

Recollections of a Late Entrant *

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1. Preamble

The title of this paper can be explained by the fact that I did not enter the academic profession until I was 37, having graduated in 1950 at the age of 30. This means that my early life has to enter into the picture to account for what must seem nowadays an odd way of becoming an economist. I was born in Tunbridge Wells in the extreme south-east of England, which is regarded as the epitome of middle-class respectability. My father was a small shopkeeper, who sold luxury goods, expensive cameras.

I went to the local boys' grammar school. Looking back, I can see that the school gave me a very good, broad education. Everybody took Latin and two foreign languages, French and Spanish in my case; I even did some German in the year I spent in the sixth form just before I left school. The languages were my favourite subjects, and they, and two others, Italian and Russian, that I picked up during the war years, turned out to be extremely useful in my research much later. English, mostly in its American version, may be spoken and written throughout Europe, but for research on European financial institutions some knowledge of the local languages is essential: however common translation into English may be, it rarely covers the detailed sources necessary for institutional research. The other part of my future career was provided for by an introductory course in economics, also in my last year. When I visited the school in the 1970s, I was saddened to find that economics and the wide spread of languages had

□ Cambridge (Great Britain).

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

become victims of the new forms of education. Mathematics caused me no difficulty; it was at university that a bad teacher took away my confidence.

At the end of the fifth year at school there was inevitably a public examination, which I passed easily. This examination had one difference from the equivalent one since the war: if it was passed at a high enough grade, it conferred the right to enter a university (matriculation) without further qualifications, whereas today it is necessary to stay on for two years in the sixth form, and entrance to a university depends on grades attained in a further examination taken at the end of the two years. This matriculation was a lucky thing for me because it meant that when I applied to a university ten years later I already had all the formal qualifications needed.

I entered grammar school in 1929, so that the economic depression (that was the term used at the time before the beginning of the present craze for euphemisms) of the 1930s was the background to my school period. We boys were not particularly conscious of the fact, but it determined a number of elements in my career, the psychological effects being as great as the economic ones. The first economic effect was that it played havoc with my father's business so that a family that had enjoyed a reasonable lower middle-class standard of living became somewhat impoverished. The only difference that my parents allowed it to make to me was the abandonment of the annual seaside holiday, but it had the psychological effect on them, in common with very many other parents, of persuading them to steer their sons and (to a lesser extent in those days) their daughters into a safe job with a pension at the end. The main avenues of employment that met the criteria were in central government (the civil service), banking, insurance and local government, and thousands of school-leavers with high achievements entered those forms of employment.

Thus it was that in 1937 I applied to two banks and sat the examination for the civil service clerical class. The banks were quicker off the mark, and I had been working for three months as a very junior bank clerk before I was told that I had succeeded in the civil service examination; the delay was not surprising because there was a record number of 12,000 candidates for 2,000 posts that year. I accepted the offer from the civil service mainly because there was a post in my home town, but I have never regretted the four months I spent in a

bank branch: they gave me useful experience of what the mechanics of banking were like before the advent of the computer thirty years later. I shall never cease to marvel at the speed and accuracy with which practised bank clerks could add up long columns of figures and pinpoint the mistakes in my additions; what made the process so expensive was that it all had to be repeated several times as the paper moved round the system.

I did not find the clerical work boring. That was partly because I had the luck to avoid the more repetitive tasks but more because the many young people who entered the lower levels of the civil service in those days were almost all those who would have gone to university had they been born two or three decades later. They were congenial companions, both during and after work.

The other effect of the recession was political. The level of unemployment and the poverty of the unemployed, coupled with direct political action in the form of 'hunger marches' and other demonstrations, made a great impression on many people. Most of the action was in the industrial areas of the north of England and Scotland, and we saw little unemployment and political action in Tunbridge Wells. The political ideas spread, however, far beyond the areas directly affected, and I was initiated into socialism, a political and economic ideal that has never left me despite the apparent overwhelming victory of capitalism since the late 1980s.

Then came the war in 1939. I was waiting to be called up when I saw a newspaper advertisement recruiting linguists for the Intelligence Corps. I was interviewed and accepted, and then there was a wait of several months before I was called up. I had to do my basic training, but to somebody who had learnt to march, drill and shoot at school in the officers' training corps this presented no terrors. Armies all over the world give a good welcome to recruits who can perform the basic drill well and who salute their officers smartly. I had no trouble and had quite a comfortable life. I saw no front-line action, and I was probably much safer than those who stayed at home.

After six months I was posted to the Intelligence Corps depot for further training. The general job of the Corps was security policing in the field, which involved travelling by motor cycle; after a week's course it was recognised by both sides that I should never make a competent motor cyclist. The Army therefore posted me to specialist work. The nature of this specialist work has been revealed

in many television programmes in recent years, including facts that we were not allowed to know at the time, but the dreaded Official Secrets Act still hangs over my head, and it is better that I give no details. Suffice to say that the work I did was largely clerical with enough elementary detective work to make it interesting. As in the civil service, I found that my colleagues were very congenial, most even of the non-commissioned ranks being graduates or halfway through a university course when they were called up. After a further year in England I was posted overseas and spent about a year altogether in Cairo, Algeria and Tunisia before finishing off the war with over two years in Italy.

The period overseas was interesting from the linguistic point of view. The voyage from Britain to the Middle East, via South Africa, in 1942 took six weeks in convoy, and a group of seven or eight of us whiled away the time by learning Russian. (This was not a political decision but resulted from a false rumour that we were going to the Caucasus to help the Red Army.) There were plenty of opportunities to speak French in the Middle East, and in Italy I found that knowledge of Latin and two modern Latin languages makes adding a third very easy.

The war ended in due course, and I returned to England twice in quick succession, first for a six-week leave that coincided with the 1945 general election, when I met my future wife, Pat, and a few weeks later on home posting. There followed a period of working in an office in England waiting for demobilisation, and we decided to get married. After three months of demobilisation leave I returned to my old job in the civil service and passed the examination for the next higher grade, which meant working in London, some 35 miles away.

At that stage I took the only big and important gamble of my sheltered life when some friends suggested that I should apply to go to the London School of Economics, reflecting the current belief that it was a 'red university'. When Pat and I discussed it, she backed me wholeheartedly in going ahead, although it meant that she would have the burden of coping with our family, which became three after a few years, largely on her own at a time when washing machines were far too expensive to buy on a student grant and disposable nappies had not even been invented. (It turned out that this grant, which included child allowances, was no lower than my civil service salary.) So much by way of preamble to my recollections as an economist.

2. London School of Economics

The first-year course at LSE was a very broad one in those days. There was some choice, but the main elements were basic economics, economic history, economic statistics, politics, geography and two languages, which were generally examined by translation of several passages with the aid of a dictionary, although one of the languages could be taken as a full paper. As my mathematics had been quite good at school, I planned to specialise in statistics in later years, which entailed taking mathematics in the first year. This was nearly my undoing because the young lecturer, faced with a class of thirty or forty students the majority of whom had left school between five and ten years previously, started straight in by writing a succession of equations on the blackboard without any indication of what part of mathematics was under consideration. This was the one part of the first-year course at that time that required written work each week, and I spent a miserable six weeks trying to cope. I gave up and was allowed to switch to French, which was an easy option for me, but the episode helps to explain my somewhat jaundiced attitude towards the spread of econometrics and the ubiquity of equations in *Economic Journal* articles. (Baumol's phrase about articles 'pock-marked' with equations is apposite.) I thus became an economist through the route of having been a statistician *manqué*, and I have never regretted the change.

LSE was an interesting place at the time. Among those who had already become famous were Robbins, Hayek, Meade, Sayers and Laski; the only one of these who lectured to first-year students was Laski, although I had seminars with Robbins and Hayek in the final year. These two and other members of the Department of Economics were strongly conservative; the epithet of 'red university' could hardly be applied to that part of LSE. The strange thing about LSE economics at that time was that one hardly dared to mention the name of Keynes; those of us who were interested in his work had to form small groups and discuss Keynesian economics on our own. The name of Karl Marx was mentioned only to pour scorn on his contribution to the development of economic thought, and again a small group of us held our own course on Marxist economics. Apart from these aberrations the course was interesting and contained some ele-

ments that have completely disappeared from modern economics courses, such as lectures on the history of economic thought.

I took my 'finals' in 1950 and began looking for a job because I had been forced to resign from the civil service before I could go to LSE. This was not easy since my political opinions were well known to those who would have acted as referees: of my tutors in the first two years one had apparently reported that my political opinions showed up in my essays and the other that they did not show up, remarks that were equally damning. The only opportunity seemed to be teaching, and I signed up for a postgraduate teaching diploma without much enthusiasm because my real interest was research. At that point I was offered a job in the Soviet news agency Tass, which I accepted.

This turned out to be a journalistic job much like any other, the political side showing up only in the choice of events that we reported, mostly left-wing meetings and demonstrations. The first two years were spent on shift work, with a summary of the day's newspapers on the night shift. Then my training in economics was recognised by my transfer to the commercial department, whose main task was to report on the world markets for commodities in which the USSR was interested. In addition to reporting of prices and market conditions there were also lengthy reports, and I found myself supervising research by others on such recondite commodities as turpentine and cork and organising monthly statistics of international trade in a number of commodities. I also wrote research reports about the computers available at the time and on commodity futures markets. I should add in fairness to my employers that there was absolutely no undercover work; we said we were from Tass, and very few people refused to answer our commercial questions. My research career could be said to have begun.

The turning point in my career was the putting down by the Soviet army in 1956 of the 'revolt' in Hungary, which was a cruel blow to the somewhat naïve views of most people with left-wing convictions. A few moved immediately over to the right wing, but most of us kept our convictions and dissociated ourselves from the Soviet Union. With a family of three children, I lacked the courage to resign immediately, but I started searching for a job. I had several interviews, but favourable interviews were always followed by curt rejection letters. Then my luck changed.

3. Department of Applied Economics, University of Cambridge

I saw an advertisement for research officer posts in the Department of Applied Economics (DAE) in the University of Cambridge; I applied and was accepted after interview by Brian Reddaway, who was the Director at the time, Richard Stone, Austin Robinson, Aubrey Silberston and some others. The research that I was allotted was the compilation of national and sector balance sheets of the United Kingdom, a formidable task for one man with one graduate assistant, who changed from time to time, and other non-graduate helpers for particular parts of the work. It had been attempted by others in the past, starting with Gregory King in the late seventeenth century and continuing with Lord Stamp, Giffen and John Hicks. In 1960, three years after I started work, Victor Morgan published a small book of the statistics of property ownership in Britain, and Raymond Goldsmith was known to be compiling national balance sheets for the United States, published in 1963. Whereas the earlier workers in Britain had been concerned with figures for the total wealth of the economy, there was no systematic and detailed matrix of intersector indebtedness and no consistent valuation of items.

It seemed to me that three steps were necessary to produce balance sheet statistics that would fit in with the existing national income and financial transaction accounts:

- 1) conceptual work on the structure of the tables and the form of consistent valuation;
- 2) sample surveys of those sectors and subsectors for whose assets and liabilities no reliable statistics were published;
- 3) estimation of particular sets of figures by as many methods as seemed reasonable on the grounds that comparisons of the differing estimates for the same figures would yield useful information.

The question of valuation seemed easy to me: the only consistent valuation was at market value or the closest approximation to it. While everybody agreed that market value was the correct valuation for equities and tangible assets, both Goldsmith and Morgan used nominal values for bonds, and two important economists from the IMF spent a lot of time trying to persuade Richard Stone, who had by

then taken over general guidance of the project from Brian Reddaway, that I was wrong; we stuck to our guns. There were many surveys, but the most important was the estimation from the share registers of a sample of listed companies of the distribution of the ownership of ordinary shares; the key point was the inclusion in the sample within the share register of all the very large holdings, which normally accounted for around 60 per cent of the total value and sometimes more.

All this work took nine years to complete, and the book describing the research and giving all the estimates, *The Wealth of the Nation*, came out in 1967. That was not the end of the story. The first consequence was that balance sheets were included for the first time in the United Nations *System of National Accounts*, for which Richard Stone was the main consultant; the form was very close to that of the DAE project. The second was that the Central Statistical Office and the Bank of England took over my work after a gap of a few years, so that Britain is now one of the very few countries that include national and sector balance sheets in their national accounts.

I had now experienced the working of two very different methods of teaching undergraduates, one at the LSE, which relied on lectures and did not move to seminars until the third year, and the other in Cambridge, in which the most important method is supervision, in which a small group of undergraduates, normally two or three, discuss an essay that they have written with their supervisor. I much prefer the Cambridge system. The key in Cambridge is the frequent writing and discussion of essays, each student often having two or more essays a week on different parts of the syllabus. One other personal factor that made Cambridge so endearing to me was the view from my room in the DAE of King's College Chapel over the trees.

4. University College of North Wales, Bangor

The Cambridge Faculty of Economics made no requirement that academic staff should take or have taken a PhD, and nearly all the distinguished economists there in the 1960s were plain 'Mr'. I regarded my ten-year research on the national balance sheet as an adequate apprenticeship; apparently other universities took the same view because

shortly after the publication of the book on that study I received two invitations to apply for chairs. So it was that in January 1969 I became professor of economics at the University College of North Wales, which is now called University of Wales, Bangor. This was in line with the record of the DAE at a time of general expansion in British university economics departments: a high proportion of my senior colleagues in the DAE became professors at other universities.

The Department of Economics that I found in Bangor was quite small, with a teaching staff of only six and a small number of students. I had been told by the Principal that my task was to develop and expand the department, and I felt that the obtaining of research grants would be the key. One fairly large grant was obtained in the first year on regional income multipliers and another a few years later on constructing 'national' accounts for Wales. One or two of the existing teaching staff took a part in guiding the projects, and we found that we could attract a very high calibre of research worker, two of whom went on to chairs in other universities. When I stopped counting about fifteen years later, I found that altogether ten people from Bangor (lecturers, research workers and research students) had been appointed as full professors in British and overseas universities in that short period; it was roughly the same as the figure achieved by the DAE.

One of the reasons for the success of the Cambridge DAE is that it provides a banner, a brand name, under which a fairly wide range of research can be conducted and become well known. In the hope that the same could be achieved at Bangor, in 1973 two research institutes were established within the economics department: the Institute of Economic Research for the continuing regional economics projects and the Institute of European Finance for my own line of research, on which I felt free to concentrate more of my time once the regional research had been securely established. Neither of these received any funds from the university; they depended entirely on research grants.

My interest in financial economics (a term that we used long before those studying company finance and portfolio theory sought to appropriate it for a branch of their subject) had been stimulated much earlier, and it was cemented by the invitation to take over the one course of banking lectures in Cambridge. In the last few years there, with the support of Richard Stone and Aubrey Silberston, I started

writing the first edition of *The British Financial System* (1973), which was finished in Bangor.

Early on we began to teach financial economics in Bangor in the form of a Masters degree. This attracted students from all the world; from memory the countries included Italy and Belgium in Europe and Nigeria, Nepal, Thailand, Chile and the West Indies in the rest of the world. For a while the Bank of England sent three or four of its younger members of staff each year; the person whose signature now appears on English banknotes was one of them. Not only did we learn a lot about the banking system of other countries from the dissertations that they wrote for the Masters degree or the PhD, but several of them stayed with the Institute of European Finance on its research projects.

Financial economics became the focus of my own research, and it acquired a European and international slant, as the title 'Institute of European Finance' suggests. This was largely because I was invited to write research and consultancy reports by various European organisations. These provided the stimulus for me to develop my study of institutional theory in the financial field in a way that cannot be done with a study of a single country and to increase my knowledge of the structure and operation of financial systems in Western Europe.

5. European research

In the early 1970s I was invited by the Organisation of Economic Co-operation and Development (OECD) in Paris to become consultant for the study of housing finance, which produced two reports, one of which (*Flexibility in Housing Finance*, 1975) I wrote and to the other of which I contributed a chapter on British building societies. This was followed by a study of bank balance sheets and income and expenditure accounts for those member countries whose central banks were willing to supply the raw material; it was published as *Costs and Margins in Banking* (1980). The last report for OECD was on the economics of information technology in banking (*Banking and Electronic Fund Transfers*, 1983); the interest of this to me was the introduction to an entirely new aspect of banking, which was at that time rarely

mentioned in banking books or journals unless they were expressly technological.

All these studies were under the aegis of the OECD Committee on Financial Markets, and it was a new experience to write reports any word of which might be subject to adverse comment from a committee of over twenty-country representatives. I was lucky, probably because my reports happened to be outside the area of expertise of the Committee members. I was told that my colleagues whose reports dealt with monetary policy had a much rougher time; indeed, the Committee spent far more time on the single chapter that dealt with the impact of electronic fund transfers on monetary policy than on any other chapter of the last report, and I had to sit back and listen while the representatives of the central banks of two large countries disagreed strongly on some points.

The research for OECD was followed by a ten-year period of research studies of the impact of developments in Western Europe for Spanish institutions of different kinds: for Banco de Vizcaya on commercial banks, for the Confederation of Spanish Savings Banks on savings banks and for Banco Exterior on public banks. This was a very pleasant period, in which my knowledge of languages could be used to the full and in which I visited many European countries, especially Italy and Spain, both for research and to give lectures to several kinds of audience. I retired from my chair in 1983 and returned to Cambridge two years later, but it has made very little difference to my life, except that I no longer have to teach students at regular intervals, to carry an ever-increasing load of administration or to spend hours in committees.

I continued to do my research and to visit European countries, but the view through my window in Cambridge has changed from King's College Chapel to my own garden. For a few years past I have not conducted specific lines of research but tried to draft a second edition of *The British Financial System*, with the addition of the words *and Global Finance* to the title to show awareness of how much financial systems have changed in the 25 years since the first edition was published. The work is slow because I have had bouts of back pain and generally impaired mobility, but I must finish it in one more year to avoid having to go through it all again to cover the many changes that have occurred during the course of the writing. It will give me an opportunity to summarise and fit into context the few theoretical ad-

vances that I can claim to have made. There are two that I may mention briefly here.

The first was developed in a study for the Banco de Vizcaya of the desirability of mergers among the seven largest Spanish banks; the title was *Mergers and the Role of Large Banks* (1987). This advanced the concept of a group of large banks that could be called 'core banks'. It is a vague concept, but it has undoubted reality in most countries. It can be defined as the group of banks that are too big to be allowed to fail and as those treated by the general public as household names. Banking economists occasionally mention such a group of large banks, but they generally see the development of the group as dependent on forces within the banking system. I saw the process of creating the core group of banks as resulting from the sharp growth in the size of the largest industrial customers and in the size of the loans demanded from banks from the last part of the nineteenth century as heavy industry in the form of railways, steel, electricity and gas began to dominate. The same line of argument can be used to explain the overall changes in the structure of financial systems from bank-dominated to market-oriented and globalised. It was not the banks that strove to become global but their large customers that had become global and demanded global financial services. Perhaps this is what the eminent Cambridge economist with a Keynesian background had in mind when he told me I was wasting my time studying banks and other financial institutions because it was well known that financial institutions adjusted automatically to the demands on them!

The second theoretical point also came out of the same study, although it still needs some development; it concerns the economies of scale, which are used as a powerful argument by banks wishing to merge. The proposition that the cost ratios (to total assets or total income) of banks decrease with size has always filled me with doubt, particularly since I remember that all my early reading on the subject emphasised the diseconomies of administrative costs as an institution becomes larger. My doubts were increased by the publication of David Humphrey's (1987) article on a study of some 13,000 US banks, which showed there was a greater variation of cost ratios within size classes than between them. The academic discussion of economies of scale stopped abruptly soon after that, and attention was diverted to the concept of overall efficiency as measured by 'efficient frontiers'. Operating efficiency should still have attention, it seems to

me; the banks themselves continue to lay stress on their cost-income ratios. There is no space to develop the argument here, but there are two points worth making. The first is that one area of a bank's operations to which economies of scale undoubtedly apply is that of processing cheque clearing and administering mortgage and other loans; when bank operations are contracted out ('outsourced'), they are nearly always of this type. The second point is that, at best, size in such operations is accompanied by *potential* economies of scale; it requires managerial action and probably investment in computer systems to turn them into realised economies.

In the early days of the European research I visited many of the professors in Continental universities who were interested in much the same aspects of banking and finance as I was. None of them apparently had any contact with the others in the group, and I suggested to them that we should all meet together. The first meeting took place in 1982 at Würzburg in Bavaria, and we decided to found a club and to call it the Wolpertinger Club (after a mythical beast that is reputed to live mainly in Bavaria). The Club also has another name, used on formal occasions and for obtaining travel funds from members' universities: European Association of University Teachers in Banking and Finance. We meet once a year in a different country to read papers; the only administration is done by the host of each meeting in turn, and there are no officers or subscriptions. The emphasis is on the enduring friendships that have been made between members, but the Club also serves as a most effective network for combined research and interchanges of staff and students.

6. Some final reflections

When I left school and entered the civil service, I had no thought of going to university and becoming a professor. Looking back it all seems to be the result of a succession of lucky chances without the vestige of a plan, but I expect many of the other authors in the series think much the same about their careers, at least in part. I do not feel that the first part of my life, including the six years of army service, was wasted: I have found it useful to have experienced life and work in environments other than that of a university. By working on dur-

ing my retirement I have managed to achieve something like the norm for the working life of an academic.

One decision that I definitely do not regret is that of retiring four years before the maximum age of 67. It happened because of the first really serious financial pressure imposed on British universities by the government, and we were made an 'offer we could not refuse' to volunteer for early retirement. I am glad that I am not still in a university. From what my friends who are still soldiering on tell me it seems that bureaucracy has multiplied many times; it still puzzles me why the application of the free-market doctrine should entail an increase in bureaucracy, but it does. The main weapon is the use of extremely crude measures of teaching and research performance. My friends seem to be constantly under pressure, and academic life no longer protects them from all the dubious effects of the performance fetish; rather it takes up an increasing part of their working lives, leaving all too little time for research, particularly in the form of lengthy projects or writing books.

Not all is gloom and doom, however. When I began my academic career, the study of the industrial economics of financial institutions was barely respectable as a separate topic: it was regarded as a part of monetary theory, with monetary policy as the main applied area – witness the large number of textbooks entitled *Money and Banking*. Now the situation is quite different. The number of banking degrees and departments has grown, and many PhDs are written on themes concerning the working of financial institutions of all kinds. The increased academic interest in the topic has come from the demand side as usual since banking and finance have not only expanded as a part of the economic system but also become infinitely more complex. The expansion has been apparent in most countries of Western Europe, but nowhere more so than in Italy and Spain. I should like to believe that a minor role in the pattern of this increased interest has been played by that club of academic friends, the Wolpertinger Club.

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