admitted, unsatisfactorily, namely on the basis of marginalist general equilibrium theory, but this is not important here. The topic of his paper is not an explanation of any experience, but the search for economic principles according to which a society could be organized. To return to the abstract system: examples are Sraffa, Pasinetti's 'natural economy,' von Neumann and Walras, if his general equilibrium is interpreted as an abstract construction without claim to descriptive use or explanation of actual events, as F. Hahn has interpreted general equilibrium.<sup>5</sup> The affinity between these various systems is of course quite a different thing from their concrete content: in the first three cases (Sraffa, Pasinetti, von Neumann) the system is dominated by the idea of reproduction, while in the general equilibrium of Walras and his followers the guiding idea is allocation. Each of the economic constructions mentioned bears a resemblance to system theory, in which the relations of parts to the whole in an organic system form the focus of interest.<sup>6</sup> Indeed the fascination of this 'system economics' is in the functioning of the system, in the way in which its elements are fitted together to form a meaningful whole. The interest of systems such as those of Sraffa and Pasinetti is very great because the 'constructivist' approach is the only one so far in economic theory which deals explicitly with the interrelations of many sectors in the whole economy. This is what macro economics has missed. No doubt it needs developing in the direction of multi-sectoral models, as Pasinetti keeps telling us. This, after all, is only a natural development from simple to more complicated concepts and methods.

What might have to be done to overcome the sterility of today's economics? The first condition is that we go back to the great tradition of the classics, Kalecki and Keynes whose work was rooted in the economic

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<sup>4</sup> LANGE O. (1938), *On the Economic Theory of Socialism*, Minneapolis: University of Minnesota Press.

<sup>5</sup> HAHN F. (1973), "The Winter of Our Discontent", *Economica*, vol. 40 n. 159, pp. 322-330.

<sup>6</sup> VON BERTALANFFY L. (1973), *General System Theory*, Harmondsworth: Penguin Books.

policy problems of their time, and derived its relevance from them. They asked what should be done and how. Economic policy is the main inspiration of economic theory.

The second condition is that a tremendous lot of new work is done in the no-man's land between the established disciplines which are entrenched in their organized fields, fearful of each other and speaking different languages. We must have close cooperation with other disciplines: engineering, science, history, sociology, biology, political science etc. Multidisciplinary work is not easy to organize because it depends on personalities. Some people like it and are suited to it, others are not. The former are a minority, most people are specialists by nature and inclination. But you can organize multidisciplinary work only with the former, the many-sided minds. This has to be heeded by organizers and financiers of research.

The potentialities of this work are enormous: when an economist is confronted with engineering or production problems he draws fancy pictures on the blackboard. He is in fact talking most of the time about things which he does not know. Must it be so? Why not step over the fence and have a look on the other side?

New orientations in economics will probably also be associated with a shift in the geographical centre of gravity, away from some prestigious universities to relatively new ones, away from the Anglo-Saxon countries to others, including Europe. Where established traditions are strongly entrenched it is less easy for new departures to take effect than in pioneering conditions.

I think the chances of a new start are not bad, because the dominant economics has largely run its course. Neo-classicism has involved itself so far in formal sophistication as to be of no use to the politicians and administrators. The success of monetarism of the old Milton Friedman type<sup>7</sup> (looked down upon by most neo-classicists) was in some part due to this, because its simplicity ensured its success with the public men. Supply economists, in their turn scorned as mere journalists by the rest of the

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<sup>7</sup> The new monetarism, of the Lucas type, proceeds on the same road of sophistication and remoteness as the neo-classicists.

academic world, were yet one stage higher in simplicity (and in the fees which they were able to charge). With them, however, economics has reached rock bottom. The time for new fashions can not be far away.