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Pierangelo Garegnani states that, as literary executor, he has initiated work on an edition of Sraffa's writings, including the correspondence. He will therefore be glad to receive any material (even photocopies) or any information (including personal recollections) which might be useful for that purpose. Please address communications to Professor Pierangelo Garegnani, Dipartimento di Economia Pubblica, Facoltà di Economia e Commercio, Via del Castro Laurenziano, 9, 00161 Roma (Italy).

A Personal Perspective on Mathematical Economics *

I was born in the heart of America, the Midwest, and grew up innocent of the great wide world of learning. On going to Harvard, I was told to study law by my grandfather, a lawyer turned banker: he may have thought I might take over his bank one day. Thus it was that I studied political science, as a preparation for law. Harvard College was for me a miraculous revelation of the world of knowledge, quickly erasing my legal aspirations. I sampled history, economics, philosophy (Whitehead), literature and art history. I began in the fateful year of 1930 and my four years witnessed the near collapse of the American economy. I had the special experience of hearing my professor of money and banking, who was also vice-president of the Federal Reserve of New York, admit in the course of his lecture, that he did not know why the President had closed all banks the day before! My grandfather's bank never was able to re-open and my father, independently, became bankrupt. Such events concentrate the mind wondrously: my own transformation may be judged by the fact that my degree dissertation was on the subject of Marxism. I had watched the incompetence and impotence of the government and I decided to change to economics, where the key to understanding of events lay — though not in the useless analysis of the orthodoxy of the time.

With a scholarship to Oxford, I went first to Germany to experience the ugly face of fascism. After that frightening experience, I spent all my vacations in Italy, which became, and still is, my promised land. In Oxford I did a degree in philosophy, politics and economics, but did little work, spending my time in political activity and travel in Italy studying painting. In my final examinations, as an exercise, I gave as far as was relevant, strictly Marxist answers, expecting a poor result, which

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naturally happened. In my third year I had to make a serious decision about what sort of economics I would pursue. I chose money and banking, as a technical subject in which my Marxism need not be visible, for I knew that as a Marxist I would be unemployable in an American university. My life has been a succession of errors, and this was one of them: after a few years of teaching the subject, I found it unbearable to devote myself to a study totally unrelated to my own interests. Hence I shifted my attention to the malfunctioning of a free economy in the form of cycles.

When I returned to Harvard, I fell under the spell of the passionate intellectualism of Schumpeter, particularly, as an intelligent reactionary, fully cognizant of the basic contribution of Marx. He enjoyed particularly the friendship of Paul Sweezy and myself precisely because we disagreed with him, and also he fancied us as typical of the degeneration of capitalism, since we both came from a background of banking. The greatest single intellectual mistake in my career occurred when Schumpeter came to me in 1938 or '39 and asked me to report on a very important new publication: the von Neumann paper given at the Menger seminar, a repetition of the one he had given in Princeton in 1932. When I got as far as realizing that he was including all remaining plant and equipment in annual output, I rashly judged it to be totally unrealistic, and I still do, though in retrospect I realize the immense simplifying power of the method. In any case, I, alas, reported back to Schumpeter that it was no more than a piece of mathematical ingenuity, failing to see that it contained two aspects close to Schumpeter's heart — a rigorous solution to Walras's central problem and a demonstration that the rate of profit arose from growth not a quantity of capital. When I came to edit his papers for the final section of his *History*, I found no reference to what now appears to me to be one of the great, seminal works of this century, the omission being possibly the result of my own blindness.

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Long ago, I once gave a seminar paper to a group of colleagues, which purported to show that it was a good thing to provide food to the unemployed at below normal price. In the middle of my presentation I was interrupted by a counter argument in the form of a well known theorem which, by means of Lagrange multipliers, demonstrates that the social maximum implies a single price for each good. This traumatic

experience induced me to undertake a lifelong, amateurish attempt to understand the use — and the abuse — of mathematics in economics. I became a "Sunday Mathematician", that is someone who pursues the black art in his spare time.

The role of applied mathematics in economics has been extraordinarily fruitful, both as to quantity and to quality. It is useful to review some of the main contributions. I propose to offer a highly personal view of it, since it would be quite impossible to treat the whole of it in a short space. Already early in the nineteenth century Cournot gave a sophisticated analysis of the behaviour of competing firms in a market. But the really striking advance was by Walras who stated the fundamental problem of a simultaneous and optimal solution of the very large number of equations necessary to determine the value of all economic goods, along with a trial and error method of solution aimed at yielding a constructive proof of the existence, presumed unique, of a solution. Pareto very subtly elaborated on the nature of the optimality of the solution, and Barone, by enumerating the very large number of equations, pointedly suggested the impossibility of actually calculating, for planning purposes, the solution. This led to a common view that a perfectly competitive price-market mechanism constituted a monster analogue computer, which would produce an optimal solution — a view still rather dominant, especially in the U.S.A.

There are serious shortcomings to this view, such as its irrelevance, since no perfectly competitive systems exist. Even in principle there is the totally unsatisfactory aspect of its timelessness. What is required is a set of solutions for all prices and outputs to the infinite future. Bold efforts have been made in this direction, but can one find them either plausible or illuminating? Wicksell by refining the Austrian conceptions of capital and time, made a careful statement of the problem but was unable to deploy it in a dynamical sense. Also Irving Fisher, a pupil of the mathematician Willard Gibbs, contributed illumination without really solving the problem. Possibly sensing this difficulty, Cassel simplified the Walrasian system, treating it positivistically as a system of linear equations in output with an inbuilt constant growth rate. Much more profound, and more influential, was the contribution of the great polymath von Neumann, who, having developed a rigorous theory of games, made one of those astonishing leaps of the imagination by applying it to economics (as well as to quantum mechanics). Starting from a kind of bilinear quadratic potential, he resolved it into dual (value and output) linear systems, with more equations than variables, thus leaving choice and the possibility of optimizing behaviour. He not only resolved Walras's problem by proving the existence of a min-max optimal solution, homogeneous and hence independent of scale, but also derived a determinate rate of profit equal to the growth rate of output. His 9 page paper, already recalled above, must, I now think, be regarded as the greatest single analytic advance in economic theory in this century, or even since Adam Smith and the Classical economists of the early years of the last century. His paper has led to a vast literature, such as linear programming, game theory of economic behaviour, the reinstatement of dynamics in economics, along with a revised theory of capital and interest.

Largely independent of this more abstract, continental approach, the Anglo-Saxon school concentrated on the more practical, empirical problems of individual firms, households and markets, i.e. microeconomics. Though less theoretically attractive, this procedure has had more substantive usefulness. Marshall, trained as a mathematician, is the outstanding example, though like Wicksell, he was very sparing in explicit use of mathematics. By elaborating a more subtle and realistic model of the parts of the economy, the aim was then to analyze the whole as the sum of the parts. The manifest failure of this approach in the Great Depression, was then corrected by Marshall's great pupil, Keynes (also a mathematician by training), who reintroduced the analysis of the whole economy by means of aggregates. My own view is that the difficult way forward lies in the simultaneous solution of a large number of dynamical equations so as to reveal the behaviour of the whole, whilst not evading the disparate behaviour of the parts.

In 1931, largely under the influence of Frisch, Tinbergen and Schumpeter, the International Econometric Society was formed. The first two had mathematical backgrounds; Schumpeter had none and never acquired the ability to use maths, but he had, as early as 1905, written a paper asserting the necessity of its use in economic analysis. These three projected a brave new world founded on a Trinity: economic theory to provide concepts and problems, maths to provide the quantitative logic to yield sure guidance from assumptions to conclusions, statistics to furnish the empirical substratum for the assumptions which would give applicable results. Schumpeter regarded this programme as a creed which every serious economist should subscribe to, and I, as one of his pupils, agreed. My initial work was a theoretical and statistical study of the supply and control of money in the U.K. in 1918-38. Thereafter I abandoned the practice, though not the faith, of econometrics, for reasons which I shall try to explain.

It is perhaps fair to say that, in their methodology, mathematical economists have been unduly influenced by the procedures of classical mechanics, not surprisingly in view of the power, the beauty, and the unparalleled success in application of those methods. No doubt because of the peculiar difficulties of their task, economists, in attempting to apply mathematics, have not had a success comparable with that of the natural sciences.

I have always suffered from an inability to understand and deploy pure mathematics. My initial effort, under the guidance of an eminent mathematician, Marston Morse, came to grief. No doubt as a form of self-justification, I have always felt that applied mathematics is more appropriate for economists. In order better to practice the art, I took advantage of the opportunity, during the war, to teach physics (without benefit of any previous knowledge of the subject!).

What are some of the differences between the problems of economics and those of the physical sciences? Not only can one not make experiments, but also individual events or elements are not isolated from the others. One can observe a gas without taking account of the phase of the moon or the weather outside. Worse still, each person, firm or market is different from all others, whereas all hydrogen atoms are much the same. The consequence is that we have to consider all the micro elements and all their interactions as one single problem: no amount of detailed observation and analysis of the parts will, by itself, tell us how the whole will behave. In this sense, there are not many different problems in economics: there is only *one* problem — and that one of a nearly insoluble complexity. A further grave complication is that very commonly economic events are substantially unique, which means that only in carefully delimited cases can one apply probabilistic analysis. It is of little use to a producer to know that, say, one third of all new ventures succeed, since he may only have one or two plays. When asked why he refused to apply his stochastic control theories to economic and social problems, Norbert Wiener replied that the run of statistics was not long enough. This is why, for all its shortcomings, game theory is more relevant to economics. Then there is the problem of dynamics: even to understand the structure of a system, one needs to see it in motion. Unfortunately the bulk of economic theory has been statical, aimed, rather successfully, at illuminating the nature of the beast, rather than how it evolves and changes.

In these and other ways, economics appears to be more akin to biology than to physics, which probably means it will need some of the rather different techniques coming into use, e.g. bifurcation theory, catastrophe, chaos (in chemistry and physics as well, e.g. the Brusselator and laser theory). Thus the German physicist Haken has introduced the concept of self-ordering systems, an approach obviously applicable to human society. We are not a herd of animals being observed by the shepherd; we are one of the herd, observing it, and, of course, influencing it, and being influenced by it.

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I come now to the most difficult and confusing aspect of economics — morphogenesis as propounded by Schumpeter. He believed one could not accept the statical formulation of economics, with some notion of change simply added on. Rather, capitalism had to be regarded as a system in a more or less perpetual state of turbulence, not merely in its motion but in its essential structural relations. In this he derived from Marx, whose stated aim was to uncover the 'law of motion' of society. In his seminal book, the *Theory of Economic Development*, he propounded his concept of innovation which meant an evolution of the morphology of the economy, an evolution which proceeded not in a steady-state but in bursts which resulted in a wave-like motion. This view of the economy poses a difficult problem. How can we analyze a system which is repeatedly changing its parameters as well as its variables? It helps to explain why, in spite of the enormous growth in economic statistics, we still have no reliable constants and few, if any, durable econometric models. This Marxian conceptualization was a fundamental departure from orthodoxy, an orthodoxy which had always aimed to characterize *all* economies, not how new ones evolved out of old ones.

When I returned to Harvard from Oxford before the war, I was full of enthusiasm for mathematical cycle theory, recently developed by Frisch, Kalecki, and Tinbergen. I pressed the necessity of this on Schumpeter, who readily agreed, and promised to attend, if I would give a course of lectures on it, which I did and which he attended, along with Haberler. However, he never succeeded in making any use of it. But he also objected to the kind of analysis I was using, and, in retrospect, I think he was right. The models were based on simple linear differential or difference equations. This implies the dynamical behaviour of a given and constant structure, whereas what he wanted was the dynamical effects of a given, major change in the structure itself. Thus

his theory envisaged a boom and subsequent collapse to a *higher* state of productivity with a changed industrial structure. Hence it was a model of *fluctuating growth* whereas simple harmonic motion is independent of growth.

I was trying to persuade him to add to his model aspects of Keynes's *General Theory*, but he resolutely and explicitly refused. At the time I was baffled, but, with hindsight, I think he did so for two main reasons, one sound and one not. Like all economists of his time, he tended to reason in terms of full employment, not unreasonably since this alone gives the rationale of relative prices. Therefore, to initiate new methods of production required new money from banks, leading to rising prices. Then, when the increased output came on the market, prices fell and real income increased. This type of theory is substantially false, because most of the time there is unemployment so that the investment is largely self-financing out of rising output and incomes. But where Keynes, and Keynesians like me, went wrong was to consider *only* the effective demand control over output, which is correct but less than the whole truth. Surely Schumpeter was right to maintain that essential to the analysis is the reduction of inputs per unit of output, especially in the labour content of output. The complex result is that the economy emerges from an expansion with a *potentially* higher output, but not necessarily an *actual* one; so, in my view, both Schumpeter and Keynes were right as well as wrong, and both should have a prize, for the profound insights they offered about economic reality. Yet precisely there lies the pitfall: one cannot use parameters determined from past statistics to determine future behaviour. Nor can one, in the Keynesian fashion, use global statistics uncritically, since the essential change is in the structure of production and relative proportions of different outputs. The creature being studied has changed its spots, and new parameters are required to monitor its behaviour.

The predominating thrust of contemporary mathematical economics is in formalizing and extending general equilibrium theory, with occasional efforts to incorporate some disequilibrium. I am not attracted by this type of theory, nor am I competent to discuss it. Rather, I shall indicate how I try to face the daunting difficulties I have alluded to. Originally, as a consequence of my concern with dynamics, I turned to business cycle theory. I took my problem from Harrod, another of my teachers. He had written a little book on the trade cycle in which he tried to extend Keynes's *General Theory* by showing that capitalism was basically unstable upward, but that it broke down (bifurcated) at full employment. Like Schumpeter, he was innocent of mathematics, but in response to sharp criticism from Tinbergen, he elaborated his theory into one which gave the conditions for steady-state growth, along with a demonstration that they were not realizable. Some years later I had the good fortune to have as a colleague in my laboratory the French mathematician, Philippe LeCorbeiller, who had specialized in oscillation theory. From him I learned that linear differential equations cannot be used to explain oscillators; he introduced me not only to the van der Pol type oscillator but also to a much wider range of types. The problem, as I saw it, was that existing types of economic cycle theory were based on the assumption that the cycle would exist even in the absence of growth, and, conversely, that growth would exist even in the absence of cycles. Already before the first world war Schumpeter had perceptively stated that technical progress did not and and could not occur steadily but rather came in bursts, thus constituting a cycle generator. My unsuccessful effort had been to convince him that it was the mutual conditioning of technical progress and effective demand that contained the fuller explanation. After long confusion and unsatisfactory formulations, I suddenly saw that Volterra's biological, nonlinear dynamical model of fish population in the Adriatic contained the formalism that we needed. Though his theory had no growth, only a cycle, it proved possible to develop it into what I was searching for, cyclical growth. My model was based on the symbiotic relation of the struggle over the shares of production between employers and employees. Professors Balducci, Candela and Ricci have perceptively reformulated the theory in terms of a game.

Such a theory, whilst embodying the fact that neither growth, nor cycle would exist without the other, remains, in some respects, unsatisfactory. The economic problem is probably so complex that it is unlikely that any one theory alone will ever suffice. Therefore I continue to look for a theory less dependent on distribution and more closely related to output and demand. The strategy goes as follows: in accordance with Harrod's formulation, the system is dynamically unstable, once excited. Therefore, sooner or later, it approaches full employment of unproduced resources, chiefly labour. This represents a bifurcation, since it breaks the condition necessary to continued growth at the pre-existing high rate. Investment is cut back, the system becomes stable and decelerates. The system is hysterical since its descent is not symmetrical with its expansion and it does not descend to the previous

low. Instead it reverses at a higher level, resumes growth and is again unstable. Thus Tinbergen had correctly stated that the multiplier-accelerator model, being a first order, linear dynamical equation, could only generate exponential growth.[1] Yet buried beneath his totally inept formalism, Harrod had very profound insight: the economy is endogenously explosive, but exogenously constrained by full employment. I continued to puzzle unsatisfactorily over this problem for the next decade, until LeCorbellier showed me how Harrod was right, given a nonlinear equation.

To incorporate Schumpeter's conception, one can proceed as follows: technical innovations require investment first and only later do the higher productivities and outputs become available. Hence a large innovation drives, through its effect on demand and output, the economy to full employment, which, in turn, precipitates a contraction, cancelling the investment and temporarily inhibiting the exploitation of the new technique. The economy drops to a level lower than the peak but higher than a previous low. There it remains until the expansion of the interrupted innovation is resumed, or until another innovation occurs. In this way we introduce history into the model, whilst retaining a certain amount of logical shape to the model. Proceeding in this way one can explain the so-called long waves. Delivering a single pulse in response to a sufficient shock, the system is perhaps better called a pulsator rather than an oscillator.

Finally one has to face the fact of the diversity of the various parts of the economy. The model must be multi-sectoral, not a crude aggregation of disparate parts. Since the number of dynamical equations must be very high, there seems to be no possibility of solving except by assuming linearity. Hence I opt for a matrix of constant input-output coefficients as a mechanism for transmitting demand signals appropriately to the many sectors. This assumption fits awkwardly with the notion of morphogenesis; its only justification is that at any one time the productive structure is completely given, and that the changes require considerable time. The system, being in principle empirical, can be diagonalized into its n distinct eigenvalues with its 2n associated eigenvectors. The great advantage of this is the separation effected

[1] Interestingly enough, this same attack had been made previously by Frisch against Hansen. Hansen, like Harrod, uninitiated into the art of mathematics, enlisted the help of Samuelson, who, by introducing a second lag, rescued the theory. But, of course, the theory remained linear and hence the cycle either died away or bumped into Harrod's upper boundary of full employment.

between the interdependence of sectors and their dynamical analysis. The result is that we can analyze many relatively simple dynamical problems and, only after, transform back to the real variables with their interdependence. In this fashion we can accept that in economics there are not many separate problems but rather just one problem, and yet retain great diversity of behaviour of the many parts. By inverting the matrix we distribute the demand from investments in highly diverse proportions amongst the sectors. Thus the investments for a new technology, coming prior to the resulting changes in the technology, are transmitted according to a given, pre-existing productive structure. Then, as the new technology becomes operative, its effects will be transmitted slowly, in a complicated way by the price mechanism to the other sectors. The difficulty with this procedure is that, when the change in morphology has been accomplished, the previous diagonalization no longer applies, and a new transformation matrix must be calculated. The pathbreaking formulation of von Neumann, although it allowed for choice of technique amongst all known ones, was fatally flawed by the assumption of perpetual growth with a constant technology, once the optimal choice was made. This destroyed the distinction between the known past and the unknown future, in the manner of classical mechanics, where time may run backwards or forwards indiscriminately. Norbert Wiener redefined this issue for a whole class of problems, and human history is plainly in that class. "Even in a Newtonian system, in which time is perfectly reversible, questions of probability and prediction lead to answers asymmetrical as between past and future, because the questions to which they are answers are asymmetrical". (*Cybernetics*, p. 43) The fact that this complicates our analysis and makes solutions very problematical, is no excuse for ignoring it. Einstein somewhere said we should always make our theories as simple as possible, but not simpler.

Many economists have developed a hostility to the flood of mathematical formalism of recent years. Probably the shrewdest and most persistent has been Leontief, who speaks with some authority, since he is himself a skilled practicioner. Yet I cannot understand his uncompromising hostility, especially since I once listened to him maintaining that mathematics was the only and proper guarantee of infallible logic. However disappointing the performance of the young discipline of econometrics, I fail to see how any serious investigator can doubt the general desirability of combining well formulated theory with

appropriate statistics, analyzed with the help of rigorous and ingenious mathematics. One surely cannot retreat into the ivory tower of 'pure' theory or relax on the soft cushion of facts as an end in themselves, 'vulgar' empiricism. In any case, I believe we need more, not less, of reliable, usable bits of all three branches of our difficult subject.

Siena

RICHARD GOODWIN