

Recollections of an Economist *

How did I come to be an economist? I was brought up in Budapest, Hungary: my father was a lawyer and my initial expectation was to become a lawyer too — though I had a sneaking desire to become a writer. The experiences during and after the First World War, with its bewildering changes in social régimes from a monarchy to a liberal Republic, then to a Communist dictatorship lasting for four months, followed by a military dictatorship soon moderated by the need to conform to the institutional framework of a Parliamentary system desired by the victorious Western powers, made me interested in the forces which govern the political evolution of society. Then in the summer of 1923, aged 15, I spent a family holiday in the Bavarian Alps, when I had the rare opportunity to observe a state of on-going hyper-inflation and the extraordinary features of behaviour to which it gave rise. At the beginning of the holiday the prices of goods in the shops were raised by large amounts — of the order of 30-40 per cent — at least twice a week. But this process was not stable — it accelerated every week and then day by day. Towards the end everyone queued up outside the offices of banks where the dollar price for the day was posted at midday. Then everyone rushed to go bargain-hunting for goods at “yesterday’s price” before the shopkeepers had time to revise their prices in accordance with the rise in the price of the dollar. At the next stage the shopkeepers closed their doors at midday and re-opened an hour later with all prices revised. Even cafes and restaurants refused to accept payments until they worked out what the afternoon price was. At a later stage, which I no longer witnessed, the same process of revising prices occurred several times a day. At the same time I noted that translated into dollars, or other stable currencies, the prices of things, despite their constant revision, were extraordinarily low. There

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was a yawning and widening gap between the prices of goods in terms of local currency and their prices in foreign currency, which were very much lower.

These extraordinary phenomena aroused all my curiosity. *Why* did these things happen — and if they happened on some occasions how were they avoided in others? No-one could give an intelligible answer to this question, though I soon discovered that there is a branch of knowledge, economics, which should be able to provide a satisfactory answer. It was then that I decided to become an economist.

Apart from a year and a half in which, after finishing school in Budapest, I attended lectures in the University of Berlin (in 1925-26), my chief training in economics was as an undergraduate at the London School of Economics in 1927-30. My first real teacher in economics, albeit for a brief period, was an American, Allyn Abbot Young, who came from Harvard to L.S.E. to succeed Edwin Cannan in 1926. This unfortunately did not last long as in the winter of 1928-29 he died quite unexpectedly of pneumonia. Nonetheless, his lectures and seminars left a lasting impression on my later development, since it was to him that I owe a basic distrust of abstract systems *per se*, and an awareness of the need to adapt the tools of theoretical analysis to the practical problems which they are intended to illuminate. Economics, in Allyn Young's view, is best defined by the particular interests which have prompted its founders — not by its "subject matter" as such.¹

Young was succeeded at L.S.E. by Lionel Robbins, young, flamboyant and enthusiastic (he was only 30 at the time of his appointment) and extremely devoted both to teaching and to economics as a subject. He lavished his energies and vitality on his pupils and identified himself fully with their success and their attainments. It was inevitable that those of us who were fortunate to have been among his first pupils — and there were a bare dozen of us then specialising at L.S.E. in the subject of "analytical economics" — should fall completely under his spell. Robbins' economics (much influenced by his contacts with Viennese economists, mainly von Mises) was the general equilibrium theory of Walras and the Austrians, rather than of Marshall, and

his lectures followed the method of presentation of Wickseil and of Knight, *Risk, Uncertainty and Profit* (a book which contains in its first half an admirably clear and concise account of neo-classical theory). Robbins as a young economist absorbed this theory — the keystone of which is the marginal productivity theory of distribution in its generalised form, as expounded by Wickseil and Wicksteed — with the fervency of a convert and propounded it with the zeal of a missionary. It was thanks to him that I acquired a thorough grasp of that theory without being hampered by doubts and hesitations — which in other circumstances might have inhibited me (as it has inhibited other critics) from mounting the intellectual effort required for mastering its content.

The theory of general economic equilibrium, in Professor Kornai's phrase, is an "intellectual experiment" — a particular method chosen for describing how a market economy works under various simplifying and unreal postulates. These postulates were not intended by its creators to be more than intermediary steps in the process of analysis — they were simplifications which were intended to be removed later when the theory was brought into closer approximation to real life. But it was an inherent consequence of the *a priori* approach of this school that its followers should be pre-occupied with the properties of the notion of "equilibrium" — which meant that progress took the form, not of removing the scaffolding but of constantly *adding* to it. Making the theory more rigorous made the whole construction even more abstract (and hence more distant from its ultimate goal) since it involved the discovery (or recognition) of additional assumptions implied in the results.

As I wrote in a paper some twenty years ago,² "it is the hallmark of the neo-classical economist to believe that however severe the abstractions from which he is forced to start, he will win through by the end of the day — bit by bit, if he only carries the analysis far enough, the scaffolding can be removed, leaving the basic structure intact". I should, perhaps, have added that it is also the hallmark of the neo-classical economist — when he takes off his hat as a pure theorist and puts on his hat as a policy adviser or as an interpreter of current events — to behave as if the scaffolding *had been* removed already, and the basic structure had been *shown* to remain intact. When it comes to judging the effects of particular policy measures — whether it relates to

¹ Cf. his classic article on "Economics" in the 14th Edition of the *Encyclopaedia Britannica* (published in 1929). On a recent re-reading this paper gives the impression of being remarkably fresh and up-to-date. However Young is mainly remembered for his famous paper on *Increasing Returns and Economic Progress* (published in the *Economic Journal*, December 1928), which created a considerable stir on its publication, even though its main message was by no means fully understood at the time.

² "Marginal Productivity and the Macroeconomic Theories of Distribution", *Review of Economic Studies*, Volume XXXIII, No. 4 (1966).

unemployment, foreign trade, the incidence of taxation, exchange rates, etc. — he applies conclusions derived from the theory of general equilibrium to the real world without hesitation: that is to say, without investigating how far his results are dependent on implied or explicit assumptions that are manifestly contrary to experience.³

The economic theory that I was taught is a theory based entirely on the deductive analytical method (though at the time it was not clothed in the language of mathematics, as it is today) and my first effort in exposition is found on the first two pages of one of my earliest published papers.⁴

Such was the almost hypnotic power of Walras' system of equations that it took me a long time to grasp that this method of making an abstract model still more abstract by discovering unsuspected assumptions implied by the results is an unscientific procedure which leads nowhere.

It was a long journey. Most of my early papers were based on the deductive *a priori* method and concentrated on unresolved inconsistencies of general equilibrium theory but without questioning the fundamentals.

For students of the present generation it is difficult to convey the atmosphere of creative tension and excitement which prevailed at L.S.E. in the early years of the 1930s. Much of it was due to the youthful leadership of Robbins; much of it was due to the presence of a number of exceptionally able young graduates who were just beginning their professional careers; much of it arose through the intellectual challenge which the severity of the economic crisis (particularly in 1931-32) presented to all economists. It was a time of endless discussions which went on at all hours of the day and night — during meals, during walks and during weekends. I benefitted enormously from Oxford weekends spent in the company of a brilliant mind, Maurice Allen, who was a year senior to me at L.S.E. and then became a don at

³ A clear example of this is the recent recrudescence of "monetarist" theories and their application (*inter alia*) to the balance of payments which assumes — reverting to pre-Keynesian ideas of the working of the market economy — that the economy is in *continuous* full-employment equilibrium and there is universal perfect competition where all markets "continuously clear" (which means both that buyers buy all that they desire to buy at the ruling price, and that sellers sell all they wish to sell at that price — an assumption which is difficult to reconcile with the facts of modern advertising).

⁴ "A Classificatory Note on the Determinateness of Equilibrium, *Review of Economic Studies*, Vol. I, No. 2 (February 1934), pp. 122-36.

Oxford, first in New College and later in Balliol, and who held views which were then to the left of mine on policies concerning unemployment, etc.⁵

The other young economist with whom I spent many hours in discussion in our neighbouring flats, on Sunday walks, or occasionally on a Continental holiday, was John Hicks, then a colleague at L.S.E. Hicks (unlike me) was an indefatigable reader of books in at least three foreign languages, and it was owing to him that I was put on the track (among others) of the younger Swedish economists, particularly Myrdal⁶ who first made me realize the shortcomings of the "monetarist" approach of the Austrian school of von Mises and von Hayek⁷ and made me such an easy convert to Keynes after the appearance of the *General Theory* three years later.⁸

However, preceding the controversy over Keynes, the problems which interested me most were those concerning the nature of competition between business enterprises. The theory of imperfect competition was 'in the air' long before the books by Joan Robinson and Edward Chamberlin made their near-simultaneous appearance in 1933. Allyn Young devoted a great deal of his lectures to various forms of imperfect competition,⁹ his main interest being in exploring the circumstances in which competition had harmful rather than beneficial effects on the workings of the economy. At the same time, there was a prolonged controversy on the theory of costs and returns in the pages of the *Economic Journal*, the most distinguished piece of which was Piero

⁵ I mention this since we ended up so differently from where we began. In 1932 I was much under the influence of the views not only of Robbins but also of Hayek (the 1930 version of Milton Friedman); whereas Maurice Allen was more under the influence of Dennis Robertson and Roy Harrod. However, he ended up, after an interlude of fighting in Burma, as an Executive Director of the Bank of England noted for his extremely conservative views.

⁶ Myrdal's short book on "Monetary Equilibrium" (published in German in 1933) contained many of the features of Keynes' system particularly as regards the role of expectations in investment and the relation of the marginal efficiency of capital to the rate of interest.

⁷ My enthusiasm for the doctrines of Professor von Hayek had already suffered a relapse when as a first year research student I undertook to translate his article "Gibt es einen Widersinn des Sparens?" into English, and in the course of struggling with the translation detected various gaps and flaws in the argument. (The paper appeared under the title of "The Paradox of Saving" in *Economica*, May 1931.)

⁸ My close friendship with Hicks did not survive his departure from L.S.E., first to Cambridge, then to Manchester and finally to Oxford. Yet on looking back, our intellectual work continued to converge at unexpected points as shown *e.g.* by HICKS' book on the *Trade Cycle* (1950), or his book on *Capital and Growth* (1965), or his most recent paper on "Monetary Theory and Monetary Experience" (in *Economic Perspectives*, Oxford 1977).

⁹ Chamberlin's book (as stated in the Preface) was written originally as a Ph.D. thesis under Young's supervision. (This happened also to be the case with another famous American book of the inter-war period, KNIGHT's *Risk Uncertainty and Profit*.)

Sraffa's famous article on "The Laws of Returns under Competitive Conditions" which appeared in December 1926. This paper anticipated many of the important "discoveries" in economic theory over the next fifty years — though in a somewhat oblique way, so that its true significance was sometimes only appreciated when one arrived at the same conclusions independently after an interval of many years.¹⁰ Sraffa's paper provided the stimulus to a whole series of subsequent papers, many of which assumed, explicitly or implicitly, imperfect competition.¹¹

My main contribution to this debate was the paper on "Market Imperfection and Excess Capacity" in the February 1935 issue of *Economica*, the purpose of which was to demonstrate that free competition in the sense of "free and unimpeded entry" into any industry or sector of the economy will only lead to a state of "perfect competition" postulated by equilibrium theory if the law of constant costs applies over the whole range of outputs from the infinitesimal to the indefinitely large. If this cannot be assumed the effect of free entry will necessarily lead to a situation in which the multiplication of "firms" is brought to a halt by the rise in costs per unit as the output of the average firms is reduced in consequence of competition. Hence the typical firm will be operating on too small a scale — near the minimum scale at which his costs are covered (*i.e.*, near its "break-even" point) and not at the optimum scale postulated by the theory of general equilibrium.¹² But the general consequences of postulating decreasing costs at the margin of production — in the short run and not only in the long run — are very far-reaching; since the existence of increasing marginal costs in the neighbourhood of equilibrium (*i.e.*, in the neighbourhood of the actual levels of output of the individual enterprise) is the keystone on which

¹⁰ Thus in a lecture at Harvard in 1974 on "What is Wrong with Economic Theory" (*Quarterly Journal of Economics*, August 1975) I came to the conclusion that constant costs, or constant returns to scale in terms of transferable resources, was the basic axiom underlying the Walrasian theory of general equilibrium. I was quite oblivious at the time that the same assertion was made in Sraffa's article which I had read more than forty years earlier.

¹¹ The most important contributions, apart from Allyn Young's seminal article, were those of R.F. Harrod in the June 1930 and December 1931 issues, and of G.F. Shove in the March 1933 issue of the *Economic Journal*.

¹² In the absence of perfect divisibility the condition of "perfect competition" will apply only to a limited class of commodities capable of strict standardisation in terms of some universally acceptable system of grading which enable such commodities to be centrally traded in highly organised markets in which "good will" (or the personal element in dealing) is wholly eliminated. (The creation of such markets is in the joint interest of the ultimate buyers and sellers, *i.e.*, of the consumers and the producers, since it serves to minimize the margin absorbed by intermediaries, traders or merchants.)

neo-classical price and distribution theory rests. Its abandonment meant, in the words of Hicks, that "the basis on which economic laws can be constructed is shorn away" thereby causing the "wreckage of the greater part of economic theory".¹³

If economics had been a "science" in the strict sense of the word, the empirical observation that most firms operate in imperfect markets would have forced economists to scrap their existing theories and to start thinking on entirely new lines — in much the same way in which the accidental discovery of an excessive amount of light emitted by pitchblende forced a fundamental reconsideration of the theory of physics. Unfortunately economists do not feel under the same compulsion to maintain a close correspondence between theoretical hypotheses and the facts of experience. When Hicks realised (in 1938) that the contemplation of imperfect markets brought him to the brink of an abyss, he hastily drew back, and his example was eagerly followed by others.¹⁴ In most theoretical work published since World War II (apart from some isolated works on oligopoly), a state of perfect competition is assumed, explicitly or implicitly.

This was even true of Keynes who accepted Marshall's micro-economics in the *General Theory* without realizing that the phenomenon he was dealing with — involuntary underutilisation of *both* productive capacity and of labour — postulates the existence of "excess supply" (*i.e.*, a situation in which the amount actually produced or sold is *less* than the optimal amount individual sellers would prefer to sell at the ruling price), a state of affairs which could not exist under perfect competition. In doing this he made an unfortunate concession to his neo-classical critics, for it meant the acceptance of the traditional postulate of a falling marginal productivity function for labour in the short period which was the main plank of Pigou's *Theory of Unemployment*. That book, which preceded Keynes' by three years, gave the then fashionable explanation of unemployment as being due to the action of trade unions which raised wages above the "equilibrium" level. Keynes' acceptance of this neo-classical postulate made it possible for his conservative critics (from Pigou, Robertson, Viner and Robbins right up

¹³ *Value and Capital* (Oxford 1939) pp. 83-4. It was already recognized by Marshall (see Appendix H of the *Principles*) that the theory of "normal value" ceases to be applicable in the case of increasing returns.

¹⁴ In fairness to Hicks it should be pointed out that in his most recent publications he completely disowned the attitude he took up in 1939. (See *Economic Perspectives*, Oxford 1977, Preface and Survey); also *Economic Record*, September 1975, pp.365-67.

to Milton Friedman) to reject Keynes on empirical grounds by asserting that there is no evidence that the workers would accept lower real wages, and since a higher level of employment would cause real wages to be lower, there is no reason to suppose that any stimulus to demand could increase employment more than temporarily.

As Keynes said at the end of the Preface to the *General Theory* "the difficulty lies, not in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds". Keynes' acceptance of increasing marginal cost in the short period (for output as a whole as well as for particular industries) followed from the Marshallian assumption that different pieces of equipment and different workers are not homogeneous in efficiency, and that as a result of the general influence of competition, the more efficient equipment and labour will be used first (and utilized to the full) before any less efficient unit is brought into use. However innocuous or logically compelling this argument may appear to be — it is after all only a simple application of Ricardo's theory of rent — it is contradicted by empirical evidence both as regards the short period elasticity of output with respect to changes in the volume of employment¹⁵ and also as regards the observed association between the movement in real wages and employment.

Though Keynes retracted on his original assertion as a result of various criticisms,¹⁶ his position remained a guarded one, and he never produced a theoretical explanation of *why* his original argument of diminishing returns being a necessary consequence of non-homogeneity was wrong, and what the consequences of its abandonment were. To do so would have required an analysis of monopolistic competition which renders the traditional rules of resource allocation inapplicable. Keynesian unemployment, as distinct from Marxian or classical unemployment, can only subsist under conditions of monopolistic competition.¹⁷⁻¹⁸

¹⁵ This is attested by a large number of statistical studies in the U.S., U.K. and other industrial countries (at least from the late 19th century onwards), the best known of which has come to be called "Okun's Law", according to which a 1 per cent increase in employment is associated with a 3 per cent increase in output. For the U.K., see R.R. NEILD, *Pricing and Employment in the Trade Cycle* (N.I.E.S.R. Occasional Paper, no. XXI, Cambridge University Press, 1963); COUTTS, GODLEY and NORTHAUS, *Industrial Pricing in the U.K.*, (D.A.E. Monograph 26, Cambridge University Press, 1978).

¹⁶ Cf. "Relative Movements of Real Wages and Output", *Economic Journal*, March 1939, pp. 34-51, quoting papers by J.G. DUNLOP (*Economic Journal*, September 1938) and L. TARSHIS (*ibid.* March, 1939, pp. 150-54) and discussions with Mihaïl Kalecki.

¹⁷ This was perceived at an early stage by Kalecki but not by Keynes, and it is (in my view) the main respect in which Kalecki's original model is intellectually superior to Keynes' *General Theory*.

As it happened, the opportunity to build a new integrated theory based on Keynesian macro-economics combined with micro-economics built on the foundation of imperfect competition and oligopoly was missed. While Keynesian macro-economics opened new avenues for the analysis of the behaviour of the economy, and for the creation of a new theory of economic policy (and was generally triumphant in most industrialised countries for the first twenty five years after World War II), the theories of monopolistic competition atrophied — partly on account of the difficulty of getting beyond the stage of elementary abstract propositions, and partly owing to the sub-conscious desire to resuscitate general equilibrium theories of the Walrasian type which lent themselves to precise formulations in mathematical form. But the cost of this was that no real research was made, and no reliable knowledge acquired, as to how competition operates and how prices and the allocation of sales among competing producers are determined, under conditions of advanced capitalism; and in the absence of such knowledge, all interpretations derived from macro-economic data (which form the basis of policy making) remain surrounded by an aura of doubt and uncertainty.¹⁹

However, even without such an integrated theory Keynes' macro-economics gave plenty of opportunities for new thought both in the field of theory and policy. It gave an immediate stimulus to new theories of the trade cycle — which meant combining the Keynesian multiplier with some form of the "accelerator" (as was first done in R.F. Harrod's book on *The Trade Cycle* in 1936). My own work in this field consisted of a demonstration (in refutation of the contentions of Prof. Pigou) of how Keynes' results can in fact be reached with the aid of a "neo-classical" model employing traditional variables, provided certain

However, it is very doubtful, to say the least, whether in the absence of Keynes' genius and personality, his exquisite style and his ability to command attention, the ideas alone would have been sufficient to bring about the intellectual break-through which the 'Keynesian revolution' created.

¹⁸ The full significance of this has not been properly appreciated even now. A world in which marginal costs are below average costs, and are normally constant and not rising (up to the point of full capacity operation) puts 'paid' to all theories which assume a trade-off between real wages and employment. If the productivity of labour is a rising and not a falling function of the level of employment, there is no such thing as a "natural rate of unemployment" and no unique real wage which secures equilibrium in the labour market.

¹⁹ It is only now, after a lapse of forty years, that the necessity for the abandonment of the perfect competition hypothesis and its far-reaching consequences have come to be asserted (or re-asserted) by economists of the "orthodox" school. (Cf. CURT B. EATON AND RICHARD G. LIPSEY "Freedom of Entry and the Existence of Pure Profit" in *Economic Journal*, September 1978.)

critical assumptions are incorporated.²⁰ This was followed by three papers which have not, I think, been rendered obsolete by subsequent work. The first, "Stability and Full Employment" appeared in the *Economic Journal*, December 1938, the second "A Model of the Trade Cycle" in the *Economic Journal*, March 1940, and the third "Speculation and Economic Stability" in the *Review of Economic Studies*, October 1939. The latter attempted to generalize Keynes' theory of the multiplier by demonstrating that it results from the stabilising influence of speculative expectations on prices which applies in all cases in which the elasticity of speculative stocks is high (in other words, the elasticity of demand for *holding* stocks is distinct from the elasticities of "flow" demand or supply of the ultimate buyers or sellers). One of the purposes of that paper was to show that Keynes' theory of interest contains two separate propositions. The first regards interest as the price to be paid for parting with liquidity, and it arises on account of the *uncertainty* of the future prices of non-liquid assets. The second concerns the dependence of the current rate of interest on the interest rates expected in the future. While the first proposition provides an explanation of why long dated bonds should normally command a higher yield than short-term paper, it is the second which explains why the traditional theory of the working of the capital market was inappropriate — why, in other words, savings and investment are brought into equality by movements in the level of incomes far more than by movements in interest rates. And this second effect will be the more powerful the *less* is the uncertainty concerning the future, or the greater the firmness with which the idea of "a normal price" is embedded in the minds of professional speculators and dealers.²¹

This paper is supplemented by a further paper on Keynes' theory of the own rate of interest which was written about the same time but remained unfinished and was published only after a long delay in

²⁰ "Professor Pigou on Money Wages in Relation to Unemployment", *Economic Journal*, December 1937, pp. 745-53.

²¹ This appeared in October 1939. When I met Keynes a few weeks later at a Cambridge tea party I was greatly surprised that he had already read my article and said that I might well be right in my contention that it is the price stabilising influence of the policies of dealers and speculators, rather than the premium which the public requires for parting with liquidity, which explains why the increase in the propensity to save is not in itself capable of generating more investment. Though I met Keynes on a number of occasions later on I never had an opportunity of discussing that point further. But the point of that discussion was, I believe, very much the same as that raised some thirty years later by Leijonhufvud in his book on "*Keynesian Economics and the Economics of Keynes*".

1960.²² The significance of this latter paper, in the context of present day discussions, lies in its interpretation of the "transmission mechanism" through which changes in the amount of "money" in circulation can affect the level of prices. In Keynes' theory, this presupposes first a fall in the money rates of interest, followed by a corresponding fall in the own rates of *money* interest of assets relatively to their own rates of *own*-interest, which in turn should induce larger stocks to be held of the various assets and thereby stimulate their production; only if production is not elastic, will it raise prices.

All the above refers to papers written and ideas developed before the Second World War. The war caused a change in my surroundings and interests. Physically, it meant the transference of L.S.E. to Cambridge which brought me into much closer contact with the Cambridge economists (some of whom, like Piero Sraffa and Joan Robinson I had already known from pre-war encounters). The immediate effect of this was that I took a much closer interest in current issues of economic policy — primarily stimulated by listening to the lectures on the problems of war finance given by Keynes and to the discussions they gave rise to — I remember particularly the long debates with one of my earliest pupils, Erwin Rothbarth, then Keynes' assistant, in collecting the material for Keynes' pamphlet on *How to Pay for the War*.²³ At Keynes' request I wrote a review article on the White Paper on War Finance in the *Economic Journal* — which later became an annual feature in that Journal and an occasion for reviewing the economic management of the war. I also participated in numerous discussion groups on post-war reconstruction, one of which was organised by William Beveridge and which had as its outcome his book on *Full Employment in a Free Society*, to which I contributed an Appendix on the "quantitative aspects" of the full employment policies.²⁴ This created quite a stir, and became the prototype of far more sophisticated econometric models to serve the purpose both of economic management and forecasting. It also brought me a reputation of being able to combine theory with close factual analysis and led to various invitations — from the U.S. Strategic Bombing Survey for an analysis of the effects of the bombing campaign on the German war economy;²⁵ from the

²² In *Essays on Economic Stability and Growth*, London, Duckworth, 1960, pp. 59-74.

²³ He was later killed in action at Arnhem in 1944.

²⁴ Appendix C in BEVERIDGE'S *Full Employment in a Free Society*, London Allen and Unwin, 1944.

²⁵ Some of the surprising results of this investigation are summarised in a paper on the "German War Economy", *Review of Economic Studies*, 1945-46, Vol. XIII (1).

Commissariat du Plan Français to examine the requirements of financial stabilization in France, and finally from Gunnar Myrdal, the Executive Secretary of the newly created U.N. Economic Commission for Europe in Geneva, to become the Head of the Research and Planning Division of the Secretariat. I accepted this offer — which entailed my resignation from L.S.E. as I was refused leave of absence — and my first task was to recruit a staff of some twenty five economists and statisticians for the Division which in the circumstances of the immediate post-war period proved to be none too easy. Nevertheless, the work of the Division got sufficiently well organised to complete within nine months — that is, by March 1948 — the first Annual Survey on the Economic Situation and Prospects of Europe. Appearing at the moment when the U.S. Congress was in the throes of debate over the Marshall Plan, its diagnosis of the causes of Europe's difficulties and in particular its huge imbalance of trade with the U.S., attracted instant attention.²⁶ Though the subsequent annual Surveys improved greatly in the quality and quantity of their information, the basic design — the comparative treatment of the rates of progress of different countries and the conclusion drawn from a commodity analysis of international trade — have remained the same.²⁷

My period as a U.N. official involved also some special assignments away from Geneva, two of which deserve mention. One consisted of becoming a kind of 'Counsel' to the Committee of the non-aligned members of the Security Council meeting in Paris in the final months of 1948 on the subject of restoring a common currency for Berlin (which was the Soviet condition for lifting the blockade of Berlin). This involved intricate technical questions which the members of the Committee (mostly diplomats or Civil Servants, under the Chairmanship of Mr. Norman Robertson of Canada) were not in a position to formulate themselves, and to try to hammer out an agreement between the Soviet Union (represented by Mr. Malietin, the Soviet Finance Minister) and the Western Powers (represented by Mr. Burke Knapp

²⁶ It had the distinction of being the subject of the first leader on the day of its release in *The Times*, the *New York Times* and the *Guardian*. Its first mimeographed version was printed by the U.S. Government Printing Office for use of members of Congress, months before it appeared in print as a U.N. document; and within a year an unofficial translation appeared in German.

²⁷ Though the few years spent in Geneva were some of the most stimulating (as well as pleasant) in my life, I would not claim more than that I succeeded in creating a team which worked together with some enthusiasm and produced a unified piece which was both instructive and revealing. Though it is impossible to do justice in a matter of acknowledgements, the members of the team who contributed most to the first Survey included Hal B. Lary, the late Hans Staehle, Mrs. Esther Boserup, Albert Kervyn and Robert Neild.

from the State Department and Mr. Charles Gifford from the Foreign Office). The procedure adopted was to see the Soviet and the Western delegates alternately, and to put questions to them in the light of each other's pronouncements. The meetings dragged on from October until Christmas, and were chiefly notable for a continued softening of the Soviet attitude, and the continued hardening of the American line, in accordance with the growing success of the airlift in securing adequate supplies for the people of West Berlin. Towards the end the Russians were ready to meet all the essential requirements initially laid down by the Americans but by that time the desire of the Americans to come to a settlement had well-nigh evaporated. Their attitude was justified by subsequent events, for a few weeks after the breakdown of negotiations, Stalin lifted the blockade without any *quid pro quo*.

The other assignment consisted of serving on an Expert Committee appointed by the Secretary General of the U.N. (then Trygve Lie) which was asked to draw up a plan that would enable member States to follow full employment policies. The Committee met in the autumn of 1949, under the shadow of an impending economic recession, and the widespread fear that this would recreate an acute shortage of dollars (due to fall in U.S. purchases) which would force other countries into deflationary measures. The Committee which included two distinguished middle-of-the-road U.S. economists, John Maurice Clark from Columbia and Arthur Smithies from Harvard, in addition to Gilbert Walker from Australia, Pierre Uri from France and myself, managed to hammer out a unanimous report²⁸ containing a far-reaching proposal for guaranteeing the external supply of currency of any major country against reductions in its foreign disbursements on account of imports, capital exports, etc.

However, the proposals which were well received in some quarters (notably by the British Chancellor of the Exchequer, Hugh Gaitskell) did not find favour in Washington, and this plan, like numerous others which were put forward subsequently by international expert committees of varying kinds, led to some extensive but barren discussions and was then forgotten. Yet on re-reading it one is struck by how well its analysis of the nature of the international propagation of cyclical recession fits the present world situation, once the sea-change in *dramatis personae* is allowed for. However, at the time the expected

²⁸ *National and International Measures for Full Employment*, United Nations, New York, 1949.

world recession did not materialise — the outbreak of the Korean war put a stop to that. The dollar shortage, contrary to everyone's expectation — with the possible exception of Keynes, who foresaw that something of this kind might happen in an article written shortly before his death²⁹ — gradually gave way to a dollar glut. And America's role as a "chronic surplus country" was gradually taken over by Germany, Japan and Switzerland, joined later by Saudi Arabia and Kuwait. However, owing to the rapid emergence of unregulated international money in the shape of the Euro-currency market, the financial or payments aspects of an international disequilibrium take on a different appearance from what was then expected and they tend to mask the shortfall in effective demand (*in real terms*) which causes the persistence of heavy unemployment combined with inflationary price movements and an almost universal feeling of impotence in dealing with them.

It was during my first year in Geneva that I received an approach from the Provost of King's about a teaching Fellowship in the College. I regarded this as a unique opportunity to return to academic life — in intellectual surroundings that were far more congenial to me than the one I left behind at L.S.E.,³⁰ or indeed I could have found anywhere outside Cambridge.

I was familiar with the syllabus and the methods of teaching in Cambridge as on Keynes' invitation I gave a special course of lectures on the theory of distribution to the Cambridge Faculty throughout the war and afterwards (until I went to Geneva) and was also an examiner to Part II of the Economics Tripos on several occasions. I accepted, subject to being allowed to postpone my going to Cambridge until October 1949, so as to be able to get the Research Division of the E.C.E. properly going.

The return to academic life brought me back to economic theory again. The focus of interest had in the meantime shifted from the trade

²⁹ "The Balance of Payments of the United States", *Economic Journal*, June 1946, pp. 171-187.

³⁰ As should be evident to the reader, my later years at L.S.E. in the 1930s were not altogether happy ones. Though the place never lacked intellectual stimulus — and there was plenty of opportunity to expound one's views in Lionel Robbins' weekly seminars — I felt out on a limbo as an early and enthusiastic supporter of Keynes, and out of sympathy with the rigid neo-classicism of Robbins, Hayek and most of the senior members of the Economics Department. Though L.S.E. was always regarded as "left-wing" by outsiders, this was an image largely created by the "media". During the period while I was there 'left-wing' views were confined to Harold Laski and to a few lecturers in law and sociology. The economics department was dominated by those who held orthodox views both on money and the functioning of a free market system — an ideology which I embraced for a brief period, but abandoned well before the appearance of Keynes' *General Theory*.

cycle to economic growth. This was greatly stimulated by the publication of Harrod's *Towards a Dynamic Economics* in 1949 which reintroduced the classical dichotomy in the notion of the 'growth potential', by distinguishing between the growth of its labour-potential, or work-performance potential (defined as the rate of growth of the *effective* labour force, which is the product of productivity per man and the available number of workers) and its capital growth potential (which is identical with the share of savings in income divided by the capital/output ratio). As Harrod (and later Domar) treated these factors as exogenously given, and mutually invariant, the problem of reconciling the two growth potentials — the "warranted" rate of capital accumulation and the "natural" rate of growth in the effective labour force — appeared as the basic "dynamic" problem.

The search for the inter-relationship between the rate of capital accumulation and the rate of growth of labour productivity led me to two important ideas. The first was that technical progress and capital investment are inextricably mixed up — inventions require to be embodied in "machines" or equipment of some kind which did not exist prior to the invention (or not in that form) but the full potentialities of which can only be realized after a long interval as a result of the design improvements that can only be gained from operational experience. It took over a hundred years to get the "best design" in steam locomotives; over fifty years to get the best design (or at least a 'stabilized' design) in sewing machines.³¹ It is impossible therefore to isolate the effects of capital accumulation and the effects of "technical progress" on the productivity of labour (or in technical jargon, the "movements along" the production function from "shifts" in that function); all that can be said is that the growth of productivity will be greater the more technological change is "activated" through new investment. Hence all that is legitimate to postulate is a relationship between the rate of productivity growth and the rate of new investment per worker — which I called a "technical progress function" — which

³¹ Of course we cannot know when (if ever) the 'best design' embodying a particular engineering idea is accomplished. But the universal experience of the last two centuries has been that while there is always a fairly extended period during which important improvements in design take place, well after the first adoption of the new invention, these come to a gradual halt after a further interval of time (which may extend to half a century or more) until a new invention crops up which gradually displaces the original "machine" altogether. (A good example is the steam locomotive, which reached its 'best design' around 1910 while its subsequent total replacement by the diesel engine came in the later 1930s, some thirty five years after the original invention of the latter.)

cannot however be assumed to be the derivative of a production function and of an exogenous rate of technical progress. Once this is accepted it inevitably follows that there is no such thing as a “technological frontier” of substitution between capital and labour, the slope of which (at any one point) would serve to determine the distribution of the product between profits (or “interest”) and wages.³²

Independently of this I felt for a long time that the share of profits in the national income was determined by macro-economic forces which ensure that the expenditures of entrepreneurs themselves generate the profits which serve to finance that expenditure. I was led to this at an early stage through the contemplation of the puzzle of the widow’s cruze in Keynes’ *Treatise on Money*³³ which was highly suggestive but not properly integrated with the theoretical framework of the *Treatise on Money* nor considered explicitly in the *General Theory*. Kalecki’s paper on *A Theory of Profits*³⁴ carried the story a stage further by clarifying the nature of the asymmetry of the position of “capitalists” and “wage-earners” which can be summed up in the well known phrase “capitalists earn what they spend, while wage-earners spend what they earn”. But he did not develop this into a theory of distribution, for as regards the determination of distributive shares he continued to rely on the “degree of monopoly” theory of the relation of wages and prices.³⁵

It was when I fully grasped the significance of the proposition that the savings of workers and salary earners must have a *negative effect* on the profits of businesses (in the aggregate) because it means a corresponding reduction in the receipts from the sale of goods to the personal sector relatively to the business sector’s current outlay, which (in a closed economy and abstracting from the existence of Government expenditure and taxation) is really nothing else than the total wage and salary bill (assuming that all inter-business outlays and receipts arising out of current operations are set off against each other). Therefore for

³² This latter proposition — *i.e.* the marginal productivity theory — continues to dominate the economic text books (and presumably the lecture courses) of most Western Universities, even though it is impossible to endow it with any heuristic value. Apart from postulating the existence of “capital in the abstract” which can be identified (or measured) in reality, it assumes a whole paraphernalia of conditions which do not obtain even approximately in the real world — *e.g.* perfect divisibility, constant returns to scale, perfect competition etc.

³³ *A Treatise on Money*, London, Macmillan, 1939, Vol. 1, p. 39.

³⁴ *Economic Journal*, June-September 1942, p. 258.

³⁵ I have never been able to accept that theory for the same reason for which I did not regard the concept of a demand curve applied to the individual firm as a valid one, except in the special case of “polypoly” where each seller decides on his optimal profit margin independently of the prices charged by his competitors.

business receipts to exceed production costs — in other words, for aggregate business profits to be positive and not negative — the capital outlays of businesses must exceed *personal* savings; in order that profits should be a “sizeable” proportion of sales, this excess moreover must be large relative to personal savings. But this implies in turn — since (under the assumption of a closed economy) total savings must always be equal to total investment — that savings out of profits must be large relative both to the total capital outlay and to the total profit. The two basic inequalities of a ‘Keynesian’ theory of distribution³⁶

$$s_p > s_w \geq 0$$

$$s_p > \frac{I}{Y} > s_w$$

are therefore not arbitrarily chosen; they are the necessary conditions for a private enterprise system to function.³⁷

The combination of these two ideas — *i.e.*, the technical progress function, and a Keynesian (or classical) savings function — together with a Keynes-Harrod type investment function, led to the formulation of a combined growth and distribution model which I worked out with the help of David Champenowne in the summer of 1957.³⁸ This has shown that it is possible to construct a model which has a determinate solution in terms of growth rates, the capital/output ratio, the investment coefficient, the profit share and the profit-rate without involving a “production function” or indeed marginal analysis of any kind. It therefore demonstrated (if no more) that neo-classical theory is not indispensable — it is possible to build an equilibrium model using entirely different bricks.

³⁶ I put forward this theory in “Alternative Theories of Distribution”, *Review of Economic Studies*, Vol. XXIII, No. 2 (February 1956). s_p and s_w stand for the savings co-efficients out of profit and wages respectively, I for business investment, Y for income.

³⁷ Consumption expenditure out of profits is itself dependent on profits. It cannot therefore secure any excess of receipts over outlays, unless there is adequate expenditure on capital account to offset the savings (of both individuals and businesses) on income account. (For an individual country, such offsets could take the form of a surplus of exports in foreign trade or loan expenditures by the Government, as well as business investment.) Various economists (J.E. Meade, F.H. Hahn, P.A. Samuelson and F. Modigliani) called into question the universal validity of the second inequality and asserted that on a *priori* grounds there is nothing to prevent the share of investment in output being less than the share of savings in non-profit income. But they overlooked the vital fact that for profits to exist, business expenditure on capital account must exceed non-business savings, and that a capitalist system cannot function unless businesses make a profit.

³⁸ “A Model of Economic Growth”, *Economic Journal*, 1957, pp. 591-624.

However, this model had its shortcomings which neo-classical critics were not slow to point out. It was very much a "Mark I" model (as D.H. Robertson once referred to it) and led to the presentation of improved versions, Mark II and Mark III, in the course of the subsequent five years. These latter models were prepared in 1958³⁹ and 1962⁴⁰ and the Introduction to the volume of Essays⁴¹ published subsequently explains how they arose and the nature of the differences between them. There is no need to go into them here. But as I explained in that Introduction, the development of my theoretical ideas has by no means come to an end with the work on growth models. Since 1965 they have changed fairly drastically, though I have not been able to present the results (though perhaps I might still be able to do so in the future) in the comprehensive form of a "model". The last six Essays in the volume of Essays already referred to⁴¹ — starting with my Inaugural lecture,⁴² and covering papers written up to 1976 — illuminate various aspects and implications of my new ways of thought without a systematic presentation of the set of interrelated ideas as a whole.

Not wishing to repeat the account given in the Introduction to the volume of Essays referred to I think I can best explain the nature of this development in terms of the shortcomings gradually perceived in my earlier theoretical work expressed in the growth models published in the years 1956-62. The list presented below follows a logical, not a chronological sequence.

1) A macro-economic model needs to be supplemented by a micro-economic analysis on the level of the single decision making unit, the firm or business, and also of the relations between groups of competing firms [without such a supplementation it is not obvious how the forces isolated in the macro-model actually operate; and it is impossible to say how far (how effectively, or how quickly or slowly) they operate]. In particular we need a theory of how prices are determined in the oligopolistic conditions prevailing in industry, in the "perfectly competitive" conditions prevailing in agriculture and most

³⁹ "Capital Accumulation and Economic Growth", in *The Theory of Capital* (ed. F. Lutz), London, Macmillan, 1961.

⁴⁰ "A New Model of Economic Growth" (written in collaboration with J.A. MIRRELES), *Review of Economic Studies*, Vol. XXIX (1962), No. 3.

⁴¹ *Further Essays on Economic Theory*, London, Duckworth, 1978.

⁴² *Causes of the Slow Rate of Growth of the United Kingdom*, Cambridge University Press, 1966.

types of mineral extraction (where the individual producers are price-takers, not price-makers) and finally under conditions approximating the "pure" imperfect competition or "polypoly" (small-scale businesses combined with free entry, with each seller facing a limited market) which prevails over much of the tertiary sector, such as retail distribution or miscellaneous services. Oligopoly which is typical of modern manufacturing industry invariably involves price-leadership; and the considerations which enter into the determination of the profit margin of the price-leader — governed by the long-run requirements for internal finance on the one hand and the need to preserve the firm's position as price-leader on the other — provide the key to the manner in which manufacturing industry operates under conditions of modern capitalism.⁴³

2) The macro-economic growth models leaned heavily on the notion of some kind of exogenous growth potential (which could however be taken to mean a zone rather than a single or a unique rate) which was some variant of Harrod's "natural" rate of growth, and reflected the assumption of an exogenous technical progress function and of population growth. The models showed how the rate of accumulation of capital and the rate of growth of output become 'attuned' to this "natural" growth rate. Such an approach is only valid in a universal context — where it refers to the *whole* productive activity of a closed or self-contained system, which has no "real-world" analogy except when the economy of the world is considered as a whole. It is not a valid assumption for analysing the economy of a single region (and the nation, looked at as a sovereign political entity, is only a particular kind of region) which is dependent on other regions both for satisfying some of its needs and for providing a market for its products; and the "resource-endowment" of which (except for natural resources) cannot be considered as exogenously given. The development of such a region, given its natural endowments (soil and its fertility, and mineral resources) and its past heritage of human and material resources at a given point of time, will depend on the external demand for its products (existing or potential) and the nature of its supply responses which together determine whether the effective demand for its products grows

⁴³ I have never been able to publish my ideas on this subject — which were developed over a run of years as a result of lectures which were annually revised — but my approach was similar to that of Adrian Wood in his book on *A Theory of Profits* (Cambridge University Press, 1975), who however carried the subject much further in some directions than I did.

relatively fast or slowly. This in turn decides whether it will attract resources from outside (through immigration and/or capital imports) or the opposite.⁴⁴

3) The macro-models were in effect "single sector" models; they assumed that the same set of behavioural assumptions could be applied to all sectors of the economy, whereas there are important differences in technology, the type of market structure and the nature of competition as between the primary, secondary and tertiary sectors of the economy, the outputs of which are largely complementary to each other. Even in the first approximation a macro-economic model relating to a closed economy needs to be a two-sector model of Agriculture and Industry. The Keynesian type of analysis in which effective demand plays a leading role is really a theory relating to Industry (which is largely the manufacturing sector). Manufacturing plays a key role in economic development as attested by the strong empirical association between economic growth and the growth of manufacturing activities.⁴⁵

4) Manufacturing activities, on account of economies of scale, internal and external, tend to cluster around particular "growth points" which become areas of vast immigration from more distant areas as well as from surrounding centres. This creates a tendency towards the concentration of industrial development, through a process of cumulative causation, which enhances the growth of the "successful" industrial centres by retarding or inhibiting the industrial development of others.

⁴⁴ It is usual to assume (for purposes of economic planning or forecasting) that each "individual" country has a potential full-employment growth rate; the purpose of economic policy is considered as one of ensuring that the actual growth-path does not diverge too much from the "full employment potential". Yet over longer periods there is considerable mobility of labour between countries (as it is within countries). Moreover the notion of "full employment output" is itself a question-begging one since it is relative to the (inherited) distribution of the labour force between different sectors. Normally there is considerable scope for increasing output through labour transference from low to high productivity sectors, the effect of which is causally indistinguishable from that due to immigration. In both cases, it is not the limitation of resources, but the limitations on the speed of adaptation or adjustment which set temporary ceilings on production.

⁴⁵ All "developed" high income countries have a highly developed manufacturing sector and are important exporters of manufactures. The reasons for this have not perhaps been fully explored; in my view they are connected with the fact that manufacturing industry generates the means for its own "extended reproduction": it generates both the savings required for capital accumulation and also provides the capital goods in which these savings are embodied; it also produces (largely though not entirely) the capital goods for the primary and tertiary sectors. (The exception is agriculture, the savings of which are partly embodied in its own output.) This, together with the existence of static and dynamic economies of scale in manufacturing, is responsible for the fact that both the level and the rate of growth of productivity in the economy as a whole is highly correlated with the level and rate of growth of manufacturing production.

This process of cumulative causation is no doubt mainly responsible for the growing differences, in productivity and real income per head, between rich and poor areas. The spatial aspects of competition under conditions of cumulative causation constitutes a field that has not been explored yet but which may call for radical changes in the prevailing views concerning the effects of freedom of trade between different countries or regions.

And here the matter must rest. I have described at considerable length the evolution of my theoretical "ideas" both before and after the Second World War, simply because throughout my academic life economic theory remained my basic interest. This was true despite increasing pre-occupations with a range of "specialised" matters such as the principles of taxation (as a result of my work on the Royal Commission on Taxation), the international implications of full employment policies, the reform of the international monetary system, commodity policies etc., as well as devoting an increasing amount of time to being an economic or fiscal adviser to various Governments abroad and two successive Labour Governments at home.

Keynes wrote once that few people are able to absorb new ideas after they are twenty-five or thirty years of age. If this did not prove to be so in my case (as it was certainly not in his) I owe it, I think, to the need to give a course of lectures on economic theory to third-year students in Cambridge. This alone gave continuity to my interest in economic theory and forced me to think through afresh each year the reasoning which underlay particular propositions.⁴⁶ For that reason I would not recommend to anyone (in the field of social sciences at any rate) that they should concentrate on "research" as against "teaching". It is more fruitful — and in the longer run more creative — to combine both.

* * *

⁴⁶ I discovered at an early stage that to give a lecture enjoyable to oneself (let alone the audience) it must deal with ideas that are fresh in one's mind. I made it a habit therefore to write out a new set of lecture notes each year, having the previous year's notes in front of me. I found that this annual review of one's ideas caused one to see things in a different light sometimes by slow stages and sometimes by changing one's viewpoint quite unexpectedly as a result of exploring a new line of thought. For the same reason I was loathe to stop lecturing for more than a year or two at most (while I was seconded to the Treasury under the Labour Government) knowing full well that a more prolonged interruption would make it very difficult to get back to lecturing on a subject which required unusual mental concentration.

I have said nothing in the above account about my works in the field of applied economics. These are collected in three volumes of Essays, two of which were published in 1964⁴⁷ and the third in 1978⁴⁷. In addition the two further volumes⁴⁸ on problems of taxation reproduce, for purposes of record, the *Memorandum of Dissent* of the Royal Commission on Taxation (which I drafted on behalf of two other members of the Commission as well as myself), as well as the Reports submitted to various foreign Governments or Governments agencies (written at their invitation) and a miscellaneous number of papers submitted to various bodies.⁴⁹

There is no need to say much about my “applied” essays since their subject matter and the circumstances in which they were written have already been fully described in the Introduction of the volume of Essays in which they are printed. In the first of these Introductions (to *Essays on Economic Policy*, Vol. I) I describe papers on economic policies as “more ephemeral than the theoretical ideas that form their background”. I now find that this was a hasty judgment. At least two of the papers written twenty-eight years ago could have been equally well written now. One of these, a plan for a permanent incomes policy, has recently been described to me by an official dealing with this particular subject as “very good — but rather utopian — the time is not yet ripe for it”. Another paper written in 1950 on “Employment Policies and the Problem of International Balance”⁵⁰ analyses the various options facing deficit countries confronted by the chronic surpluses of countries who refuse to expand their home demand adequately. This also is very much a live topic, the only difference being that the “chronic surplus countries” of today are not the same as those of thirty years ago.⁵¹

⁴⁷ *Essays on Economic Policy*, Vol. I and II, London, Duckworth, 1964. *Further Essays in Applied Economics*, Duckworth, 1978.

⁴⁸ *Reports on Taxation*, Vol. I and II, Duckworth, 1980.

⁴⁹ These are additional to the five papers on the subject of tax reform which have been included as Part III of *Essays in Economic Policy*, Vol. I.

⁵⁰ *Essays on Economic Policy*, Vol. I, pp. 83-94.

⁵¹ That paper prepared for an early Conference of the International Economic Association in Monte Carlo proved to be “very controversial” when it was discussed by the group of academic economists assembled there. If it were put forward today, it would be criticized on much the same grounds by a similar group of establishment economists. The “battle-lines” as they were drawn up then have not really changed in the intervening thirty years; in a sense they have not changed since the debate started between the “Currency School” and the “Banking School” in the 19th century. Recently the adherents of the “currency school” — the monetarists — have become more vocal and more numerous but I feel confident that their influence will fade in much the same way as it did on earlier occasions.

The same is true of other papers written in the 1950s — such as the analysis of the relative merits of fiscal and monetary policies or⁵² my memorandum to the Radcliffe Committee on the *modus operandi* of monetary policy submitted in 1958,⁵³ or the ideas put forward in my review of the Radcliffe Report published in 1960.⁵⁴

I find that on all these matters neither the nature of the problems changed much nor my own views relating to them, though the prospect of reaching an agreed view among economists is no better now than it was twenty-five or thirty years ago. On the contrary, the upholders of the quantity theory of money have become more influential and numerous; it is their opponents, the views represented in the Radcliffe Report, which seem to be on the defensive. Yet I remain convinced that all this recrudescence of pre-Keynesian views, the new monetarism, has nothing to be said for it — I regard it as a symptom of intellectual decadence that so many people should accept it without having the least notion of how the monetary authorities regulate the “money supply” when much the greater part of the money supply consists of transferable-debt certificates of financial institutions, and when the range of institutions prepared to under-write other people’s spending is constantly widening.

There is only one important matter on which the events of the 1970s caused me to change my mind. This concerns the relative importance of price (or cost?) competition, as against other “non-price” factors, such as superiority of design or quality, length and reliability of delivery dates, after-sales service, etc. Exchange rate adjustments operate *mainly* on costs and prices, and despite vast changes in relative exchange rates — in real, and not just in nominal terms — there was little effect on the pattern of trade in manufacturing. The trade-gaining surplus countries continued to gain trade, and the trade-losing deficit countries continued to lose it (especially when their own domestic market is taken into account, as well as the foreign markets). It is possible of course that if exchange rate adjustments go far enough, and last long enough, the day will come when they will begin to show results in terms of a trade-loss due to over-valuation and trade-gain due to undervaluation. In the end, if the change in relative prices goes far

⁵² This is contained in the paper written for the centenary celebration of the Royal Economic Society of Belgium in 1955, reprinted in *Essays on Economic Policy*, Vol. I, pp. 101-08.

⁵³ *Ibid.*, pp. 128 ff.

⁵⁴ *Ibid.*, pp. 154-65.

enough, a point must come when cheapness will compensate the buyer for all non-price disadvantages. But even if that proved to be the case in the next few years, the world would have lost an enormous amount of wealth and well-being — through lost production and mass unemployment — in the intervening years (or decades) of “disequilibrium”.

The lesson of the 1970s and the 1980s, to my mind, contradicts the current intellectual trends which seek salvation through a return to a free market system. It shows that instruments which operate through market forces (such as devaluation) are much too slow in their effects to avoid unnecessary (and in the long run, intolerable) hardship caused by reliance on them. If the mainly private-enterprise market economy is to survive (as it must, if even less palatable alternatives are to be avoided) the world needs more planning and more regulation in the matter of income-distribution as well as in the field of international or inter-regional trade, and not less.

Cambridge,

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