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POLITICAL ECONOMY, ECONOMICS AND BEYOND*

I. THE CRISIS OF POLITICAL ECONOMY IN THE 1870S

A little more than a century ago, on the 31st of May 1876, the Political Economy Club held a grand dinner, followed by a discussion, in honour of the hundredth anniversary of the publication of *The Wealth of Nations*. Mr Gladstone was in the chair and the debate was opened by Robert Lowe, former Vice-President of the Board of Trade and of the Education Board, Chancellor of the Exchequer in Gladstone's first liberal government and later raised to the peerage as Lord Sherbrooke. After a survey in praise of Adam Smith and his works, Lowe concluded with some remarks about what remained to be achieved by political economy. He did not think there was much. He granted that the prospects would be altered 'should other sciences relating to mankind, which it is the barbarous jargon of the day to call Sociology, take a spring and get forward in any degree towards the certainty attained by Political Economy'. But he does not seem to have thought this very likely and ended: 'The controversies that we now have in Political Economy, although they offer a capital exercise for the logical faculties, are not of the same thrilling importance as those of earlier days; the great work has been done' (Political Economy Club (1876), pp. 20–1).

Walter Bagehot (1876) reached much the same conclusion from a re-examination of the fundamental postulates of the subject which he had been publishing at about the same time in *The Fortnightly Review*. Economists as different as Cairnes and Jevons felt some misgivings about the character and persuasiveness of political economy as it was then practised. In the British classical tradition which developed after Adam Smith, the subject was presented as a deductive science concerned primarily with material wealth, identified with the doctrine of *laissez-faire*, largely indifferent to detailed observations, which could, it was argued, at best illustrate the truth of the propositions reached by reasoning from a few primary assumptions, self-consciously narrow in relation to other branches of the study of human society, insular and, after its earlier triumphs, increasingly out of touch with the spirit of the age. Speaking at University College, London, in 1870, Cairnes had said:

* Presidential Address to the Royal Economic Society, 24 July 1980.

'I seem to observe in the literature and social discussions of the day signs of a belief that Political Economy has ceased to be a fruitful speculation. Nay, I fear I must go further, and admit that it is even regarded by some energetic minds in this country as even worse than unfruitful – as obstructive, a positive hindrance in the path of useful reform' (Cairnes (1873), p. 238). In a lecture on the future of political economy, delivered, also at University College, in 1876, Jevons summed up the position as follows: 'In short it comes to this – that one hundred years after the first publication of *The Wealth of Nations*, we find the state of the science to be almost chaotic. There is certainly less agreement now about what political economy is than there was thirty or fifty years ago' (Jevons (1876), p. 620).

Other signs of the crisis can readily be found. In 1877 Francis Galton proposed the abolition of section F (Economic Science and Statistics) of the British Association for the Advancement of Science mainly on the grounds that the scientific content of the papers presented to it was small (Statistical Society (1877), p. 471). Indeed, many years earlier a warning had been given. In 1860 Nassau Senior, in his presidential address to the section, had drawn a distinction between science, concerned with facts and principles, and the arts which use it: 'Within the strict limits of economic science and statistics', he had said, 'a large field is open to us. It appears to me that we shall do well, if, as far as may be practicable, without much inconvenience, we confine ourselves within it, and deviate as little as we can into the numerous arts to which those sciences afford principles' (Senior (1860), p. 184). Apparently this advice had not been heeded by the contributors to section F, which at the time Galton made his devastating suggestion contained papers which knew no bounds either in subject matter or in method of treatment: the list of titles for the years 1873–5 ranges from perfectly legitimate topics such as the compilation of statistics, the economic law of strikes and the science of capital and money to oddments such as the acclimatisation of the silkworm, domestic service for gentlewomen, and the cost and propriety of removing to England the fallen obelisk of Alexandria. It cannot be denied that Galton had a point, though he pressed it too hard. He was opposed by William Farr and the Council of the Statistical Society and did not get his way.

The following year the Association met in Dublin and the president of section F was John Kells Ingram of Trinity College. Ingram had a wide-ranging career, occupying in succession the Chair of Oratory and English Literature, the Regius Professorship of Greek and the post of Librarian; he also wrote a *History of Political Economy* (Ingram, 1888). The presidential address of 1878 provided a good occasion on which to defend the section and to discourse on the present position and future prospects of political economy. He summarised his discussion in four propositions: '(1) that the study of the economic phenomena of society ought to be systematically combined with that of the other aspects of social existence; (2) that the excessive tendency to abstraction and to unreal simplifications should be checked; (3) that the *a priori* deductive method should be changed for the historical; and (4) that economic laws and the practical prescriptions founded on those laws should be conceived and expressed in a less absolute form. These are, in my opinion, the great reforms which are required both in the conduct of economic research, and in the exposition of its conclusions' (Ingram (1878), p. 656).

Thus Ingram's response to the critics of British economics was to take up the cudgels in favour of the new historical school. This school had originated in Germany in the 1840s and 1850s with the works of Roscher (1843), Hildebrand (1848), and Knies (1853) and had made a considerable impact on economic thinking in other Continental countries. In the words of the Belgian Emile de Laveleye, who was a guest at the Political Economy Club dinner, the historical school held that 'distribution is governed in part doubtless by free contract; but also, and still more, by civil and political institutions, by religious beliefs, by moral sentiments, by custom and historical tradition. You see', he went on, 'that there opens itself here an immense field of studies, comprehending the relations of political economy with morals, justice, right, religion, history, and connecting it to the *ensemble* of social science. That in my humble opinion is the actual mission of political economy' (Political Economy Club (1876), p. 31; translated in Jevons (1876), p. 621).

Marx, as we all know, went further: economic phenomena were not just a part of the body social, they were the tail that wagged the dog. However, at the time I am speaking of, Marx's ideas were not widely enough known in England to be included in the ongoing debate. It was only later that his followers entered the arena.

Ingram was not alone in his distrust of the *a priori* deductive method. Cliffe Leslie (1876), for instance, held an even more extreme view, which led him to regard the deductive theory of political economy as barren, if not false.

I think I have said enough to show that a hundred years ago there was widespread dissatisfaction in this country with the way in which political economy had developed, or perhaps I should say failed to develop, in the second half of the century following the appearance of *The Wealth of Nations*. But the prophets of doom were proved wrong: the revival was just round the corner.

II. THE END OF THE NINETEENTH CENTURY: SOME NEW DIRECTIONS

On the deductive side the 1870s saw, in the works of Jevons (1871), Menger (1871), and Walras (1874), the beginning of the marginalist revolution. Marshall had similar ideas at the time (Whitaker (1975), vol. I, pp. 37–52), though their publication in the *Principles* came later. The movement was joined in the 1890s by Fisher (1892), Wicksell (1893), and Pareto (1896–7). This wave of creative vigour carried with it a need for more sophisticated mathematical techniques than had been found necessary theretofore. Many of the foremost economists of the time were mathematicians, and they made extensive use of their mathematical abilities.

On the inductive side the importance of numerical data, both past and present, was increasingly recognised. Quantitative economic history, represented in nineteenth-century England by Tooke and Newmarch (1838–57), Rogers (1866–1902, 1884), Jevons himself (1884), and later Bowley (1900), was establishing itself as a legitimate branch of economics. And the collection of data on current economic and social phenomena, which had been going on sporadically throughout the century, was gaining ground: the scope and volume of official statistics was growing, and energetic individuals made their own fact-finding enquiries in the areas that interested them. An outstanding example of this kind of private initiative is Charles Booth's *Life and Labour of the People in London*, a social survey carried out over the years 1886 to 1902 and published in its definitive form in 1902–3; incidentally, in the team of assistants engaged for this work we find the young Beatrice Potter, not the creator of Peter Rabbit but the future wife of Sidney Webb. Booth mistrusted generalisations and was resolved to make use of no fact to which he could not give a quantitative value. In a paper given to the Statistical Society in 1887, dealing with the conditions and occupations of the inhabitants of the Tower Hamlets, he declared: 'The *a priori* reasoning of political economy, orthodox and unorthodox alike, fails from want of reality. At its base are a series of assumptions very imperfectly connected with the observed facts of life.' Marshall, who was present, protested: 'he was sure that all those economists whom he had the pleasure of knowing would say that there was not a line in the paper which they ought not to think over most carefully' (Booth (1887), pp. 376 and 392).

Although Booth draws many sensible conclusions from his data, he did not have the mathematical apparatus needed to squeeze out of them all the information they could yield. Indeed, the people who collected data seldom had. But the existence of such data stimulated among the more scientifically minded the search for methods of analysis that could deal with their complexity. Edgeworth and Bowley are cases in point (Bowley, 1901; 1928).

As theories evolved and observations multiplied, economists became more and more convinced of the desirability of combining them to throw light on each other. Nobody at the turn of the century would have said of economic theory what John Stuart Mill had said in 1848 of the theory of value: 'Happily, there is nothing in the laws of Value which remains for the present or any future writer to clear up; the theory of the subject is complete . . .' (Mill (1848), 1st edn, bk. III, ch. 1). Contrast this with Marshall's statement to Edgeworth in 1902: 'The keynote of my *Plea* is that *the* work of the economist is "to disentangle the interwoven effects of complex causes"; and that for this general reasoning is essential, but a wide and thorough study of facts is equally essential, and that a combination of the two sides of the work is *alone* economics *proper*. Economic theory is, in my opinion, as mischievous an imposter when it claims to be economics *proper* as is mere crude unanalysed history. Six of ye one, ½ dozen of ye other!' (Pigou (1925), p. 437). Pareto held the same opinion. In his *Manual* he says, borrowing a phrase from Henri Poincaré (1892): 'my purpose is to make, with regard to the social sciences, "observations as precise as possible and then compare them to the results of theories"' (Pareto (1906), translation p. 11).

Another recurrent theme in the writings of the time is the interconnection of social phenomena and the dependence of economic events on forces other than purely economic ones. This was nothing new – indeed, as we have seen, it was one of the fundamental tenets of the historical school – but by the end of the century this tenet numbered among its adherents some of the leaders of economic thought, first and foremost Pareto. Marshall too, though less wholeheartedly, recognised its validity. In the *Principles* he summarises his views in these words: 'Comte did good service in insisting on the unity that underlies social phenomena, but failed to show that there is no use in making special studies of certain classes of them' (Marshall (1890), 1st edn, pp. 72–3).

Though unanimity of opinion is hardly to be expected, I think that by the end of the nineteenth century few people would have denied that: (*i*) on the deductive side, marginal analysis had gained the upper hand; (*ii*) on the inductive side, statistics could no longer be dismissed as unimportant; (*iii*) the use of mathematics was increasingly required both for the formulation of economic theories and for the analysis of data; (*iv*) economics 'proper' rested on a combination of induction and deduction; and (*v*) economic phenomena should, in principle, be studied within their social context. Thus new fields were opened up and the old ones fertilised for more intensive cultivation.

This process of expansion has continued ever since and is still continuing. Naturally it has led to greater complexity and consequent subdivision of labour. Numerous as the specialisms are, however, it seems to me that their practitioners can be grouped into three types: the speculative, the active and the inquisitive. The speculative look at the machine and try to interpret its workings from the signals it emits; the active do not like the way it appears to work, and devise improvements; the inquisitive try to take it to bits and see how it actually works. In our century all three types have made and are still making useful contributions to the common task, especially where the lines of communication are kept open. In the rest of this lecture I shall consider some of these contributions, since time does not allow me to consider them all. If I were inclined to speculation I would talk about advances in pure theory. If I were an activist I might talk about welfare economics or taxation policy or Keynes or perhaps even Marx. Being merely one of the inquisitive ones, I shall talk about the areas I have enquired into and the tools I have used for the purpose: more specifically, about quantitative analysis, economic dynamics, and what, for want of a better term, I shall call social econometrics.

III. QUANTITATIVE ANALYSIS

In 1871 Jevons had written: 'I know not when we shall have a perfect system of statistics, but the want of it is the only insuperable obstacle in the way of making Political Economy an exact science. In the absence of complete statistics, the science will not be less mathematical, though it will be infinitely less useful than if it were, comparatively speaking, exact' (Jevons (1871), 1st edn, p. 14). Twenty-five years later Marshall delivered an address to the Cambridge Economic Club on the old generation of economists and the new. In it he said: 'Speaking generally, the nineteenth century has in great measure achieved *qualitative* analysis in economics; but it has not gone farther. It has felt the necessity for *quantitative* analysis, and has made some rough preliminary surveys of the way in which it is to be achieved: but the achievement itself stands over for you' (Pigou (1925), pp. 295–311).

By quantitative analysis Marshall did not mean economic statistics or even statistics worked into an economic argument and used to illustrate it. His wording makes it clear that what he had in mind was a quantitative analysis parallel to his qualitative analysis, in which each arbitrary function would be given a specific form, each arbitrary parameter a numerical value, each arbitrary time-lag a definite length, and so on. Indeed, in the *Principles* he remarks: 'It is perhaps not unreasonable to hope that as time goes on, the statistics of consumption will be so organized as to afford demand schedules sufficiently trustworthy, to show in diagrams that will appeal to the eye, the quantities of Consumers' Rent that will result from different courses of public and private action' (Marshall (1890): 1st edn, p. 471; 8th edn, pp. 492–3). In other words, the programme would be to fill Clapham's empty economic boxes (Clapham, 1922) and then to combine them into a computable model of the whole economic process. This, it seems to me, is just what the twentieth century has been doing, with a considerable amount of success.

At the beginning of the century quantitative economics consisted of a few isolated studies and a great deal of exhortation. Statistics in the sense of numerical data were still scarce and ill organised, despite the recent improvements; statistics in the sense of methods of estimation and inference were comparatively new and not well understood by most economists; and computation was dependent on primitive desk calculators. No institution existed that could give concrete guidance and support to the scattered and inexperienced workers. Of all the distinguished men I have mentioned, the first to see the need for an international forum for the exchange of ideas was Irving Fisher. I quote from Carl Christ: 'As early as 1912, while Fisher was vice-president of the American Association for the Advancement of Science, he had attempted to organize a society to promote research in quantitative and mathematical economics. Wesley C. Mitchell, Henry L. Moore, and a few others had been interested but they were too few, and for the time being nothing came of their vision.' Fisher was discouraged. When many years later, in 1928, Frisch and Roos had the same idea and asked for his support, he was pessimistic. At length, however, he said that if they could 'name one hundred people in the world who would join a society established for the encouragement of econometric work and the exchange of econometric papers, he would become an enthusiastic partner in organizing such a society' (Christ (1952), p. 5). The first list could not be stretched beyond eighty names, but Fisher was satisfied with it. He joined Frisch and Roos in the recruiting drive and two years later, in 1930, the Econometric Society was founded, with Fisher as its first president. In 1978 it numbered 2,518 members of various kinds and 3,143 institutional subscribers.

To illustrate the development of quantitative analysis I shall take the example of consumers' behaviour. In 1907 Benini estimated a demand function for coffee in Italy by multiple regression methods; in 1910 Pigou described and illustrated his indirect method of deriving price elasticities from family budget data; in 1913 Lenoir published his brilliant but largely neglected *Etudes sur la*

Formation et le Mouvement des Prix; and in 1914 Lehfeldt estimated the elasticity of demand for wheat in England. Lehfeldt's study was the first serious work on the demand for wheat since the price-quantity schedule published by Davenant in 1699 (1st edn, p. 83). Although 'Gregory King's law', as it is usually called, was frequently quoted, it was Philip Wicksteed in 1889 and, independently, Udny Yule in 1915 who showed that Davenant's data could be exactly fitted by a cubic equation.

But it seems to have been Henry Ludwell Moore, a writer given, in the words of George Stigler (1962), to 'many puzzling juxtapositions of vision and blindness', who made the 'statistical estimation of economic functions an integral part of modern economics'. Moore devoted his life to statistical economics, as he called it, and for my purposes it is perhaps sufficient to refer to the second of his five books: *Economic Cycles: Their Law and Cause*, published in 1914. There he dealt with the statistical laws of demand, not so much for their own sake as for the link they provided in his exogenous theory of cycles. Moore later abandoned this theory, but the statistical estimation of economic relationships gathered momentum. Beginning in the 1920s, a number of studies of the demand and supply of agricultural products were made by agricultural economists such as E. J. Working and Frederick Waugh. How far they were directly influenced by Moore I cannot say; but he certainly had one disciple in the person of Henry Schultz, whose book *The Theory and Measurement of Demand*, published in 1938, is perhaps the principal monument of demand analysis before the second world war. In England, the outstanding work in this field was Allen and Bowley's *Family Expenditure* (1935).

The demand studies undertaken immediately after the war followed the methods used in the 30s: single equations based on time series, as in Schultz, or on family budgets, as in Allen and Bowley. An example of the former is my early work on consumers' behaviour (Stone, 1945; 1954 with others); of the latter, Prais and Houthakker's *The Analysis of Family Budgets* (1955). Illustrations of both methods can be found in Wold and Jureen's *Demand Analysis* (1953). Although these methods gave useful results, they did not enable the interdependence of wants to be handled in a systematic and consistent way. Accordingly, experiments began to be made with systems of equations in which each price entered into each equation. The difficulty with these systems is that they involve a large number of constants that have to be estimated from the data. For instance, suppose we make the simple assumption that the expenditure on each commodity is a homogeneous linear function of total expenditure and of each of the prices. Then, with 10 commodities there are 110 constants to be estimated. True, these are not all independent: by adding up the ten equations we can see that the number of independent constants is 99. But this is still a very large number. One way to reduce it substantially is to assume that the substitution matrix for the average consumer is symmetric, as it would be in theory for the individual consumer. This greatly restricts the form of the matrix of price effects and reduces the number of independent constants to 19, which is a manageable number. In 1954 I gave a numerical example of this model, which I christened the linear expenditure system (Stone, 1954). The same method had been used in 1948 by Klein and Rubin as a basis for constructing a constant utility index of the cost of living; and in 1950 had been shown by Roy Geary to be associated with an explicit ordinal utility function.

Despite its restrictive nature, this model has been much used in econometric work and many generalisations have been proposed. In the 1960s alternative demand systems made their appearance, notably Theil's and Barten's Rotterdam models and Houthakker's addilog models. Anybody interested in the extensive literature on the subject up to 1972 should consult Brown and Deaton's (1972) survey in this JOURNAL. The translog model of Christensen, Jorgensen and Lau appeared later in the 70s (1975).

In 1974 Angus Deaton pointed out that the assumption of additive preferences, which in one way or another underlies the demand systems in common use, implies an approximate linear relationship between own-price and income elasticities. He concluded: 'the deficiencies of additive models should thus be taken very seriously, and in spite of their enormous practical advantage, the whole question of their suitability for applied work should be reconsidered' (pp. 338–9). In other words, manageability has not yet been reconciled with realism. An important step in this direction has recently been taken by Deaton himself in collaboration with John Muellbauer (1980).

This short account of the development of demand analysis brings out some of the difficulties of combining induction and deduction: of requiring that theories shall accord with the facts and that the facts shall lend themselves to a coherent theoretical interpretation. As is clear from the literature I have been citing, my example is virtually confined to static formulations. Dynamics poses further problems, to which I shall now turn.

IV. ECONOMIC DYNAMICS

'The dynamical side of economics has never yet received systematic treatment', wrote Irving Fisher in 1891 (p. 104). Fisher did not, of course, mean that economists had never interested themselves in growth and similar processes; nor did he mean that their theories could not be given an explicit dynamic form, as has been demonstrated by Irma Adelman (1961) for Smith, Ricardo and several others. He meant, I think, that economists, like most other scientists, had begun by using a static formulation to analyse the stationary points of their variables, exemplified by the intersection of a supply and a demand curve, without going into the question of the paths the variables might follow through time if they were not at a stationary point. By similar methods, known as comparative statics, the change in the position of the stationary point which follows from a change in circumstances, such as a change in the taste for a commodity, can be worked out; but nothing can be said about the path between the one point and the other. An attempt to deal with some aspects of dynamic phenomena within an essentially static framework was made in Marshall's distinction between long and short periods.

In a closed deterministic model formulated in discrete time, the basis of what is usually called period analysis, the variables in the different equations can be dated. If the dates are all the same the model is static and, if its equations are consistent, enables us to work out the stationary or equilibrium values of the variables. If the dates are different the model contains dynamic elements, and if these are so arranged that they enable us to calculate forward, period by period, from a given set of starting values, the model is dynamic. All this can be repeated for models in continuous time, with time derivatives in place of lags. Time delays can be introduced into this last type of model, giving rise to a mixed system of differential and difference equations. Thus just as the passage from qualitative to quantitative analysis takes us into a new world, so does the passage from static to dynamic analysis.

The outstanding early contribution to dynamic economics came in 1933 with Ragnar Frisch's essay published in a volume in honour of Gustav Cassel (Frisch, 1933). In it Frisch sets up a deterministic macro-dynamic system of three equations. He discusses the factors on which the time paths of the three variables depend and the conditions for stability, and then introduces plausible values for the parameters. He shows that with these values the path of each variable takes the form of a damped oscillatory movement about a trend. As he points out, this form is found very generally in econometric models.

In Frisch's model there is a primary cycle of 8·6 years and a secondary cycle of 3·5, as well as a number of lesser cycles with shorter periods. So the structure of the system gives rise to a tendency to oscillation which would, however, die away quite quickly. This is not what we observe and so there is something more to be accounted for: what is responsible for maintaining the oscillations? The need to explain not only how cycles originate but also how they are maintained was apparently first recognised in the early years of this century by Wicksell (1907), who surmised that the source of energy was to be found in erratic shocks. But he did not work out how random impulses lead to maintained cycles; this issue was taken up independently in 1927 by Slutsky and by Yule.

The first macro-dynamic model of a real economy was built by Tinbergen for the Netherlands in 1935 and reported to the meeting of the Econometric Society in that year (Staehle, (1937), (Tinbergen, 1935; 1959)). This model was policy-oriented, being designed to explore the question of what Holland, or any similarly situated small country, could do to help itself in a world struggling with the great depression. It preceded Tinbergen's celebrated studies for the League of Nations (1939*a*; *b*) and his model of the British economy in 1870–1914, which was published after the war (1951).

Many have followed in Tinbergen's footsteps, notably Lawrence Klein, who by the early 1960s had built two models of the American economy (Klein (1950), Klein and Goldberger (1955) and one of the British, Klein and others (1961)). At the time of the second American model, Irma and Frank Adelman (1959) made an interesting examination of its dynamic properties. First, as is the common practice, they made projections allowing the exogenous variables to follow smooth trends and using only the systematic part of the equations; they then repeated this exercise allowing for random shocks in the exogenous variables and, more importantly, in the stochastic equations themselves. Their results bore out the conclusion reached by Frisch in his paper of 1933: if projections are made without random shocks the variables soon begin to move along smooth trends; but if random shocks are introduced, the smooth trends are replaced by irregular movements which reproduce fairly well the quasi-cyclical paths observed in the real world.

All this happened twenty years ago. In the last two decades macro-dynamic model building has spread enormously. It has also linked up with another strand of economic model building, based essentially on Leontief's input–output analysis, which explores the likely position in a target year, say five or ten years in the future, without attempting to work out the path to that year. These models are often referred to as medium-term planning models. A case in point is the Cambridge Growth Project model whose history I have summarised elsewhere (Stone, 1977). It began twenty years ago as a static model, though many of its equations contained lagged values of the variables. As the years progressed, more and more variables which had originally been exogenous were endogenised until in 1975 Terence Barker showed how, by introducing appropriate modifications into the structure of the relationships, the model could be rendered dynamic: that is, given the relevant values of the variables up to a base year and the values of the exogenous variables thereafter, the future values of the endogenous variables could be worked out year by year. The current version of the model, with its 15,000 coefficients, 8,000 variables and 3,000 equations, can be solved by a Gauss–Seidel iterative algorithm and requires for each year between 7 and 25 iterations involving some 4 to 6 seconds computing time (Barker *et al.*, 1979).

Testing a model of this type and using it to make projections is in principle straightforward. First, one needs the values of all the variables in the base year, that is at the beginning of the period used in estimating the coefficients; in the case of lagged variables some earlier values will also be necessary. Second, one needs values for the exogenous variables for each year after the base year; these exogenous values will be known for the observation period and for several years after if the observation period does not extend to the present; but for the future they will have to be guessed. Then, starting in the base year, one can run the model through the observation period, then on to the present time and, finally, out into the future. The first part of this run provides a test of the model's ability to reproduce the movements in the economy over the observation period; the second part provides a test of the model's ability to predict, since the values of the exogenous variables are known; and the third part provides predictions whose accuracy will depend partly on the continued suitability of the model and partly on the assumptions made about the future paths of the exogenous variables.

So far, at Cambridge, we have not found time to undertake the first part of this programme; but we have undertaken the second and third, with encouraging and interesting results.

What I have outlined is an example, only one of many, of a descriptive model of an economy. With caution, it can be used as it stands to throw light on policy and planning issues. I say with caution because for these purposes it is incomplete. The missing part is a procedure which would enable us to examine problems of control and optimisation. By control I mean a method of setting policy instruments so as to hit targets, and by optimisation I mean a formalisation of the policy-makers' views about targets and the trade-offs between them.

As regards control, the last version of the Growth Project's static model (Barker, 1976) contained a procedure based up Tinbergen's (1952; 1956) targets and instruments approach. Given a maximum of ten targets, such as the rate of unemployment, the level of the balance of payments and so on, the model could be solved so that all the targets were hit, the instruments, now endogenised, taking on values that would ensure this result. The relevance of this for policy purposes is clear. Introducing a control mechanism into the dynamic version of the model is a bigger problem and, although we think we have solved it, we have not yet made use of the solution.

As regards optimisation, it is only very recently that we have started working on it. The difficulties are well known. First, one must find a form of function which not only does express preferences but is also manageable computationally. Second, one must have an explicit and coherent statement about what preferences one is trying to represent; it is unlikely that the policy-makers will have precise ideas on the subject or, if they have, that they will find it easy to allow for uncertainties and reconcile conflicts of interest. The first difficulty is a technical one and not insurmountable, we hope. The second is political and thus outside our scope as model-builders. All we may one day be able to contribute to the discussion is a means of quantifying 'the best', however this may be defined.

Though still something of a novelty, econometric models are coming to be accepted as an aid to policy formulation and planning. From one point of view it is remarkable how much they have improved since Tinbergen's pioneering effort forty-five years ago; from another, it is a commonplace how far they still have to go. But however much progress is made, they will remain unsatisfactory as long as they concentrate on economic variables to the exclusion of social, demographic and environmental ones. The world we live in is as much influenced by these factors as by economic ones, and we are not likely to prove very good guides of the economy if we do not attend to them. Which brings me to social econometrics.

V. THE ECONOMY, SOCIETY AND THE ENVIRONMENT

I shall take as a starting point the distribution of income, a subject with both economic and social aspects which is particularly associated with the name of Pareto. To a far greater extent than any other famous economist writing at the end of the nineteenth century, Pareto concerned himself with social issues. His work on the distribution of income by size culminated in his well-known law: the number of incomes above a given level is related to that level by an exponential decay curve. Pareto (1896) emphasised that the relationship could not be expected to hold for incomes below a certain minimum, and so his law applies only to the upper part of the income range. Another way of approaching the distribution of income is to treat it as a stochastic process. This approach was first proposed by David Champernowne in a fellowship dissertation written in 1936 but not published until much later (1953; 1973). Champernowne tells us that his choice of the subject had arisen from a suggestion made by Keynes 'that he should search for an explanation of the remarkable degree of conformity with Pareto's law displayed by many statistics of income-distribution published by the taxation authorites of various countries'. Champernowne's model expresses income changes by means of a matrix of transition probabilities between income groups. This way of regarding the matter makes possible an extension of multiplier analysis which captures the effects of income transfers within the income and outlay accounts just as Goodwin's (1949) matrix multiplier captures the effects of inter-industrial transactions, as demonstrated recently by Pyatt and his colleagues (1977).

So far we are still unequivocally in the field of economics, albeit with strong social overtones. But Pareto went further. In his main work on sociology, the *Trattato di sociologia generale*, published in 1916, he looks on society as a system of which the economy forms a part and tries to describe the laws of motion of this system and to illustrate these laws historically. Many of his ideas, such as his theory of the circulation of élites, whereby the ruling class in a society is continually transformed by a succession of innovative groups and conservative groups of changing complexion, seem to invite formalisation as part of a comprehensive model. However, although Pareto, following Walras, succeeded in giving a formal, mathematical treatment to many of the economic aspects of his system, he did not get so far with the social aspects. This is hardly surprising, since it is difficult to model a complex system before experience has been gained with some of its components. The challenge was taken up by later sociologists.

Let me take as an example the phenomenon of social mobility, which bears some affinity to Pareto's circulation of élites. This subject is usually studied in terms of the movements of families from generation to generation between prestige groups. If occupations are assigned to prestige groups, pairs of fathers and sons can be arranged in a transition matrix whose columns relate to the father's group and whose rows relate to the son's group. Such a matrix was constructed in 1954 by David Glass and his associates (p. 183), and in 1955 Sigbert Prais showed how much could be learnt by treating the mobility process as a regular Markov chain. At about the same time Blumen, Kogan, and McCarthy (1955) at Cornell assembled information on intra-generational occupational mobility in the United States and found that the workers in their sample did not move according to a Markov process: those who had moved in one period were more likely to move again in a later period than those who had not made a move in the earlier period. They dealt with this lack of homogeneity by proposing the mover–stayer model in which the population is divided into two groups, one with zero transition probabilities.

This is a purely sociological example, but it points the way to a means of integrating social and economic variables within a single model. In 1960 Kemeny and Snell discussed Leontief's open input–output model and Prais' social mobility model as examples of Markov chains, without however bringing them together. In my early papers on modelling the educational system, published in *Minerva* (Stone, 1965; 1966), I showed that one could display the flows of pupils and students in a demographic matrix with the human being as the unit of account, and the costs of education in a corresponding economic matrix with the currency as the unit of account. Since the fundamental matrix of an absorbing Markov chain has the same mathematical form as the Leontief inverse, these two bodies of data can readily be integrated, provided only that they are based on a common set of definitions and classifications. Thus socio-demographic accounts and economic accounts offer a promising way of organising the two kinds of data so that they can be combined into one model. The subject is developed with particular reference to education in my report for the OECD (Stone, 1971).

A wider range of applications is described and illustrated numerically in a report I later prepared for the United Nations (1975). This report describes how the population included in a transition matrix can be classified by any category that is actually or potentially amenable to statistical recording: age, sex, family grouping, geographical location, educational level, occupation, marital status, income group, religion, race, health condition, criminal record and so on. Which of these categories we choose and how we mix them will depend on which aspect of the social scene we want to study. The same can be said of the models we use to analyse these data: our method of analysis must depend on our area of interest. To take an example that has been examined by Jack Meredith (1973), we might be interested in working out the financial implications for a geriatric hospital of boarding out its patients instead of keeping them year after year in the wards of the hospital. If fairly long spans in boarding homes could be assured by a careful selection of suitable patients, not only would those patients benefit by being enabled to lead a more normal life but the hospital might gain financially despite the cost of the selection procedure. Given the transition probabilities between the selection ward, the ordinary wards and the boarding homes, and given the costs of keeping a patient for a unit period in each of these locations, it is not difficult to calculate the probable gain per patient accruing to the hospital from the existence of the selection procedure. In Meredith's example this gain was considerable, and it can easily be shown that it remains sizeable even if allowance is made for discounted costs.

Although socio-demographic accounting and model building are still young, their methodological equipment is already fairly extensive. The main difficulty for the time being is the inconsistency of the available data on human stocks and the total lack of data on human flows. What transition matrices have been constructed so far have been constructed by private individuals with data laboriously pieced together from different and often conflicting statistical sources. It is all too reminiscent of the early days of demand analysis. Progress is being made, however, as in the work of Rees and Wilson (1977) in this country and of Kenneth Land and his colleagues in America (Felson and Land (1978), Land and McMillen (1980), Pampel *et al.* (1977)).

The final topic I shall mention in the context of society and the environment is pollution. In 1970 Leontief proposed a means of handling pollution analytically within an input–output framework. In principle the idea is a simple one. All that is needed is to expand the input–output table for 'regular' industries by adding a row and column for each treatment service designed to handle one of the pollutants. Each new column would contain the inputs needed to operate one of the treatment services and, on the assumption that industrial pollution is to be entirely eliminated, each new row would contain the quantity of the relevant pollutant emitted by each activity, whether a regular industry or a treatment service; and the emissions would provide a measure of the cost that must be incurred by the polluter. However, it is not obvious that total elimination should be the target: it might be better to aim at partial elimination, thereby leaving more resources for the production of 'regular' goods and services. This issue and the question of who should pay for the treatment costs are discussed in a paper I wrote on the evaluation of pollution (Stone, 1972). Following this, James Meade pointed out that people are more interested in the cleaner air, land or water resulting from treatment than in the amount of treatment itself, and that the calculations can still be made if, instead of basing our model on the demand for treatment services, we based it on the demand for their ultimate product (Meade, 1972).

These are the extensions I had in mind when I introduced into my title the words 'and beyond'. The distribution of income, socio-demographic accounting, the evaluation of pollution are only the first steps towards realising Pareto's vision. But to begin you have to begin, as Gertrude Stein is sure to have said somewhere.

VI. CONCLUSIONS

This brings to a close what I have to say about the progress of our subject over the past three generations. Many of the innovations I have mentioned were received coolly, if not altogether ignored, when they were first proposed. And yet these were not developments *sui generis*, but more or less direct responses to lines of advance suggested and to some extent laid out by our predecessors at the end of the nineteenth century. I think it is important from time to time to recognise the strong element of continuity in the thought of successive generations.

The vast amount of research in economic theory, in econometric and statistical methods and in all branches of applied economics has borne fruit: the younger generations of economists are technically streets ahead of mine, the gap between the *a priori* and the empirical seems to me to have narrowed considerably, and the tools on offer to the policy-maker are getting more and more sophisticated.

Nevertheless people are discontented. Society has in some measure benefited from the progress of economic science: the prosperity of the 50s and 60s was partly brought about by the application of a number of economic prescriptions recommended in the 20s and 30s if not earlier. Now that this prosperity is threatened, partly because the correct doses have been exceeded, partly because the physicians have been oblivious of side-effects, partly because new diseases have manifested themselves, the man on the Clapham omnibus grouses because economics does not at once get the answer out of a hat. But the man on the omnibus always grouses, especially if the omnibus is bound for Westminster.

For my own part, when I compare the knowledge available a hundred years ago and the knowledge available today, I am led to revise my habitual pessimism about modern times. The world may be going to the dogs, but economics certainly is not.

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