

## The Formation of an Economist\*

If I am asked how it was that I became an economist, I can give nothing better than the regular economic answer: in order to earn my living. At the moment when the decision had to be made, I had just taken my first degree at Oxford. I had had a very good general education, but a very unspecialised education, which did not clearly point in one direction rather than another. It had been paid for by "scholarships", awarded on competitive examination (at the ages of 13 and 17); at that stage my main subject was mathematics. But I had turned away from mathematics; I took my degree in "philosophy, politics and economics", a new course just established at Oxford, a course which was perhaps better devised for the training of politicians than of academics. (Hugh Gaitskell, Harold Wilson, Edward Heath and Reginald Maudling all had that background.) But I wanted to be academic; and though I had done very little economics, I was advised that economics was an expanding industry, so I would have a better chance of employment if I went that way. So I did.

Economics, at Oxford, was very "social"; so they started me working on labour problems. I did my thesis on skill differentials in the building and engineering trades. But I had been well advised that there was a market for economists; so when I came to seek employment for myself, I was able to get what I wanted. From 1926-35 I taught at the London School of Economics; and I learned at the London School of Economics. Within those nine years I passed from the state of appalling ignorance, from which I started, to my first theoretical achievements: the invention of the elasticity of substitution (*Theory of Wages*, 1932), the distinction of income and substitution effects ("Reconsideration of the Theory of Value", in collaboration with Roy Allen, *Economica*, 1934) and the liquidity

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\* *The Review has included in its programme a series of recollections by distinguished economists. No rigid model has been laid down for the authors. The series, which is now opened by a contribution from Professor Hicks, will therefore reflect a variety of approaches, interests and experiences. (The Editor)*

spectrum ("A Suggestion for Simplifying the Theory of Money", *Economica*, 1935). Already, before I left LSE, I had done what I still feel to be some of my best work.

How had this happened? Those nine years at LSE fall very sharply, from my point of view, into two parts. They are separated, in 1929, by the arrival of Lionel Robbins as head of department. In the three years before that time I had been working mainly by myself. I had access to that already splendid library, and I got advice from my colleagues on what I should read, but I was not a member of a group. After 1929 I was a member of a group, the group which Robbins built up around him. We were all of us quite young people and most of us are still surviving. Apart from Robbins himself, there were Hayek and Roy Allen, Richard Sayers, Nicholas Kaldor and Abba Lerner, together with Marian Bowley and Ursula Webb (Ursula Hicks after 1935). So the work which I did in these latter years was in large measure a collective work.

I go back to the years of preparation which preceded. There were two things which happened during those years which need to be recorded.

One of them, in the first of those years, was that Hugh Dalton<sup>1</sup> (then temporary head of the economics department) said to me "you read Italian, you ought to read Pareto". So it was reading the *Manuale* which started me off on economic theory.<sup>2</sup> I was deep in Pareto, before I got much out of Marshall.

<sup>1</sup> Dalton had learned his economics at Cambridge, where he was a pupil of Pigou; but by the time I knew him his interest in economics was waning. He had started upon his political career, and was aiming at being Foreign Secretary in a future Labour government. It is well known that when the time came, he was disappointed in that ambition, and had to go back to economics as Chancellor of the Exchequer. But by 1945 his economics was seriously out of date.

His lectures, which I attended in 1926, were a bit like political speeches. "I always begin with population — good spicy subject, gets 'em interested" he said to me himself.

He had learned Italian when serving with the British army in Italy in 1918. He had a great affection for Italy, but felt himself unable to visit Italy during fascism. My Italian had begun by stumbling through Dante, while I was still at school; I had gone on to read fairly widely in Italian literature. But it was not until 1933, after I had published *Theory of Wages*, that I made my first contact with Italian economists, visiting, in Turin, Einaudi and Cabiati, del Vecchio at Bologna and Marco Fanno at Padua.

<sup>2</sup> I was naturally led from Pareto to Walras and Edgeworth. My time at Oxford was too late for me to have been able to go to Edgeworth's lectures; and I doubt if by my teachers at Oxford he was even mentioned. So it was not until I got to LSE that I found *Mathematical Psychics*.

The other was a long interlude in the second year, when I went to South Africa. The professor at the University at Johannesburg (their sole teacher of economics!) had died very suddenly. The authorities sent to London for a temporary replacement, while they made up their minds on the appointment of a successor. No one senior to me would take it, but I was tempted — on the whole very fortunately. I had to lecture on a wide variety of subjects, from statistics to economic history; but somehow I managed.

My own interest, at that time, was still in labour problems; and from that angle South Africa was a revelation. I came from a country where Trade Unions could still be thought of, by their well-wishers, of whom I had been one, as agents for the advancement of labour in general. But in South Africa they stood for no more than the interests of a minority, for White labour only. So much has been heard, in later years, of the colour problem in South Africa that it will hardly be credited that Dalton had given me an introduction to his "fellow-socialist", the leader of the South African Labour Party, then in coalition with the Nationalists, the begetters of *apartheid*, with whom I could soon see that they belonged. Thus I got a new view of Trade Unions; I began to think of them as monopolists, so that it was by the application of monopoly theory that their effects were to be understood. The reservation of skilled jobs to White labour, and the confinement of the best land in the country to White ownership, were the economic obstacles in the way of progress for the Black majority. In a free market system these would wither away, so I became a free market man, even before I left South Africa.

Thus, when the Robbins circle began to form, I fitted in. I readily accepted his rejection of inter-personal comparability of utilities (then considered as a rationale for progressive taxation), for the rejection was in line with the ordinalism I had got from Pareto. And I was readily seduced by the great "neo-classical synthesis" (as it effectively was, though that name has been mainly applied to later varieties), according to which a competitive system, free of monopoly elements, which would only grow if they were buttressed by state "interference", would easily find an "equilibrium". I was willing to apply this doctrine, even to the labour market; though there I had some reservations, which survive in some chapters of *Wages*. My *Wages* book, however, is in its main lines thoroughly "neo-classical".

It was surprising, to outside observers, that these very Right-ish doctrines could have had such a vogue at the London School, which

was popularly considered to be a hotbed of socialists. We did indeed have our eminent socialists, such as Laski and Tawney (Dalton, by now, had gone off into politics); but it was significant of the tolerant atmosphere at the School that personal relations with them were friendly. There was indeed a substratum of "liberal" political principles which our socialists and our free market men had in common.

LSE was not only tolerant; it was also, to a high degree, international. (It has become even more international since that time!) What we economists thought we were doing was not only to bring to life the inheritance of the British Classical Economists, but also to widen the horizons of the British economists of our own time by bringing in a refreshment from what was being done, and had been done, in other countries. I got mine, as has been seen, from Walras and Pareto; Robbins, on the other hand, was looking to the Americans (Chicago was already another home of free market economics) and even more to the Austrians. Books written in other languages had not then been translated into English; but I managed enough German to read the Austrians, and also Wicksell and Myrdal (at that time only available to me in German). I have never learned Swedish, but, as will be seen, I have been deeply influenced by Swedish economics.

It was not only through books that one made these contacts. Eminent economists, from many countries, would pass through London, and when they came to London they would come to the School. Thus it was that I made the acquaintance of Taussig and Viner, of Mises and Schumpeter, of Ohlin and Lindahl; as well as of a younger generation of Austrians, often on their way to exile, for Austria was already falling under Hitler's shadow.<sup>3</sup> Hayek himself

<sup>3</sup> Ursula spent a semester at the University of Vienna in 1931, so she had first-hand experience of the incipient Nazification. But it was not difficult for the rest of us, associating with German and Austrian exiles, to have a feeling of what was coming. When I went to Cambridge in 1935 (of which more below) I found an atmosphere that was very different. I remember how shocked I was to hear Pigou, a very great economist but curiously insular, remarking at that time that he supposed that Hitler was going to "bomb the frogs" (i.e. the French). None of our business! And it was even later that Claude Guillebaud (Marshall's nephew and later editor) wrote a book on the *Economic Recovery of Germany*, praising the economic policy of Hitler as an application of Keynesian economics. (I would not like to leave that reference without saying that Guillebaud was a good friend of mine in Cambridge; he was the only other British economist I have known who knew the last canto of the *Paradiso* by heart). The vogue of appeasement at Oxford during those years is notorious; but the sleep at Cambridge was still more profound.

came to London before the Hitler revolution; he came to tell us about Austrian economics; and he did.

My reaction to Hayek's teaching, at that time, I have described elsewhere;<sup>4</sup> and I have also set out, in another place,<sup>5</sup> the change in my own ideas which was intertwined with it. Here I will merely say that I began, once again, from Pareto, making an attempt, first of all a very crude attempt, to make the Paretian system less static, so as to be able to incorporate planning over time, planning for a future which was not known in advance. Hayek was making us think of the productive process as a process in time, inputs coming before outputs; but his completest, and most logical, account of intertemporal relations was confined to a model in which everything worked out as intended — a model of "perfect foresight".<sup>6</sup> In his *Prices and Production* (1932), the lectures through which we first got to know him, things were allowed to go wrong, but only for monetary reasons; it was only because of monetary disturbances that an exception was allowed to the rule that market forces must tend to establish an equilibrium. If money could only be kept "neutral", all would be well. (An anticipation of latter-day monetarism!) In the models I tried to construct, in which people did not know what was going to happen, and knew that they did not know what was going to happen, there was no place for "neutral money".

I was aware, before I left LSE in 1935, and before the appearance of Keynes's *General Theory* at the beginning of 1936, that the direction in which my mind was moving was not dissimilar to his. (He told me so himself, in some correspondence I had with him.<sup>7</sup>) But I did not begin from Keynes; I began from Pareto, and Hayek.<sup>8</sup> But I had gone on by 1935 to draw consequences from my new approach; and I had realised that I had separated myself from the faith in the free market which had been dominant among my colleagues. After I had read them my *Simplification* paper (at the end of 1934) they must have been aware of what was happening;

<sup>4</sup> "The Hayek story", (*Critical Essays in Monetary Theory*, 1967).

<sup>5</sup> "Recollections and Documents", (*Economic Perspectives*, 1977).

<sup>6</sup> ("Das intertemporale Gleichgewichtssystem", in *Weltwirtschaftliches Archiv*, 1928).

<sup>7</sup> Reprinted in "Recollections and Documents", cited.

<sup>8</sup> There is evidence for this, in a paper which I published (in a German translation) in the *Zeitschrift für Nationalökonomie* in 1933.

but, as I have said, the atmosphere at LSE was tolerant, and I have been able to keep them among my friends.<sup>9</sup>

It was not because I was becoming Keynesian (as in a sense I was) that in the summer of 1935 I removed to Cambridge. I went there in consequence of an invitation from Pigou, and it was because of the friendship I had already formed with Robertson<sup>10</sup> that I was attracted. Cambridge, however, was already riven by disputes between Keynesians and anti-Keynesians; and since I was associated with Pigou and Robertson, I found myself regarded, at least by some Keynesians, as being in the "anti" camp. The ISLM version of Keynes's theory,<sup>11</sup> which I myself produced, but which has never been highly regarded by orthodox Keynesians, did not help me.

My chief occupation during those years at Cambridge (1935-38) was the writing of *Value and Capital*. This is not at all a Cambridge book; it is a systematisation of the work I had done at LSE. It is represented as a work of bridge-building, not so much between micro- and macro-economics (as others have often regarded it) as between the static neo-classical system, which had been regarded as the foundation of free market economics, and the "dynamic" models where past and future are properly distinguished, in which I had by that time become more interested. My own dynamic model is presented in terms that have some relation with Keynes's work; but it is not very Keynesian. It owes much more to what I had got from the Swedes, from Myrdal and Lindahl. It was from Myrdal that I got the idea of "temporary equilibrium", a momentary market equilibrium in which price-expectations are taken as data; it was Lindahl, with his pioneering work on the social accounting framework, who taught me how (formally at least) to string my temporary equilibria together.<sup>12</sup>

<sup>9</sup> I think that Hayek, and perhaps Vera Lutz, have been the only ones of us who in later years have been fully constant in the old faith. Even Robbins has departed from it, to a considerable extent.

<sup>10</sup> I have described my early relations with Robertson in "Recollections and Documents". See also the memoir of him which I wrote for the British Academy, and which is reprinted as a preface to the selection of his *Essays on Money and Interest* (1966).

<sup>11</sup> "Mr. Keynes and the Classics", (*Econometrica*, 1937); reprinted in *Critical Essays*.

<sup>12</sup> I read Myrdal's *Monetary Equilibrium*, in German, at the beginning of 1934; it was through Myrdal's references to him that I first heard of Lindahl. I found these references very exciting; but I could not follow them up, since I could not read Swedish. So it was a great moment when I actually met him at LSE: He

I don't now think that the monetary chapters in *Value and Capital* are at all good; it is not from them, but from the *Simplification* paper of 1935, that my later work on money has proceeded. There is little about liquidity in *Value and Capital*.

By the time *Value and Capital* was published, I had removed to Manchester, where I remained during the war years. The British universities were only partly closed down, so there still was work to be done, though most of the teaching I had to do was rather elementary. I took advantage of this to write my *Social Framework*, which seems to have had the widest circulation of any of my books. It should have been called *The Social Accounts*, for its novelty consisted in the systematic use of social accounting material for elementary teaching; but the idea of social accounting was then unfamiliar, so I was persuaded to fall back on that unsatisfactory title.

*Value and Capital* had been published at the beginning of 1939; so it got distributed throughout the world before the War broke out. But I was thereafter cut off from the reactions that were forming to it; it was only after the War that I found out what had been happening.<sup>13</sup>

In the second half of 1946, I made my first visit to the United States. I there met again some old friends, such as Schumpeter and Viner; but I also made my first contacts with the younger generation, who were soon to become famous. At Cambridge (Mass.) I met Samuelson; in New York I met Arrow; and at Chicago Milton Friedman and Don Patinkin. I did not know them, but they knew me; for I was the author of *Value and Capital*, which (as has since become obvious) was deeply influencing their work. They regarded it as the beginning of *their* "neo-classical synthesis" — no more than the beginning, for they and their contemporaries, with far more skill in mathematics than mine, were sharpening the analysis I had merely roughed out. But I am afraid I disappointed them; and have continued to disappoint them. Their achievements have been great;

had come to London to get help in the translation of his essays into English; I was able to find a helper for him. A year later, on another visit to see that helper, she had to tell him that we had decided to get married. "Ah!" he said in his imperfect English "I had my doubts".

<sup>13</sup> Years later, when visiting Japan, I was assured that my book had been a set book at Kyoto University since 1943. I was astonished, and asked them how it could have been possible for them to get copies. They reminded me that until December 1941 they could import through America; and then, they said, we captured some in Singapore!

but they are not in my line. I have felt little sympathy with the theory for theory's sake, which has been characteristic of one strand in American economics; nor with the idealisation of the free market, which has been characteristic of another; and I have little faith in the econometrics, on which they have so largely relied to make their contact with reality. But I make no pretence that in 1946 I was even beginning to get clear about all this. It took me many years before I could even begin to define my new position.

I can see, looking back, that there is quite a gap between my early contributions, substantially completed by 1950, and the work on which I have been engaged from 1960 onwards. It is not that in the gap I was idle. There was work to be done in Oxford, where from 1946-52 I took part in the formation of Nuffield College; and where from 1952-65 I held the Drummond Professorship, with some general responsibilities for the organisation of post-graduate studies. And I was also much engaged in other activities, which sprang initially from Ursula's work in Public Finance, and from other work in that field in which I joined her. I have always held (as I said in the preface to *Value and Capital*) that theory should be "the servant of applied economics"; but I have also been aware that theory gives one no right to pronounce on practical problems unless one has been through the labour, so often the formidable labour, of mastering the relevant facts. Those which have to be mastered before one can pronounce on the macro-economic problems of developed countries are so extensive that the task of mastering them has usually to be left to specialists; but there have been simpler cases where it has appeared more manageable. During the years when the British Empire was breaking up, there were many such opportunities for British economists; they were often called on for advice in easing the transition to self-government and then independence. We have done a bit in that field, in Nigeria and in the Carribean, in India and in Ceylon; during the nineteen-fifties it was a major interest.

I pass on, as here is appropriate, to the years about 1960, which I reckon as the time of my *Risorgimento*. The first thing I had to do, on resuming my former work, was to bring myself up-to-date with what others had been doing; and I knew that I could not understand what others had been doing unless I could re-state it in my own terms. I did two exercises of that kind,<sup>14</sup> which took a good

<sup>14</sup> The first was published as "A Survey of Linear Theory" (*Economic Journal*, 1960); the second is embedded in the middle chapters of *Capital and*

deal of time. But I do not feel that these things are fully my own work; they are just "translations".

Nevertheless, with these behind me, I could go on. I could start to build on the work I had done in the thirties, but I could do so in my own way. I could take those parts of Keynes's system which I wanted, and could reject those which I did not want. I then found myself led, only incidentally to formal models, but chiefly to new analytical concepts, which may have some power to improve understanding of what has happened in the world, and what is happening.

There are three of these which I now feel to be important enough to be distinguished.

The first is the contrast between what I have called flexprice and fixprice markets: the former being those in which prices are made by the market (by demand and supply, as in the textbooks), the latter being those in which prices are made by producers, a change in price being an act of policy. This already appears in *Capital and Growth*,<sup>15</sup> but its fruits have been gathered throughout my later work. I contend that flexprice markets, as they have existed in practice, depend upon the existence of intermediaries, neither producers nor final consumers of the products in which they trade. My *Theory of Economic History* is largely an attempt to see the main lines of economic development as a matter of the evolution of the merchant-intermediary, and its consequences. But I have fully recognised that in the most modern times it is the fixprice market which is taking over. Thus, when I am concerned with contemporary problems, I have tried to think in terms of a *mixed* fixprice-flexprice economy.

The second is a deepening of the concept of liquidity, which, though it is Keynes's concept, was (I now feel) imperfectly explored by Keynes. He did not (at least in the *General Theory*) sufficiently stress the relation between liquidity and time. "Liquidity is not a matter of a single choice; it is a matter of a sequence of choices, a related sequence. It is concerned with the passage from the unknown to the known — with the knowledge that if we wait we can have more knowledge".<sup>16</sup>

*Growth* (1965). The writing of the latter owed much to the tuition which I received from Michio Morishima, while he was a Visiting Fellow of All Souls College in 1963-64.

<sup>15</sup> Especially in chapter 5.

<sup>16</sup> *Crisis in Keynesian Economics* (1974), pp. 38-39. See also my *Economic Perspectives* (1977) essay 3, and *Causality in Economics* (1979) chapters 6 and 7.

The third is the concept of the Impulse, which grew out of *Capital and Time*, but which did not finally emerge until the essay on "Industrialism" in *Economic Perspectives*. I think of a major invention, or other major change in circumstances, like the opening up of a new market, as generating a chain of consequences, some of which by theory can be followed out. I did not have this idea when I wrote my *Theory of Economic History*; it is needed to complete the analysis which I gave in that earlier book.

During the years since 1965, while I have been writing my later books, I have been a retired professor; but I have been allowed to continue to work in Oxford, at All Souls College. Though I have useful discussions with colleagues at Oxford, I have not been a member of a group, as I was in early days at LSE. Those who have worked closest with me have been visitors to Oxford, and post-graduate students, who themselves come and go. For though in Oxford our first degree students are mainly British, most of our post-graduate students come from abroad. When they have done their two or three years, they go back to the places, often very distant places, from which they have come. Such contact as one can then maintain with them must then be largely by correspondence — unless one can go and see them at their homes or places of work. I have indeed done a good deal of that.

It has so happened that a considerable number of economics post-graduates, and of other economists who have visited Oxford, have come from Italy. And it is not so far from England to Italy as it is to places further afield! I have explained the importance of my knowledge of Italian (which is still, I fear, little more than a reading knowledge) in the beginnings of my economics. It has been a great thing for me that I have again been able to use it in the contacts with Italian economists which I have been able to develop during the last twenty years. We now feel that a year which does not contain a visit to Italy is a year in which there is something missing. And now, when we come to Italy, we come to see our friends.

*Oxford*

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