

From Theory to Policy-Making: Economics as a Profession *

I came to the study of economics by accident. My ambition was to become an accountant, although I was just as ignorant of accountancy as I was of economics. But accountancy in those days meant a five years' apprenticeship which would have ruled out taking a university degree and in addition I was reluctant to discard two scholarships sufficient to meet the cost of university study. I was able to square the circle by embarking on a degree in economics which would allow me to cut two years off an apprenticeship for which I had little appetite and which in the end I never even began. As the years went by, economics became more enticing and accountancy lost all attraction. I never afterwards felt any impulse to return to accountancy, not even when I came to teach economics to every intending accountant in the West of Scotland in nine successive courses between 1935 and 1939, covering the entire subject twice a year in forty lectures delivered in ten weeks.

It was an assignment I had expected to share with my colleague Alec Macfie, one of us taking the winter course and the other the spring. But it was at a time when he kept suffering from perforations of an ulcerated stomach and had to be removed to hospital whenever his turn came round, until finally he had to have his intestines removed in a novel operation that featured in the *British Medical Journal*.

I completed a four year course at Glasgow, combining economics with a variety of other subjects, but not, as I had intended, mathematics: English and history in my first year, more history, logic and moral philosophy in my second, and politics in my last two years as part of the honours course. It was one of the virtues of arts degrees in Glasgow that you could take a wide spread of subjects and were required to combine two subjects in an honours course.

* Contribution to a series of recollections on professional experiences of distinguished economists. This series opened with the September 1979 issue of this *Review*.

Economics was a very different subject in 1928 from what it is in the 1990s. For example, it leaned much further towards philosophy. Price theory, for example, was taught as the theory of value; and it took me quite a time to realize that value theory dealt with price determination under another name. Even in the honours class the word "price" was hardly used and more attention was given to a work by Professor John Laird of the University of Aberdeen on the philosophical theory of value. In keeping with this, the subject was largely non-quantitative: apart from a reference to the approximate size of the national income, no figures of any kind were introduced, even by way of illustration. Again, the theory to which we were introduced concentrated on microeconomics. Macroeconomics came in separate packages labelled: "the trade cycle"; "money and banking"; "international trade", and so on (but international trade was almost completely neglected).

A further difficulty was the lack of up-to-date textbooks. We were expected to begin with William Smart's *Introduction to the Theory of Value* (Smart had been the first holder of the Adam Smith Chair and was the translator of Böhm-Bawerk). This was to be followed by the abbreviated version of Marshall's *Principles* which I found unbearably dull. The only elementary textbook covering the whole of the subject was Henry Clay's *Economics for the General Reader* which even then was rather out-of-date. Fortunately the Cambridge Economic Handbooks provided an excellent and readable guide, especially Henderson's *Supply and Demand* and including also Dennis Robertson's *Money* and Austin Robinson's *Structure of Competitive Industry*.

I don't think I learned a great deal about economic theory from my teachers. At least that was how I felt when I arrived in Cambridge in 1932 all set to begin a Ph.D. My professor, W.R. Scott, was an eminent economic historian and much respected by his peers (he was, for example, Treasurer of the British Academy) but his lectures seemed to consist of one long sentence at dictation speed. He brought with him to the lecture room a fat leather-bound volume which he never consulted. But when I was able to have a look at it some years later when I joined the staff I found that it contained a set of lectures in the Marshallian tradition that were far in advance of what we were offered verbally. In the honours class (which we took for two successive years) we learned more about the life of Adam Smith (which Scott was busy writing) than about advanced economic theory.

Nominally the class was in Public Finance – a promising subject but given very rarefied treatment.

Nearly all lectures from start to finish were given by Scott. But occasionally a course was given by the two lecturers on the staff, Alec Macfie and J.W. Nisbet. They also appeared when Scott was away on official business, as frequently happened, and helped to give us some insight into the economic forces at work in the real world. Nisbet had a racy, popular style and was a firm believer in market forces. I suspect that he played an important part in the development of Thatcherite economics, not in any direct way, but as the teacher at St Andrews University of Lord Harris and others who played leading roles in the development of the Institute of Economic Affairs. Alec Macfie had a very different personality, quieter and subtler. But for war injuries he would probably have been a Scottish rugby international. Later in life he built up out of his earnings as a lecturer a remarkable collection of paintings by Scottish artists which he left to the University. One course which he gave us that was of particular interest was in theories of the trade cycle (subsequently published in a book under that title).

One of the merits of the Glasgow course was that honours students were expected to submit a thesis (with no limit of length that I can recall). Stimulated by the controversy on German reparations I spent much time preparing a long essay on *Capital Transfer and the Terms of Trade*. This caused me to read widely in the theory of international trade, starting with Taussig and including of course the Keynes-Ohlin debate in the *Economic Journal*. A good deal of the theory seemed to me to take a long time to establish rather simple points, but I liked the "verification" approach to international trade theory adopted by Taussig, Viner and the Harvard economists. Only, they seemed to me to get things the wrong way round and assume that it was capital flows that dictated the terms of trade when it seemed far more likely that it was the price relationships that governed capital flows. Thus I was an early disciple of Erik Lundberg whose warcry was "Why not the other way round?"

The thesis served two other purposes. First of all, I had come to be a devoted student of the Cambridge economists, eager if possible to study in Cambridge. As there were no scholarships in Glasgow sufficient for the purpose I applied in the summer of 1932, before "finals" in September, for a Trinity College (Rouse Ball) Scholarship, but with little expectation of success. For this purpose I submitted

some of my work, including a copy of the thesis. It sufficiently impressed Dennis Robertson to win me the scholarship. I was astonished and delighted beyond measure.

When I got to Cambridge what could be more natural than to build on the thesis to frame a dissertation for the Ph.D.? I had observed that international trade theory seemed to be left to the Americans, the Swedes and the Austrians; and it was soon apparent to me that Keynes was developing his ideas in terms of a closed economy without applying them, as he had in the *Treatise* to international trade and finance. I made up my mind, therefore, to review the period covered by my thesis (1870-1914) in the light of the Keynesian theory of employment. This meant concentrating not just on foreign investment, as existing work did, but examining the fluctuations in domestic investment to see how the two interacted. For this purpose it was necessary to dig deeply into forgotten statistical sources.

Cambridge in those days had a great wealth of talent both among the dons and the undergraduates. Keynes, Pigou, Dennis Robertson, Gerald Shove, Richard Kahn, Joan Robinson, Colin Clark and others were all on the lecture list and the undergraduates in economics included Brian Reddaway, Dick Stone, David Champernowne, Bryan Hopkin, David Butt and many from abroad who later occupied important positions such as Bob Bryce and Lorie Tarshis from Canada, V.K.R.V. Rao from India and Sid Butlin from Australia. There were, however, few – if any – undergraduates from Europe. Looking back, the most surprising thing is that in 1932 nobody in Cambridge had a Ph. D. One Ph.D. had been awarded – to G.T. Jones, author of *Increasing Return* – but he had been killed in an accident. The next to be awarded was in 1933, to E.R. Walker, an Australian, and I was no. 3 in 1936. There were, it is true, other research students, all of them from abroad, who came and went without studying for a degree; and we managed to form a group that met on Sunday mornings, latterly with an admixture of students from the LSE, which blossomed into the London and Cambridge group.

My knowledge of economics up to 1939 derived largely from lectures and discussions in Cambridge. I spent most of my time with the undergraduates, attending lectures with them, but mixing also with the graduate students, particularly Walter Salant, Harry Wolfson and Hans Singer. I recall the excitement of listening to Keynes on

Mondays at 12 in the Michaelmas term for three years running while the *General Theory* was in gestation: particularly his attempt to convey how new ideas are born in a cloudy whirl and later acquire shape and form and later still harder edges than their creator envisaged – a fate to which he seemed reconciled for his own ideas. But his was only one of the revolutions in progress. There was Colin Clark, whom I first met walking across the lawn at King's in carpet slippers on his way to Keynes' Monday night discussion club. Colin has claims to be a pioneer in the revolution in economic statistics and at that time was working on the statistics of capital formation. Since this was exactly what I proposed to work on – but for an earlier period – I picked him as my supervisor in succession to Pigou. Then there was Shove who was in the middle of the revolution in competition theory and seemed to me to have more important things to say than either Joan Robinson or Edward Chamberlin but hated letting anything of his appear in print. Unfortunately no one has succeeded in doing for his lectures what Professor Rymes has done for Keynes' in those years.

At Glasgow I had been particularly struck by Richard Kahn's 1931 article on the multiplier and took care to attend his lectures on "the short period" (based on his fellowship dissertation and published in 1989 as *The Economics of the Short Period*). I recall very little of these although they made a deep impression on me at the time. Nor have I any distinct recollections of Joan Robinson's lectures except their trenchancy. But I tried to boil down what I had learned from Shove, Kahn, Joan Robinson and Duncan Burn when I came to write a textbook in 1938, as described below. Unfortunately the Shovian influence was so muted that nobody noticed it.

One of the great experiences of those days was attending meetings of the Keynes Club on Monday nights. A paper was given, sometimes by an undergraduate, sometimes by one of the dons or a visitor, and occasionally by Keynes himself. Before the reading of the paper Richard Kahn came round with small slips of paper in his hand from which each of the junior members was asked to draw. Six of the slips were numbered and if you drew a number you had to speak, however briefly, advancing to the blazing fire and addressing the gathering with your back to it. The dons followed, if they chose to comment, and Keynes summed up. I gave two papers in different years, both on capital investment, and was misguided enough to

include in the first the limerick (in Latin) about the young lady of Niger.¹

It was at these meetings that I heard Melville and Shann, on their way from Canada to Australia in 1933, defend the Ottawa agreements, Hubert Henderson attacking public works as a cure for unemployment, Ronald Walker demonstrating the futility of wage-cuts as a cure for unemployment, Dennis Robertson characterizing the world depression as "a gloom followed by a bump" and Keynes reading a cut version of his memoir of Malthus. Once, when someone remarked that Rexford Tugwell (a member of Roosevelt's Brains Trust) was on a razor's edge, Keynes interjected like a flash, "Then there will soon be two Tugwells." Later, when Bryan Hopkin and I had lunch with him early in 1935, he explained his method of writing: "I don't really start" he said, "until I get my proofs back from the printer. Then I can begin serious writing."

At Glasgow, even in the depths of the depression, student political activity was minimal. This was not so in Cambridge where student interest in politics was much livelier. There were demonstrations, attacked by quasi-Fascists, in which I took part. After the outbreak of the Spanish Civil War, several of my contemporaries went out to fight and at least two were killed. At Trinity, in my final year, I had rooms immediately above Anthony Blunt, the art critic who spied for Russia, but I never exchanged a word with him. I also knew Philby well at a time when he showed no particular interest in politics. Many undergraduates at that time despaired of seeing an economic recovery under the existing regime and were sympathetic to Marxist ideas, but the graduate students found enough to excite them in the intellectual revolutions in progress without seeking to launch a political revolution as well.

When I left Cambridge, Dennis Robertson told me that high theory was not my forte and advised me to write a book on the building industry. I accepted Dennis's verdict but was unpersuaded that I had nothing to contribute to theory of a more policy-oriented kind. It seemed to me then, and seems to me still, that however powerful and intoxicating high theory may be as an application of

¹ *Puella nigensis ridebat
Quem tigris in tergo vehebat
Externa profecta, interno revecta
Sed risus cum tigre manebat.*

There was a young lady of Niger
Who went for a ride on a tiger
They came back from the ride with the lady inside
And a smile on the face of the tiger.

logic to hypothetical postulates it has to be mixed with a grasp of what goes on in the real world, a familiarity with the magnitudes and institutions, a sense of how things change and why. The gin of mathematics has to be mixed with the tonic of history.

I had at that time in my mind two illustrations of this thesis and the war added a third. The first related to capital theory. Much of what I read treated capital as an undifferentiated aggregate that yielded a flow of income and profit that had to be explained and justified. But for some purposes it made a great deal of difference what concrete form capital took: housing, public utilities, machine tools, inventories and so on. While some writers clearly had industrial capital in mind, this was a limited proportion of the total and reacted differently to economic change from other forms of capital. In the early years of this century, for example, when foreign investment was at its peak, most forms of domestic capital formation shrank correspondingly but industrial investment continued to expand. It was housing and public utilities that suffered from the diversion of savings into foreign investment. Power, transport and housing made claims on capital far in excess of manufacturing industry, and foreign investment on occasion was larger than any of these. Whatever generalizations could be made in relation to the aggregate – and many of them seemed to me very doubtful – it was necessary to supplement them with a clear understanding of the behaviour of the constituent parts.

A second illustration related to international capital flows. I found it a little strange that the classical theory of international trade assumed complete immobility of the factors of production when economists spent a good deal of time trying to explain what governed emigration of labour and foreign investment. The Cambridge economists had engaged in much theoretical analysis of the likely effect of reparations payments on the terms of trade: but it seemed to me that they overlooked one important effect that stood out in the German case. The movement of capital in one direction might be associated with or even bring about a movement in the reverse direction. Payment of reparations, if on a sufficient scale, could give rise to a shortage of capital in the paying country, drive up interest rates and attract capital from abroad. It was not possible to consider the matter exclusively in terms of the current account.

Apart from this, it was a mistake to assume that reparations payments and foreign investment could be analysed in the same

terms. Foreign investment, unlike reparations, responded to profit opportunities and these in turn were related to favourable movements in prices. Any change in the terms of trade, therefore, might precede investment, instead of being a consequence of it. This obviously applied to direct foreign investment in exportable products. In addition, countries like Canada and Australia were more likely to borrow abroad, for example for railway construction, when their agricultural exports were obtaining high prices, than when they were facing difficult market conditions.

A third illustration followed war-time contact with the aircraft industry. I had experience day by day of the problems of development in an industry at the frontiers of technology where rapid advance was all-important to eventual victory. In the competition in the air, success depended critically on whether designs took the fullest advantage of the latest technology. One could not help reflecting that this might hold true also in peace-time in large sectors of industry. I had not at that stage read Schumpeter, but I could see the importance of technical change in the competitive process and, more to the point, in economic growth. I was less inclined than Schumpeter to link innovation with unusual entrepreneurial gifts. I saw it more as a commercial process that drew on new knowledge, and might involve investment to produce new knowledge, but was much like other investment using existing knowledge. Developing a new process or product did not seem fundamentally different from exploring for oil.

It was only later that I came to distinguish knowledge from knowhow and recognize that while technology might draw on scientific knowledge, which is almost invariably public knowledge capable of precise transmission, it is not itself a form of knowledge but rather capacity to produce certain results. There may be a recipe, but the work of a good cook is not adequately conveyed in a recipe. As became very clear after the war in attempts by British firms to take advantage of German technology, there was a great deal more to profiting from somebody else's technology than borrowing the prescription. I recall how one firm in Yorkshire trying to adopt a German process was obliged to bring over German engineers to help it before it had any success; and that kind of experience is by no means unusual.

Observing the pace of change in the average industry, it struck me that with technical change the return to new capital investment

would dwindle rapidly. It was as a carrier of technical change – not the only carrier but the most important – that capital sustained its usefulness. I developed the point in a paper for an International Economic Association conference on “Economic Progress” in 1953 but the proceedings of the conference had only a limited circulation until re-issued in 1957.² Although I did not use the expression “the residual”, I did point out that if the annual additions to labour and capital were assigned normal earnings under static conditions they would account for much less than the growth in GNP. I don't suppose that I was the first to point this out. Abramovitz, who reached a similar conclusion about the same time, attributes priority to Tinbergen.

In later years the significance of the point seemed to me to be frequently overlooked by economists who dwelt on the virtues of higher investment *per se*, as if that alone governed the growth of GNP. It was, as a rule, not the capital but the technical change that went with it that counted; and there was much technical change that could be incorporated with little effort in existing assets, without much new investment, as well as other kinds of change (in organization, redesign etc.) that were highly productive and required little or no capital. Of course, where countries lacked the infrastructure of an industrial society there might be an urgent need for more capital as such. But even then it was often a psychological infrastructure of skills, habits and responses – what Abramovitz calls “social capability” – that was the major deficiency. In industrial countries, too, psychological factors determined how technical change was used and absorbed: commercial judgment of what to select or reject, how to secure acceptance by the workforce and in the marketplace, what prior tests and training to provide, how rapidly to proceed, and so on, could be just as important as the change itself.

If what lies behind a steadily rising standard of living is in the main advances in technique, then economists whose field of study is the Wealth of Nations can hardly refrain from examining what makes for more rapid technical progress. When I was a student no one mentioned technical progress in courses in economics except perhaps as a source of unemployment. One of the major changes in the subject in my lifetime is that technology is no longer left aside by

² *Economic Progress*, Proceedings of a Conference at Santa Margherita; first edition 1955 edited by LEON DUPRIEZ; second edition 1987 edited by E.A.G. ROBINSON (London and Basingstoke: Macmillan).

economists as not their affair. Instead it is studied as of critical importance to economic growth and responding to market forces: responding to a limited extent even at the stage of invention and inevitably and evidently at the stage of introduction to the market.

Although it is not a point that occurred to me at the time, there is a parallel with the treatment by economists of what goes on inside the firm as lying more or less outside their subject while the interaction between firms and the market is very much the business of economists. Yet when one reflects that all economic activity might be organized within a single organization, presumably state-run, and that the market is fundamentally an instrument of decentralization into a large number of units, it is hard to see why economists give so much more attention to the strengths and weaknesses of the market than they give to the strengths and weaknesses of alternative patterns of economic organization.

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I had been lucky in ever getting to Cambridge and I was lucky again in returning in 1935 to a full lectureship in Glasgow the moment my thesis was submitted. I lectured episodically to University classes, regularly to agricultural students at the West of Scotland Agricultural College and four times a week to accountants. The latter seemed to me to need a simplified course in economics that could be readily absorbed when they arrived at 5.15 p.m. after a day's work. I therefore prepared a summary of what I proposed to say on a single sheet of foolscap and handed them out at the end of each lecture, charging a penny a copy, and admonishing them to listen carefully and take no notes. After I had been doing this for some time, I was approached by the Glasgow School of Accountancy with the proposition that if I would prepare a textbook they would undertake to buy 2000 copies a year. When I explained that I needed any spare time I had to prepare my thesis for publication they pointed out, to my amusement, that I already had a textbook in the form of forty sheets of foolscap. What moved me to go ahead was a sight of the textbook they were using, which appalled me as both unreadable and full of mistakes.

I had nearly finished when war broke out in 1939 and I put away the draft. But in 1943 I managed to find time for a final chapter. I had made arrangements for publication with a firm which I mistakenly took to be Thornton Butterworth but which turned out to be Butterworth, a legal and medical publisher whose chairman aimed to make his firm the

British McGraw Hill and had accepted my textbook as a first step in that direction. Unfortunately he died during the war and when I called in 1943 I was assured by the person who had taken over responsibility (newly arrived from India) that "no one will want to read economics after the war." However, instead of the 2000 copies they proposed to print, sales ultimately reached well over 100,000 and the book is still in print nearly fifty years later.³

My experience of lecturing in Glasgow gave me a more rounded training in economics. At Cambridge what we argued about was mainly employment theory and macroeconomics. Now I had to work out for myself the counterpart in microeconomics. I doubt that there was anything original in the textbook except in exposition and phrases such as "marginal preference." But it was the first post-Keynesian elementary textbook if James Meade's *Introduction to Economic Analysis and Policy* (1936) is left out of account and it was widely used by bankers, accountants and university students.

By 1939 my education in economics was still far from complete. I had yet another piece of luck when Austin Robinson rang me up at Christmas to invite me to join him and John Jewkes as part of the staff in the War Cabinet Offices of what was called the Stamp Survey – a small group consisting of Lord Stamp, Hubert Henderson and Henry Clay – that had been asked shortly before the outbreak of war to review the government's economic and financial plans for the conduct of the war. I spent the next eighteen months as a member of what by the time I left had become the Economic Section of the Cabinet Office, expanded to include many of the most distinguished economists of my generation, of whom two (James Meade and Dick Stone) subsequently became Nobel Laureates for work begun in the Economic Section,⁴ while another (Harold Wilson) became Prime Minister.

There could be no better way of studying economic anatomy. We engaged in what was elementary input-output analysis, taking to bits the structure of a war economy and assembling the statistics necessary for its operation. My job centred on imports, and on the shipping, transport and storage they required. But of course I was involved in all the problems of a war economy that occupied my colleagues, especially Ely Devons and Norman Chester. I took part in an inquiry into port congestion in Liverpool, another on warehousing

³ *Introduction to Economics* (London: Butterworth), first edition 1944; sixth edition (with PETER SINCLAIR) 1982.

⁴ I wrote an account of the Economic Section to 1961 with NITA WATTS in *The Economic Section 1939-1961: A Study in Economic Advising* (London: Routledge, 1989).

and storage facilities and a third on the scope for a planning unit in the Ministry of Aircraft Production. The second of these took me later into the Control of Factory and Storage Premises in the Board of Trade for about six months in 1941 where I was engaged in trying to build up a reserve of premises for blitzed factories to move to; and the third led ultimately to a transfer to the newly established planning directorate in the Ministry of Aircraft Production at the end of 1941. There I learned at first hand what planning is really like in a Ministry co-ordinating the efforts of nearly two million workers and producing technologically advanced products for an intensely competitive market.

The planning staff included seven future professors of economics and formed a kind of island of analysts in a sea of engineers, most of whom were at home in the specifics and lost in the general. It was interesting to see how economic activity was co-ordinated in a world in which market mechanisms had been superseded by officialdom. I had seen planning in the large from the viewpoint of the Cabinet Office for example, when drawing up an import programme. Now I spent four years in contact with industrial planning in detail. The experience might yield little by way of economic theory: but it added abundantly to my grasp of economic reality.⁵

When I was trying to make up my mind what to do after the war, Geoffrey Crowther persuaded me to join the staff of *The Economist*. I thought that I had committed myself firmly to the private sector. But in October 1945 I found myself in Berlin as Treasury representative in the quadripartite negotiations on reparations and the future level of German industry. This was a surrealist kind of planning with four countries trying to frame a plan for the fifth, disagreeing on almost every item and trying to resolve their differences by adding up the figures submitted by each country and dividing by four. I have written a personal account of that experience in *A Country to Play With*.⁶ The net effect on German industry was negligible but the protracted negotiations did give time for American public opinion to swing round to a more balanced view of what was at stake.

In the spring of 1946 I was at last able to join *The Economist*: but not for long. I spent a good part of my time writing a report on

⁵ For an account of the experience see *Planning in War-time: Aircraft Production in Britain, Germany and the USA* (London and Basingstoke: Macmillan, 1991).

⁶ *A Country to Play With: Level of Industry Negotiations in Berlin 1945-46* (Gerrards Cross: Colin Smythe, 1987). See also my *The Price of War: British Policy on German Reparations* (Oxford: Blackwell, 1986).

the wool textile industry and, when pressed to rejoin government service as Economic Adviser to the Board of Trade, decided that since I was already working half-time for government without pay I might as well work full-time for pay. I spent three years in the Board of Trade, first under Stafford Cripps and then under Harold Wilson. As Economic Adviser I was attached more closely to the Permanent Secretary than to the Minister and active as a kind of intelligence officer raising issues for top-level consideration and trying to improve policy co-ordination. I came to the conclusion at the end of 1948 that devaluation was inevitable and should take place within the next six months, before it became the subject of public debate. But my advice had no effect then and was equally ineffective when pressed on Harold Wilson in March 1949.⁷ It was good luck, not good advice, that produced increasing pressure on the pound that summer and paved the way for devaluation in September.

I had planned to return to academic life in 1950 but was pressed by Stafford Cripps to go to OEEC in Paris as Economic Adviser in succession to Donald MacDougall. I spent an eventful year in which the Schuman Plan was launched in May and the European Payments Union in July. What I chiefly remember was my involvement with Per Jacobsson (then at the Bank for International Settlements in Basle) in the German balance of payments crisis of 1950-51. After a visit to Frankfurt, we reported to the Managing Board of the European Payments Union recommending a credit of \$200 million to Germany, and the Board, under the chairmanship of Guido Carli, agreed on a credit of \$120 million. We had hoped to avoid a suspension by the Germans of the measures to liberalize trade which they had taken as part of an OEEC programme, but the credit proved a little too small and in February 1951, a month before the balance of payments showed unmistakable signs of improvement, import restrictions had to be introduced. It was my first experience of an international mission to assess a country's prospects and needs; and in retrospect I don't think that we did badly. My predictions of a rapid expansion in German exports were soon borne out and the credit to Germany was a useful investment in international goodwill. From that day to this Germany has never again had to be baled out in a balance of payments crisis.

⁷ I gave an account of the 1949 devaluation of sterling (and of the later 1967 devaluation) in CAIRNCROSS and EICHENGREEN, *Sterling in Decline* (Oxford: Blackwell, 1983).

When I moved to Glasgow in January 1951 to take up my appointment as Professor of Applied Economics I had been away from academic life for eleven years, all but six months of it in government, and had a very different conception of what economics was about. I was Director of one of the first economic research departments in the United Kingdom. In an industrial city like Glasgow it seemed to me natural to concentrate on labour and industry. But as I soon discovered, my staff were also engaged in teaching, and what research they undertook had to comply with their personal interests, not follow instructions from above. I succeeded, however, in uniting them in writing contributions to a study of the Scottish economy, re-writing a good deal myself in the process.⁸

One thing this brought home to me was that although Scottish industry seemed highly diversified in the sense that there was a wide range of industries and products, it was almost completely lacking in industries making durable consumer goods or making use of mass production methods. The output consisted largely of one-off engineering products such as ships and locomotives using skill to excess, with very little line production of the kind familiar to me from aircraft manufacture. As I later discovered, the Japanese applied to the building of ships the technique of pre-fabrication they learned from aircraft production. But this did not happen on the Clyde, partly because the yards were not well located for the purpose.

The Professor of Accountancy enlisted my help in 1952 in beginning courses in management and this became an important part of my Department's work. I reckoned that if we could attract to the University the young and livelier managers in the area we might not only teach them something but in time they could be of great help to us in assembling research material. The work done in Glasgow in the 1950s on the functioning of the labour market and on the problems of the shipbuilding industry were pioneering efforts which were of great value to me when I returned to Whitehall. As for the management courses, we were besieged with applications from young managers who came back year after year.

I had in the meantime managed to publish my Cambridge thesis eighteen years after its completion. I cut out the theoretical sections, retaining only the parts analysing the behaviour of home and foreign investment in the late 19th century and adding work I had done

⁸ *The Scottish Economy* (edited by A.K. Cairncross, Cambridge: Cambridge University Press, 1954).

subsequently on allied themes such as fluctuations in the building industry, the trends in internal migration from 1841 to 1911 and the relation between emigration and foreign investment.⁹ All this was in a sense economic history: but in the light it cast on the workings of the British (and the international) economy it was also an exercise in economic theory. It showed, for example, how in successive decades a boom in foreign investment tended to alternate with a boom in domestic investment; how emigration expanded as capital flowed abroad; how in the decades of high domestic investment the outflow of labour from the rural areas moved to the towns instead of swelling emigration; and how, to complete the circle, house-building flourished with this move to the towns and fell off with high emigration, so boosting domestic investment in the one case and releasing savings for investment abroad in the other.

The volume also included an application of Keynesian theory to investment in Canada between 1900 and 1913. Viner had done a classic analysis of the adjustment in the Canadian balance of trade to match the inflow of capital. But he had left out the repercussions on domestic investment in Canada. It was possible to show how the pace of expansion was set by a combination of investment and exports that had to be matched, at an appropriate level of income, by Canadian savings, capital imports and commodity imports. I had published the analysis in a rather badly translated article before the war in *Weltwirtschaftliches Archiv* and revised it slightly for the collection of essays. I think now that it needed more drastic revision, for example to take account of harvest fluctuations. But at the time I was delighted to be able to show how economic history could profit from Keynesian ideas.

In the mid 1950s my interests took a new turn when I was asked by the World Bank to launch the Economic Development Institute (EDI). I had already prepared a report to the Bank in 1954 suggesting a kind of staff college in Washington for officials from the less developed countries and was invited to act as the first Director. I was not, however, prepared to abandon my job in Glasgow, asked for two years' leave of absence and had to settle for eighteen months. John Adler and Bill Diamond helped me to organize a series of seminars for a course lasting six months and covering all aspects of de-

⁹ *Home and Foreign Investment 1870-1913* (Cambridge: Cambridge University Press, 1953).

velopment. I had hoped to find ample case studies in the Bank's experience to use as teaching material but in this I was disappointed. We did teach ourselves as much as was then known about development and evolved a few ideas of our own, but how much we got across to the participants in the course is hard to say. I think that we may have had a hand in starting Korea off in the right direction but it is hard to trace any effects of those early efforts in other countries.

What was apparent was that there was a great lack of material suitable for study by officials dealing with economic development in the countries represented. They took back with them piles of the papers and seminar outlines with which we had supplied them and the EDI maintained this tradition in later years. We probably attempted too much: but at least we avoided concentration on some limited formula such as discounted cash flow calculations (which were useless unless you knew how to establish the appropriate rate of discount, as few participants did). I had hoped for a series of texts such as one on development banks by Bill Diamond (which did get published)¹⁰ and one on fiscal policy in developing countries (which was crowded out). The most disappointing feature of the courses to me was how little those taking part absorbed of the American use of technology when I drew their attention to significant aspects in our factory visits.

Almost as soon as I was back in Glasgow I found myself on two government committees, both of which occupied much of the next two years. The Radcliffe Committee embarked on an exhaustive study of monetary control and reached conclusions that were thought to belittle the influence of monetary policy. Richard Sayers and I, as the two economists on the Committee, heard little evidence that disposed us to put a great deal of weight on monetary instruments unless they were used *à outrance* with far larger swings in interest rates than seemed desirable. But we did suggest that from time to time it might be necessary to make a deliberate change of gear by raising or lowering long-term rates of interest. Needless to say, we did not envisage circumstances in which prices might be rising at 20 per cent per annum; nor did we foresee the enormous scale on which liquid funds would slosh around the world. We might well have agreed that in highly inflationary conditions it was more appropriate to judge the

¹⁰ W. DIAMOND, *Development Banks* (Washington: World Bank, 1957).

tightness of monetary policy by what was happening to the quantity of money rather than its nominal price in the form of interest rates. But we should never have thought that manipulation of the stock of money should be given pride of place as a weapon against inflation. We should still, I think, regard monetary weapons as feeble and slow-acting if kept within limits and highly dangerous and damaging if used beyond those limits.

I found myself involved in the formulation of monetary policy in the 1960s when for nearly eight years I was either the government's chief economic adviser or head of the newly-created Government Economic Service. It was not a period in which monetary policy was allowed a leading role. To some extent – but far less at that time than theorists assume – monetary policy had to keep in step with monetary developments abroad. What chiefly surprised me in that decade was the eagerness with which academic economists advocated an immediate devaluation of the pound. It was very rare to find a warning that it might be quickly undone by more rapid inflation. Nowadays the fashionable view is that devaluation does no good at all and that therefore nothing is lost by abandoning the power to devalue and making use of a common currency. In my judgment this is just as mistaken, even now, as the passion for devaluation or a floating rate was in the past. It is one more example of how opinion in economics on major issues is often a matter of fashion, not careful judgment.

When I left the Treasury for Oxford at the beginning of 1969, I reckoned that I had spent half of my working life in government (including international government) and half in teaching and research. Even in academic life I found myself serving on one official committee after another: for example, on old age, crofting, anthrax and the monetary system. My experience in government service disposed me to look at economics as a consumer, interested only in those parts of economic theory on which I could draw in offering advice. I recognized that there were other parts that represented a great intellectual achievement and provided a standpoint from which to look out over the whole economic system. But my interest was in more workaday economics using the short chains of reasoning that Marshall recommended and never straying too far from the changing features of the real world. I distrusted elaborate models built on a succession of hypotheses and relying heavily on logic to make up for inadequate observation. It might be a necessary part of the training of economists to construct such models and build into them the rigour and precision associated with science. There is undoubtedly a place

for model-building in economics – all thinking rests on models of a kind – but it is a mistake to try to be more scientific than the subject matter allows.

When it comes to action, economic theory is only one input among many. It has to be combined with a grasp of political and administrative feasibility and above all has to take advantage of experience and observation, not rely wholly on logic. As has often been remarked, logic can be a way of going wrong with confidence.

One effect of life in government is to direct attention to new problems and to make one see old problems in a different light. After a spell in government, able theoreticians often find their interests moving in a new direction and value the contribution their experience has made to their understanding of how the economic system works. In my view it is important in any society that there should be some circulation between government and academic life (as well, I might add, as some interchange between business and both government and academic life). I tried, in my time in the Treasury, to develop the practice of bringing in academic economists for a spell of two years – the maximum for which their universities were prepared to release them – but this practice seems now to have been abandoned. Where economists have no contact with government, they easily misconceive the real problems and possibilities for action and become absorbed in theoretical developments of little practical value; while at the same time government may be deprived of useful new ideas and forego useful academic comment on economic problems in later years when economists have returned to their universities.

After my long pilgrimage from one job to another I find myself looking back more and more over the successive episodes in which I have taken part and seeking to draw lessons from each. I have already written five books and many articles, filling out what I remember with what I can now discover in the Public Record Office, in order to throw light on past policy and performance. Economic history for me is an economist's workshop in which one can observe how things worked in the past and may well work in the future. It is also the study of economic change and hangs out warnings against too ready an assumption that things will continue to work in the same way. It is not perhaps the best approach to pure theory but it strengthens the judgment in drawing on theory in the formulation of policy. It adds the tonic to the gin.

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