FRANK HORACE HAHN *Photo*

Frank Horace Hahn
1925–2013

Frank Hahn had a transformative impact on British economics wherever he went, building on his love of, and competence in, mathematics and greatly influenced by his numerous American colleagues. He was at the heart of the development of General Equilibrium theory, which he criticised for its inability to include money. As a mathematical economist he was aware of the power of maximisation to deliver testable propositions about competitive equilibrium, but he was also sceptical about competitive equilibrium as a useful description of any actual economy. Later he was to attack what he termed Lucasian macroeconomists (Robert Lucas and his followers) for assuming that economies were in competitive equilibrium. His published works live on along with his huge impact on the profession. He brought together and worked with the best economists of his generation, nurtured and launched his students and colleagues, and, crucially, introduced the modern American rigorous theoretical economic approach to the more literary-minded and even anti-mathematical English style that had been prevalent since Marshall.

Hahn was a leader—he held the highest offices in the profession as President of the Econometric Society (in 1968), was elected to the British Academy (in 1975), and was a transformative President of the Royal Economic Society (1986–9)—but he engaged intensely with everyone and anyone willing to talk economics, whether the undergraduates that he invited weekly to his house, or his colleagues in the faculty common room at coffee and tea time, at faculty seminars and when visiting. He was hugely supportive of his friends and students, and to quote Solow on Hahn's 1956 visit to Massachusetts Institute of Technology (MIT), 'I

Biographical Memoirs of Fellows of the British Academy, XVI, 485–525.

cannot remember whether it was literally love at first sight. By the end of that year, however, a lifelong friendship had come into being' (Solow, 1992, 3). That was a sentiment echoed by many other visiting economists, who duly arrived as Overseas Fellows at Churchill College in Cambridge in response to invitations from Hahn after he joined the college as a founding fellow in 1960, many of whom were subsequently to be awarded the Nobel Prize.

He could be abrupt, combative and impatient with what he considered to be intellectually mediocre; he was often politically incorrect and sometimes dismissive of those with whom he disagreed: worse, he was sometimes impatient with the administrative staff for not intuiting exactly what he wanted. He was always fiercely intellectual, and his first question on seeing a colleague might be 'What new theorems have you proved?'. He recounted how when invited to give a lecture at Berkeley in 1959 he reminded them that 'America is the only country that went from barbarism to decadence without civilization in between.' Apparently that went down a treat and he was offered a position in their economics department.

Life and career

Frank Horace Hahn was born in Berlin on 26 April 1925, the younger of two sons, to Arnold Hahn, German-speaking, and Maria Hahn, Czech-speaking, with roots in the Jewish community. The family were Central European intellectuals. Hahn would delight in reminding one that he was a mitteleuropäisches Jew[1] (who toyed with the idea of becoming a Catholic when a young student at Oxford, and later would hanker for the life of a Trollopian rural dean). He would exaggerate his accent—'wee Breeteesh'—and stated most emphatically that 'England made me'.

His father was a chemist, but became a rather well-known literary figure, author of a book of sonnets, writing a weekly column for *Simplicissimus*, a satirical magazine founded in 1896, as well as popular science books and novels. Hahn described him as 'formidably learned' and his mother as 'beautiful and rather pleasure-loving'. His father was clearly a disciplinarian, not allowing Frank any dessert until he had equalled his older brother in solving problems. His upbringing gave him a

[1] I was with Hahn at a conference at Varenna, Lake Como, in June 1967 at the outbreak of the Six-Day War, and Hahn was all for offering his services to Israel—the war was over too soon for this to take effect.

'voracious appetite for reading and intellectual speculation', which he retained throughout his life.

The family left Berlin for Prague in 1931, where he and his elder brother, Peter, (born 1923), were sent to English schools but also learned Czech. At the relatively late date of 1938 they wisely left permanently for England, first for London and then Oxford. His father continued to publish, perhaps remarkably for wartime London, in German (Hahn, A., 1943), while Hahn was sent at the age of 13 to board at Bournemouth Grammar School, for which he retained an abiding fondness. Like his brother Peter, who went to University College of Swansea in 1941 but enlisted in the RAF after a year, Frank went to Oxford to read mathematics, and also enlisted in the RAF after a year's study in 1943.

Frank's father had suggested that he study economics at university, but his real love was always pure mathematics, although he recognised early that he was unlikely to make a good enough mathematician to justify continuing. It is hardly surprising that Hahn is always described as a mathematical economist and admitted that as an economist he might 'lack some of the attributes of an economist that Keynes thought necessary' with a 'weak interest in the practical end of the subject'.

While serving in the RAF (as a navigator latterly hunting for U-boats) he started reading economics books, and notably Hicks's *Value and Capital*, at the suggestion of his father's friend Michal Kalecki. Kalecki was a distinguished Polish economist, hired by the Oxford Institute of Statistics in 1940, who established close links with the Cambridge 'post-Keynesians' around Richard Kahn and Joan Robinson. Kalecki's approach to macroeconomics based on imperfectly competitive firms should have been as influential with that group as it was with Hahn. *Value and Capital* had a lasting impact on the young Hahn, leading to a sustained interest for the rest of his life with General Equilibrium theory. Hicks stressed, and Hahn was persuaded, of the critical importance of the micro-foundations of macroeconomics. Later, in the preface to their joint book, Hahn and Solow (1995, vii) explained what this meant: '… we both regarded ourselves as neoclassical economists in the sense that we required theories of the economy to be firmly based on the rationality of agents and on decentralized modes of economic communication among them. Indeed, it was this general approach that led us to the view that the new macroeconomists were claiming much more than could be deduced from fundamental neoclassical principles.' Kalecki clearly had a major impact, as Hahn took his advice that it would be easy to read for a London external degree in economics while in the forces, which he did, graduating in 1945.

While his brother Peter decided to return to Prague after the war (and remained there until escaping with his family, a suitcase and £500 at the end of the Prague spring in 1968), Frank went to the London School of Economics (LSE) to pursue a PhD at the tender age of 20. Hahn wrote that his time at the LSE was 'fortunate in many ways. I met my wife (also an economist) whose intelligence exceeds mine by an order of magnitude and whose good sense has been invaluable' (Szenberg, 1992, 162). The story is that he met Dorothy Salter and proposed to her the following day, marrying in 1946 at the age of 21—an excellent example of his rapid perception followed by decisive action.

The LSE was an international hothouse of economists. Lionel Robbins, Nicky Kaldor, Ronald Coase, Arthur Lewis and Friedrich Hayek were active in the seminars that Hahn attended. His initial PhD supervisor for the first three months was Kaldor, but Hahn only had two supervisions from him before moving to Robbins. His thesis, *The Share of Wages in National Income*, was completed in 1951. Kaldor was later to write 'According to the preface of Ricardo's *Principles*, the discovery of the laws which regulate distributive shares is the "principal problem in Political Economy"' (Kaldor, 1955, 83; distributive shares are the shares of wages and profits in national income). In 1950 Hahn published a condensed version of part of the thesis as the short article 'The share of wages in the trade cycle' in the *Economic Journal*, followed by Hahn (1951), which has the same title as the thesis but is essentially just chapter 3.

The thesis was eventually published in full (Hahn, 1972) and illuminatingly discussed by Solow (1992, 3), who argues that 'Hahn has an excellent claim to be the originator of the "macroeconomic theory of distribution", to have been—dare I say it? a sort of proto-Kaldor.' In the Preface to the published thesis Hahn first disarmingly notes that 'To publish unchanged a thesis written over twenty years ago is a dubious enterprise and certainly requires an explanation.' He also notes in the first sentence that 'Professor Kaldor recalls a conversation with me about 1947 in which he expressed the view that "the best approach to distribution theory is macro-economic"' (Hahn, 1972, 1). The reason for finally publishing his thesis was that it was 'largely concerned with an exploration of the consequences for the distribution of income of a postulated difference in the savings propensities out of wages and profits. This avenue of exploration has been much discussed in recent years …' (i.e. in the 1950s Cambridge discussions about growth and distribution), but Hahn argued that long-run theory was 'basically uninteresting' and that he was

concerned with 'the study of disequilibrium, which I took to be the "normal" state of a capitalist economy.'

Kaldor, who had moved from the LSE to Cambridge and was probably instrumental in persuading Hahn to come to Cambridge in 1960, later wrote extensively on the importance of differences in savings out of wages and profits in determining the long-run share of wages in growth models (Kaldor, 1956, 1957). Hahn surveyed these models in the masterful work with Robin Matthews 'The theory of economic growth: a survey' (hereafter the *Growth Survey,* Hahn and Matthews, 1964) but without mentioning any connection with his own earlier work at that time.

In 1948, Hahn left for a teaching post in Birmingham University where he met, and found irresistible, Terence Gorman, who had 'by far the best and clearest mind I had yet come across' (Szenberg, 1992, 162). They remained lifelong friends, surprisingly different in manner but equally devoted to the application of mathematics to understanding and illuminating claims in economics. They both decided to accept professorships at the LSE in 1967, Gorman moving from a chair in Oxford while Hahn moved from Cambridge.

Birmingham under Gilbert Walker was one of the first departments to take mathematical economics seriously, and although small (it still had only seven staff in 1962) had in addition Alan Walters (later Hahn's *bête noire* as a monetarist and advisor to Prime Minister Thatcher), Michael Beesley, David Rowan and Esra Bennathan (who both remained life-long friends). While at Birmingham, Hahn supervised Maurice McManus' PhD, another distinguished mathematical economist.

In 1956 Hahn took a sabbatical at MIT, which was hugely influential, both in creating strong links to the leading American economists of the day, particularly Bob Solow and Paul Samuelson, and in reassuring him of the validity of the mathematical approach to economic theory. Hicks in *Value and Capital* relegated mathematical arguments to the appendix, rather as Marshall used mathematics to clarify his thinking, before putting the argument into words and relegating any essential mathematics to footnotes. The same was even more the case with Keynes, who consciously eschewed mathematics in his economics writings (opening himself to the criticism that he was as a result imprecise and failed to clinch arguments). This was despite graduating as the twelfth-best undergraduate mathematician at Cambridge in 1905 and starting his research on probability, producing an early draft of his *Treatise on Probability* in 1908.

Much of this very British approach to economics was the feeling that the 'real world' was too complex and chaotic to capture in a simple model,

and, as Keynes is claimed (after his death) to have said, 'It is better to be roughly right than precisely wrong.'[2] Hahn, always a mathematician at heart, had read Hicks and his contemporaries, but was hugely influenced by Samuelson's (1947) *Foundations of Economic Analysis*, which he cites in the first chapter of his thesis. Nevertheless, the thesis shows its origins in the literary style of his British contemporaries, in contrast to the more carefully axiomatic approach of the later Hahn. (Although he was sometimes willing to write what he would describe as 'blah blah' articles that reflected on debates in a more literary style.) Given the opacity of some of Hahn's mathematical arguments these were often a useful supplement to understanding the central points he was trying to make, and in some articles (e.g. Hahn, 1966) he would write 'some comments' at the end, in which he explained what was really going on behind the mathematical opacity.

The insight that so appealed to any mathematician encountering the *Foundations* was the revelation that maximising behaviour (firms maximising profits, or consumers maximising their utility) and the dynamic stability of market equilibria could generate powerful and testable propositions, the mark of a proper science. Later Hahn, enunciating his philosophy, stated that the claim of economics to be a science is premature and pretentious, but defended economics as useful and important. 'It provides grammatical arguments and methods for summarizing economic data. … a powerful aid to thought in providing clear limits to understanding, and can demonstrate genuine nonsense' (Szenberg, 1992, 163).

Terence Gorman further convinced him of the power of mathematics and was later to demonstrate that by employing maximisation to derive utility and profit as functions of competitive prices rather than quantities. That immediately made them directly applicable to observable market data and hence testable. Solow, reflecting on that first visit of Hahn to MIT, observes that 'By 1956, Hahn was already recognizably the economic theorist the world knows now' (Solow, 1984, 3). Hahn also met Ken Arrow, with whom he started a lifelong collaboration that resulted in arguably his definitive legacy—*General Competitive Analysis* (Arrow and Hahn, 1971), started when Arrow was visiting Cambridge in 1963–4.

Hahn was elected Reader in Mathematical Economics at Birmingham in 1958. In 1959 he visited the University of California at Berkeley where

[2] The correct attribution is to Carveth Read in *Logic, Deductive and Inductive* (1898, p. 351): 'It is better to be vaguely right than exactly wrong' <https://en.wikiquote.org/wiki/John_Maynard_Keynes>- (accessed 3 July 2017).

Kaldor was also visiting (as was Aubrey Silberston, also on leave from Cambridge, and Donald Winch, another lifelong friend). By all accounts Kaldor, by then at Cambridge, urged Hahn to come to Cambridge in 1960 to a lectureship, but it was Richard Kahn, perhaps surprisingly given his suspicion of mathematical economics,[3] who secured Hahn's appointment.

Churchill College was founded in 1960 as the national memorial to Sir Winston Churchill. In March 1960 the master of the embryonic college, Sir John Cockcroft, considered Hahn for a fellowship while he was still on sabbatical at Berkeley. It is fascinating to read some of the references solicited as part of that very careful scrutiny for the new college. Hicks, whose book had been the strongest influence on Hahn's early interest in economics, wrote rather disarmingly that 'he really is a good man and should go far'. Paul Samuelson, who had met him on his visit to MIT in 1956, wrote that he has the 'priceless ingredient (in our profession!) of enthusiasm'; In contrast with some coming from mathematics he has an 'excellent 'feel' for economically important aspects of a problem'. Kaldor, writing from Berkeley, was even more fulsome: 'an economic theorist of considerable ingenuity and subtlety' with 'exceptional intellectual ability' and 'scrupulous intellectual honesty' who 'gives his help unstintingly and generously to all who need it in their own intellectual problems'.

As Hahn's lectureship started on 1 October 1960, Cockcroft wrote in April 1960 to offer Hahn a fellowship from the same date. Hahn thus became a founding fellow, although as Churchill had no buildings he was hosted at Kaldor's college, King's, for a short period. He also became Director of Studies in Economics, responsible for admitting and arranging the teaching of economics undergraduates, a task that I took over from him when he left in 1966.

Hahn's arrival in Cambridge was like a refreshing blast for those wishing to drag Cambridge out of its disputatious and backward-looking controversies. Keynes had died in 1946, exhausted by wartime financial negotiations. Sir Dennis Robertson, a close collaborator of Keynes in the early development of *The General Theory*, had been cast out by Keynes and his circle for criticising some of Keynes's arguments. The bitter feuding of the 1930s continued after Robertson had retired and Keynes had died, carried on by Keynes's acolytes, Richard Kahn and Joan Robinson. Kahn had been responsible for bringing Hahn to Cambridge to continue the faculty's standing as a world-class economics faculty, despite his

[3] Kahn studied mathematics in his first year as an undergraduate, physics in his second year, but Finals of the Economics Tripos in his third year.

scepticism and even hostility to mathematical economics. Joan, however, was a fierce opponent of neoclassical economics, a position she maintained long after her retirement. Austin Robinson, husband of Joan, was a professor (as was Kahn, but not at that time), and both he and Joan had worked closely with Keynes in the 1930s. Austin and the Canadian economist Donald Moggridge were chiefly responsible for editing the thirty volumes of Keynes's *Collected Writings*.

Austin Robinson had been the prime mover behind the creation of a new faculty building, completed in 1961 and named after him on his ninetieth birthday in 1987. Harcourt's obituary describes him as '… the unsung hero of Cambridge economics. Through selfless service, often as secretary, sometimes as chairman of the Faculty Board of Economics and Politics, before and after the Second World War, Robinson, more than anyone else, enabled the various opposing factions of the faculty to coexist, and its intellectual life thereby to thrive.'[4] His peace-making was not always successful, and Austin and Joan could not have been more different, seeming distant from each other even when both in the faculty coffee room.

The Faculty of Economics and Politics (as it then was) already had an impressive set of academics when Hahn arrived. James Meade, who had spent a postgraduate year in Cambridge in 1930–1 working with Kahn on the development of Keynes's *General Theory*, had moved from a chair at the LSE in 1957 to become the Professor of Political Economy (the sole chair before the war and always the senior chair) and was later to win the Nobel Prize. Joan Robinson made his life a misery, to the point that he took early retirement to continue writing his books.

Richard Stone (Nobel Prize winner in 1984) had worked with James Meade for the wartime British Government and became the first Director of the Cambridge Department of Applied Economics (DAE, that Keynes had proposed before the war) from 1945–55. Under Stone, the DAE became a leading centre of economic theory and statistical methodology. With postwar funding from the USA, the DAE attracted some of the world's leading econometricians, including James Durbin and Geoffrey Watson. Stone's Directorship had been brought to an end by Kahn and Robinson, who blocked his reappointment as Director. The University elected him to the newly secured P. D. Leake Chair of Finance and Accounting, which Stone, to the dismay of the sponsoring firm, interpreted

[4] At <http://www.independent.co.uk/news/people/obituary-sir-austin-robinson-1489686.html> (accessed 3 July 2017)

as Social and National Accounting. The Faculty Board ensured that the DAE was entrusted to, in its view, a safer pair of hands in Brian Reddaway, Director from 1955–80, a commonsense economist sceptical of abstract theory.

Nevertheless, Stone, at the suggestion of Alan Brown, bundled up his various projects on consumer demand, input-output and national accounts into the *Growth Project*, separately funded (by the UK Social Science Research Council) but located in the DAE,[5] taking full advantage of the right, at that time, of professors not to have to lecture. Charles Feinstein (later Chichele Professor of Economic History at Oxford), joined the DAE in 1958 to work on National Income and Expenditure, and later Stone recruited Angus Deaton (Noble Laureate) and Mervyn King (later Governor of the Bank of England) to work on the *Growth Project*.

Michael Farrell had joined the DAE in 1949 to work with Stone and was subsequently appointed to a lectureship and later to a Readership in the faculty. Farrell, an editor of the *Review of Economic Studies* and fellow of the Econometric Society, developed empirical methods of identifying the efficient production frontier, and was the first economist to point out that under certain aggregation conditions, non-convexities need not rule out the existence of competitive general equilibrium. Farrell was therefore very much at home in Hahn's world of mathematical economics and the two much admired each other. One measure of the pettiness and vindictiveness of Kahn and Robinson is that when Kahn took over Keynes's Monday seminar, known as the 'secret' seminar,[6] Farrell, with some other prominent faculty like Malcolm Fisher and Ron Henderson, supporters of Robertson, were excluded. Meade and Hahn were invited to the 'secret' seminar, although Meade then withdrew.

Several other members of the faculty should be mentioned to demonstrate its pre-eminence and schizophrenia in 1960. David Champernowne had been at Cambridge as an undergraduate in the 1930s and was appointed a lecturer in statistics in 1938. In 1940 he was drafted into Churchill's wartime Government statistical department. In 1944 he was instrumental with Keynes in setting up the Department of Applied

[5] Until the Faculty closed it down (and took all its funds) the DAE housed the staff employed on all externally funded research projects. Reddaway had secured an excellent financial settlement when he became Director that enabled it to operate as an autonomous and self-sustaining budget holder.

[6] So-called to give the misleading impression that those specifically not invited would not know that they had been excluded.

Economics in Cambridge, but after the war he moved to Oxford to become Director of the Institute of Statistics from 1945 to 1948, and professor of statistics from 1948 to 1959. He apparently missed being in Cambridge and was willing to accept the more junior post of Reader there in 1959, which he held until he became Professor of Economics and Statistics in 1970.

Nicky Kaldor has already been mentioned, and was perfectly happy talking to both sides of the great divide in the faculty, frequently earning the wrath of Joan Robinson for his heretical views (such as the tendency of capitalist economies towards full employment). He later collaborated with Jim Mirrlees who helped with the mathematics of his growth model. Kaldor was a larger than life figure (as was Hahn), hugely engaging and a brilliantly perceptive economist, frequently remarking that the world was not linear—and his sideways profile certainly was not. Like Hahn, he was supremely self-confident, outspoken and could be hostile in seminars. Joan Robinson had '(in Shove's view) a very poor understanding of what neoclassical economics said about a lot of theoretical problems' (Harcourt, 1995) but was anxious to discuss economics with Hahn. On hearing this, Kaldor rang Hahn and said 'Don't you dare talk to that woman! She will steal my ideas from you.' Kaldor was concerned on this occasion about his theory of distribution and growth. According to Harcourt, Joan Robinson and Kaldor 'were "at it" with Joan Robinson trying to mend fences, Nicky being impossible, and Kahn stirring the waters whenever peace looked like breaking out'.[7]

Robin Matthews (who had graduated in PPE from Oxford) had been at Cambridge since 1949. As the author of the highly regarded 1954 *A Study in Trade Cycle History*, he was less in the neo-Keynesian firing line, and followed that up with the influential book on *The Trade Cycle* (Matthews, 1958). He was to join forces with Hahn to write the Hahn-Matthews *Growth Survey* (Hahn and Matthews, 1964) but left Cambridge in 1965 to become the Drummond Professor of Political Economy at Oxford from 1965 to 1975. Dick Goodwin, who had been a Rhodes Scholar in the 1930s, and described himself as 'a lifelong but wayward Marxist',[8] had arrived in Cambridge after being forced out of Harvard (where he had taught since 1942) by McCarthyism. Goodwin had also published two articles on the trade cycle (Goodwin, 1953, 1955), worrying, as had Harrod in his original article, about how the short-term rate of

[7] Email of 23 August 2016.
[8] Desai and Ormorod (1998).

growth determined by investment could bear any relationship to the long-run rate of growth determined by technology and population growth. Goodwin, as a well-trained American economist, was quite comfortable with sophisticated mathematics, and later constructed a very elegant predator–prey differential equation model of the trade cycle (Goodwin, 1967).

Luigi Pasinetti had been a student of Piero Sraffa and Richard Kahn in the 1950s and returned to Cambridge (and King's College) at the behest of Kahn in 1962. He followed Kaldor in studying the impact of different savings propensities on the distribution of income, but in this case distinguishing the savings behaviour of workers (who might receive profits) and capitalists who received profits (Pasinetti, 1962). That model was also reviewed in the Hahn-Matthews *Growth Survey.*

Cambridge in 1960 when Hahn arrived therefore had a distinguished but factious faculty, although Ken Binmore, in his obituary in *The Times* (3 February 2013) and writing from the distance of the LSE, gave a rather more downbeat assessment: 'In the time of such Cambridge luminaries as Marshall and Keynes, British economists led the world, but the mantle of leadership had passed to America by the 1950s, and Cambridge had become an ineffectual talking shop. However, a new generation of young economists led by Frank Hahn kept the United Kingdom in the running by abandoning the literary style that had been the norm since Adam Smith.'

Christopher Bliss, who read economics from 1959 to 1962, in correspondence before Hahn died, wrote:

> Frank Hahn came to Cambridge in 1960, when I was a second-year undergraduate. The important point to appreciate concerning the Cambridge Economics of that time is that it suffered greatly from the mess that Keynes (who died 14 years earlier) had left behind him. Despite his unquestionable greatness, Keynes could be an arrogant and intolerant person. Once his ideas had settled he often had no time for anyone who ventured to disagree with him. This affected in particular Sir Denis Robertson, who had been his chum, but who refused to be born again into the new Keynesian faith, although he pursued a revised macroeconomics of his own. The aftermath of all this was a sharp and fractious division in the Faculty between the 'neo-Keynesians' (notably Kahn and Joan Robinson) and the 'Neoclassicals' (in particular James Meade).[9] Like the religious divisions of Sixteenth-Century Europe, this mainly proved to be unproductive, with different parties talking past each other, although it did sometimes spin off new ideas and good work. Broadly one can say that the 'Keynesian'

[9] Meade was a lifelong Keynesian, and not simply a 'neoclassical' economist.

> stream was backward-looking and sterile. The 'Neoclassical' stream on the other hand, for all its faults, and they were several, was dynamic and creative; part of a world-wide research programme involving mathematical economics, game theory and econometrics. The arrival of Frank Hahn into this fetid atmosphere was like a breath of fresh air, and the same could be said of Amartya Sen's influence. Both these men refused to sign on to one side or the other in the holy war of Cambridge Economics. Yet they both communicated vigorously with all parties, and they were evidently too strong to be ignored.[10]

Later, in another communication, Bliss wrote of Hahn: 'His impact was huge. Like John Nash he had a beautiful mind, and his energy was massive. Aside from conducting his own research, he engaged with anyone who cared to talk to him. He was not intimidated either by distinction or by aggression from Joan Robinson.'[11] Later on Hahn and the growing number of young Turks that he attracted were to set up the Churchill seminar as a rival to the 'secret seminar', which they outcompeted in attracting attendees, until Kahn dissolved the seminar in 1969.

However, in the early 1960s the 'secret seminar' was still in full swing, with Hahn very much in attendance. Harcourt recalls that in November 1963 Ken Arrow, visiting Cambridge and Churchill College, read from the proofs of his classic 1963 paper 'Uncertainty and the welfare economics of medical care', while the following February Bob Solow, who was also visiting Cambridge (and gave the Marshall Lectures on 'Effective Demand and Capital Theory'), commented on a draft of the Hahn-Matthews *Growth Survey*.

I arrived at Trinity College to read for Part II of Mathematics Tripos in 1961, and, after taking Finals in 1963, and unclear what to do with my third year, met with Jim Mirrlees at the suggestion of a fellow Scot and my Director of Studies, Keith Moffatt. Mirrlees had no difficulty in persuading me to read for Part II of the Economics Tripos (1963–5). My first encounter with Hahn was in 1964, attending his lectures on general equilibrium. They were quite brilliant, not only for setting out the elegant and powerful theory but also for pointing out its limitations—its inability to account for the role of money, and hence for the problems that afflict real-life economies, such as unemployment.

Monjit Chatterji recalls a similar experience of Hahn: 'His Lectures were exhaustively demanding. Frank himself made no bones about it. His cyclostyled lecture notes with hand drawn diagrams (without the benefit of a French curve) were a real *tour de force*.' Chatterji cites from Hahn's

[10] Email, 2013.
[11] Memorandum from Christopher Bliss, August 2016.

handout: 'The lectures (and notes) are intended to serve a small minority of those reading Economics. They are not required for examinations. Moreover there are many successful practical economists everywhere who are quite innocent of the matters to be discussed so that the lectures are not required to ensure comfort in later life. There are also many people who regard this kind of careful and abstract approach as not worthwhile and they are just as often respected academically as those who hold the opposite view. So the lectures are not required for academic respectability.' And again '… if you do not like "difficult lectures" these are not for you' (Chatterji, 2013).

No undergraduate could be unaware of the seething disagreements within the faculty. Joan Robinson was lecturing as though their school (the post-Keynesians) had refuted neoclassical economics, while Hahn and others were providing convincing arguments (to at least the majority of students) that this was far from the case. Later, as a final-year student, I was slightly shocked to hear Hahn crowing that he had just delivered the final nail in the coffin of capitalism, perhaps referring to his paper on 'Equilibrium dynamics with heterogeneous capital goods' (Hahn, 1966). Hahn was always quite clear about important distinctions such as that between the efficiency of competitive equilibrium and its distributional justice, and between equilibrium as a concept and its likelihood of describing an economy at any moment. In contrast, the Kahn-Robinson clique (and many of their circle) considered that undermining neoclassical economics was essential to remove what they saw as its justification for the existence of profits in a capitalist society.

We were all convinced that economics was of the utmost significance, so passionate were the arguments we were exposed to. It helped that Keynes was viewed as the economist who had provided the tools to enable governments to avoid the Great Depression, and that numerous faculty members were for ever rushing off to advise the Government of the day. Reddaway (then Director of the DAE and the inventor of war-time points rationing) convinced us to become familiar with the *Blue Book* of National Accounts (and other statistical publications) and that we should be able to ground our arguments using such statistical evidence in language that an intelligent, but non-economist, civil servant could understand.

Tony Atkinson, who graduated from Churchill College in economics in 1966, recalls that he first met Hahn as his Director of Studies. His initial greeting was characteristically off-putting: 'are you as stupid as you look?' Later, after an initial year's supervision with Jan Graaff (another Churchill Overseas Fellow invited by Hahn), Atkinson faced Hahn's 'idiosyncratic—

and sometimes counter-productive—style of teaching: 'Surely', he would say of my essay, '"you can do better than Professor X", the aforementioned X having recently published an article on the topic in a leading journal. I like to think that we got on well, perhaps because I did not take him too seriously, or perhaps because I took him seriously when he meant to be.'[12]

Churchill College's founder Winston Churchill had a vision of the college as an MIT for Britain and the statutes require that 70 per cent of its students are in sciences, engineering and mathematics. At its foundation, the college received a generous endowment to invite distinguished American academics (later extended to all nationalities). Following Churchill's vision, Hahn thought that Economics could play a similar role in a science- and engineering-oriented college as it did in MIT. He took delight in persuading a steady stream of future Nobel laureates and other almost equally distinguished economists to visit the college for between one and three terms.

The first buildings in the college were the Sheppard Flats, named for the architect of the whole college, wonderfully located in a cluster surrounded by formal gardens and trees, looking down over the playing fields to the college buildings proper, and these were provided to the Overseas Fellows. In short order, Arrow, Diamond, Uzawa, Radner, Solow, Maskin, Debreu and Scarf arrived to stay in the Sheppard Flats and visit the faculty. Joe Stiglitz was visiting from MIT in 1965 (and indeed spent a tumultuous few weeks nominally as Joan Robinson's student, before moving to Hahn) and was rapidly elected to the Tapp Research Fellowship at Gonville and Caius College, Cambridge (1966–70, although he was Assistant Professor of Economics at MIT, 1966–7, before moving to Yale).

The faculty was also recruiting young economists. Jim Mirrlees was the first. Mirrlees had taken his PhD in 1964 with a thesis on 'Optimum planning for a dynamic economy' (supervised by Richard Stone) and was appointed to the faculty in 1963. Although Hahn had recruited me to replace him as the economics teaching fellow at Churchill College starting in 1966, I spent my first year after graduation (1965–6) as an Overseas Development Institute Nuffield Fellow in the Treasury of the Tanzanian Government. I was invited while there to apply for an assistant lectureship in the faculty, and was appointed without even an interview, so powerful was Hahn's influence at that time. Christopher Bliss, who had been an

[12] Atkinson's talk at Hahn's memorial event.

undergraduate and then PhD student at Cambridge, gained his PhD in 1966 on 'Capital-labour substitution and economic growth', and was appointed to a lectureship also in 1966. A year later Tony Atkinson was appointed (also a year after graduating—those were the days) and we shared an office, before he and Bliss were both appointed professors at Essex University. Partha Dasgupta had also switched from Mathematics to Economics and was being supervised for the PhD under Mirrlees, winning a Research Fellowship at Trinity Hall in 1968.

I was a seriously undereducated economist, at that time without any graduate experience. Hahn immediately saw that I needed some further education and arranged with his friend Herb Scarf to invite me for a sabbatical term at the Cowles Foundation in 1969, where Joe Stiglitz was now an Associate Professor. Joe ran an informal small seminar that provided the bulk of my exposure to research in economics, while sharing an office with Atkinson provided further stimulation.

Hahn was offered a chair at the LSE in 1966 at the same time as his close friend, Terence Gorman, making the offer irresistible. The college then elected him to a Title E Fellowship (E for Extraordinary, which certainly fitted Hahn perfectly). He continued to live in Cambridge (in the house in Adams Road that he purchased from Matthews when he moved to Oxford), and continued to engage with the young Turks in the faculty, at the rival Monday seminar held in Churchill College and at his house, where he hospitably entertained with Dorothy. (Dorothy was teaching economics at Newnham College as well a holding the very responsible position of Bursar, and came to my rescue by looking after first-year students when I took up my fellowship as a totally inexperienced supervisor.)

Hahn arrived at the LSE in 1967 like a whirlwind, just as he had at Cambridge. Richard Jackman (taught by both Hahn and myself at Churchill) wrote in the LSE's obituary:

> According to the folklore LSE Economics was in decline and the then Convener, Ely Devons, was advised by his junior colleagues that the situation could be saved only by the appointment of world class (as we would now say) scholars. It is alleged that each of those recruited (Frank Hahn, Terence Gorman, Harry Johnson and Alan Walters) agreed to come only in the belief that the other three had already accepted. Though not long at LSE (he returned to Cambridge in 1972), the years he spent here were amongst the most momentous in the history of the Department. Hahn and the other newly appointed professors set about imposing serious academic standards with traumatic consequences. Several

> junior lecturers were denied tenure on the grounds, which seemed extraordinary at the time, that they hadn't published anything.[13]

Dasgupta's (2013) obituary for the Royal Economic Society echoed these comments: 'They (Hahn, Gorman and Sargan) re-structured the graduate programme into its modern form, persuaded the other Professors to call a moratorium on appointments to Lectureships until a suitable cohort had been trained (David Hendry and Stephen Nickell were among the first of the new batch of Lecturers there), and organized the establishment of Chairs so as to attract Amartya Sen and Michio Morishima.' Desai remarks that 'He even on occasion came to the Econometrics Workshop which Denis Sargan ran.' Desai also recounts a wonderful occasion at the Association of University Teachers of Economics (AUTE) meeting in Aberystwyth in April 1972: 'As we all trooped into a large hall completely packed, Frank came on stage and began "Although you see a small Hungarian (sic[14]) Jew before you, let me tell you that I am John the Baptist. I have come to tell you about what is coming." Having got our attention, he went on to give a memorable lecture about what was passionately occupying him at that time. This was the collective effort by several young theorists and himself to integrate money into Walrasian General Equilibrium theory. He told us about the young French theorists Jean Michel Grandmont, Jean Pascal Benassy, Roger Guesnerie et al. He got us all engaged in what he told us was an absolutely central problem of economics.'

Meanwhile, back in Cambridge, the disputes and disagreements in the faculty were going from bad to worse, hardly providing an attractive place to stay. Mirrlees had moved to a chair in Oxford in 1969, shortly followed in 1971 by his former student, Partha Dasgupta, who moved to the LSE. Bliss left for a chair in Essex in 1971, as did Atkinson. Geoffrey Heal, an undergraduate student of Hahn with Atkinson in Churchill, and later a PhD student there, was appointed Assistant Lecturer in 1969 but left for a chair at Essex in 1973, following Atkinson and Bliss. Later the faculty was to fail to appoint Angus Deaton (a subsequent Nobel Laureate who left the DAE for a chair at Bristol in 1976 before emigrating to the USA), just as they failed to promote Oliver Hart (another future Nobel Laureate),

[13] At <http://www.lse.ac.uk/economics/newsEventsSeminars/files/EconomicsReview20122013.pdf>.

[14] Hahn described his father as an Austro-Hungarian with a bust of Franz-Joszef on his desk, but Frank was born in Germany.

despite support from Hahn, and lost him first to the LSE and then to MIT.

Aubrey Silberston had been Chairman of the Faculty Board and found it a challenging time to be running the Faculty, for despite his diplomatic skills the factions continued to fight. The Kahn-Robinson axis fought for control of the Faculty Board in order to appoint the Appointments Committee and make nominations to professorial electoral boards, thereby controlling appointments to the Faculty. 1968–9 saw the students in economics and sociology playing a leading part in their version of the student revolution, and the faculty left was now divided between the Maoists and the anti-Maoists (this was a period in which Joan Robinson was impressed with the Chinese approach to economic development).

The Appointments Committee started making doubtful appointments that sometimes seemed based more on whether the candidate signed up to the Kahn-Robinson line than whether they were potentially outstanding. Kaldor manoeuvred the Electoral Board to appoint Robert Neild as Joan Robinson's replacement when she retired from her professorship in order to keep Hahn out, even though Kaldor and Hahn lived in Adams Road and continued to see each other. Silberston (Faculty Chairman) had a terrific row with Kaldor over Neild's appointment, because by then Kaldor had become anti-mathematical and also anti-Hahn. Apparently Neild's appointment in preference to Hahn caused so much outrage that when Richard Kahn retired eighteen months later, Hahn was elected to Kahn's chair in Economics in 1972, and to a professorial fellowship at Churchill.

Hahn's inaugural lecture, given in February 1973, was entitled 'On the notion of equilibrium in economics' and followed hot on the heels of *General Competitive Analysis* (Arrow and Hahn, 1972), begun when Arrow had been visiting Hahn at Cambridge in 1962–3. In it he also responds sharply to Kaldor's (1972) 'On the irrelevance of equilibrium economics', criticising him for his incorrect view of Debreu's classic (1959) *The Theory of Value*. In the same year Hahn was to publish another critique, this time of Kornai's book *Anti-equilibrium* (Hahn, 1973b), so Hahn returned to Cambridge in combative mood.

The Cambridge faculty, in contrast to the LSE that Hahn left, was neither a happy nor intellectually vibrant environment in 1972. Dasgupta observed that 'Hahn faced an insular and worse-than-mediocre Faculty, displaying nevertheless an academic self-confidence unsurpassed anywhere I have seen. … Unable to modernize the Faculty (the best deal he was able to reach with those wielding political power in the Faculty Board

was to have one Lectureship appointment of his choice for every three), Hahn made a move that displayed for a second time his gifts as an academic visionary and administrator. He obtained, what would be impossible today, a loosely specified research grant for studying risk and incentives from the then Economic and Social Research Council.'[15]

Hahn's intellectual energy knew no bounds and he used this remarkably successful research programme to redress some of the shortcomings of General Equilibrium theory—its lack of a theory of unemployment, money and market adjustments. The so-call *Risk Project* attracted an amazingly impressive group of young researchers such as Eric Maskin, David Kreps, Oliver Hart, Mark Machina, Lou Makowski, Douglas Gale, Ben Lockwood, Jonathan Thomas, Paolo Gottardi, David Canning, Bob Evans, Paul Seabright, Luca Anderlini, Costas Gatsios and David Kelsey, as well as many members of the Faculty, such as myself. Most of the researchers funded under this project went on to distinguished academic careers, many in the United States; Paul Seabright moved from Churchill College to Toulouse in France. Some fortunately stayed in Britain (where those who left Cambridge all became professors).

Hahn's weekly internal *Risk* seminars were typical of his idiosyncratic but effective research style. They became known as 'Quaker' meetings, as they had no formal agenda but let the spirit move participants to speak—if they were quick enough to seize the chalk. Newly minted post-docs could hold forth before visiting Nobel laureates, rapidly gaining insights, experience and confidence that stood them in good stead later on. These Quakers were intensely productive, producing a steady stream of green discussion papers that in those pre-pdf times were posted around the world, signalling the vigour of the Hahn enterprise.

The 1970s were turbulent times politically, with the high inflation following the oil shocks of 1973, strikes and labour unrest culminating in the 'winter of discontent' of 1978/9 (curiously echoing the title of Hahn, 1973b). In 1979 the country elected Margaret Thatcher's Conservative Government to replace a failing Labour Government. Hahn was much incensed by the monetarist advice his former colleague, Alan Walters, had been providing the Government. In the view of Milton Friedman, much lauded by Sir Keith Joseph and other Conservatives, high inflation was simply due to an excessive expansion of the money supply. This is where the great value of the Cambridge faculty coffee room showed its worth. The faculty (and the DAE) met for coffee every day in term time, and

[15] See <http://www.res.org.uk/view/article3Apr13Correspondence.html> (accessed 3 July 2017).

argued vigorously not just over theory (these were disputatious academics, after all) but also about policy (many of the faculty were active policy advisors and commentators).

Perhaps surprisingly, given their prior history, Hahn and Neild (who had pre-empted Hahn's earlier return) criticised monetarist doctrine in an article in *The Times* (25 February 1980) 'Monetarism: why Mrs Thatcher should beware'.[16] Friedman responded aggressively, claiming that reducing monetary growth 'may increase unemployment temporarily, to be rewarded by a much sharper reduction in unemployment later' (*The Times*, 3 March 1980). By 1981 unemployment had risen from 7 per cent in 1980 to 10 per cent in 1981. Geoffrey Howe, Chancellor of the Exchequer, delivered the Budget on 10 March 1981, after which Hahn and Neild sat down in the coffee room and drew up a response, circulated it and secured the signatures of 364 economist academics (including themselves) to a letter published in *The Times* on 29 March 1981, stating that:

a) There is no basis in economic theory or supporting evidence for the Government's belief that by deflating demand they will bring inflation permanently under control and thereby induce a recovery in output and employment;
b) present policies will deepen the depression, erode the industrial base of our economy and threaten its social and political stability;
c) there are alternative policies; and
d) the time has come to reject monetarist policies and consider urgently which alternative offers the best hope of sustained economic recovery.

By 1984 unemployment reached 12 per cent and remained above 10 per cent until the end of 1987, accompanied by a rapid fall in inflation from 18 per cent in 1980 to 5 per cent in 1984. There was much discussion about these policies (the Thatcher revolution also set in motion subsequent waves of privatisation and rolling back the frontiers of the state). Neild's measured assessment written in 2012 was that 'When inflation struck in Britain the necessary response was (a) a short hard dose of deflation and (b) a radical reform of the trades unions. A hard-headed Keynesian analysis, or common sense, would have led to that conclusion. But before 1979 Labour and Conservative governments jibbed at such

[16] *The Times*, 25 February 1980, p.19. I am indebted to Robert Neild's note 'The 1981 budget and the letter by 364 economists', 23 July 2012.

harsh policies, and so did the great majority of economists of whom I was one. Much as I abhor the social philosophy of Mrs Thatcher (and her follower, Mr Blair) I now give her credit for having introduced these two controversial policies that were necessary to check inflation—though I deplore the fact that monetarism so blinded the government that it pressed home deflation too hard and too long' (Neild, 2012).

Peter Clarke, then writing his *The Keynesian Revolution in the Making*, remembers discussing with Hahn that Keynesians would agree that deflationary policies (a slump) would reduce inflation, along with the economic activity that generated it. Howe's 1981 budget, though deflationary, acted mainly through fiscal policy, and the monetary targeting was thereafter more a political charade to cover this shift in policy.

Hahn spent many of his summers visiting his close colleague, Kenneth Arrow, at Stanford, which annually gathered an impressive range of visitors to the Institute for Mathematical Studies in the Social Sciences (IMSSS). In 1992 his colleagues presented him with the suitably weighty Festschrift on his retirement from Cambridge (Dasgupta et al., 1992). Of course, Hahn could not retire from active economics, and promptly took a post in Siena, and he and Dorothy (whose Italian was considerably better than Frank's) moved, living in a modest flat within the city walls, very supportive of their local Contrada (Giraffa). He would invite his colleagues for stimulating conferences in the glorious Certosa outside the city walls in a monastery on a Tuscan hill—a wholly suitable place for Quakers to meet, though with the gastronomic delights to tempt a pope. As Hahn says in his autobiographical notes (Szenberg, 1992, 160), mentioning a Borgia pope (Leo X) 'God has given us the papacy, now let us enjoy it'—very much the spirit in which he and Dorothy entertained colleagues and students at seminars at his Cambridge house, as well as the dinner parties for visitors and friends. I have vivid memories as a guest of Secondo Tarditi with the Hahns at the Palio in Siena on one such visit to the Hahns.

In his obituary, Dasgupta (2013) paid tribute to the closeness of their marriage: '… over the nearly five decades that I knew him it has always seemed to me that without Dorothy there would be no Frank. She was the practical and emotional centre of his life; she had a professional career, but it was her support at home that enabled him to spend his days thinking, reading, scribbling (his words), conversing, and listening to music and to others. He loved his garden at 16 Adams Road, but beyond cutting the odd flower head, I don't believe he did any gardening himself. I cannot

remember an occasion when on arriving at his home for tea or a glass of wine I didn't find him reading.'

If there is one theme that runs through Hahn's life, it was to attract the right people and move decisively to support them. He was also intensely loyal to the college right from its foundation to the last days of his life, indeed he was sitting next to me at dinner in college when he was taken ill at the end of his life (he died on 29 January 2013). He believed passionately in the merits of the Cambridge supervision system and the Socratic Method—which his Quaker seminars and the Churchill seminars exemplified. He brought outstanding teaching fellows to Churchill—Oliver Hart, Roger Witcomb, Margaret Bray, Jayasri Dutta and others.

He was delighted when first Douglas Gale and then David Kelsey became Junior Research Fellows (both moved quickly to become professors), and subsequently Daniel Sgroi joined Churchill first as a Junior Research Fellow before becoming a teaching fellow. Junior Research Fellows in economics are relatively rare in Cambridge, partly because in the past the natural career path normally involved a lectureship fairly soon after the PhD, partly because economists are so much more critical of each other than those in other subjects, whose students invariably walk on water. But Hahn appreciated that time to pursue research without the pressures that a full-time faculty appointment requires could be of immense value for an economist at the start of his or her career. The *Risk Project* supported many such, often in partnership with Churchill where they supervised—and the combination of young and enthusiastic researchers rubbing shoulders with their seniors at Quaker meetings and then passing on that enthusiasm to undergraduates at Churchill in turn prompted many of them to pursue glittering academic careers.

In that spirit Hahn strongly supported the College's proposal to create the Hahn Fellowship in Economics—and became its overwhelmingly most generous donor. It is a fitting memorial to his contributions to the profession, his college and Cambridge friends.

Works

The early years

Hahn was greatly influenced by Hicks's (1939) *Value and Capital* and even more by Samuelson's (1947) *Foundations of Economic Analysis*. Paul

Romer, in his blog,[17] gives a clear sense of the significance of 'the Samuelson program', starting from the observation that from 1890 to 1940 'economists avoided the use even of calculus and spent 50 years mired in the confusion spawned by the talky, market-by-market, supply-and-demand-ish approach to economic analysis codified in 1890 in Alfred Marshall's *Principles of Economics*. Samuelson saw that recovery for economics would require both the precision of mathematics and a commitment to models that could handle more than two variables at the same time.' Samuelson himself later wrote that 'Shortly after 1930 economics burst out into new life. At least four revolutions erupted: the monopolistic competition revolution, the Keynesian macro revolution, the mathematicization revolution, and the econometric inference revolution' (Medema and Waterman, 2014, 26).

One relevant example that demonstrated the power of general equilibrium over simple market-by-market analysis was to take just two industries with constant returns to scale in capital and labour and therefore perfectly horizontal Marshallian supply curves with apparently constant prices independent of demand. Stolper and Samuelson (1941) showed that imposing an economy-wide constraint on the amounts of labour and capital gave a smooth production possibility frontier that showed that prices would vary with the pattern of demand—familiar enough in the Edgeworth tradition but lacking in the atheoretical Marshallian approach. Not surprisingly, Hahn saw the importance of rigorous proofs, requiring mathematics, a general equilibrium approach and the significance of the Keynesian revolution, which still lacked a rigorous foundation.

Harrod (1939), a strong supporter of the Keynesian revolution, was to influence Hahn's approach to economic stability, and whether there was any reason in the longer run for an economy to be able to sustain full-employment growth. Hahn's PhD thesis 'The share of wages in national income' demonstrates his early commitment to the application of micro-economic theory to understanding macro-economic phenomena, consistent with his first supervisor Kaldor's advice that 'the best approach to distribution theory is macro-economic'.

Keynesian macroeconomics calls for a general equilibrium approach in the loose sense that agents in different sectors (consumers, investors, banks, not to mention the rest of the world through trade and capital

[17] See <https://paulromer.net/what-went-wrong-in-macro-historical-details/≥ (accessed 3 July 2017).

flows) interact to determine the level of output, employment, prices and the rate of interest. Keynes and his followers were also concerned with equilibrium, and whether in particular an equilibrium with unemployment could persist (that is, was stable) without a strong tendency to return to full employment. However, the theory of general equilibrium in the narrower sense (GE for short) of a full specification of agents, endowments, preferences and production possibilities interacting through markets was seriously incomplete in 1945. This was despite 'a long and fairly imposing line of economists from Adam Smith to the present who have sought to show that a decentralized economy motivated by self-interest and guided by price signals could be compatible with a coherent disposition of economic resources that could be regarded, in a well-defined sense, as superior to a large class of alternative dispositions. … it is important to know not only whether it *is* true, but also whether it *could* be true' (Arrow and Hahn, 1971, vi–vii).

Hahn (1977) later commented on the relationship between Keynesian economics and GE, noting that GE theorists 'have been unable to deliver one half at least of the required story: how does general equilibrium come to be established? Closely related to this lacuna is the question of what signals are perceived and transmitted in a decentralised economy and how. The importance of Keynesian economics to the general equilibrium theorist is two-fold. It seems to be addressed to these kinds of questions and it is plainly in need of proper theoretical foundations.'

Given this background, it is not surprising that Hahn began to develop a proper general equilibrium model with Keynesian features as early as his doctoral thesis. Later, he, with many of his mathematical economic contemporaries, would gradually develop a fully rigorous theory of general competitive equilibrium. This would require a study of stability and, crucially, whether it was possible to include money in the theory of general competitive equilibrium.

In his PhD thesis Hahn embeds a risk-averse imperfectly competitive firm with some power to influence prices in a one-good general equilibrium model of the whole economy. His interest goes beyond the Keynesian problem of the determination of output and employment to investigate what happens to the share of wage and profits in total output, building a rigorous model of the short-run evolution of the economy from an initial starting point. The first chapter notes a number of problems with the classical production function approach, specifically that it conflicts 'with what we now know about the role of money in the system'. He notes that 'much of modern trade cycle theory is based on the assumption that such

(a dynamic general) equilibrium is in fact unstable, so that … a "comparative dynamic equilibrium" approach to distribution may be impossible', and that if 'the assumption of perfect competition is dropped … the production function approach is useless'.

Solow (1992) stresses the novelty of both considering imperfect competition in a general equilibrium model and allowing firms to be risk averse, developments that became more familiar considerably later. The third novel element was to allow the supply of savings to depend on the distribution of income, as Hahn argued that workers would save less than firms (or their owners). This last element was much used later by Kaldor and other Cambridge post-Keynesian economists such as Pasinetti, often to argue against the idea that wages and profits were returns to labour and capital, and therefore ethically defensible. Kaldor (1956) summarises Kalecki's theory as 'capitalists earn what they spend, and workers spend what they earn', which suggests that profits are not earned and therefore not ethically defensible.

Hahn (1972), in the preface to his (belatedly) published thesis, reacts to the muddle in which he finds the debate. On one side 'neoclassical practitioners have not been able to resist the temptation to make the theory yield simple answers to sociologically motivated questions' such as the distribution of income. In contrast, the other side has 'criticised it on logical grounds where, as it happens, it is particularly robust. To make matters worse the controversy has been overlaid by ideological clap-trap: the neoclassical theorist is said to be justifying the *status quo* while his opponent is the harbinger of progress.'

In his collected works (Hahn, 1984b, 1–2) Hahn set out very clearly what he meant by a neoclassical economist:

1. 'I am a reductionist in that I attempt to locate explanations in the actions of individual agents.
2. In theorising about the agent I look for some axioms of rationality.
3. I hold that some notion of equilibrium is required and that the study of equilibrium is useful.'

Hahn goes on to comment on the first point that while he has no problem with the idea of class, any theory of class interest would on his view need to be grounded in the interests of individual members. The notion of equilibrium is of course central to GE theory, but far wider than that. The Hahn-Matthews *Growth Survey* discusses the confusions of interpretation and the dubious nature of such political-economic claims at considerable length.

The final building block in the PhD model is the demand for investment, which looks like an accelerator function of national income. Together these relations give rise to an equilibrium in which the supply of savings that depends on the share of wages in total income is equal to the demand for investment, which is an increasing function of the profit margin. Firms will be happy when they can sell what they produce (in this imperfectly competitive world in which they cannot sell any amount at an externally set price), and in turn demand, income, wages and profits are such that supply and demand are balanced. Given this model it is possible to explore how its elements (output, the share of wages, the level of employment, etc.) respond to various shocks or parameter changes—very much part of the Samuelson programme.

Solow's celebratory essay on Hahn's PhD commends its delivery of a complete short-run macro-model, but goes on to say 'I resist the 1972 Hahn's rejection of loosely aggregative economics, and I would defend the approach of the younger Hahn against his maturer self' (Solow, 1992, 16). Earlier, Solow remarks that his own belief is that 'economics, as an applied science, is about approximations, not theorems. So I have a deep interest in the aggregative use of microeconomic ideas.' It says much for their mutual respect and friendship that such apparently disparate views of the subject did not prevent them collaborating together on a major book (Hahn and Solow, 1995).

Money and general equilibrium

During the first part of his academic career, Hahn was still formulating his response to the Keynesian revolution, and specifically whether Keynes's claims could be true and, if so, under what assumptions. His first paper after those of his PhD was a critique of Patinkin's attempt to include money in a general equilibrium model (Hahn, 1952, 1960a), arguing that it 'failed to model the essential intertemporal aspect of money' (Hahn, 1985, 2). The disputes of this period are reflected in his paper on the rate of interest (Hahn, 1955), which revisited the old Keynes-Robertson dispute over Loanable Funds (LF) vs Liquidity Preference (LP) as 'determining' the rate of interest. Hahn dismisses this confusion as one of determining the period chosen for consideration. The LF theory differs from the LP theory 'only in so far as it is concerned with the value of the rate of interest at any one moment of time during the income period'.

Clearly this was a hotly disputed area, and Hahn acknowledges prior interactions with Joan Robinson, Harrod, Patinkin and Harry Johnson. It

resulted in the 1962 IEA conference 'The theory of interest and money' to examine developments since Keynes's *General Theory of Employment, Interest and Money*. In the preface to the conference volume Hahn notes that 'all essential points of interest may be lost if we construct models of a monetary economy in which price uncertainty and market imperfections have been assumed away' (Brechling and Hahn, 1965). The concept of equilibrium becomes problematic in the face of differing expectations, and strengths of belief in the absence of a complete set of futures markets. The main contribution of the conference was not any new theory but the clearing away of various obscurities. Hahn was clearly somewhat disappointed that a successful integration of real and monetary forces failed to emerge, leaving growth theory in an unsatisfactory state. The role of money in satisfactory equilibrium models was to engage Hahn for the rest of his career (Hahn, 1965, 1971, 1973, 1975).

In 1968 Jerome Stein secured NSF funds for a 'Conference on money and economic growth' at Brown University, subsequently published in the then new *Journal of Money, Credit and Banking* in 1969. It was intended to discuss 'the basic questions (that) concern the extent to which financial policies and institutional arrangements can affect the time profiles and steady-state values of the capital–labor ratio (or capital intensity) $k(t)$, the real wage $w(t)$ and the rent $r(t)$ per unit of capital' (Stein, 1969).

Hahn's (1969) paper starts 'Economic theory still lacks a "Monetary Debreu".' Hahn constructs a simple neoclassical growth model with money to see whether, given active government intervention, 'mediation by money must restrict the accumulation choices of an economy. The answer is no, and so, in a proper sense, for a rational society, money is neutral' (Hahn, 1969, 180). On the other hand, the government is required to be active (taxing, transferring and investing) to ensure a desirable outcome (or dynamic evolution).

It should be hardly surprising that there is a problem in providing a motive for holding non-interest bearing money in a competitive general equilibrium model which, in its properly articulated form, has a set of well-defined, homogeneous products, whose prices are already known at every date in the future (and, in the extension to deal with risk, prices in each state of nature). As there are no transaction costs, contracts (to borrow) can be costlessly enforced, and as agents cannot influence prices, they can draw up their lifetime work, asset accumulation and consumption plan when they reach the age of competence. There would be no need for money either as a means of exchange or as a store of value.

Hahn (1975a) sets out his views on the subject in a non-technical way, pointing out all the difficulties involved in the path along which others such as Patinkin and Clower had set out. They start with the axiom that only money buys goods, and then consider sequence economies in which agents hold money from one period to the next. In such economies agents must form expectations about future prices. Even supposing that these price expectations are correct, there are problems in establishing the existence of a short-period equilibrium. The article is typical of Hahn's approach to the subject, setting out clearly the gaps in the theory and the unsatisfactory nature of the current state of understanding, before concluding that we are still far from a satisfactory theory.

In their definitive summary of GE theory, Arrow and Hahn (1971) turn, in their concluding chapter 13,[18] to the Keynesian Model and the relation 'of certain features of this model to what has gone before' (i.e. GE theory). Money and the lack of futures markets, with the implication that expectations are important, immediately introduces the idea of bankruptcy, and that, as they put it, 'may make it impossible to guarantee the continuity properties of the various functions and correspondences and this is bad for existence proofs' (Arrow and Hahn, 1971, 354).

One of their key questions is whether Keynes discovered 'features of an economy that … make it impossible to establish the existence of a temporary equilibrium' (Arrow and Hahn, 1971, 354) in which all markets including the labour market clear and the prices of labour and money are non-zero). While they are not claiming to be able to establish any general non-existence results they are able to construct an example in which there exists no temporary equilibrium, thus vindicating a central Keynesian proposition.

This leads to a discussion of what might happen out of equilibrium, when a temporary equilibrium does not exist. After quoting Keynes on how money wages might respond to unemployment (and what might then happen to real wages) they note that it is hard to relate the kinds of adjustment processes they have considered and 'the kind of forces that Keynes thought to be important. This is partly due to the fact that he was quite imprecise in these matters, but *largely because a precise formulation would be extremely complex*' (Arrow and Hahn, 1971, 367, emphasis added). Their conclusion is 'that the Keynesian revolution cannot be understood if proper account is not taken of the powerful influence exerted by the

[18] Chapter 12, section 6, almost at the end of the book, is the first time that money is mentioned in any significant way.

future and past on the present and by the large modifications that must be introduced into both value theory and stability analysis, if the requisite futures markets are missing' (Arrow and Hahn, 1971, 369).

Hahn's final remarks on GE were presented after he retired from Cambridge and took up a post at the University of Siena in 1992, where he engaged actively in the annual summer schools organised at the Certosa near that wonderful city. The XII Workshop of 1999 took as its topic 'General equilibrium: problems, prospects, alternatives', and led to the conference volume (Petri and Hahn, 2003). Much of the conference was taken up with raking over the old capital controversies on the 1960s that Italy and Siena had kept alive, set out at some length in Petri's co-introduction to that volume.[19] Hahn's rejoinder in his co-introduction remarks that the most strident critics of GE are the neo-Ricardians, but he has 'never found it easy to see their objections …'. Hahn's own contribution (Hahn, 2003) summarises some of the problems with the Arrow-Debreu form of GE and approaches that have been taken to address them. He admits that there remains no satisfactory economy-wide theory that takes account of these problems, concluding that instead of looking for micro-foundations for macroeconomics (where Hahn started his career) perhaps what is needed is a macro-foundation for microeconomics. When agents need to form expectations about the future, macro-variables such as the rate of inflation, the level of unemployment and others are likely to influence these expectations and hence their resulting actions.

General equilibrium and stability

Samuelson (1941) had already drawn attention to the limited nature of nineteenth-century Walrasian competitive theory, noting that stability is central to any equilibrium theory, for unless there are forces that tend to drive an economy towards an equilibrium, then such a state would not likely persist. Conversely, if the economy is driven towards equilibrium, then it may be legitimate to make predictions about where the economy will move if some parameter changes (e.g. the productivity of labour in some sector) by examining the new equilibrium defined by the changed parameter.

[19] Kirman in his contribution remarks that 'taking part in a meeting on general equilibrium in Siena is very much like taking part in an intellectual Palio' (Kirman, 2003, 468). Il Palio is an ancient and particularly brutal, no-holds barred horse race held in the Piazza del Campo in Siena.

Hahn was therefore concerned with establishing the stability of general equilibrium, as evidenced by a rapid series of papers on the subject (Hahn, 1960b, 1961, 1962a, 1962b). Starting with Hicks (1939), Arrow and Debreu (1954) and Debreu (1957), rigorous foundations for the static theory of GE were laid down, specifically establishing conditions under which a competitive equilibrium exists (relatively weak), that it is unique (where local uniqueness is generally the case, but global uniqueness requires strong assumptions) and its optimality—the relationship between competitive equilibrium and its efficiency.

A competitive equilibrium can be shown to be Pareto efficient given a complete set of markets, all agents have full information about all prices, and there are no transactions costs and no-one is satiated. Pareto efficiency means there is no other feasible allocation of goods in which no-one is worse off and at least one person is better off. The second welfare theorem makes the more significant claim that with the additional assumption of convexity (i.e. no economies of scale) any feasible Pareto-efficient allocation can be decentralised as a competitive equilibrium with lump-sum transfers. While competitive equilibria have no claim to fairness or social equity, given some social preference function that allows one to rank alternative allocations among households, the social optimum can be supported by a competitive price system, with the massive proviso that transfers can be arranged in a way that does not affect choices. The force of a lump-sum transfer is that they must not depend on any observable action, such as earning income or spending money, and thus requires the Benevolent Dictator to see into the hearts and minds of all agents.

Clearly such lump-sum transfers are quite impractical, but the modern theory of public economics has been concerned with finding the best feasible outcome, subject to the information available to the tax authorities and the incentives that agents face when confronted with taxes. Diamond and Mirrlees (1971a, 1971b) in their theory of optimal indirect taxes (on goods and services) showed that allocations on the second-best Pareto Frontier satisfy aggregate efficiency and can be achieved in a decentralised setting under the usual assumptions, even taking account of the limited information of the tax authorities.

Following the rigorous static part of GE, Arrow et al. (1958, 1959) set out to establish conditions for the stability of competitive equilibria (CE), firmly retaining the Walrasian concept of *tâtonnement* in which an auctioneer would call out prices for every good, receive information back as to demands and supply at each price, and then adjust prices until supply is at least as great as demand in every market (and if strictly greater, the

price has fallen to zero). The crucial assumption is that no exchanges would take place until the process had found a set of equilibrium prices for every good, under which they were able to find conditions ensuring that the CE was stable.

Something like this process on a much simpler scale happens in the European day-ahead electricity auction, to which all those wishing to offer to supply or bid to demand electricity for a given hour next day submit an offer or demand schedule, and the intersection of the aggregate demand and supply will set the price (this is a simplified story). In this auction market only one price (that of electricity in the hour) is to be determined and the prices of all other goods are assumed to remain unchanged (or have already been contracted, such as the gas and coal for generation).

Hahn and Negishi (1962), following these papers of Arrow et al., abandoned the Walrasian assumption that no trade takes place before a complete set of equilibrium prices is determined, and allowed trade to take place even if some markets were not in equilibrium. In this model of a pure trading economy (i.e. without production) prices are still 'called' (presumably by an auctioneer) and at those prices trading continues until there is never an individual with any unsold good on hand when that good is in excess demand nor with an unpurchased good when that good is in excess supply.

Whereas Hicks had approached the question of stability using classical differential equation theory (examining the Jacobian of responses of demands to price changes) Hahn and Negishi picked up the newly rediscovered Second Method of Lyapunov, originally published in Russian in 1892 as *The General Problem of Stability of Motion*. Operations researchers anxious to develop algorithms for guiding missiles enthusiastically plundered this Russian gem (and that of Pontryagin, whose 1956 article laid out principles of optimal control), making this powerful technique generally accessible (although Hahn and Negishi reference a 1956 article by Wolfgang Hahn in German). Hicks's method only establishes local stability—that is, the system will converge to the equilibrium providing it starts sufficiently close to that equilibrium. The Lyapunov method, in contrast, establishes global stability, starting from any initial condition.

The crucial step in the proof, which is all but hidden by some heavyweight and pretty obscure mathematics, is that each step in the price adjustment process is one in which agents become disappointed, so their expected utility decreases. Sellers, when they cannot find buyers, have to accept lower prices, while buyers, if they cannot find sellers, have to face

increasing prices. Expected utility therefore decreases but is bounded below, hence by the Lyapunov argument, converges to the competitive equilibrium.

This article is justifiably considered a seminal contribution, as it shows that under reasonable conditions the model trading economy will tend towards an equilibrium under weaker conditions than those required for the stability of a Walrasian equilibrium (with no out-of-equilibrium trading). This model still assumed an auctioneer to ensure that at any moment everyone faces the same price for a good (even if it is not an equilibrium price), as otherwise there would need to be agents who changed prices. Giving an agent the power to change prices in response to perceived demand immediately introduces imperfect competition, raising the next question of whether such market power would ultimately vanish at the final CE—a question Franklin Fisher devoted fifteen years of his life pursuing (Fisher, 2011). But the Hahn-Negishi approach was a critical step in moving away from the Walrasian *tâtonnement* and modelling sequential trading in markets.[20]

In the subsequent development in Arrow-Hahn (1971, chapter 13) this non-*tâtonnement* process is extended to allow the use of money to mediate exchange, building on Clower (1965). The paper with Negishi was also important in sparking further research to generalise the results by the brilliant mathematician, Smale, and in encouraging Fisher to extend the model to include production. A rather pessimistic assessment of the stability of GE is provided by Kirman (2003, 473), who counts the failure to establish any tendency towards equilibrium as the most striking failure of GE theory, while 'the (Arrow-Debreu) model is intact but now looks almost irrelevant to the understanding of real economic phenomena' (Kirman, 2003, 483).

Growth theory

The *Growth Survey* (Hahn and Matthews, 1964) is an excellent example of bringing the approach that clarified GE theory to the simple but dynamic models of growth that had been sparked by the early work of Harrod (1939) and its neoclassical responses of Solow (1956) and Swan (1956).

[20] Later there was considerable interest in the stability properties of Agent-based models, that allow different agents to interact in a sequence of production and trading dates, but such developments took place both later and with different objectives in mind. Newbery and Greve (2015, Appendix B) gives a brief summary of some of these developments.

The *Growth Survey* came when growth theory was evolving rapidly and needed clarifying, which the *Growth Survey* achieved magnificently. Harrod (1939) and Domar (1946) introduced growth theory to the English-speaking audience, although von Neumann (1938) had already published a highly sophisticated multi-sector model of growth in German, but that was not available until translated by Morgenstern (von Neumann, 1946, which Hahn covers extensively in the *Growth Survey* and reprints in Hahn, 1971, with Champernowne's 1946 note).

Growth theory concerns itself with the prospect of steady growth, an important shift from earlier concerns with 'long-period' equilibrium that was presumably considered as static. Harrod was much concerned with both the existence of steady-state growth with full employment (a dynamic counterpart to Keynesian concerns) and with the 'knife-edge' instability of steady growth paths. Stability in growth theory has two dimensions. The first, *equilibrium dynamics*, asks whether equilibrium non-steady state paths converge to steady-state growth. The second, *disequilibrium dynamics*, asks whether, if the economy is disturbed and hence departs from equilibrium, agents react to restore equilibrium. The latter requires additional specifications of out-of-equilibrium behaviour, a subject that Hahn accepts as a necessary evil, but is concerned that it can so easily become arbitrary. It leads to trade-cycle theory and, with that, a need to reconcile growth and fluctuations.

Harrod's existence problem arises because the natural rate of growth, n, determined by the growth of the labour force at full employment, and the warranted rate of growth, determined by the ratio of the savings share, s, to the capital-output ratio, v, or $g_w = s/v$, are unlikely to be equal given the assumption that all three variables are given and independently determined. The *Growth Survey* therefore considers models that differ in which of these can be variable and adjust to the required relationship, $n = s/v$. Hahn in his thesis noted that the average savings rate could depend on the distribution of income, and hence changes in that distribution could allow the necessary change in the average s. The neoclassical models of Solow and Swan assume that capital and labour can be substituted to allow v to adjust. Hahn-Matthews accept that Harrod did not rule out a flexible v, but that the Keynesian approach saw difficulties in reaching short-run full employment equilibrium, which Harrod extended to the longer run, and which others like Kaldor picked up.

Hahn (1960) was already concerned with the stability of growth paths in simpler models, and specifically whether they were stable and how money might be included in a simple Solow-type model. In these models

producers can make mistakes and investment is not necessarily equal to full employment savings, while money plays a role and can influence investment decisions. Even though capital and labour can be smoothly substituted (a neoclassical production function) Harrod's knife-edge stability remains problematic.

After discussing one-good, one-sector models in which capital is like corn, capable of investment and consumption, the *Growth Survey* considers two-sector models where capital is physically different from consumption and needs a price in terms of consumption. This creates a mini-general equilibrium model in which there can be multiple equilibria and, even more exciting, discontinuities with capital accumulation in which multiple equilibria collapse to a single equilibrium, possibly with massive redistributions of income (assuming full employment could ever be maintained).

The final part, largely written by Hahn, considers multi-sector linear models in which there are many different types of capital good, and hence the concept of a single capital aggregate is no longer simple (but nor is it needed to derive rates of profit in equilibrium). The section considers an extension of the dynamic input-output model with a single non-produced factor of production (labour), many goods in which there is a large number of different processes to produce each good. Provided the system is productive, the set of competitive prices is independent of the pattern of demand—the dynamic (non-) substitution theorem.[21] This had been noted in the static context by Samuelson (1951) and was made much of by Sraffa (1960). In the *Growth Survey* Sraffa's model is somewhat dismissively treated on a par with similar 'Leontief-Samuelson-Sraffa' multi-sector models.

Hahn followed this final line of questioning in another of his most influential papers on 'Equilibrium dynamics with heterogeneous capital goods' (Hahn, 1966). This is a simple stripped-down model to demonstrate the instability of growth models that have more than one type of capital good, even if there is only one consumer good. Agents have to decide what kind of capital to invest in, and given competitive markets they will seek those with the highest rate of return, including capital gains. In equilibrium as all types of capital are needed all must earn the same rate of return, but even when agents start with a set of initial expectations, the economy may pursue a variety of equilibrium paths. The problem remains even if there are only two types of capital when, even if a single

[21] Mirrlees (1969) gives a proof of the most general formulation.

equilibrium path is found, there are many initial conditions for which such equilibrium paths do not approach the balanced growth path.

This article was written when Hahn had returned to Cambridge amid the so-called Cambridge capital controversy, sometimes called 'the two Cambridges debate' as economists at Cambridge MIT (Solow, Samuelson and Stiglitz) were also participants. That debate, which need not detain us here, in one sense revolved around whether one could attach any meaning to an aggregate capital concept, convenient in the original single sector growth models, and what might be the relationship, if any, between 'the' rate of profit and 'the' quantity of capital. Of course, in the full Arrow-Debreu model there are a complete set of intertemporal prices for all goods, and Bliss argues 'that capital theory should be liberated from the concept of the rate of interest, meaning by that one rate... Instead, we will find the concept of inter- temporal prices to be fundamental and will see that working with the rate of interest is a clumsy groping for that concept' (Bliss, 1975, 10).

Hahn's 1966 paper certainly chimes nicely with Bliss's comment that 'It has always seemed to me to be supremely ironic that in the war between the post-Keynesians and the Neoclassical school the major damage to orthodox theory came from the latter.' After he retired from Cambridge in 1992, Hahn moved to the economics department at the University of Siena, a hotbed of neo-Ricardian economics. Hahn (1982c) had earlier written on the neo-Ricardians (specifically on Sraffa, 1960) and states on the first page 'I … show that there is no correct neo-Ricardian proposition which is not contained in the set of propositions which can be generated by orthodoxy. I shall therefore conclude that the neo-Ricardian attack via logic is easily beaten off' (Hahn, 1982c, 353). Hahn concludes that marginal productivity concerns an economy in full neoclassical equilibrium: 'But on the manner in which such an equilibrium is supposed to come about, neoclassical theory is highly unsatisfactory. Sraffa's work shows that certain simplified routes are very risky and not free from logical difficulties. The remarkable fact is that neither he nor the Sraffians have made anything of this' (Hahn, 1982c, 373).

The Cambridge 'Risk' Project 1976–94

In his Presidential address to the Econometric Society in 1968 Hahn expressed concern about the performance of the 'invisible hand', in other words with the idea that a general competitive equilibrium could be established by any plausible market process (Hahn, 1970). While recognising

the achievements of GE theory in the previous twenty years, Hahn considered it somewhat scandalous that so much effort was devoted to 'refining the analyses of economic states which they give no reason to suppose will ever, or have ever, come about. Equilibrium economics, because of its well-known welfare economics implication, is easily convertible into an apologia for existing economic arrangements and it is frequently so converted' (Hahn, 1970, 1).

The last remark on welfare economics is illuminating, and goes some way to explaining his views in *Reflections* (Szenberg and Ramrattan, 2004) that he was rather repelled by utilitarianism ('mechanical morality'), and so was uninterested in welfare economics: 'Nevertheless, I often find myself on the utilitarian side.' He concludes in his presidential address that he is assailed by 'Doubts' that GE can deliver: 'The most intellectually exciting question of our subject remains: is it true that the pursuit of private interest produces not chaos but coherence, and if so, how is it done?' (Hahn, 1970, 12).

That thought germinated in a proposal to the Economic and Social Research Council, the UK social science funding body, for the project Information, risk and quantity signals in economics', which would bring together academics within the Cambridge Economics Faculty, younger PhDs, post-docs and visitors to address the serious incompleteness of Walrasian theory. This *Risk* project was funded and extended for three-year periods until 1994, two years after Hahn's formal retirement from Cambridge in 1992, by which time it had published 199 working papers, most of which emerged as published articles. Hahn (1989, 1) summarises its aim as 'to move beyond the Walrasian paradigm without abandoning the commitment to lucid and rigorous thinking'.

The incompleteness of this Walrasian theory resided partly in the implausibility of agents taking prices as given when firms were often large and clearly could act strategically, which required the rapidly developing field of game theory for its analysis.[22] At a more fundamental level, Arrow-Debreu GE assumed market completeness, but evidently markets were seriously incomplete, with missing futures and Arrow security markets.

[22] Hahn was interested in what Game Theory had to offer, but not in actively working in the area. Chapter 8 of Arrow and Hahn (1971) is devoted to the core of the economy, a game-theoretic concept that picks up from Edgeworth's bargaining approach to equilibrium, and shows that under certain conditions the core converges on the Walrasian equilibrium). Hahn clearly found this an attractive way of reconciling the Edgeworth and Walrasian approaches to GE, but was always somewhat repelled by the multiplicity of possible equilibria that game theory could support with no obvious way of selecting any one single 'solution'.

When trading has to take place in their absence, agents need to trade on the basis of expectations and in response to signals which provide some but incomplete information about opportunities and prospects. That in turn raises questions about how agents learn about their environment, and what role prices play in aggregating, revealing and transmitting information.

The importance of incomplete and asymmetric information for market functioning was recognised by Akerlof (1970) and Stiglitz and Rothschild (1976)—for which Akerlof and Stiglitz with Spence won the 2001 Nobel Prize—so the *Risk* project was very much in at the start of the resulting revolution in economics, with implications for incentive compatibility, and for the necessity of other institutional arrangements (contracts, standards, monitoring, accreditation, guarantees, etc.) to address the missing markets and missing information (Newbery, 1989). Hahn, in the introduction, had to admit that 'neither we, nor, as far as I know, anyone else has managed to integrate the new insights into a comprehensive theory of the economy in the manner of Walrasian theory'.

The *Risk* project may have failed to deliver that comprehensive theory, but it did produce a remarkable amount of good and useful insights, adding to the sum total of knowledge and, equally important, stimulating a whole generation of economists to think deeply about hard problems, to learn from the brightest and best that Hahn gathered around, and to be stimulated by his probing questions and continued engagement with these hard problems. By forcing theorists to recognise the inadequacies of the only really well-articulated theory of an economy, it encouraged the development of theories to address some of these inadequacies, although in a partial, rather than general equilibrium setting. The theory of the firm, of modern public finance, of insurance and health markets have all been enriched by theorists tackling these hard problems.

Hahn's retirement was marked with a weighty Festschrift (Dasgupta et al., 1992), notable for containing twenty-seven contributions from a galaxy of the talents that Hahn had gathered around himself during his career including, among many, Solow, Atkinson, Hart, Maskin, Stiglitz, Arrow, Aumann, Radner, Diamond, Mirrlees, Gorman and Samuelson, as well as many other active participants in the *Risk* project. The book had three thematic parts—on the Microeconomic Foundations of Macroeconomics, Information and the Theory of Games, and Equilibrium with Missing Markets, with a final Miscellany, but it would take us too far afield to discuss their contributions.

Frank Hahn started his career at a critical moment for British economics, after the monopolistic competition revolution and the Keynesian macro-revolution, but not yet impacted by the other two revolutions sweeping the USA, noted by Samuelson—the mathematicisation revolution and the econometric inference revolution. Hahn led successive generations of economists to adopt 'the rigorous analysis made possible by the mathematical modelling introduced by American economists like Arrow, Samuelson and Solow. Hahn and his emulators thereby created an intellectual culture in British economic theory that remains dominant to this day' (Binmore, 2013). His published legacy is substantial and important; the stimulus he imparted to his students, colleagues and the profession is equally impressive.

DAVID NEWBERY
Fellow of the Academy

Note: I am indebted to conversations, interviews and correspondence with a large number of Hahn's friends and colleagues, but particularly to Kenneth Arrow, Tony Atkinson, Christopher Bliss, Monojit Chatterji, Partha Dasgupta, Peter Diamond, Dorothy Hahn, Geoff Harcourt and his many friends who came and made tributes at his memorial. I have drawn on his personal contribution in Szenberg (1992) as well as many obituaries.

References

Akerlof, G. A. (1970) 'The market for "lemons": quality uncertainty and the market mechanism', *Quarterly Journal of Economics*, 84, 488–500.

Arrow, K. J. (1962) 'The economic implications of learning by doing', *Review of Economic Studies*, 39, 155–73.

Arrow, K. J. and G. Debreu (1954) 'Existence of equilibrium for a competitive economy', *Econometrica*, 22, 265–90.

Arrow, K. J. and L. Hurwicz (1958) 'On the stability of the competitive equilibrium, I', *Econometrica*, 26, 522–52.

Arrow, K. J., Block and L. Hurwicz (1958) 'On the stability of the competitive equilibrium, II', *Econometrica*, 27, 82–109.

Arrow, K. J. and Hahn (1971) *General Competitive Analysis* (San Francisco, CA).

Arrow, K. J. and Hahn (1999) 'Notes on sequence economies, transaction costs and uncertainty', *Journal of Economic Theory,* 86, 201–18.

Binmore, K. (2013) 'Obituary', *The Times*, 3 February 2013.

Bliss, C. J. (1975) *Capital Theory and the Distribution of Income* (Amsterdam).

Chatterji, M. (2013) Frank Hahn—Economist, Thinker and Teacher Extraordinaire. Memorial address at Churchill College, 29 September 2013
Clower, R. W. (1965) 'The Keynesian counter-revolution: a theoretical appraisal', in F. H. Hahn and F. P. R. Brechling (eds.), *The Theory of Interest Rates: Proceedings of a Conference held by the International Economic Association* (London), pp. 103–25.
Dasgupta, P. (2013) 'Obituary', *Royal Economic Society Newsletter*, April.
Dasgupta, P., D. Gale, O. Hart and E. Maskin (eds.) (1992) *Economic Analysis of Markets and Games: Essays in Honor of Frank Hahn* (Cambridge, MA).
Debreu, G. (1959) *The Theory of Value: an Axiomatic Analysis of Economic Equilibrium* (New Haven, CT).
Debreu, G. (1962) 'New concepts and techniques for equilibrium analysis', *International Economic Review,* 3, 257–73.
Desai, M. and P. Ormerod (1998) 'Richard Goodwin: a short appreciation', *The Economic Journal*, 108, 1431–5.
Diamond, P. and J. Mirrlees (1971a) 'Optimal taxation and public production I: production efficiency', *American Economic Review*, 61, 8–27.
Diamond, P. and J. Mirrlees (1971b) 'Optimal taxation and public production II: tax rules', *American economic Review*, 261–78.
Domar, E. D. (1946) 'Capital expansion, rate of growth and employment,' *Econometrica*, 14, 137–57.
Fisher, F. M. (2011) 'The stability of general equilibrium—what do we know and why is it important?', in P. Bridel (ed.), *General Equilibrium Analysis* (London and New York), pp. 34–45.
Friedman, B. and Hahn (eds.) (1990) *Handbook of Monetary Economics, I—II* (Amsterdam).
Goodwin, R. M. (1953) 'The problem of trend and cycle', *Yorkshire Bulletin of Economic and Social Research*, 5, 89–97.
Goodwin, R. M. (1955) 'A model of cyclical growth', in E. Lundberg (ed.), *The Business Cycle in the Post-war World*, *Proceedings of International Economic Association Conference* (London, 1955), pp. 203–21.
Goodwin, R. M. (1967) 'A growth cycle', in C. H. Feinstein (ed.), *Socialism, Capitalism and Economic Growth* (Cambridge), pp. 54–8.
Hahn, A. (1943) *Das Volk Messias (Sonnets in Favour of the Jews)* (London).
Hahn, F. H. (1952) 'The general equilibrium theory of money: a comment', *Review of Economic Studies*, 19, 179–85.
Hahn, F. H. (1955) 'The rate of interest and general equilibrium analysis', *The Economic Journal*, 65, 52–66
Hahn, F. H. (1958) 'Gross substitutes and the dynamic stability of general equilibrium', *Econometrica*, 26, 169–70.
Hahn, F. H. (1960a) 'The Patinkin controversy', *Review of Economic Studies*, 28, 37–43.
Hahn, F. H. (1960b) 'The stability of growth equilibrium', *Quarterly Journal of Economics*, 74, 206–26.
Hahn, F. H. (1961) 'Money, dynamic stability and growth', *Metroeconomica*, 13, 57–76.
Hahn, F. H. (1962a) 'A stable adjustment process for a competitive economy', *Review of Economic Studies*, 29, 62–5.

Hahn, F. H. (1962b) 'On the stability of a pure exchange equilibrium', *International Economic Review*, 62, 206–13.
Hahn, F. H. (1962c) 'The stability of the Cournot oligopoly solution', *Review of Economic Studies*, 29, 329–31.
Hahn, F. H. (1963) 'On the disequilibrium behaviour of a multi-sectoral growth model', *The Economic Journal,* 73, 442–57.
Hahn, F. H. (1965) 'On some problems of proving the existence of an equilibrium in a monetary economy', in F. H. Hahn and F. P. R. Brechling (eds.), *The Theory of Interest Rates: Proceedings of a Conference Held by the International Economic Association* (London), pp. 126–35.
Hahn, F. H. (1966) 'Equilibrium dynamics with heterogeneous capital goods', *Quarterly Journal of Economics*, 80, 633–45.
Hahn, F. H. (1968) 'On warranted growth paths', *Review of Economic Studies*, 35, 175–84.
Hahn, F. H. (1969) 'On money and growth, 1969', *Journal of Money, Credit and Banking, Conference on Money and Economic Growth*, 1, 172–87.
Hahn, F. H. (1970) 'Some adjustment problems', *Econometrica*, 38, 1–17 (Presidential Address)
Hahn, F. H. (1971) 'Equilibrium with transaction costs', *Econometrica*, 39, 417–39.
Hahn, F. H. (ed.) (1971) *Readings in the Theory of Growth* (London).
Hahn, F. H. (1973a) *On the Notion of Equilibrium in Economics: an Inaugural Lecture* (Cambridge).
Hahn, F. H. (1973b) 'The winter of our discontent', *Economica*, 40, 322–30.
Hahn, F. H. (1973c) 'On some equilibrium growth paths', in J. A. Mirrlees and N. H. Stern (eds.), *Models of Economic Growth* (London), pp. 193–206.
Hahn, F. H. (1973d) 'On transaction costs, inessential sequence economies and money', *Review of Economic Studies*, 40, 449–61.
Hahn, F. H. (1975a) 'Money and general equilibrium', *Indian Economic Journal*, 23, 109–22.
Hahn, F. H. (1975b). 'Revival of political economy: the wrong issues and the wrong arguments', *The Economic Record*, 51, 360–4.
Hahn, F. H. (1977) 'Keynesian economics and general equilibrium theory: reflections on some current debates', in G. C. Harcourt (ed.), *Microeconomic Foundations of Macroeconomics* (London), pp. 25–40.
Hahn, F. H. (1980) 'Monetarism and economic theory', *Economica*, 47, 1–17.
Hahn, F. H. (1981) 'General equilibrium theory', in D. Bell and I. Kristol (eds.), *Crisis in Economic Theory* (New York), pp. 123–38.
Hahn, F. H. (1983) *Money and Inflation* (Cambridge, MA)
Hahn, F. H. (1982b) 'Reflections on the invisible hand', *Lloyd's Bank Review*.
Hahn, F. H. (1982c) 'The neo-Ricardians', *Cambridge Journal of Economics*, 6, 353–74.
Hahn, F. H. (1982d) 'Stability', in K. J. Arrow and M. D. Intriligator (eds.), *Handbook of Mathematical Economics* (Amsterdam), pp. 745–93.
Hahn, F. H. (1984a) 'Why I am not a monetarist', in F. H. Hahn (ed.), *Equilibrium and Macroeconomics* (Cambridge, MA), pp. 307–26.
Hahn, F. H. (ed.) (1984b) *Equilibrium and Macroeconomics* (Cambridge, MA).

Hahn, F. H. (ed.) (1985a) 'In praise of economic theory', in F. H. Hahn (ed.), *Money, Growth and Stability* (Oxford), pp. 10–28 (The Jevons Lecture).
Hahn, F. H. (ed.) (1985b) *Money, Growth and Stability* (Oxford).
Hahn, F. H. (ed.) (1989) *The Economics of Missing Markets, Information, and Games* (Oxford).
Hahn, F. H. (1989) 'Robinson-Hahn love-hate relationship; an interview', in G. R. Feiwel (ed.), *Joan Robinson and Modern Economic Theory* (London), pp. 85–91.
Hahn, F. H. (1990) 'Liquidity', in B. Friedman and F. H. Hahn (eds.) (1990) *Handbook of Monetary Economics, I—II* (Amsterdam), pp. 63–80.
Hahn, F. H. (1999) 'A remark on incomplete market equilibrium', in G. Chichilnisky (ed.), *Markets, Information and Uncertainty: Essays in Economic Theory in Honour of Kenneth J. Arrow* (Cambridge), pp. 67–71.
Hahn, F. H. and Brechling (eds.) 1965 *The Theory of Interest Rates: Proceedings of a Conference Held by the International Economic Association* (London).
Hahn, F. H. and Matthews (1964) 'The theory of economic growth: a survey', *The Economic Journal*, 74, 779–902.
Hahn, F. H. and T. Negishi (1962) 'A theorem on non-tâtonnement stability', *Econometrica*, 30, 463–9.
Hahn, F. H. and Solow (1995) *Critical Essay on Modern Macroeconomic Theory* (Cambridge, MA).
Harcourt, G. and J. King (1995) 'Talking about Joan Robinson: Geoff Harcourt in conversation with John King,' *Review of Social Economy*, 53, 31–64.
Harrod, R. F. (1939) 'An essay in dynamic theory', *The Economic Journal*, 49, 14–33.
Harrod, R. F. (1950) *The Trade Cycle* (Oxford).
Harrod, R. F. (1948) *Towards a Dynamic Economics* (London).
Hicks, J. R. (1939) *Value and Capital*: *an Inquiry into Some Fundamental Principles of Economic Theory* (Oxford).
Kaldor, N. (1956) 'Alternative theories of distribution', *Review of Economic Studies*, 23, 83–100.
Kaldor, N. (1957) 'A model of economic growth', *The Economic Journal*, 67, 591–624.
Kaldor, N. (1972) 'On the irrelevance of equilibrium economics', *The Economic Journal*, 82, 1237–55.
Keynes, J. M. (1936) *The General Theory of Employment, Interest and Money* (London).
Kirman, A. (2003) 'General equilibrium: problems, prospects, alternatives—an attempt at synthesis', in F. Petri and F. H. Hahn (eds.), *General Equilibrium: Problems and Prospects* (London), pp. 468–85.
Kornai, J. (1971) *Anti-equilibrium: on Economic Systems Theory and the Tasks of Research* (Amsterdam).
Medema, S. G. and Waterman (eds.) (2014) *Paul Samuelson on the History of Economic Analysis: Selected Essays* (Cambridge).
Mirrlees, J. A. (1969) 'The dynamic nonsubstitution theorem', *The Review of Economic Studies*, 36, 67–76.
Mirrlees, J. A. (1971) 'An exploration in the theory of optimum income taxation', *The Review of Economic Studies*, 38, 175–208.
Neild, R. (2012) The 1981 Budget and the Letter by 364 Economists, mimeo (Cambridge).

von Neumann, J. (1945) A Model of General Equilibrium, *The Review of Economic Studies*, 13, 1–9.

von Neumann, J. (1938) 'Über ein ökononomisches Gleichungsystem und eine Verallgemeinerung des Brouwerschen Fixpunktsatzes', in K Menger (ed.), *Ergebnisse eines Mathematische Seminars* (Vienna), pp. 73–83.

Newbery, D. M. (1989) 'Missing markets: consequences and remedies', in F. H. Hahn (ed.), *Economics of Missing Markets, Information, and Games* (Oxford), pp. 211–42.

Newbery, D. M. and T. Greve (2015) 'The robustness of industrial commodity oligopoly pricing strategies'. *EPRG 1522* at http://www.eprg.group.cam.ac.uk/wp-content/uploads/2015/12/1522-PDF.pdf (accessed 4 July 2017).

Panayi, P. (1996) *Germans in Britain since 1500* (London).

Pasinetti, L. L. (1962) 'Rate of profit and income distribution in relation to the rate of economic growth', *The Review of Economic Studies*, 29, 267–79.

Petri, F. and Hahn (eds.) (2003) *General Equilibrium: Problems and Prospects* (London).

Samuelson, P. A. (1941) 'The stability of equilibrium: comparative statics and dynamics', *Econometrica*, 9, 97–120.

Samuelson, P. A. (1947) *Foundations of Economic Analysis* (Cambridge, MA).

Solow, R. M. (1956) 'A contribution to the theory of economic growth', *Quarterly Journal of Economics*, 70, 65–94.

Solow, R. (1992) 'Hahn on the share of wages in national income', in P. Dasgupta, D. Gale, O. Hart and E. Maskin (eds.), *Economic Analysis of Markets and Games: Essays in Honor of Frank Hahn* (Cambridge, MA), pp. 3–18.

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

Stein, J. L. (1969) 'Conference on money and economic growth: introduction', *Journal of Money, Credit and Banking*, 1, 131–7.

Stiglitz, J. E. and M. Rothschild (1976) 'equilibrium in competitive insurance markets: an essay on the economics of imperfect information', *Quarterly Journal of Economics*, 90, 629–49.

Stolper, W. F. and Samuelson, P. A. (1941) 'Protection and real wages', *The Review of Economic Studies*, 9, 58–73.

Swan, T. W. (1956) 'Economic growth and capital accumulation', *The Economic Record*, 32, 334–61.

Szenberg, M. (ed.) (1992) *Eminent Economists: their Life Philosophies* (Cambridge).

Szenberg, M. and Ramrattan, L. (eds.) (2004) *Reflections of Eminent Economists* (Cheltenham).