

Autobiographical Note: Extract from the Introduction to Peter E. Earl
(2002) *Information, Opportunism and Economic Coordination*,
Cheltenham, Edward Elgar

The process of carving an academic career that did not follow today's rules began immediately after I graduated from Cambridge University with First Class Honours in Economics in 1977 – an achievement that would normally be followed, if one were pursuing an academic career, by a high-flyer record of papers in top journals and jobs in top institutions. By that stage, I was hooked on problems of information and knowledge and was already an admirer of behavioural and Post Keynesian approaches to economics, via reading suggestions from, particularly, Ajit Singh, Alan Hughes and Tony Cramp. However, even whilst at school (Alleyne's Grammar School, Stevenage), I had been interested in the process of structural change and the problem of economic coordination: the first two economics books that I read were not textbooks but G.C. Allen's (1959) *British Industries and their Organization*, which gave me a sense of business history, and Michael Stewart's (1967) *Keynes and After*. Moreover, of the half-dozen textbooks that I read, Alchian and Allen (1967), with its discussion of speculation in commodity markets, was the one that interested me most. In fact, my interest in competition as a process rather than a state, and in the capabilities of firms and barriers to change, began a few years before I started economics, when my geography teacher talked about 'industrial inertia' as he explained why pottery continued to be produced in Staffordshire despite the deposits of china-clay having long been used up.

I remember vividly my discovery that, for some economists, the 'theory of the firm' was the perfect competition price/output diagram rather than something rather more concerned with the firm as an evolving organization. This came during an interview with John Creedy for a place at Durham University (as a back-up in case I did not get good enough A-Level results to try for Cambridge): he was already looking rather sceptical about the range of economics books I informed him I had been reading and asked me to draw 'the diagram of the theory of the firm'. After an embarrassing silence, I had to ask him what he meant. Durham promptly rejected me, which was rather ironic given its reputation for taking those that could not quite get into Cambridge.

After graduation, I stayed in Cambridge for a further two years, reading ravenously, with little supervision and virtually no interaction with other postgraduates. (I never took up a desk in the research students' room because, if I recall correctly, in those days students were allowed to smoke there.) Instead I cultivated a strong reputation as a supervisor of undergraduates. My reading began with Loasby's (1976) *Choice, Complexity and Ignorance* – which I had noticed on display in Heffers bookshop during my final year as an undergraduate – and most of the items in its list of references, and it spread out from there via the network of citations and by seeing what was sitting close by cited works on the library shelves. I must have been one of the first, if not even the first, to pick up Williamson's (1975) *Markets and Hierarchies* in the Marshall Library and thereby be introduced to his notion of opportunism.

Early in my studies, I was summoned to be interviewed one Saturday morning by senior faculty members about my intentions/interests. My main memory of the interview was that when Frank Hahn was asked if there was anything he wanted to raise, he replied, 'No, he's an economics poet.' After eight months, I was required to submit a 'substantial piece of work', as a prelude to a thesis proposal. What I tried to

submit, only to be told that I must prune it to 30 pages so that my supposed supervisor would be willing to read it, was a 150-page document entitled 'A Keynesian approach to structural change'. This was a precursor to my (1984) book *The Corporate Imagination*, that focused on problems of economic coordination in the face of uncertainty, sources of corporate inertia, non-price competition and the uptake of slack. It lacked, however, the 'corporate strategies as Kuhnian paradigms' perspective, that struck me six months later as I reflected on the fact that it is not merely scientists that cling to ideas in which they have invested. Two problems stood in the way of turning the ideas into a Ph.D. One was that no suitable supervisor was available in Cambridge. The other was that, in empirical terms, what I would have liked to do was akin to an update of Downie's (1958) analysis, but, unlike Downie, I did not have access to the kinds of raw, disaggregated performance data from firms that could only be obtained by someone working inside the Central Statistical Office.

When I told Geoff Meeks, the Assistant Director of Research, that I was thinking of giving up and turning to a career in management consulting, he raised the possibility of an external supervisor. I suggested Brian Loasby, of the University of Stirling, as the ideal person and, whilst approaches were being made, several positions were advertised at Stirling. Meeks encouraged me to apply – if nothing else, I might get a trip to Scotland to meet Loasby – and, a few months later, at age 23 and with my Ph.D. studies in disarray, I found myself with the offer of a lectureship. How different things were in those days! Several weeks after I joined the department, Sheila Dow was hired and we soon discovered overlapping interests in economic method and Post Keynesian monetary economics, that resulted in a number of enjoyable collaborative projects.

Even with Loasby's support, it took a further three years to get a topic approved in Cambridge and it was while this struggle was going on that I got on to the treadmill of being mainly focused on books rather than articles. This was in no small part as a consequence of being impressed by the ability of Mark Casson to produce a book every year and wondering whether I could do the same. The thesis that was eventually accepted in 1984, entitled 'A behavioural analysis of choice', contained elements from my (1983b) and (1986) books.

The Stirling approach to teaching economics was pluralistic and, in teaching management economics and marketing with Richard Shaw and Sue Shaw, I was introduced to the case-study method. Via the lively seminar programme, I made my first contact with Stirling alumnus Neil Kay, whose work continues to have a major influence on my thinking.

The four years and eleven months I spent at Stirling – one month short of the time that would have been sensible for superannuation purposes – were a great introduction to a lecturing career but were overshadowed by the struggle to get the Ph.D. through its various hoops, along with poor pay and, after major funding cutbacks, a lack of any immediate chance of accelerated promotion. Given the financial situation and my liking of mountainous scenery (despite having failed to make the most of what Scotland had to offer, owing to work on the books and thesis), a vacancy at the University of Tasmania looked appealing. After some initial hesitation, since the offer I received was for a three-year contract and I already had tenure at Stirling, I decided to give Tasmania a try, oblivious of the implications that the subsequent erosion of the Australian dollar and explosion of house prices in the UK would have for my ability to return to the British system.

The seven years that I spent at the University of Tasmania (1984–1991) were mostly focused, in terms of research, on monetary economics (leading to Earl, 1990)

or on economics and psychology. There was little on industrial economics and the theory of the firm, aside from a growing file of correspondence with Fred Lee about the work of P.W.S. Andrews, that eventually led us to edit his collected papers into a book (Andrews, 1993). However, towards the end of the 1980s, I found myself increasingly pushed out of teaching economics, since my Post Keynesian macroeconomics and monetary theory proved an unpalatable second-year filling to a three-year sequence of courses that began and ended with units taught in an uncompromisingly neoclassical style. The consequence of this was that I ended up teaching marketing, business strategy and organizational behaviour and developing many case study resources, some of which appear here in updated form (chapters 2 and 7) or in my (1995) microeconomics text. This period also saw me moving towards the pluralistic, context-based mode of thinking about economic problems that is evident in chapter 10 (originally written in 1989) and which culminated in the (1995) text.

As with Stirling, I ended up leaving Tasmania without making the most of its leisure opportunities. Despite a CV that was dominated by books, I found myself getting short-listed for chairs. Having been marginalized amongst my economics colleagues, and with a growing vision of how economics could provide a foundation for marketing and business strategy, I accepted the position of Professor of Economics in the Department of Economics and Marketing at Lincoln University, located near Christchurch, New Zealand.

On paper, this looked like an opportune move for doing the sorts of things that I wanted to do. Lincoln University was formerly an agricultural college of some note, but was growing rapidly in the commerce area (with plenty of scope for hiring new staff) and with a head of department (the marketing professor) who had trained as an agricultural economist and taught transaction cost analysis. Three staff shared my interest in Post Keynesian macroeconomics. On top of this, Christchurch was a city with good cultural facilities, cheap and architecturally interesting housing, excellent scenery nearby and with little congestion. In material terms, the decade there was very comfortable, but stagnant house prices made departure a costly experience when I eventually managed to escape what turned out to be a most unpleasant academic environment that matched few of my expectations of what was normal in a university.

Very soon after arriving, I realized that I was not going to get time for the sort of reading or reflective thinking that I had been used to, even if I sacrificed much of my leisure. The problem was not merely that research time was limited but that it was difficult to get a sustained chunk of it; everything had to be done in small bursts, on the run. There were many reasons for this, such as a crazy timetabling system that tended to require one to teach each subject on four or five days of the week, far more contact hours per subject than I had ever known and woefully inadequate tutorial resources, quite apart from administrative joys such as having to establish and then for many years chair the Human Ethics Committee, which seemed to chew up a day every week. Any hopes that I had held of building up a group of economists committed to pluralistic thinking and with an interest in cross-disciplinary research also vanished rapidly when I discovered that I only had one vote in the hiring process and that potential supporters seemed reluctant to alienate certain powerful figures from the ruling 'mafia'. (The bullying that went on was not just implied or verbal: at one stage, when standing my ground as examinations moderator, I was even picked up by a junior colleague and thrown out of his office.) Whilst there was hostility towards pluralism from the economists, which increased as more of them came with orthodox North American training, I was also unable to win support from other

business disciplines. Influential marketing and management colleagues guarded their turf carefully, such that their postgraduates often only discovered my expertise in their areas when they came to see me to discuss their upcoming applications for ethical approval. Increasingly, I wondered if this was a case of what is known in Australia and New Zealand as ‘the tall poppy syndrome’ in which steps are taken to cut high achievers down to normal size. The only rays of hope came via contacts that I made with social scientists in other departments, who made me welcome from the outset and with whom I developed both Bachelor and Master of Social Science degree programmes that enabled heterodox approaches to economics to continue to be taught to small classes under the ‘society and business’ banner after it had been pushed out of the microeconomics core.

In this context, I concluded that my best hope of maintaining a profile would be to produce the radically innovative microeconomics text and to edit books that explored things that time did not allow me to do on my own: both these kinds of work could be done in small doses during semester or in the evenings. Many of the papers that appear in revised form in this book were produced as bonuses in the midst of this, squeezed in because someone asked me to contribute something and because it turned out that I had something I felt to be worth saying and the occasion was an appropriate venue at which to say it. As a consequence, a lot more of the book editing than I had planned ended up being done in what should have been leisure time.

In the end, after being marginalized once again by neoclassically trained economists, I concluded that I really had to escape to a pluralistic environment that offered more time to think, even if it meant stepping back down the ladder for a time. In what might appear as a minor blow for the axiom of transitivity, I thus accepted a position at the University of Queensland despite having declined a position at a similar grade there at the end of 1987. Transitivity was actually not violated, though, for the working environment would now be air-conditioned – a vital ingredient during the long vacation research period – and Australian academic salaries had shot up whilst those in New Zealand had stagnated with the failure of microeconomic reform to deliver faster economic growth (cf. chapter 5). I would be working alongside my former Lincoln Ph.D. student Jason Potts, with a head of department (John Foster) who understood where I was coming from in terms of economic theory and teaching philosophy. In prospect, it felt as close as it could to being in Stirling twenty years earlier. Work on the present book commenced in my final months at Lincoln in 2001, as a closing chapter on an exhausting and frustrating decade.