Whither Now? *

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1. Introduction

I doubt whether I rank as an 'eminent economist'. I have become a leading figure in a limited number of special areas, notably central banking and monetary policy and, later on in my career, the microstructure of the foreign exchange market; but I have added nothing to the body of accepted theory, and my role as policy adviser, though frequently exciting and fulfilling, has mostly been at too junior a level to count as eminent. Nevertheless vanity affects us all; having been asked to contribute to this series, and thereby become a member of the elect, who am I to refuse on grounds of unworthiness?

I should, nevertheless, like to use my experiences in the three main phases of my career, learning at Cambridge(s), 1958-65, policy advice at the Bank of England, 1968-85, and teaching at the London School of Economics, 1966-68 and 1985-to date, as a springboard for discussing some ongoing issues in economics, to wit the treatment of expectations, the use and role of economists in the City, and the analysis of markets. And now that I have returned to an advisory role, as an independent external member of the newly-formed (1997) Monetary Policy Committee of the Bank of England, I shall end with a few comments on central bank autonomy.

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2. Biography (1936-58)

I probably come from a more privileged background than any previous contributor. My paternal grandmother was a Lehman, of the great New York Lehman family.[1] Besides the bankers and the bank, that family had the distinction of providing simultaneously one of the most famous Governors and Senators from New York (Herbert) and its Chief Justice (Irving). My paternal grandfather was a senior stock-broker on the New York Stock Exchange.

My father, Arthur Lehman Goodhart,[2] was sent by his parents to Trinity College, Cambridge, to study economics in 1912. At that time there were no economists at Trinity, and – as my father told it – he was advised *not* to go next door to Kings College to study under the young Keynes because his advisers at Trinity doubted whether Keynes was quite 'sound'. Anyhow, he turned instead to the study of law with a young Trinity don called Harry Hollond, and that was the start of his life-long academic career as a lawyer, specialising in jurisprudence.

My father returned to New York in 1914 to practice and then, after a spell in the Artillery in World War I and a more important period as a junior legal adviser in the US commission to Poland (1919) to investigate ethnic, essentially anti-semitic, problems there, which became the basis of his first book,[3] he returned to Cambridge on a more permanent basis becoming a law don himself at Corpus Christi College. From there, he moved his family to Oxford, where he had become professor of jurisprudence, in 1936 (the year of my birth), and became master of university college, 1951-1963.

Although my father's American family is jewish, many were not by that time strictly observant, and my father was not a religious believer. While at Cambridge he married an English girl, Cecily Carter, daughter of a Birmingham accountant, who was a staunch member of the Church of England, and brought up her three sons as such.

The Anglo-American theme was reinforced in World War II. As an outspoken opponent of Nazism, my father was on the Nazi black-

[1] The family figures prominently in Birmingham (1968); also see Nevins (1963) and Flade (1996).

[2] See *The Dictionary of National Biography* (1986, pp. 350-51).

[3] A.L. Goodhart (1920).

list. With many close New York relatives, my two elder brothers and I were evacuated to the USA, my brother William (aged 6) and me (aged 2) under the command of a Norland nanny, since my parents remained in Oxford. My mother had to be pointed out to me on the boat-train platform when we returned after the war was over.

Since American primary schools then taught no Latin, a major requirement for entry into British public (i.e. private fee-charging) schools, little French, and the 'wrong' history, my brother was scholastically several years 'behind' his English contemporaries, though in fact intellectually brilliant. Anyhow the only (fee-paying) British preparatory school that would accept him was the St Leonards branch of the (Oxford) Summerfields School, intended for those intellectually too weak to go to the main Oxford school. I was given the choice of staying at home with my parents as a day-boy at an (excellent) Oxford school, or accompanying my brother to the boarding school; since he was the only constant in an otherwise kaleidoscope world, I chose to go with him.

Intellectually challenged, or not, it was a lovely place, but in its sixty years of existence it had had but two scholarships to any school, and although there was some thought given to trying to prepare me to sit a scholarship exam, when the time came I was too far behind-hand.

My father, despite his academic career, was a worldly man, comfortable with, and interested to maintain, the power to influence events. He wished to pass that on to his children. So, he put us all down for Eton, the dominant English public-school. If you did not obtain a scholarship to College (as a King's Scholar, KS), you had to be accepted by an ordinary House Master (as an Oppidan). Owing to the war, my father had been late in doing so. The only reason that I was accepted was that I was then good at cricket, and the prospective house-master (Whitfield) wanted above all to win the house cricket cup. Alas, my eyesight then deteriorated rapidly, and I could not see fast bowling!

Whitfield's house was far from an academic milieu. One was encouraged to avoid being 'too clever by half', a serious failing in oppidan eyes. Nevertheless the teaching was excellent, and the streaming by ability meant that we were always stretched academically. However, the English education system specializes far too early, and I concentrated on history (plus languages) from the age of 16, giving up all the natural sciences and maths, before I had even started calculus!

As in many other fields of English life, Eton has been a major source of leading figures in economics (though no economics was taught there before the 1970s). Keynes and Dennis Robertson were both Etonians (KS), and in my own generation Richard Layard (KS) and Nick Dimsdale have made a mark. Someone should do a note on the contribution of Etonian economists.

When I went up to Cambridge in October 1957, after two years compulsory military National Service,[4] I put myself down to read economics. This was not because I wanted to become an economist then; instead it was part of my father's grand design for his three sons. My eldest brother, Sir Philip Goodhart, conservative member of Parliament, Beckenham (1957-1992), was already embarking on a political career; my elder brother, Sir William Goodhart, Q.C., was the cleverest of us three; my father saw him as a worthy successor to his own work in the (academic) legal field. But academics and politicians do not earn much money. My father had me typed to go into finance, probably as an investment banker, probably in New York, in the footsteps of many other members of my extended New York family, Altschuls, Lehmans, Loebs, Morgenthaus, etc.

I did not work very hard in my first year at Cambridge, and expected to get a moderately good exam class. To my astonishment, and delight, I got a 1st. That changed my life completely. I had never been top of anything before (except the Summerfields, St Leonards School which does not really count). Now I had found something which I could do. Moreover, economics was fun and a challenge because it seemed so unsure of itself (so bad). Despite the formal models, no one really knew, or knows now, what determines the level, or rate of growth, of most of the key economic variables. In all my previous education there had been one correct pronunciation, one correct date, one proper proof; and the main exercise all too often was just to learn how to regurgitate that. To find a subject wherein one's teachers admitted that there were several possible answers, and that *none* of those yet developed might be correct was profoundly liberating. I believed as a result that I might be able to contribute, and to do so in a socially worthwhile manner.

[4] During this Service, I was tangentially involved in the events of 1956, the Hungarian uprising and the Suez crisis. My battalion was responsible for running barracks to house those Hungarians who fled to the UK. During the Suez crisis, I was appointed Intelligence officer in a brigade to be formed to go in to Suez in a second wave. That wave was cancelled. In the meantime the Brigadier had asked me to go through his private safe and burn all the secret papers that would not be needed. Out of several hundred, I burnt all but three, an early grounding for my subsequent conviction that people (*not* just bureaucrats, see Section 5), grossly over-classify papers as confidential or secret when there is no need for that.

Despite the particularities of my earlier experiences, a comparison of these with those of other previous contributors reveals some common threads. First, there is my Jewish connection; why have Jews been so clearly predisposed to economics? This strikes me as a stylized fact, but one which most commentators purposefully overlook. Second (perhaps related also to Jewishness?), there is my feeling of not belonging to any group/religion/country in depth, but sharing a broad international (but essentially Atlantic) culture in general. Third, there is the common tendency to come to economics from an outside specialisation, there being two main streams, the first from maths and physics, the second from history, together with the other social sciences and humanities. The interaction between these two separate strands provides most of the personal creative tensions in the subject. Maths, without understanding of the institutional/social/political background, will just lead to empty formalism. Historical and institutional knowledge, without a hard theoretical core, will be wordy, vague and often wrong. I have always known that I needed more maths to become a really good, rounded economist, but time was always too short, my aptitude too slight, and too many other fascinating exercises to work on to undertake the necessary investment.

3. The Cambridges, 1957-65

The Cambridge (UK) economics faculty was dominated during these years by the triad of Kaldor, Kahn and Joan Robinson. Kahn seemed a reclusive, and somewhat sinister, figure (to me at least), who never lectured and had little contact with undergraduates, but who was supposed to be the *eminence grise* maintaining doctrinal purity. Joan Robinson was in her Chinese period, and she used to wear fantastically beautiful silk robes, reputedly given to her by Mao personally, in which she lectured. She was ferocious and strident in debate, but much more so with other academics, especially with the neoclassicals, than with undergraduates; if we were obviously making an effort, Joan would be really quite kindly to us (though I am glad that she was never my own supervisor). Nicky Kaldor was far the most approachable, and in my view the best economist of the three. He had a fount of original ideas (of admittedly varying quality), with enormous enthusiasm for all of them, and for economics in general. Nicky used to doze (feign sleep?) during seminars, and then come alive with a sharp, and usually apposite, interjection.

The ideological front maintained by these three dominated Cambridge; Piero Sraffa was a charming, but largely invisible,[5] figure in the Marshall Library; Austin Robinson appeared a minor, self-effacing attachment; James Meade was yet to be invited to come, and Dennis Robertson had just retired; though the dominant triad managed successfully to marginalize any serious competitive challenge from these latter two. Dennis invited me to tea, with his cats. I still remember my embarrassment when I immaturely suggested that his exchanges with Keynes over the General Theory must have been stimulating, and saw the expression of pain in his face.

The best lecturers, however, were the younger economists, and Cambridge had another trio of these, Michael Farrell, Frank Hahn and Robin Matthews. Of these, Robin Matthews gave the clearest lectures; Frank Hahn was the most technically advanced; but it was Michael Farrell who struck me as the most original. His early death was a great loss. Of course, Cambridge had a much larger faculty with experts in many other fields, e.g. Robin Marris in industrial economics and Dick Goodwin on trade cycles (though no one, after Dennis Robertson, really much good in monetary economics). At that time economic history formed an integral part of the tripos, and was generally taught exceedingly well.[6] By contrast, the statistics course was elementary and rudimentary; there was nothing recognizable as econometrics; any mathematics for economists or mathematical economics was just optional, and an option that few, and not me, took.

In any case lectures did not form the main basis for education at Cambridge; the lecture course/class system was unknown. The lecturers often did not set the exams. Many, probably most, lectures were poorly attended, the more so as the term went on.[7]

Instead the main form of instruction was via tutorials. This is enormously labour-intensive, representing a one-on-one, or a one-on-two, hourly meeting between tutor and undergraduate, at which the undergraduate reads out his essay and the tutor then commented. Sometimes the tutor would have read it in advance, and sometimes not. It is almost certainly too labour intensive to endure. Nevertheless at the time the selection, associated reading and writing of the weekly essay, formed the main work of the term. Tutors mostly came from the College at which you were. My first two tutors were, therefore, Trinity economists, Robert Neild and Maurice Dobb. Dobb was supposed to be a communist, but he was useless as a tutor, since he refused to criticise, take up any position and would barely even comment. He seemed rather a retired gentleman than a communist economist.[8] My best tutor, by far, was not from Trinity, but Michael Posner (from Pembroke College). Michael would often profess to know less about an essay subject than you, but would then, apparently guilelessly, slip in a couple of apparently 'simple-man' questions that would make one have to reconsider the whole subject from a new light.

In my final year, I was paired in tutorial with a student who had just moved over into economics from pure mathematics. His name was Jim Mirrlees. Jim and I got on well together, though our approaches and aptitudes were quite different. I recall being rather put out that our tutor in that year, David Champernowne, clearly preferred Mirrlees, despite his slight background then in economics and my two prior 1sts. Subsequent events demonstrated the acuity of Champernowne's preferences, with Sir James having received the Nobel Prize in 1997.

The main figures in the faculty also ran an evening seminar for undergraduates who had done best in the first (and second) year economics exam, usually about 12-15 undergraduates. Those asked to join in their second year would deliver a paper to this seminar in their

[5] Invisible to undergraduates. His great (1960) book, *Production of Commodities by Means of Commodities*, had, however, a major influence on the theoretical outlook of the Cambridge faculty at the time, but failed to make any significant breakthrough into the increasingly dominant school(s) of North American economists.

[6] Apart from one lecturer on US economic history whose views I so disliked that I learnt a lot from trying to think up mental refutations as he proceeded.

[7] By far the best lecturer of that period was not an economist, but Noel Annan who lectured on the great philosophers. He was the only social scientist who could fix his lecture hour at 9 a.m. and still command a sizeable, and sustained, audience.

[8] There was a well-known quip about a foreigner (reputedly Kalecki) who came to Trinity in search of the prototype English gentleman. Eventually he claimed to have found two, but one was Italian (Sraffa) and the other a communist (Dobb).

third year. The other undergraduates would draw lots, with the lottery determining the order in which you *had* to comment on the prior paper, if you drew a number at all. Faculty members attended, often in surprising numbers, but kept any brief comments right till the end.

While I have now forgotten the essay which won part share of the Adam Smith essay prize, I remember my paper to the Marshall Society rather well. It was a rendition of Shackle's theory[9] of how agents approached the uncertain future, with the potential surprise function, three dimensional graph (made by me out of multi-coloured plasticine), *et al.*

Shackle claimed that people would in practice concentrate their attention on a single focus-gain (or focus-loss) that might result from a decision, where the focus (gain/loss) was a function of the potential surprise of the outcome together with the intensity of anticipation of that outcome. Where the two variables interactively reach their maximum is the focus (loss/gain); people would then compare the focus gain with the focus loss and come to a decision. When the outcome is on an either/or basis (e.g. either I will catch new-style CJD from eating beef, or I will not: either my next plane trip will end in disaster, or it will not), this kind of approach still seems reasonable. Indeed, outcomes with intensely felt anticipations (as in the examples above) frequently induce much stronger behavioural reactions (airports became almost deserted after the Lockerbie disaster), than rational probability analysis would have suggested made any sense (after the BSE/CJD scare, beef consumption fell as sharply in Germany, where no cases of BSE had occurred, as in the UK!).

This approach, of trying to simplify the decision-making process to a comparison of two focus values, is less appropriate when the outcome can take a continuum of values, as is the case with most economic variables, e.g. prices and quantities. But, even so, Shackle would argue that the exercise of trying to build up an expected probability distribution would be excessively time consuming (and thus not utility maximising), especially when we are still uncertain of what confidence we can attach to our subjective probability distribution. There is an infinite regress of what probability we can attach to the probability we have attributed to an outcome.

[9] See for example Shackle (1949).

As in the Grossman/Stiglitz paradox, shortage of time and the costs of acquiring information make it rational to stop short of trying to incorporate all available information into our expectations' formation process. The simplest, and most obvious, way of economising on time and effort is to try to learn (e.g. about probabilities, outcomes, models, etc.) from others who may possess more, or better, information than oneself. The 'representative agent' paradigm may make computation and analysis easier, but is patently invalid for any analysis where expectations are important. While we do learn from our own, and others, mistakes, we learn much more from the arguments, ideas and behaviour of other people.

Given that people are heterogeneous and fallible, and that everyone is always learning, both from events and from everyone else, the concept that 'everyone' knows the true model of the world is ridiculous; no one knows the 'true' model. Even the logically more seductive idea that one should assume model-consistent expectations, i.e. that in setting out a model one should assume that all agents will behave as if that particular model was correct, is not only false in reality, but is also likely to lead to an underestimate of the extent to which learning by observation of others is likely to lead to 'herding' in behavioural response and, apparently irrational, sudden shifts in market behaviour, etc.

Whereas the minimalist form of rational expectations, i.e. that people will not persist indefinitely in making systematic errors, is mostly correct (you cannot fool all the people all the time), the more ambitious extensions of that theoretical approach, whether expressed in terms of the extent of information assimilated, or of common knowledge of a 'true' model, go too far. A preferable approach would be one stressing bounded, or near, rationality in a static context, and learning processes (in a world itself subject to change), in a dynamic context. There is still much to be done to improve our understanding and analysis in this field. Shackle will, I expect, go down in the history of economic thought as an idiosyncratic pioneer; his work was stimulating and enjoyable for a young but enthusiastic undergraduate.

It took me about thirty years before I stopped having nightmares about sitting exams, and the (self-induced) pressure of Finals was severe, but I got my first class result – though not the starred first for which I had hoped; and then I turned to the US for the graduate training that a professional academic needs. At that time (1960), there was none to be had in the UK. It had been thought that a well-trained undergraduate could move directly into research. But since undergraduate courses in economics then included only relatively low-level, and optional, courses in maths and econometrics, this meant that entrants into economic academia in the UK would only carry with them the technical aptitudes learnt earlier and on other courses; and I had none.

I chose to go to Harvard. The subject on which I wanted to do research was trade cycles. There was an inconsistency between the *models* of such cycles, which predicted lengthy periods of slump/stagnation as excess capital slowly got worked off and brief booms – checked by capacity ceilings – and *real* economic experience, which was that slumps were much shorter than booms (usually). In particular I wanted to work with James Duesenberry, whose work on macro-economic subjects, notably on the consumption function, was exciting.

But first I had, and wanted, to do course work on maths and econometrics. The maths mostly involved difference equations, but not calculus, and attempts to teach myself calculus (on the boat trip over to the USA – to the annoyance of my newly-married wife, who complained of being left alone – and subsequently in a maths class at Harvard) proved largely unavailing. The econometrics course was more comprehensive, taking us nearer to the professional front-line. At that time the print-outs from the latest IBM were done by a typewriter on top of the main frame, whose striking arms moved by electronic command; it really looked like the ghost in the machine.

Most of the other courses, however, covered ground already taught at undergraduate level in Cambridge, UK, since economic majors at US universities had far less exposure to courses on economics than economic specialists at Cambridge.[10] Anyhow the thought of doing another two years of repetitive material was deeply depressing, and the Chairman of the Faculty, Arthur Smithies, allowed me to telescope the normal two-years Masters into one, which I managed to do.

Research started the next autumn – after the only long vacation my wife and I have ever had, touring most of the coast-lines of North America – and was enormous fun. There was an initial glitch. I had intended to try to explore why the US economy had rebounded so sharply after the 1907 collapse, but had failed to do so in 1929. But to do that exercise properly, higher frequency national income data (quarterly and monthly) were needed, and these were not available for the 1906-09 episode, which was my starting point. Instead there was a copious wealth of high frequency (e.g. call report) data on money and banking. The question of the interaction between regular seasonal financial fluctuations (in a banking system without a central bank) on the one hand, and cyclical and other shocks on the other, was quite intricate and central to the history of the 1907 crisis.[11] Moreover, prior studies had, I believed, got much of that analysis wrong. Everything went swimmingly. My PhD thesis (1962) was completed within the year, by June 1962; and a Harvard Economic Study book (1969) and a *Journal of Political Economy* article (1965) followed shortly thereafter.

While young married life in Cambridge, Mass., was delightful, and I had had dual nationality (US/UK) until army national service had forced a choice (in 1955), I had become essentially English over time, and my wife was even more so. In 1962 we returned to Cambridge, UK, to a Prize Fellowship at Trinity College and an assistant lectureship in the faculty. Having successfully (to my own satisfaction) reinterpreted US (1900-1914) monetary history, using a high frequency data base, the obvious continuation was to try the same trick for the UK. Monthly banking data were also available (though heavily window-dressed in some respects) in the form of the monthly reports of the London Joint Stock Banks, which Chancellor of the Exchequer, Goschen, had required to be collected and published, following the first Barings crisis in 1890. The problem was that no one had previously systematically collected, checked, and analysed these. So the better part of two years (1963/64) was then taken up with primary historical research, collecting, checking and assembling as much monthly banking and macro-economic data as existed into us-

[10] The same was *not* the case for Oxford graduates where the PPE (Politics, Philosophy and Economics) course put them on roughly the same level as their US counterparts.

[11] Having entered monetary economics from the historical vantage point, I was largely unaware then (1962) of theoretical stirrings in Chicago. It was because of my historical expertise that Duesenberry showed me the manuscript (of Chapter 4) of Friedman and Schwartz' great book, *A Monetary History of the United States, 1867-1960*. And having been named in the preface as a (small) helper, I was later privileged to write one of the first reviews in the UK (in *Economica*) of that book (1964).

able time series format. This was a tedious chore. With these data published in *The Business of Banking, 1891-1914* (1972), nobody should ever need to repeat that exercise.

In the early 1960s the Cambridge faculty was embroiled, with the dominant US mainstream, on questions over the measurement and definition of capital (e.g. the re-switching issue), and on growth theory (see Harcourt 1972). Bob Solow visited on sabbatical, and he and Joan Robinson used to go at it hammer and tongs. Although intellectually intriguing at the outset, through constant repetition of the arguments, the debate became (to my eyes) both strident and sterile; I have cordially disliked and distrusted growth theory ever since.

But while I quietly kept out of the main academic squabbles in Cambridge, I found it more difficult to avoid administrative duties. Traditionally the Secretary of the Faculty was a post taken for two years by a junior faculty member. It was (is) a hideous job. The faculty chairman, Ken Berrill, led me to believe that my chances of tenure depended on taking it, and in a weak moment I agreed. When I realised that I had sacrificed two years of decent academic work to faculty administration (and in Cambridge!), I sought the first good job outside.

In 1964 an incoming Labour Government (after 13 years of Conservative misrule, so the slogan went) was keen to introduce indicative planning (French style) in order to try to speed up the (comparatively low) UK growth rate. They set up a new Ministry, the Department of Economic Affairs (DEA), under George Brown, with Sir Donald MacDougall as Chief Economist, to promote that; though its relationship with the Treasury, which continued to wield all the levers of demand management, was never clear. Anyhow they needed economists, and working in the DEA, albeit not in my own area of research, was preferable to being an administrator at Cambridge.

The 'National Plan' in the event turned out to be a non-starter, because there was a black hole where the balance of payments was supposed to be, and the Labour Government was neither prepared to countenance devaluation (until later in 1967), as almost all its senior economic advisers advised (in private), nor to retreat to a siege economy with quotas on imports, etc., as some of the Left advocated. Instead it fumbled along from crisis to crisis, as beautifully described in Cairncross (1996). That was hardly conducive to planning.

Fortunately I was not involved in the wider, macro-economic policy discussion. Instead, the plans, e.g. for future growth, of each of the main sectors needed to be made consistent with the overall plan target growth rate (4%). So the DEA needed economists who would assess sectoral/industrial plans/forecasts/objectives for such consistency. I worked under John Jukes, a sensible economist and nice man, on the energy sector, where a *White Paper* was under preparation, and on housing and construction. This was quite interesting, but not enormously intellectually demanding, and with the DEA and the National Plan clearly heading for the rocks, it was time to return to monetary economics.

After Dennis Robertson retired, monetary economics was not a leading field at Cambridge. By contrast the London School of Economics had made monetary economics a specialty. In the more institutional/historical wing, there were Richard Sayers (a key figure in the 1959 *Radcliffe Report*), Leslie Pressnell and Roger Alford; while on the analytical/theoretical wing, Alan Walters had passed through, being followed by Harry Johnson (holding a joint Chicago/LSE chair), plus there were several lively younger monetary economists (Morris Perlman and Laurence Harris). Harry's weekly seminar was *the* key feature of LSE. So I was happy to go there as a lecturer (in 1966), and pick up the traces of my monetary research. This involved trying to complete the work on the pre-1914 UK banking system (though I had to bring work on the operation of the gold standard to a premature end in 1968 when I moved to the Bank of England), together with several other (new) research exercises, of which two stand out. The first was a study on current monetary policy in the UK, commissioned by Holbik of the Federal Reserve Bank of Boston for a comparative study of *Monetary Policy in Twelve Industrial Countries*. This was completed in 1967, but Holbik was so inefficient at putting pressure on co-authors that it was not published till 1973, by which time a 'post-script' was, to my annoyance, required. For the second, I did, I believe, the first serious empirical article in the field of 'political economy' regressing political popularity, as measured by Gallup poll data, on a series of macro-economic (e.g. inflation and unemployment) and political cycle variables ("Political economy", jointly with R.J. Bhansali, in *Political Studies*, 1970).

By comparison with the spacious and gracious living conditions at Cambridge, LSE was (and remains) an inner-city slum – though the inhabitants have a vibrant intellectual life. In 1968 conditions at LSE worsened sharply, as the students there became infected with the contagious epidemic of revolt that had spread from Berkeley, via Paris, to LSE. In the winter term of 1968 the atmosphere at LSE was hysterically febrile and unconducive to any form of civilised academic activity. So I was glad to get a call from the Bank of England to join them for a two-year temporary secondment.

4. The Bank of England and the formation of monetary policy

4.1. *The battle of ideas: Monetarists and Keynesians*

The Bank had begun a policy of inviting young monetary economists to come into the Bank on a two-year secondment, one at a time, earlier in the 1960s. Roger Alford, Tony Cramp and Brian Reading had been my immediate predecessors when I arrived there. When I became installed, I discovered that I was effectively the only person there reasonably expert on the latest developments in monetary theory, especially what the Monetarists (led by Milton Friedman) were arguing.

The Bank had earlier recruited two economists with some background in monetary economics, Kit McMahon, whose specialty was in international monetary economics, and John Fforde, who had written a book on the history and workings of the Fed (Fforde 1954), but these now had senior positions, on the international and domestic monetary policy side respectively, and did not have the time (or perhaps the inclination) to go into the minutiae of the academic debate. The executive director responsible for economics in the Bank was Maurice Allen, who was sharp but, by then, quirky, and, while his experience of, and feel for, monetary affairs was excellent, his formal training had been many decades earlier. Meanwhile the head of the Economic Intelligence Department, Michael Thornton, was a man of great charm and ability, but not a professional economist, and his chief economist, Leslie Dicks-Mireaux, had been a general macro and labour market specialist.

Readers may think it odd, as I did, that the Bank then had no resident expert in monetary theory, but it was somewhat symptomatic of attitudes in the Bank at that time. Economists in the Bank were generally content with, and supportive of, the Keynesian economic analysis as outlined in the *Radcliffe Report*. The crucial requirement for domestic stability was, they believed, an appropriate fiscal policy, perhaps supported at times of crisis by an incomes policy of some kind. Without good fiscal policy, monetary policy by itself (at least within the bounds of practical politics), so it was argued, would be unable to stem the tide; variations in interest rates could be used for a time to counter speculation and to protect the balance of payments, but the essential requirement was to use fiscal policy to keep the economy in balance. The paper on "The operation of monetary policy since the Radcliffe Report", Bank of England (1969a), largely the work of Kit McMahon, provides a very fair picture of attitudes at that time.[12] Sometimes it felt as if the Bank considered its (private) advice to the Chancellor on fiscal policies to be its main input into macro policy.[13]

But the Radcliffe/Keynesian view of the role and functions of monetary policy was under threat and attack from the moment that I had arrived. The devaluation of 1967, so long resisted by the Government, did not seem, in 1968, to be working successfully to improve the trade balance. Speculation against sterling restarted, and the IMF were called in for support. Under the influence of Jacques Polak and Marcus Flemming, they had developed an international monetarist approach; and they attributed our problems in the UK in part to an unduly lax monetary stance.

Their (IMF) approach to a country in balance of payments difficulties was then broadly as follows. Discuss, and agree, planned future objectives for output, prices, the balance of trade, the broad context of fiscal policy and interest rates, with the country involved. This would give an estimate of future nominal incomes, consistent with a

[12] This paper, and most of the other key published papers issued by the Bank, have been gathered together in Bank of England (1984).

[13] While most of the formal economic analysis was Radcliffe/Keynesian, there remained some residual monetarist-type gut feelings among the domestic monetary operators in the Bank. They *hated* the application of credit ceilings, which were, they believed, intrusive, inefficient, cumbersome and ultimately ineffective; so they were more willing to give (sharper) fluctuations in interest rates a try as an instrument for domestic monetary control. Also, the concern of the domestic monetary operators in the Bank with trying to maintain confidence and good conditions in the gilts market was, I believe, *au fond* based on a worry about having to flood the country with additional money, or near-money assets, if the gilts market should go on strike.

desired recovery in the trade (and fiscal) balance. Then feed those estimates of nominal incomes (and interest rates) into a demand for money function. Given the expected (forecast) external contribution to monetary expansion, that demand for money calculation then led directly to a figure for domestic credit expansion (DCE) consistent with the achievement of the agreed forecast. Subject to some margin for error, those (quarterly) DCE forecasts then became the IMF's ceilings, which the deficit country had to achieve in order to receive further tranches of loans from the Fund. The idea was clear; any unplanned domestic expansion would raise nominal incomes and (via the demand for money function) increase DCE, which would then have to be cut back by some deflationary action (fiscal, interest rates, debt sales, credit ceilings); only if the expansionary impulse arose from (unexpectedly large) inflows from abroad – a larger external monetary component – could it be accommodated.

Much of this was a novel concept to British economists, especially the key role of an (assumed predictable) demand-for-money function, and antipathetic to many – recall that the *Radcliffe Report* had denied the stability, or even the usefulness as a concept, of velocity. So my first role at the Bank was to try to explain the concept, and role of DCE, both within and without the Bank.[14] Actually we had to go further. To protect British *amour propre* there had to be some pretence that we, in the UK, had thought up this wonderful new wheeze, rather than had it foisted upon us, out of weakness, by the IMF.

Anyhow I had already found a niche in the Bank,[15] which was to try to explain internally to the Bank what the outside (monetarist) economists were arguing, while at the same time trying to explain to those same outside economists what the Bank's viewpoint was. This meant that within the Bank (and perhaps the Treasury) I was perceived as almost the resident 'Monetarist', while to the Monetarists outside, notably at the Konstanz conferences organized by Karl Brunner and Allan Meltzer, I was seen as an 'unreconstructed Keynesian'.

It was obvious that a, perhaps *the*, crucial difference between the Monetarist and the Radcliffe camps (though not so much in the case

[14] I was the main author of Bank of England (1969b).

[15] The importance of this niche to the Bank, and the interest and satisfaction of the work to me, led me to move on from a position of temporary secondment to a full-time position, as an adviser.

of the neo-Keynesians under the leadership of James Tobin in the USA) lay in the question of the predictability of the demand for money. So the Bank next set me the task of assessing this empirically for the UK, under the supervision of McMahon and Dicks-Mireaux, and with the assistance of Andrew Crockett. This resulted in the *Quarterly Bulletin* (1970) paper on "The Importance of Money", together with Andrew Crockett's paper (1970), on whether money was a leading indicator for subsequent movements in output and prices.

The results suggested an econometrically quite stable relationship, both for broad money (£ M3) including interest-bearing as well as non-interest-bearing deposits, as well as for narrow money, M1. Largely because the movements in £ M3 could be analysed in terms of the credit counterparts (and DCE), it was preferred as the main measure for assessing monetary conditions.

The main clearing banks in the UK had been subject to direct credit controls, more or less continuously, since 1939, and the Bank, especially John Fforde, rightly argued that such constraints were both increasingly deleterious to the efficiency of the system and, over time, became more and more ineffective, via disintermediation. The Bank used my work as one argument against maintaining such controls. If the demand for money function was stable *and* there was a significant negative coefficient on interest rates, then you could rely on interest rate adjustments – and did not need direct credit controls – to maintain monetary stability.

The Treasury, then under Sir Douglas Allen, were cautious, and worried about the likelihood of a credit explosion if controls were to be lifted. But after extensive discussions,[16] mainly in 1970, they relented, and *Competition and Credit Control* (Bank of England 1971) appeared just in time for the 1972/73 boom, and subsequent bust.

In the event, bank lending and broad money accelerated very sharply in 1972/73, far faster than consistent with current and prior nominal incomes and interest rates, which were raised to 13% in the autumn of 1973. 'My' demand-for-money function broke down

[16] I was involved in most of the discussions, but the real work was done by the Home Finance side, primarily John Fforde, the Executive Director, and John Page, the Chief Cashier, with the assistance of Andrew Crockett. Andrew drafted the paper that went to the Treasury in 1971, following Fforde's internal paper to the Governor, Leslie O'Brien, at the end of 1970. *Competition and Credit Control* (Bank of England 1971, and also 1984, Chapter 2), as titled and published, was mainly written by the Chief Cashier, John Page.

within a couple of years of being estimated! What had gone wrong? Unduly lax monetary policy – a supply shock – said the critics. My own view is that a large part of the explanation is due to the regime shift encouraging banks to compete in offering much more attractive deposit liabilities, notably CDs, with much more competitive, money-market related interest rates. Indeed, the ambience of the time, with a boom and rapid expansion, led the banks to compete so strongly for market share that they were prepared to raise interest rates on wholesale deposits, relative to lending rates, to a point where 'round-tripping' arbitrage, whereby some well-placed borrowers took the loans just to reinvest the money in such bank deposits, became (arguably) profitable.

Meanwhile, however, the demand-for-money function of M1 remained well-behaved and stable, and M1 growth slackened as interest rates rose in 1973. What was intriguing during these disturbed years, 1972-74, was that previously estimated demand-for-money functions (e.g. Goldfeld's in the USA, 1973) generally misbehaved in most developed countries, but almost always it was the function for that definition chosen by the central bank as its preferred monetary indicator that broke down most emphatically. When the Reserve Bank of Australia invited me to a Conference in 1975, as the third visitor in a trio alongside Jim Tobin and Dick Cooper, I used that observation (see Goodhart 1984, p. 96) as the basis for a jocular footnote about 'Goodhart's Law', that, "whenever a government seeks to rely on a previously observed statistical regularity for control purposes, that regularity will collapse". To the British Press and wider public, that quip, which was picked up and seems to lead a life of its own, is the only memorable thing about my work! While this 'law' does have its serious analytical side (*q.v.* the Lucas critique), it does feel slightly odd to have one's public reputation largely based on a minor footnote.

But whether, or not, the data for £ M3 were artificially inflated, it is impossible to deny that the boom, especially the bubble in housing and property prices, in the UK in 1972/73 got out of hand. *Ex post*, policy was far too weak. Even though the demand for money function in 1972/73 broke down, the fact that the surge (1972/73) and subsequent fall-back in the growth of £ M3 preceded the surge and fall-back in inflation led virtually all UK Monetarists, and most outside commentators in the country, to reinforce their conviction that broad money was *the* key monetary aggregate.[17]

The Prime Minister, Ted Heath, had sought to rely on incomes policy in 1973 to hold the line on inflation. Having interest rates rise (an input cost to businessmen), while prices were supposed to be held pegged, was an obvious embarrassment. So, towards the end of 1973[18] an edict reached the Bank that the continuing fast growth of the monetary aggregates must be curtailed *without* any further rise in interest rates. That could only mean a resort once again to direct credit controls. John Fforde asked me to spell out the options, and I wrote a note stating that we could place a limit on either the level, or the marginal increase, of either loans or deposits. The option which both I and John Fforde preferred was a limit on the marginal increase of *interest-bearing* deposits. No limit on non-interest bearing deposits could be applied since banks could not refuse them. But they could discourage additional interest-bearing deposits by cutting the interest rate offered. Since much of the previous rise in £ M3 had been engendered by 'excessive' competition between banks to offer ever more attractive interest rates, the punishment seemed to fit the crime. Moreover the banks could hardly scream too loudly about a measure aimed at raising the spread between loan and deposit rates. Moreover, and for some the clinching argument, the 'corset' (as my colleague Gilbert Wood christened the scheme), was sufficiently different from, and rather more complex than, simple direct controls on bank lending to the private sector. So its imposition did not appear to be such an abject reversion to the *status quo ante Competition and Credit Control*. In fact its initial imposition worked rather well; the £ M3 bubble burst and its growth slackened rapidly.

There were a number of bubbles in the UK economy in 1973, including a property and housing price boom. These burst towards the end of 1973, leading to the fringe bank crisis and 'lifeboat' rescue scheme, with which I was not involved. Indeed the downturn in 1974/75, somewhat exacerbated by the incoming Labour Govern-

[17] See Walters (1986, chapter 6).

[18] From the end of 1972 until the autumn of 1973, I had taken leave from the Bank to write my main textbook, *Money, Information and Uncertainty*. I reckoned that I was, by then, as well placed as I ever would be to combine theory with practical policy insights. Because prices of paper and wood-pulp were then rising so rapidly, my publishers, Macmillan, were uncertain how to price it. So they sat on it, not publishing it until 1975, and then with a tiny type-face. I was not best pleased.

ment's fiscal squeeze on the company sector, led to a period of comparative calm on the monetary policy front. Moreover, the 1976 external crisis and speculative attack on sterling was not, I believe that history will tell, much related to the conduct of domestic monetary policy.

Instead the main subject of interest in my field was analytical in form. Following the breakdown of the Bretton Woods system, and the disturbed and unhappy experience of 1972-74, the Monetarists were arguing that domestic monetary targets should become the centrepiece, and rule, for monetary policy. While not accepting the full panoply of monetarist theory, neither the rigidity of rules nor the use of monetary base control as an operating mechanism, central banks around the world were tending to describe themselves as 'pragmatic monetarists', and to publish monetary bands as general guidelines and indicators against which the conduct of monetary policy could be judged, beginning with the Bundesbank in 1974 and continuing with the Fed in 1975 (see Goodhart 1989).

Opinions in the UK were mixed. Advisers of the new Conservative opposition leader, Mrs Thatcher, such as Gordon Pepper and Brian Griffiths, were strongly in favour of monetary targets. Many of the Keynesian, and the more left-wing, advisers of the Labour Government were vehemently against. Meanwhile the new (June 1973) Governor of the Bank, Gordon Richardson, was listening to his central bank colleagues in his regular meetings with them in Basle, especially perhaps Governor Bouey of Canada. But there were also differences of view within the Bank. While the experience of the breakdown of the £ M3 demand-for-money function had made me unwilling to advocate the acceptance of rigid rules, whereby monetary policy would be conducted solely on the basis of a quantified monetary target, I could see the benefits of an indicative quantified forecast of how the monetary aggregates could be expected to develop consistent with the Government's objectives for the growth of nominal incomes.

Christopher Dow arrived in the Bank, coming from the OECD, just about the same time as Richardson took over, and became Executive Director and Chief Economist, when Kit McMahon moved from that position to becoming overseas director in October 1972.[19] I believe that the Governor saw Christopher as someone who could warn him whenever the Bank might be moving in such a way as to inflame the sensibilities of the Labour Government and the Left. Anyhow Christopher was much more suspicious and sceptical of monetary targetry than I. The Governor's, and the Bank's, pronouncements on this subject, such as the Governor's (Richardson 1978) Mais lecture, usually followed a lengthy process of redrafting after redrafting, partly, but only partly, to reconcile the differing analytical standpoints of Christopher and myself.

As usual, events decided, and the pressures of the 1976 sterling crisis led to the publication (by the Government) of quantified monetary targets, first in a normative, subsequently in a positive, manner; and the 'corset' was reimposed, though (as usual with direct credit controls) by now the banking system was better prepared to disintermediate through the 'bill leak', a technicality whose details are not worth restating here.

In practice the tightened policy measures (mostly fiscal) taken in 1976, once again under IMF tutelage, slowed the economy less than had been expected – especially by the more hysterical members of the Labour party. The period 1977-79 was again one of relative calm for domestic monetary policy,[20] in some part because the rapport between Governor and Chancellor, Denis Healey, was then particularly close.

As earlier noted, the new Conservative leadership, Mrs Thatcher, and Keith Joseph had espoused many of the tenets of monetarism. One facet of monetarism which I believed to be unworkable in practice in a monetary and banking system, such as existed in the UK, is monetary base control.[21] So I felt it desirable to make these arguments known and public, before they might be regarded as contrary to the expressed views of an incoming Conservative Government. This was done in a paper, jointly written with Michael Foot and Tony Hotson, in the *Quarterly Bulletin* in 1979.

[19] His predecessor as overseas director, Jeremy Morse, left to take on the thankless task of trying to patch up the collapsing Bretton Woods international exchange rate system in the Committee of Twenty, taking the young Eddie George with him as his personal assistant.

[20] The main debate, in 1977, was whether interest rates should be used primarily to control the exchange rate (i.e. to keep it at the low, competitive level established in 1976), or to control the domestic monetary aggregates (and hence inflation), or some combination of the two.

[21] I have written on this several times subsequently (notably in 1994 and 1995a).

The Conservative Party duly won the 1979 election, and shortly thereafter re-affirmed a target for broad money, £ M3, which target then became the centrepiece of their medium term financial strategy (H.M. Treasury 1980); the best analytical account of the strategy is to be found in the Zurich speech given by Nigel Lawson (1981), then financial secretary to the Treasury. The new Government was warned by the Bank, *not* least by myself, that the presumed underlying stability of the relationship between £ M3 and nominal incomes was *not* sufficiently reliable for the weight being placed upon it. In order, however, to make their new policy seem firm and credible, specific quantified targets for £ M3 were nevertheless promulgated, with no caveats.

From the outset circumstances led to great pressures being placed on the monetary target. The second oil price shock led to sharp upwards increases in input costs; the new Government had felt bound to allow a negotiated (post-incomes-policy type restraint) surge in public sector pay to proceed unchallenged; the Chancellor had announced a major shift from direct income tax to VAT, raising it from 8% to 15% in his first Budget in June 1979. All this led to sharp price increases in 1979/80. At the same time, however, the UK's new position as a prospective large oil exporter, confidence in Mrs Thatcher's policies and the tightening of monetary policy, with interest rate increases, were putting very strong upwards pressure on the UK nominal exchange rate. So real exchange rates were going through the roof and the competitiveness of the tradable goods sector, essentially manufacturing, was being put to the sword.

While this conjuncture was causing consternation among many economists, e.g. the famous letter to *The Times* of 364 economists (March 31, 1981), the pace of monetary growth was right at the top end of the target range. It was shortly to go way over the top in embarrassing circumstances.

Exchange controls had been summarily discarded in October 1979, with no adverse effects (given the concurrent sharp upwards pressure in sterling). But once exchange controls had been dropped, it was hardly possible, or sensible, to continue with direct controls over domestic bank expansion, since they could now be avoided by simple disintermediation abroad. The 'corset' was still in place, but was accordingly to be removed in the summer of 1980.

The difficulty lay in predicting how large had been the prior build-up of disintermediation that now might come flooding back into bank deposits and £ M3, after the corset's removal. My colleagues and I at the Bank made a rough stab at a prediction, forecasting a rise of somewhat over 2% in £ M3 in the month of June 1980, data becoming available in late July. That would have been bad enough by itself for meeting the target. In fact the rise in the month was more than double our prediction, nearly 5%. But such a huge jump, when published, would make a nonsense, a laughing-stock, of the recently established (with much fanfare) monetary centrepiece, the target for £ M3, in the medium term financial strategy. There was a great need (from the Bank's viewpoint), for some urgent quiet diplomacy. It did not receive sufficient. It was the start of the holiday period, and almost all the key senior *dramatis personae* were away on holiday. Mrs Thatcher was on holiday in Switzerland, and discussed the British monetary surge with some monetarist experts in Switzerland, before the Bank had had a proper chance to talk with her about it. Anyhow she returned unsure whether the Bank were fools or knaves; the Bank was well and truly in the dog-house.

We, in the Bank, had to explain at regular intervals why we were so ineffectual in slowing monetary growth, and we were regularly chastised for our shortcomings. To her credit Mrs Thatcher never considered reverting to direct controls. When we pointed out that any market-oriented method for slowing monetary growth would involve raising interest rates yet further, the tone of such discussions always changed abruptly. As it happened, the surge in £ M3, and its subsequent fast growth in the remainder of the early 1980s, were not accompanied, or followed, by a similar surge in nominal incomes; indeed the upwards trend in the velocity of £ M3 broke precisely in 1979; though, of course, in the early 1980s we were not to know that, and for several years we waited anxiously for the 'overhang' of excess money balances to feed through into expenditures – as it had (or so it is believed) in 1973/74.

Instead, the high level of interest rates, and especially of real exchange rates, and an increasingly tough and determined willingness to confront the unions were bringing down inflation at just about exactly the rate which the Government had always wanted.[22] And with

[22] Lawson (1992, p. 72).

real output going through a brief, but sharp, deflation, this was hardly the time to raise interest rates further, even for monetarist purity. Moreover, doubts were increasing about what such monetarist purity actually entailed. Milton Friedman had been critical of the UK choice of target.[23] Even more important, Alan Walters returned to the UK as Mrs Thatcher's adviser, and he encouraged Jurg Niehans to do a study (1981) on UK monetary policy. Their advice was that the narrow monetary aggregates were a better guide to policy, than broad money, and that by those standards monetary policy between 1979 and 1980 had been, if anything, too tight rather than too lax![24] Alan and the Bank were in broad agreement on that.

Against this background the Government, and the Bank, retreated to a multiplicity of target aggregates, narrow and broad. But with the relationship between the original cynosure £ M3 and nominal incomes having become patently unreliable, and with uncertainty about which was the proper aggregate to target anyhow, the Government's earlier confidence in this approach was ebbing fast. This matched, and was reinforced by, similar problems that the Fed was having in steering by M1, with the operational method of non-borrowed monetary base control. As John Crow, Governor of the Bank of Canada quipped, "We did not leave the monetary aggregates; they left us". That faced Nigel Lawson, who became Chancellor in 1983, with the problem of finding some alternative anchor for steering monetary policy towards price stability. His subsequent attempts to find such an anchor in a link with the DM and the Bundesbank, via the ERM, against the wishes of Mrs Thatcher (and Sir Alan Walters) is, however, another, often-told, story almost entirely played out after I had left the Bank.

As earlier recounted, Mrs Thatcher's personal economic advisers (e.g. Griffiths, Pepper, to a lesser extent Walters)[25] were, I believe, all strongly in favour of trying to move operationally to a system of monetary base control. But the Bank, the banks and the City viewed the proposal with horror (certainly including me). As I recall, the Treasury tried to keep out of this argument (on the one hand ... on the other hand). The details of the subject were, however, quite arcane, and the issue did not have as much resonance with Mrs

[23] Select Committee evidence, see Treasury and Civil Service Committee (1981).
[24] Thatcher (1993, p. 125); Walters (1986, chapters 6 and 8).
[25] Walters (1986, pp. 123-24 and 147).

Thatcher and Keith Joseph as the monetary targets/medium term financial strategy had had. So the defenders of the *status quo* won the day, as I have described at greater length (1989). A truce was declared with the publication by the Bank of England of new arrangements for monetary control in August 1981 (Bank of England 1981) which actually left the Bank's basic *modus operandi* unchanged.

4.2. *Structural developments at the Bank and the use of economists*

When I arrived at the Bank, there was more relative attention then given to the analysis of economic events abroad and less to domestic economic concerns, with the economists split between the Economic Intelligence Department (EID, with responsibility also for financial statistics – including seasonal adjustment techniques – and the *Quarterly Bulletin*), and the Overseas Department. I was in EID. Our job was forecasting, analysis and policy simulation for internal consumption, and published commentary, mainly for the *Quarterly Bulletin*. One question that kept on being repeated throughout my time at the Bank was whether we should have our own model, or just rely on the Treasury's for forecasting and simulation purposes. A key concern was that, if the Bank was to publish its own forecast projections, the Press would concentrate their attention on differences, however minor, between the Government's and the Bank's outlook. Such a focus on (minor) differences in forecasts was felt to be unhealthy. So discussions with the Chancellor and Treasury were, almost always, held on the basis of their (HMT) set of consistent forecast figures – though the Bank contributed to (and discussed aspects of) HMT's forecasts, particularly on the financial side.

HMT economists not only had the advantage of playing on their home ground on the forecasts; they also had immediate access to the Chancellor and Treasury Ministers. So, whenever there might be a debate on purely analytical economic matters between HMT (and their economists) and the Bank (and its staff), HMT would be the likely winners. Not surprisingly, therefore, the Bank's main card was usually its practical experience of market responses. "I must warn you, Mr Chancellor, that whatever the academics may claim, the markets would not be happy with policy X." With sterling endemically weak, and with an uncomfortably high borrowing requirement, Chancellors were often frightened by such warnings.

The Bank saw its main strengths, therefore, "as a Bank, not a study group".[26] Economists were necessary as a potential counterweight to economic analysis elsewhere, for expressing the Bank's views and policies in an academically acceptable light (PR), and for forecasting purposes. They were, when I first joined, not welcomed into the operational areas of the Bank. In the case of monetary (and to a slightly lesser extent, the gilts) markets, this gap between academic/analytical advice and operational decisions narrowed greatly during my time at the Bank. This was partly because it was perceived that the senior executives in the Bank needed to combine proficiency in the language of monetary/macro economics with practical/practitioner command over operational activities.[27] Ability as a practitioner was partly a matter of personal aptitudes, common sense, unflappability, etc., and partly a matter of on-the-job training; but the discipline of economics required professional university training. Hence the recruitment policies of the Bank shifted consciously towards gifted young economists. Usually these high flyers (e.g. Andrew Crockett, Lionel Price, Michael Foot, Tony Hotson, Bill Allen and many more) first passed through my monetary analysis and forecasting sections, before moving on to the next (operational) stage of their careers in the market management part of the Bank. Over time the personal and analytical inter-twining of economic analysis and market operations grew stronger.

The same was not true, during my time at the Bank, on the supervisory/regulatory side, but has become so since, as I shall discuss later. The 1973/74 fringe bank crisis and 'lifeboat', noted earlier, led to a mushroom-growth of Bank formal supervisory functions. But the principles on which this worked initially were strictly practical, and, apparently consciously, eschewed academic input (though, in fairness, there was not much useful input then to be had); instead the idea was that you should find out what was widely accepted as 'best practice' among the banks, and other relevant financial institutions, involved, and then chivy the laggards into abiding by such better behavioural norms.

Despite the greater formalism of economics now, and the effects of the Information Technology revolution in enabling us to access and analyse mountains of data, we do not really understand much more about, or feel any better able to predict, the macro-economy, than in the 1960s. Indeed almost the reverse; in the early days of (computer-assisted) macro-modelling, we (i.e. macro-economists in the public sector) really felt that we were enhancing our ability to understand *and* control the economy. Since then the Lucas critique, the rational expectations revolution and the failure of the large (Keynesian-type) forecasts have thrown forecasting (and parts of the previous canon of macro-economics) into disarray.

What has, instead, developed with great success has been the study of finance and the analysis of the relationship between risk and asset prices, and the determinants of risk, e.g. variance, co-variance, fat-tailed distributions, risk factor analysis, etc. From the Black/Scholes option pricing formula onwards, it became clear that the design, analysis and pricing of assets, and the measurement and assessment of risk lay in the domain of the economist.

The rational expectations hypothesis explains why anyone, whether economist, chartist, or sooth-sayer is bound to fail to predict the movement of asset markets at all well (since they will be mainly driven by unanticipated news); together with the Lucas critique, it also explains why the economist will have perennial problems even in predicting those parts of the economy subject to inertia, rigidities, etc. So the primary use of economists, in the public sector and in the City, as forecasters exposed them to circumstances where they would be inherently fallible, treated as witch doctors one minute, and derided as charlatans the next. Such a condition was (is) exacerbated by the unwillingness of the audience for forecasts to accept, or for forecasters to insist on the provision of, probability/confidence bounds for those forecasts. The inflation fan forecast in the Bank's recent *Inflation Reports* is one of the few praise-worthy exceptions. So the treatment of City economists as mainly forecasting/PR merchants has not, in my view, been helpful to the profession.

Where economists really can help is in the analysis of risk. There are much more systemic and predictable fluctuations in the *variance* than in the *level* of asset prices (variances often follow an

[26] I am not sure who first said this. Some attribute it to Montagu Norman; in any case it has been frequently repeated.

[27] I was asked, once or twice, in my early years at the Bank, whether I wanted to move from an economic policy advisory to an operational post. Although it was implied, but unstated, that such broadening would be a prerequisite to eventual promotion to a really top job, I always felt that my comparative advantage lay in sticking to my academic last, and I refused.

ARCH process – mean levels are, close to, random walk). Thus where City firms really would get the best out of their economists is in the risk control areas – and in the regulatory/supervisory areas in the public sector, which in the 1970s and much of the 1980s was mostly a 'no go' area for economists. All that, however, is now changing fast, and very much for the better. The growing partnership between academic work in financial economics and practical operations in risk management in the City is one of the most encouraging developments of recent years, but it largely post-dated my stay in the Bank.

In the early 1970s, following *Competition and Credit Control*, there was a systematic formalisation of our analysis of monetary developments, and their subsequent discussion with the Treasury and Chancellor. This was structured around the arrival of monthly balance sheet data from the banks. After processing and analysis, the data were presented to the Monetary Review Committee (MRC), chaired by John Fforde, the home finance director. I was responsible for (most of) the papers going to MRC and was its first secretary. Following the discussion in MRC, a summary of (considered) views on these monetary developments was put to the governors and executive directors, and formed the basis for subsequent regular discussions with HMT officials, and, if felt necessary, between Governor and Chancellor.

With monetary developments playing an increasingly large role in determining what the market operators were asked to achieve through the 1970s and 1980s, and the perceived resultant need to unify analytical advice with market operation, I myself formally moved into the home finance division, under John Fforde and alongside Eddie George (gilts market) and Tony Coleby (money markets) in the early 1980s. All that created something of a gap for complementary analysis of the 'real' economy and for another senior economist to work with Christopher Dow on that side, and John Flemming was recruited in the mid 1970s. The arrival of Robin Leigh-Pemberton in 1983 then led to another structural reshuffle in 1984. Among the constituent elements in that reshuffle was the need to find a replacement for Christopher Dow, the retiring executive director with responsibility for economics. The Governor chose John Flemming. This was a severe blow to me since it was the only step up the promotion ladder to which I could seriously aspire, and John was younger than I.

My personal hurt was lessened by the fact that John was (and remains) both a close friend and an excellent economist. Even so, it left the prospect of continuing with the same job that I had, in effect, been doing for the last 16 years for a further 13 years (till retirement). I could see myself becoming both bitter and stale. I learnt about the various promotions and reshuffles – in which I did not figure – almost accidentally, as those having been promoted discussed what was then to happen. Apart from a very brief and largely formal word with the Governor, a couple of weeks later ("Difficult choice", etc.), no one spoke to me at all about my own future prospects. It struck me then, and strikes me still, as appalling man management not to talk as carefully, or even more so, with the big 'losers' as with the big 'winners' from any reorganisation. So, despite the fact that my job at the Bank had been satisfying and fulfilling (until 1984), I decided to return to academic life.

5. Return to Academia (LSE); analysis of (foreign exchange) markets; central bank autonomy

5.1. *LSE and the Financial Markets Group*

Returning to academic life was easier decided than achieved. Although I had continued with research and publication at the Bank, I had been increasingly absorbed with the work of a senior official. My technical abilities, weak at best, had atrophied further. The preferred forms of analysis and teaching in the academic profession had moved on, and become more mathematical and formal. Some of my (LSE) colleagues had doubts whether I could still rank as a professional academic economist at all, or, if so, whether my appointment, as a Professor, would not use up one of the rare Chairs that could go to someone younger and more proficient.

Fortunately for me Eric Sosnow[28] wanted to endow a Chair at LSE in honour of his son, Norman, who had tragically died in an air

[28] Eric had escaped from Poland before World War II intending to go on to Israel, but had taken on a temporary job in London as a financial journalist – his prior occupation – and had stayed. After the war he moved on to financing trade with the Eastern bloc, and built up his own financial, import/export firm.

crash, in banking and finance. The special Appointments board for this named Chair contained non-academics, as well as academics, and my background was considered suitable; anyhow I was appointed, and have remained the Norman Sosnow professor of banking and finance ever since.

LSE is not a wealthy University with, by Oxbridge and US comparatives, minuscule endowments. Being sited in mid-London where space is expensive, it is extremely cramped, and, having grown in size over time, is a rabbit warren of inter-connecting buildings, whose geographical juxtaposition is somewhat random (you deserve an MSc in geography for finding your way around). Apart from its superb Robbins Library, and good IT, its other support staff are similarly skimped, crammed in and penurious. It is a miracle that the LSE administration manages to keep the place afloat at all, a miracle largely achieved by a few really dedicated key personnel.

In this inner-city slum, however, lives a world-class set of social science faculties,[29] notably one of the very best economics faculties in the UK. When I arrived Richard Layard was Departmental Convener (Head), shortly to be followed by Meghnad Desai (now Lord Desai). Other eminent figures were Tony Atkinson (now Master of Nuffield College, Oxford), Nick Stern (now at European Bank for Reconstruction and Development) and Mervyn King (now at the Bank of England). David Hendry and Steve Nickell had recently gone to Oxford. John Moore and Charlie Bean were prominent among the younger faculty.

[29] LSE is frequently perceived as a left-wing university. It is unclear why this reputation should linger on, being often attributed to the role and presence of Laski after World War II. It is true that, at the end of the 60s and early 70s, the Law department was unusually left-leaning; but the Economics department has Lionel Robbins as its great figure – and was home for a time to Hayek. So the Economics tradition at LSE has been more (neo) Classical than almost anywhere else in the UK. In particular, LSE economics never became infected with a quasi-Marxist sub-group, of the kind which embroiled certain other UK universities in the 1970s and 80s. Of course, LSE economics, and other faculties, had the usual mixture of left, middle and right political supporters, but the economics faculty never split, or became seriously internally at odds, on ideological grounds.

A greater division lay between those who had taken up applied specialties, e.g. transport, development, welfare, housing economics, and those who believed that the core of economics lay in theoretical analysis. The former group tended to feel treated as second-class citizens, and several of them, over time, migrated towards other faculties.

Given the heavy teaching and administrative load, academics in the UK nowadays have to use and protect their remaining time with fierce devotion if they are going to keep up with reading and research. The general perception is that academic life is comfortable and full of holidays compared with life in the public sector or the City. My own experience is that, so long as you want to continue making your mark in the academic profession, then the reverse is true.

Meanwhile the squeeze on resources for higher education meant that not only secretarial assistance was disappearing (becoming totally replaced by individual word processing on PCs), but also research assistance was not affordable for the universities. Moreover, research activity and methodology increasingly involved – often required – joint work. When I left academic life for the Bank in 1968, it was considered slightly disreputable to involve one's PhD research students in your own research. By 1985, and increasingly thereafter, it had become the norm!

Another culture shock, on returning to LSE, was that in the Bank nobody talked about the need to raise money for this, or that project. At LSE it was a perennial focus for discussion. Apart from a few theoreticians, content to live alone with their thoughts and equations, research now meant groups of faculty with research officers and assistants, and that required raising external finance, because LSE had none to spare.

The main economics research centre at LSE then (the Suntory-Toyota International Centre for Economics and Related Disciplines – STICERD – established by Michio Morishima) covered many aspects of the social sciences, but not money and finance. So, I was happy to join with Mervyn King who had the idea of trying to set up a research group, the Financial Markets Group (FMG), concentrating on such issues. Given our proximity to the City, and the focus of our research, we hoped that we could raise sufficient finance from the private sector, especially from City financial firms, to do the kind of basic research into such issues, that should (in the longer run) help to support the City's development (and in the process train a few of its brightest recruits). But we were adamant that we would *not* do direct consultancy; moreover, we would do research in-house, rather than try to supplement, or compete with, Richard Portes' brilliantly successful Centre for Economic Policy Research (CEPR) role in networking economists doing research at separate establishments around Europe.

With the help of Sir David Walker, our first Chairman, and the blessing and assistance of the Bank of England, we did manage in 1986/87 to obtain sufficient financial support[30] to open our doors for business (with a party) on January 14, 1987.[31] Mervyn and I were joint directors, but in reality the FMG was his creation, and he ran it with devotion (to every detail).

The FMG prospered greatly. We attracted excellent research students, good research officers, and we had sufficient funding to attract a flow of eminent visitors. It would be anomalous to pick out names, which are anyhow set out in the FMG's *Annual Reports*. Besides occasional conferences, the research output of the FMG usually first sees the day in the form of *Discussion* and *Special Papers*, though they subsequently often get published later in journal and book form. The *Discussion Papers* (DP) are more analytical/theoretical/econometric in content; the *Special Papers* (SP) are more institutional/policy/practical-oriented. The main fields that the FMG covered were corporate finance and governance, market structure, asset price determination and volatility, and monetary policy and financial regulation. Since 1987, until July 1997, the FMG has published 268 DPs and 97 SPs.

It was, of course, a serious blow for the FMG when Mervyn was picked by the Bank in 1991 as the new Executive Director in charge of economics, following John Flemming's move to the EBRD (and thence to being Warden of Wadham College, Oxford). At that time, moreover, I was acting as Head of Department, and neither could, nor wanted to, take over as Director myself. We were fortunate to have David Webb, who was moving from economics to become a professor in the Department of accounting and finance, to take on this (increasingly arduous) role. Since then David has greatly strengthened the finance wing of that Department, and has succeeded in supplementing our private sector funds with a (largely matching) public sector contribution from the Economic and Social Research Council (ESRC), for whom we have become (since 1993) a Research Centre. With the shift from Mervyn to David, the balance of our work has moved slightly from economics towards finance.

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30 Our main donors initially were Citibank, County NatWest, Investors in Industry (3i), Salomon Brothers and Nomura International Finance.

31 Financial Markets Groups (1987).

My own publications and research have been primarily in two main areas. First, I have written a large number of papers on current issues in monetary policy, both on international matters such as ERM/EMU, and on domestic questions, for example relating to the role and functions of the central bank, and also on financial regulation, where I was fortunate to be assisted first by Dirk Schoenmaker and then by Philipp Hartmann as research officers. Many of these papers have been gathered together in *The Central Bank and the Financial System* (1995b), and a further set of papers on *The Emerging Framework of Financial Regulation* (1998) should be published soon (also see Goodhart *et al.* 1998). Most of these papers were policy-oriented, so it is not surprising that the number of SPs of which I have been an author, 32, of which 6 were jointly authored (by August 1997), greatly outnumbers my contributions to the FMG DP series, 18, of which all but 3 were jointly authored.

Indeed, the majority of my DPs – and serious journal articles – related to my second main field of research, to which I turn next.

5.2. *Analysis of foreign exchange markets*

The standard theory of asset price determination, the rational efficient markets hypothesis, proposes that all, publicly available, relevant information should be factored into existing prices, so that asset prices should move in future primarily[32] in response to unanticipated 'news'. Furthermore, if one subscribes to the (Dornbusch) overshooting hypothesis, some asset prices should jump beyond their eventual 'equilibrium' on the receipt of certain news, e.g. of monetary changes in the case of the foreign exchange (forex) market, and then slowly revert to equilibrium.

But when I was regularly watching markets at the Bank, this stylized picture seemed far from reality. With a few exceptions (such as, for example, the release of monetary announcements in the USA during the period of Volcker's adoption of non-borrowed reserve base control, 1980-82), the response of the forex market to identifiable

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32 Given the pattern of existing interest differentials, a small, anticipated, rate of change of foreign exchange prices is generally expected over the future. In very high frequency data, e.g. seconds, minutes, hours, even days, such expected changes approximate zero.

economic (and political) 'news' seemed to account for only a small proportion of the market's gyrations – and a long way from the supposed 'jumps' that were supposed to occur. Moreover, much of the movements and volatility in the forex market seemed largely unrelated to anything that could be identified as public 'news'. In the stock-market, of course, one might relate fluctuations in individual shares to the release of 'private' news on each firm; but in the huge forex market, say in the enormous spot market for $/DM, would one really expect private news, e.g. on customer orders for forex transactions at the many competing individual banks, to have much effect on rates? After all, it is conventional wisdom that (sterilised) intervention by central banks is too comparatively small to be successful. If their orders are too small to move markets, why should other customers' orders be any more effective?

What determines movements in forex prices seemed a mystery, far from fully explicable in terms of the advent of unanticipated (public) news.[33] Anyhow that mystery struck me as a worthwhile subject for academic research, one probably requiring sufficiently detailed and patient pursuit that only academics would be likely to resolve it.

Anyhow this question, the determination of the movement of forex rates became a second focus of my research, and the basis for my inaugural lecture at LSE, on 'The foreign exchange market: a random walk with a dragging anchor', given in Autumn 1987, and reprinted in *Economica* (1988).

'News' is continuously occurring. It fills the pages of the newspapers, and television screens, every day. If one wants to isolate the effect of *individual* 'news' items on asset markets, it is necessary to go to very high frequency data (at a *minimum* hour by hour). But, it may be said that it may take quite a long time for news to be transmitted, assimilated and appreciated. This is *not* so in the case of economic news. The timing of most such announcements is known; the expected values for such variables is collected and reported in advance by institutions such as Money Market International Ltd.; bank traders are briefed at the outset of each day about what to expect, and on what response to take to an unexpected deviation (by the inhouse economists and technical analysts), and those same experts are on hand to give instant commentary and advice after the announcement.

[33] Whenever I used to ask the Bank forex dealers, perplexed, why some sharp surge in an exchange rate had occurred, I would usually receive the reply: "More buyers than sellers!".

Indeed my own (and others) research shows that the full effect of any economic 'news' with a pre-announced release date is factored into forex prices within about five minutes (early research on economic news which arrives *unannounced* during market-open periods indicates that full assimilation takes significantly longer, around half an hour); moreover the associated spike in volatility subsides back to normality (for news with pre-set release times) in about twenty minutes. Thus, with the use of high frequency data, at hourly, or shorter, intervals, one can isolate, with a reasonable degree of (statistical) confidence, the market impact of individual 'news' items.

In any case, the higher the frequency, the closer one comes to the actual continuous operations of the market. It becomes possible to study the interaction between many (but not all, see further below) of the market variables of interest to an economist, e.g. the (absolute) size of price change, its volatility and the size of the bid-ask spread. This work was labour and data intensive, and I have worked with a succession of good PhD research students. I provided the data base and ideas; they provided their time, and often the latest econometric techniques. The series started with hourly data, with Marcelo Giugale as research assistant (Goodhart and Giugale 1989, 1993); he later joined the World Bank; then minute by minute data, with Lorenzo Figliuoli (Goodhart and Figliuoli 1991, 1992), who subsequently joined the IMF.

The basis of virtually all forex data is the indicative price of bids and asks for bilateral spot rates put out by electronic screen vendors, such as Reuters, Telerate or Knight Ridder, on a continuous basis. End monthly, daily or hourly data are simply taken as snapshots from a continuous data stream. Why throw away all the intervening data? So with the kind help of Reuters PLC, and the IT assistance of Russell Lloyd, I installed a data feed direct from the FXFX, FXFY (and AAMM) pages of Reuters screens, and collected three months of continuous (April 9-July 3, 1989) data on forex bilateral spot rates (from the FXFX and FXFY pages) and associated news (from the AAMM page) (1989/1990).

Unbeknownst to me, a specialist consultancy/advisory/research firm in Zurich, Olsen and Associates, was currently doing even better on this front, collecting continuous forex and interest rate data from electronic screens from the mid 1980s to date; they have, I believe, the best library of such data in existence. We met subsequently, and I encouraged Richard Olsen to extend his, already widespread, links with the economic academic community by holding conferences on the use of High Frequency Data in Finance (HFDF); the first was held most successfully in Zurich in March 1995, the second now planned for March 1998, connected with which Richard has, with characteristic generosity and enthusiasm, made freely available much of his own data base for academic research use. But I run ahead of my own story.

Anyhow, with Antonio Demos (Goodhart and Demos 1990, 1991; Demos and Goodhart 1996), and Riccardo Curcio (Goodhart and Curcio 1991; Curcio and Goodhart 1992, 1993, 1997), I undertook research into this continuous data series, for example confirming the existence of first order negative auto-correlation between quotes at very high frequencies, e.g. at periodicities less than five minutes, which I had earlier discovered in my work with Figliuoli. With Riccardo, I also tried to explore – using similar high frequency data series – whether, and possibly how, chartist (technical analysis) might work. There was also related work on the microstructure of the forex market with Patrick McMahon, who sadly died early, and his research associate, Yerima Ngama (1992, 1993, 1997), with Mark Taylor (1992), Thomas Hesse, (1993), Hall, Henry and Pesaran (1993), and a survey article with Maureen O'Hara (1997), initially prepared as the introductory paper presented at the Olsen HFDF (1995) Conference, already noted. I had intended to, and may still, put most of these papers together into a collection of studies on the working of the foreign exchange market; but there was always another key paper in the series yet to be published, or just in the process of being drafted.

This still continues; and, as often beforehand, it revolves around the attempt to obtain yet another, and a better, data base to study. The Reuters FXFX page provides continuous data on indicative bid and ask quotes.[34] But the series has several shortcomings:

1) the data show the quotes of the latest bank to enter its quotes, *not* the best bid or ask available in the market;

[34] Though not *all* such quotes, since the FXFX technology can only handle one new quote every second, or so. That shortcoming has now been overcome with their new RICs pages, which shows all such quotes.

2) the quotes are indicative of prices ruling, and *not* firm, and either better, or in some market conditions worse, terms can be obtained by direct (telephone) contact;

3) the spreads are conventional in size, and again *not* representative of the true market spread;

4) the data may be unreliable at times when the market is particularly busy and volatile (because dealers may be too busy to update entries);

5) there are no associated transaction data available at all.

Meanwhile Reuters, and its main competitor EBS (which merged with Minex recently) have been developing electronic broking systems; the Reuters system is called D-2002. These can provide greater immediacy than telephone search, and are cheaper to use than inter-dealer brokers. On these (private) systems, the member banks can input firm offers to buy, or sell, for chosen quantities expressed in standardised units. The quantities available at the best firm bid, and ask, are shown on screen, and then another bank can 'hit' the best bid (or ask) for an amount up to that shown to be offered at that price. Although electronic broking only accounts for a fraction of the total market, this data set is clearly vastly superior in many ways to the FXFX series.

When I first approached Reuters they were hesitant to make any of their D-2002 data available, for confidentiality reasons. However they had themselves made videotapes of their own screen, for seven hours, on 16 June 1993, for promotional and presentational reasons, and they were prepared to pass these videotapes on to me. With the assistance of Professor Taka Ito and Richard Payne, the next in the line of research assistants, we exploited this (brief) data set in a series of papers (1996; Payne 1996a and 1996b).

I persevered with requests to Reuters to release more D-2002 data for academic research. Currently I am hoping to have them release, under my care, but for the use of all accredited academic research workers, one week of continuous data from D-2002 for the $/DM spot market. If I can achieve that, my first plan is to try to show the key features of how this market works *visually* in graphic form over CD/Rom. That would be a new venture for me. There will then, I hope, be more to follow. Watch this space.

5.3. *Central bank autonomy*

I have been fortunate to have been quite closely involved in three occasions of major regime changes in central banks, in Hong Kong in 1983 (though the Hong Kong Monetary Authority (HKMA) was not then a fully-fledged central bank), in New Zealand in 1988/89 and now in the UK in 1997.

I was somewhat distantly aware of the monetary crisis in Hong Kong in September 1983. Prior negotiations between Chairman Deng and Mrs Thatcher on the future of Hong Kong had not gone well. Flight capital began to leave Hong Kong, driving down the exchange rate, which had no anchor. The fall in the exchange rate began to raise local prices sufficiently rapidly to cause domestic concern. The People's Republic of China (PRC) attributed the developing panic to stratagems by the British to remove their money from Hong Kong in good time, which was untrue. But their sabre-rattling further heightened the panic. In turn, the panic caused property prices to drop, and that made the Hong Kong Association of Banks (HKAB) reluctant to raise interest rates sharply for fear of collapsing asset values.[35]

So, at the behest of the Chancellor,[36] two officials with some knowledge of monetary economics, David Peretz of HMT and I, were flown out to Hong Kong, to find that the senior monetary adviser, Douglas Bly, had already publicly committed to achieving a monetary reform, but that there were no clear plans as to what it should be. There was, however, a blueprint for reform already on the table, in the shape of a currency board system linked to the US $, which had been proposed by John Greenwood, a senior economist at G.T. Management Plc. A problem was that Greenwood had made himself *persona non grata* with the then chief secretary by his prior, biting criticisms of the unanchored, flexible regime which Sir Philip Haddon-Cave had been personally responsible for putting in place. Sir John Bremridge, the Financial Secretary, and Bly were not monetary experts and too unsure of themselves in this field to accept a scheme from an outspoken local critic.

[35] I have written a fuller account of this panic in my entry in Glasner, *Business Cycles and Depressions* (1997).

[36] Lawson (1992, p. 523).

So, our job was to assess the proposed 'link' to the US $, decide if it was a good idea – which it was, and remains – and to work out both transitional details, and, with much help from the local commercial bank executives, especially from the Hong Kong and Shanghai Bank, the various technical details of applying a currency board system to the particularities of the Hong Kong financial system (this latter was not an easy task). Hong Kong was particularly well suited to the 'link' since its extraordinarily flexible markets enable it to adjust to monetary conditions and interest rates, established by the Fed in the USA for domestic American objectives (i.e. which might not be conjuncturally best suited to current Hong Kong conditions). Equally Hong Kong's complex political position, as a UK colony shortly to change, in 1997, its status to a Special Administrative Region of the People's Republic of China, with two economic systems in one country, made the establishment of a currency board linked to the US $ (and *not* to the currency of the colonial power), extremely helpful, simultaneously both a strong and calming influence.[37]

I remained since then in fairly close touch with Hong Kong monetary affairs, serving on the Exchange Fund Advisory Council (an Advisory Board for HKMA), for the better part of a decade – which involved quite frequent lengthy plane trips – and also maintaining connections with the City University of Hong Kong, where I now have a position as external visiting professor for a couple of years.

Let me turn next to my connection with the Reserve Bank of New Zealand (RBNZ). During my time at the Bank I had had the opportunity to meet, and become friends with, the senior economists and officials at the RBNZ, especially Rod Deane and Peter Nicholl. I was asked to give a public lecture in Wellington on the occasion of their 50th anniversary, and then, more important, to act as one of their external advisers (Geof Wood of the City University Business School being another), when the Labour government (under Lange and Douglas), proposed an Act[38] to give the RBNZ autonomy to vary

[37] Also see Thatcher (1993, pp. 489-90).

[38] I have argued for several years that Labour governments are more likely to give autonomy to central banks, than Conservative governments. Their credibility gain (in reduced interest rates) is likely to be greater; moreover the Conservative opposition party can hardly object, given their ideological position. Hence such a central bank regime change, introduced by a Labour government, should receive all-

interest rates in pursuit of an inflation target agreed between Minister and Governor, and openly published and laid before Parliament.

I have argued, in numerous papers, that this framework, with the government determining the quantified inflation objective to be pursued, and then giving the central bank autonomy to vary interest rates so as to achieve that target, is optimal. It is, for example, in my view much preferable to the proposal for the European System of Central Banks, whereby the ESCB decides on its own (inflation) objectives; this leaves an excessive democratic deficit. But the main framework of the RBNZ Act of 1989 was determined by themselves in Wellington, not by external advisers,[39] though I was delighted to have the opportunity to comment in writing and to appear publicly in support of the draft Bill before one of their Select Committees.

Indeed, my particular memory of this episode relates to one piece of advice that I pushed strongly, which was *not* accepted. I advised that the Governor's salary should be linked to his success in achieving the inflation target. I had advocated a Walsh contract in practice[40] (see Walsh 1995), before it was shown to be optimal in some theoretical contexts. The reason why it was turned down, on Treasury advice, was primarily presentational. When inflation threatened, the Governor could be perceived as increasing his own income by raising interest rates that (in the short term) would lower the disposable income and employment of others. What this argument, which has some force, illustrated to me was that the short-term demand for higher employment, without proper concern for medium-term price stability or sustainable growth, emanated from the general public (and parts of the Press), as much as, or rather than, from supposedly self-seeking politicians.[41]

While I have consistently supported central bank autonomy in setting interest rates, to achieve an inflation objective set by the political authorities, I have at the same time had doubts about the virtues of the main theoretical analysis paraded in support of that step,

party support, as indeed happened in New Zealand. That greatly enhances the credibility of the whole exercise.

[39] The Governor, Don Brash, has been far too generous in his allocation of partial responsibility to me.

[40] Rumours of this suggestion somehow leaked. Although it was not accepted, for some years there was a common misapprehension that it had been.

[41] I have expressed these views more formally in a paper, with Dr Huang (1996).

i.e. the time inconsistency argument. There is little compelling empirical evidence that governments have sought consciously to use expectational inertia to trick people into working harder, in pursuit of a short-run electoral feel-good factor, and, indeed, little evidence, given the long lags with which monetary policy works, that they could do so even if they wanted. In my view, key, central elements in the conduct of monetary policy are the long lags in monetary policy and the wide range of uncertainty surrounding the effects of such policies over time on nominal incomes and prices. Yet in most time-inconsistency models, the monetary authorities can control prices instantly and perfectly! Absolute nonsense.

Yet this model not only survives, but is highly influential. This is partly because it combines technical, mathematical virtuosity with a fashionable cynicism about the motives and agenda of politicians. The reality in my view is rather more mundane. The future is always uncertain and debatable, so it is never easy to take a step that is currently painful in order to correct some uncertain future problem (i.e. increasing inflation). The temptation is always (and admittedly especially so before elections) to defer raising interest rates until actual hard current data show undeniable proof of worsening inflation. Given the lags, however, it is then too late to stop the dynamic process easily or quickly. Politicians are clearly liable to vary interest rates "too little, too late", but not, in my view, essentially out of a conscious desire to fool people into working harder.

Given the lags, the aim of monetary policy must be to control the level of the future (technically best constructed) forecast of inflation.[42] For the reasons stated above, this is best done by an autonomous central bank, working to an objective set out by the political authorities. It was, therefore, with great pleasure that I learnt in early May 1997 that the Chancellor of the Exchequer, Gordon Brown, of the incoming Labour government had initiated a regime change in the UK more or less exactly along these lines, and even more personal pleasure to find out a few weeks later that I was to be an external (i.e. non-Bank) member of the newly-created Monetary Policy Committee. It is an unusual privilege for an economist to try to make work in practice what he has advocated in theory. This task will provide purpose and excitement to my remaining years in the profession.

[42] As has been advocated in a number of recent, excellent papers by Lars Svensson (e.g. Svensson 1997a and 1997b).

REFERENCES

BANK OF ENGLAND (1969a), "The operation of monetary policy since the Radcliffe Report", *Bank of England Quarterly Bulletin*, 9 (4), pp. 448-60.

BANK OF ENGLAND (1969b), "Domestic credit expansion", *Bank of England Quarterly Bulletin*, 9 (3), pp. 363-82.

BANK OF ENGLAND (1971), *Competition and Credit Control*, May, London.

BANK OF ENGLAND (1981), "Monetary control – Provisions", *Bank of England Quarterly Bulletin*, 21 (3), pp. 347-50.

BANK OF ENGLAND (1984), *The Development and Operation of Monetary Policy, 1960-1983*, Clarendon Press, Oxford.

BIRMINGHAM, S. (1968), *Our Crowd. The Great Jewish Families of New York*, Longman, London.

CAIRNCROSS, A. (1996), *Managing the British Economy in the 1960s: A Treasury Perspective*, Macmillan, London.

CROCKETT, A.D. (1970), "Timing relationships between movements of monetary and national income variables", *Bank of England Quarterly Bulletin*, 10 (4), pp. 459-72.

CURCIO, R. and C.A.E. GOODHART (1992), "When support/resistance levels are broken, can profits be made?", *Financial Markets Group Discussion Paper*, no. 142, July; revised and reworked as Curcio, R., C. Goodhart, D. Guillaume and R. Payne (1997), "Do technical trading rules generate profits? Conclusions from the intra-day foreign exchange market", *International Journal of Finance and Economics*, 2(4), pp. 267-80.

CURCIO, R. and C.A.E. GOODHART (1993), "Chartism: a controlled experiment", *Journal of International Securities Markets*, 7, Autumn, pp. 173-86.

DEMOS, A.A. and C.A.E. GOODHART (1996), "The interaction between the frequency of market quotations, spread and volatility in the foreign exchange market", *Applied Economics*, 28, pp. 377-86.

Dictionary of National Biography (1986), Oxford University Press, Oxford.

FFORDE, J.S. (1954), *The Federal Reserve System, 1945-49*, Clarendon Press, Oxford.

FINANCIAL MARKETS GROUP (1987-to date), *Annual Report*, London School of Economics, London.

FLADE, R. (1996), *The Lehmans: From Rimpar to the New World*, Königshausen & Neuman, Würzburg.

FOOT, M.D.K.W., C.A.E GOODHART and A.C. HOTSON (1979), "Monetary base control", *Bank of England Quarterly Bulletin*, 19 (2), pp. 149-59.

FRANKEL, J.A., G. GALLI and A. GIOVANNINI (1996), *The Microstructure of Foreign Exchange Markets*, for the NBER, University of Chicago Press, Chicago.

FRIEDMAN, M. and A.J. SCHWARTZ (1963), *A Monetary History of the United States, 1867-1960*, NBER, Princeton University Press, Princeton.

GLASNER, D., ed. (1997), *Business Cycles and Depressions: An Encyclopedia*, Garland Publishing Inc., New York.

GOLDFELD, S.M. (1973), "The demand for money revisited", *Brookings Papers on Economic Activity*, no. 3, pp. 577-638.

GOODHART, A.L. (1920), *Poland and the Minority Races*, George Allen & Unwin Ltd, London.

GOODHART, C.A.E. (1962), "National banking system reactions to seasonal variations, 1900-1913", 2 vols, PhD Thesis, Harvard University, Cambridge, Mass.

GOODHART, C.A.E. (1964), "Review of a monetary history of the United States, 1867-1960, by M. Friedman and A. Schwartz", *Economica*, XXXI (123), pp. 314-18.

GOODHART, C.A.E. (1965), "Profit on national bank notes, 1900-1913", *Journal of Political Economy*, LXXIII (5), pp. 516-22.

GOODHART, C.A.E. (1969), *The New York Money Market and the Finance of Trade, 1900-1913*, Harvard Economic Studies, vol. 132, Harvard University Press, Cambridge, Mass.

GOODHART, C.A.E. (1972), *The Business of Banking, 1891-1914*, Weidenfeld and Nicolson, London.

GOODHART, C.A.E. (1973), "Monetary policy in the United Kingdom", in K. Holbik ed., *Monetary Policy in Twelve Industrial Countries*, chapter 12, Federal Reserve Bank of Boston, Boston.

GOODHART, C.A.E. (1975), *Money, Information and Uncertainty*, revised 2nd edition 1989, Macmillan, London.

GOODHART, C.A.E. (1984), "The problems of monetary management: the UK experience", in *Monetary Theory and Practice*, chapter III, Macmillan, London.

GOODHART, C.A.E. (1988), "The foreign exchange market: a random walk with a dragging anchor", *Economica*, 55 (220), pp. 437-60.

GOODHART, C.A.E. (1988), "The conduct of monetary policy", *Economic Journal*, 99 (396), pp. 293-346.

GOODHART, C.A.E. (1989), "'News' and the foreign exchange market", Manchester Statistical Society, Pamphlet, October 17; reprinted as *Financial Markets Group Discussion Paper*, no. 71, January 1990.

GOODHART, C.A.E. (1994), "What should central banks do? What should be their macroeconomic objectives and operations?", *Economic Journal*, 104 (427), pp. 1424-36.

GOODHART, C.A.E. (1995a), "Money supply control: base or interest rates", in K.D. Hoover and S.M. Sheffrin eds, *Monetarism and the Methodology of Economics*, chapter 7, Edward Elgar, Aldershot.

GOODHART, C.A.E. (1995b), *The Central Bank and the Financial System*, Macmillan, London.

GOODHART, C.A.E. (1997), "Hong Kong financial crisis (1983)", in D. Glasner ed., *Business Cycles and Depressions: An Encyclopedia*, Garland Publishing Inc., New York.

GOODHART, C.A.E. ed. (1998), *The Emerging Framework of Financial Regulation*, FMG Papers on financial regulation, Central Banking Publications Ltd, London, forthcoming.

GOODHART, C.A.E. and R.J. BHANSALI (1970), "Political economy", *Political Studies*, XVIII (1), pp. 43-106.

GOODHART, C.A.E. and A.D. CROCKETT (1970), "The importance of money", *Bank of England Quarterly Bulletin*, 10 (2), pp. 159-98.

GOODHART, C.A.E. and R. CURCIO (1991), "The clustering of bid/ask prices and the spread in the foreign exchange market", *Financial Markets Group Discussion Paper*, no. 110, January.

GOODHART, C.A.E. and A.A. DEMOS (1990), "Reuters screen images of the foreign exchange market; the Deutschemark/dollar spot rate", *The Journal of International Securities Markets*, 4, Winter, pp. 333-57.

GOODHART, C.A.E. and A.A. DEMOS (1991), "Reuters screen images of the foreign exchange markets: the yen/dollar and the sterling/dollar spot market", *The Journal of International Securities Markets*, 5, Spring, pp. 35-64.

GOODHART, C.A.E. and L. FIGLIUOLI (1991), "Every minute counts in financial markets", *Journal of International Money and Finance*, 10(1), pp. 23-52.

GOODHART, C.A.E. and L. FIGLIUOLI (1992), "The geographical location of the foreign exchange market: a test on an 'islands' hypothesis", *Journal of International and Comparative Economics*, (JOICE), 1 (1), pp. 13-28.

GOODHART, C.A.E. and M. GIUGALE (1989), "Some evidence on daily trading in the London foreign exchange market", *The Journal of International Securities Markets*, Summer, pp. 137-46.

GOODHART, C.A.E. and M. GIUGALE (1993), "From hour to hour in the foreign exchange market", *Manchester School Journal*, LXI (1), pp. 1-34.

GOODHART, C.A.E., S.G. HALL, S.G.B. HENRY and B. PESARAN (1993), "News effects in a high frequency model of the sterling-dollar exchange rate", *Journal of Applied Econometrics*, 8 (1), pp. 1-13.

GOODHART, C.A.E., P. HARTMANN, D.T. LLEWELLYN, L. ROJAS-SUAREZ and S. WEISBROD (1998), *Financial Regulation: Why, How and Where Now?*, Routledge, London, forthcoming.

GOODHART, C.A.E. and T. HESSE (1993), "Central bank forex intervention assessed in continuous time", *Journal of International Money and Finance*, 12 (9), pp. 368-89.

GOODHART, C.A.E. and H. HUANG (1996), "What are the central bank's and the private sector's objectives", Financial Markets Group, mimeo, forthcoming later in published form.

GOODHART, C.A.E., T. ITO and R. PAYNE (1996), "One day in June 1993: a study of the working of the Reuters 2000-2 electronic foreign exchange trading system", in J.A. Frankel, G. Galli and A. Giovannini eds, *The Microstructure of Foreign Exchange Markets*, chapter 4, University of Chicago Press; also published as *NBER Technical Working Paper*, no. 179, April 1995.

GOODHART, C.A.E., P.C. MCMAHON and Y.L. NGAMA (1992), "Does the forward premium/discount help to predict the future change in the exchange rate?", *Scottish Journal of Political Economy*, 39 (2), pp. 129-40.

GOODHART, C.A.E., P.C. MCMAHON and Y.L. NGAMA (1993), "Testing for unit roots with very high frequency spot exchange rate data", *Journal of Macroeconomics*, 15 (3), pp. 423-38.

GOODHART, C.A.E., P.C. MCMAHON and Y.L. NGAMA (1997), "Why does the spot-forward discount fail to predict changes in future spot prices", *International Journal of Financial Economics*, 2 (2), pp. 121-30.

GOODHART, C.A.E. and M. O'HARA (1997), "High frequency data in financial markets: issues and applications", *Journal of Empirical Finance*, 4 (2/3), pp. 73-114.

GOODHART, C.A.E. and R. PAYNE (1996b), "Microstructural dynamics in a foreign exchange electronic broking system", *Journal of International Money and Finance*, 15 (6), pp. 829-52.

GOODHART, C.A.E. and M.P. TAYLOR (1992), "Why don't individuals speculate in forward foreign exchange?", *Scottish Journal of Political Economy*, 39 (1), pp. 1-13.

HARCOURT, G.C. (1972), *Some Cambridge Controversies in the Theory of Capital*, Cambridge University Press, Cambridge.

HOLBIK, K. (1973), *Monetary Policy in Twelve Industrial Countries*, Federal Reserve Bank of Boston, Boston.

LAWSON, N. (1981), "Thatcherism in practice. A progress report", Speech to the Zurich Society of Economics, 14th January, H.M. Treasury Press Release, London.

LAWSON, N. (1992), *The View from No. 11, Memoirs of a Tory Radical*, Corgi Books, London.

NEVINS, A. (1963), *Herbert H. Lehman and his Era*, Scribners, New York.

NIEHANS, J. (1981), "The appreciation of sterling – Causes, effects, policies", Money Study Group, *Discussion Paper*, mimeo, February.

PAYNE, R. (1996a), "Announcement effects and seasonality in the intra-day foreign exchange market", *Financial Markets Group Discussion Paper*, no. 238, London School of Economics.

PAYNE, R. (1996b), "Information transmission in inter-dealer foreign exchange transactions", *Working Paper*, Financial Markets Group London School of Economics.

RICHARDSON, G. (1978), "Reflections on the conduct of monetary policy", *Bank of England Quarterly Bulletin*, 18 (1), pp. 51-58.

SHACKLE, G.L.S. (1949), *Expectations in Economics*, Cambridge University Press, Cambridge.

SRAFFA, P. (1960), *Production of Commodities by Means of Commodities: Prelude to a Critique of Economic Theory*, Cambridge University Press, Cambridge.

SVENSSON, L. (1997a), "Optimal inflation targets, 'conservative' central banks, and linear inflation contracts", *American Economic Review*, 87 (1), pp. 98-114.

SVENSSON, L. (1997b), "Inflation forecast targeting: implementing and monitoring inflation targets", *European Economic Review*, 41(6), pp. 111-46.

THATCHER, M. (1993), *The Downing Street Years*, Harper Collins, London.

TREASURY and CIVIL SERVICE COMMITTEE (1981), *Monetary Policy: Report*, HMSO, London.

TREASURY, HER MAJESTY'S (1980), *Financial Statement and Budget Report*, incorporating the Medium Term Financial Strategy, Her Majesty's Stationery Office (HMSO), London.

WALSH, C.E. (1995), "Optimal contracts for central bankers", *American Economic Review*, 85 (1), pp. 150-67.

WALTERS, A. (1986), *Britain's Economic Renaissance: Margaret Thatcher's Reforms, 1979-1984*, Oxford University Press, Oxford.