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A Cambridge Economist From Down Under

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# ***Geoffrey Colin Harcourt (1931-2021): A Cambridge Economist From Down Under***

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**Abstract.** The paper offers a view of Geoff Harcourt's – b. 1931 in Melbourne; d. 2021 in Sydney – life trajectory as an Australian economist educated and active in the Cambridge UK tradition. His main contributions – to the Cambridge capital debates, history of economic thought and post-Keynesian economics – are discussed against aspects of his biographical background.

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**Keywords.** Harcourt, biography, capital debates, post-Keynesianism, history of economics

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## 1. Studying Geoff

Former students of Geoff Harcourt, who passed away on the 7<sup>th</sup> of December 2021 in Sydney, probably remember how he used to begin his courses of lectures – mostly on post-Keynesian economics – by discussing how values, ideology and theory were indissolubly interconnected, followed by a statement of his own political, philosophical, religious and economic views.<sup>1</sup> Harcourt had started doing so around the time he was working on his widely read 1969 survey on the capital debates between some prominent members of the economics departments at Cambridge University and at the Massachusetts Institute of Technology (MIT), which brought him international reputation (Harcourt 1969a; followed by Harcourt 1972, a book length version of the article). The survey was written in the second semester of 1968, when Harcourt shut himself away from his intense direct action anti-Vietnam War activities started in mid 1967, as the Campaign for Peace in Vietnam in South Australia was set up in Adelaide – where he lived at the time – in reaction to Australia’s extended involvement in that conflict. He was then deeply influenced by contemporary works by Australian historian Hugh Stretton (1969) and American linguist Noam Chomsky (1967) on the public role of intellectuals (Harcourt 1992a: 5-6; 1995: 240; [1999] 2012a: 26; 2011a: 123-24).

Born in Melbourne on the 27<sup>th</sup> of June 1931, Harcourt never wrote a complete autobiography despite invitations from publishers to do so, as he preferred to retain his large economic network of many friends instead of revealing “episodes and attitudes often known only to the persons concerned and of which they (in any event, I) are thoroughly ashamed” (Harcourt 2012a: 1; see also Harcourt 2018: 2-3). Nevertheless, he produced a relatively large number of “intellectual autobiographical” essays, with recollections of his family background, intellectual formation and professional life in periods in Cambridge UK (1955-58; 1963-66; 1971-72, 1980, 1982-2010) and Australia (Melbourne 1931-55, Adelaide for most of the 1960s until 1982, and Sydney 2010-21).

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<sup>1</sup> I took Geoff’s course on post-Keynesian macroeconomics and did my PhD thesis under his supervision at the Faculty of Economics and Politics of Cambridge University in the early 1990s.

Harcourt was one of the economists who responded positively to requests for autobiographical memoir essays that became fashionable from the 1980s-90s, from journals such as *Banca Nazionale del Lavoro Quarterly Review*, *The American Economist* and the *European Journal of Economics and Economic Policies: Intervention*, and from collections edited by Arestis and Sawyer (1992), Hertjee (1999) and, more recently, Sinha and Thomas (2019) (see Harcourt 1992a, 1995a, 1998, [1999] 2012a, 2011a, 2019a). Moreover, he published recollection pieces as new opening chapters of some of his collections of previously published essays (Harcourt 1992b, 2001a). Although there is some repetition (Harcourt 1992a is virtually identical with Harcourt 1992b), those autobiographical essays altogether are quite informative, particularly when his lengthy interviews are also taken into account (Harcourt and King 1995, King 1995, Mongiovi 2001, Macfarlane 2007, Harcourt 2011b, Tran-Nam and Harcourt 2017).

Two obituary articles (apart from a number of shorter pieces on the press etc.) about Geoff Harcourt have been published so far, both by Australian economists (Kriesler 2022; Cornish & Hawkins 2022). Before that, during Harcourt's lifetime, a number of biographical accounts and/or assessments of his contributions to economics came out, firstly as part of Groenewegen & Macfarlane's (1990: 196-99) discussion of seven internationally prominent Australian economists from 1950.<sup>2</sup> That was followed by Hatch and Petrides (1997) – written for Harcourt's Festschrift on the occasion of his 65<sup>th</sup> birthday – and by Hatch and Rogers (1997) in celebration of his election as Distinguished Fellow of the Economic Society of Australia (which he had presided over 1974-77). More recently, Millmow (2017: 169-71, 188-89) discussed aspects of Harcourt's contributions to economic theory and to Australian debates on economic policy, including a couple of references to the "Papers of Geoff Harcourt", formed by 175 boxes covering the period 1939-2016, held at the National Library of Australia.<sup>3</sup>

Mark Perlman (1995), who, as founding-editor of the *Journal Economic Literature* had commissioned Harcourt's (1969a) survey, provided a first general account of his contributions to economics, together with Perlman (1997) on problems

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<sup>2</sup> The others were H.W. Arndt, T.W. Swan, M.C. Kemp, W.M. Corden, W.E.G. Salter and S.J. Turnovsky. Harcourt was the only heterodox economist on the list.

<sup>3</sup> I did not have the opportunity to access the Harcourt Papers for the purposes of this essay.

in the reception, after the 1980s, of what Perlman called Harcourt's humanitarian and communitarian economics. Twenty years later, Velupillai (2017) would offer a general assessment of Harcourt's economics, in a chapter of a collection about Cambridge economists. Davis (1997) and Repapis (2014), respectively, have discussed important features of Harcourt's work as a historian of economics and as a dedicated book-reviewer.

Arrow (1999) – in his foreword to the third volume of Harcourt's *Festschrift*, formed by 28 essays contributed by Harcourt's former students – and Cohen (2022) – in his afterword to the 50<sup>th</sup> anniversary edition of Harcourt's 1972 book – have reflected about Geoff Harcourt's character and personality, pointing to his sweet temper, humanitarian and egalitarian values, roles as motivator, catalyst and community-builder, wit, and courtesy for others' ideas. Other commentators have also recognized such attributes. Arrow (1999: xviii), who interacted with Harcourt at the Cambridge Faculty of Economics and Politics in 1963-64 and 1971-72, recalled how he “only came to realise later the extent to which [Harcourt's] concerns were motivated by strong humanitarian and egalitarian values derived from his religion”.<sup>4</sup> Like other, better known, distinct instances (e.g. Frank Knight and Philip Wicksteed), religion was a significant element of Harcourt's outlook as an economist (see Harcourt 1994a and section 2 below).

Harcourt (1972: 165) once wrote that Knut Wicksell (1851-1926) was the “most loveable of the ‘great’ economists” due, *inter alia*, to the Swedish economist's acknowledgement of contributions by colleagues and predecessors, concern with economic justice, and intellectual honesty – shown, for instance, in Wicksell's pioneer discussion of hurdles faced by neoclassical capital theory (the famous “Wicksell effects”), which provided a starting-point for the capital debates surveyed

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<sup>4</sup> Arrow (1999: xviii) pointed out that the fact that Harcourt clearly expressed the nature of differences in viewpoints between contending parties – and the precise ways they agreed or disagreed – did not mean he was a “wishy-washy eclectic”, as he had firm convictions and maintained them. The admiration was mutual. Harcourt (2017) agreed with many in that Arrow was the greatest economist of his generation. Harcourt lectured about the British National Health Service at Cambridge in 1964-65 on the basis of Arrow's classic article on uncertainty and medical care. Arrow, who was one of the referees of Harcourt (1969a) – the others were J. Stiglitz and P. Samuelson – wrote to Harcourt upon publication asking: “May I use your brilliant survey for my graduate class at Harvard?”, which is the letter Harcourt prized “most of any I've received in all the world” (King 1995: 177).

by Harcourt. We may say now that, to many, Geoff Harcourt is among the most loveable of the economists who enlightened our trade over the last 60 years or so.

An examination of Harcourt's intellectual and personal trajectories provides insights about the emergence of heterodox economics, especially its post-Keynesian alternative. Economic heterodoxies burgeoned in the 1970s, when they were institutionalized as reactions to the establishment of a dominant neoclassical orthodoxy (see Backhouse 2000). Interwar economics was essentially pluralist, a feature that declined but continued to survive until the 1960s. Harcourt contributed a number of formal economic models throughout the 1960s up to the early 1970s, about growth, pricing, investment, distribution, and the rate of return. Although recognizable with hindsight as "post-Keynesians", they were addressed to and read by a broad audience at the time (see e.g. Harcourt 1963a, b; 1965a, b; 1966; 1968; 1969b; Harcourt & Massaro 1964a, b; Harcourt & Kenyon 1976;).<sup>5</sup>

Harcourt's research agenda as a model-builder deepened during the three and a half years (1963-1966) he spent in Cambridge – cherished as the most exciting and productive of his professional life (see e.g. Harcourt 1992b: 4-5) – when he interacted with Joan Robinson, Richard Kahn, Piero Sraffa, Frank Hahn, James Mirrlees and visitors such as Robert Solow, Kenneth Arrow and Joseph Stiglitz, among others.

Harcourt's (1969a, 1972, 1976) surveys of the capital debates – especially in its 1972 version, which embedded many of his 1960s models and a few new ones on distribution and accumulation – represented a culmination of that agenda, even though he came to produce the *JEL* survey by chance, as he had not contributed to the 1950s-60s capital debates before.<sup>6</sup> Harcourt (1969a, 1972) attracted attention from economists in general, sometimes critically. The Cambridge capital controversies was one of the last episodes when economists that would be soon labelled "orthodox" and "heterodox" agreed to have an extended conversation – although beset by communication problems, which Harcourt's surveys attempted to sort out – around a common theme.

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<sup>5</sup> Harcourt & Kenyon (1976), based on a first version Harcourt drafted around 1966, was rejected by the *Economic Journal* in 1974 – even though the editors D.G. Champernowne and B. Reddaway were then both members of the Cambridge Faculty – probably reflecting the new times of mainstream dominance (see Harcourt [1995c] 2012a).

<sup>6</sup> One may say that was a reason why he was asked to do it in the first place.

The *JEL* survey was instrumental in bringing out what the conversation was all about and calling attention to fundamental theoretical issues that seemed to seriously damage the neoclassical framework, especially in its aggregative version. A few years later, Harcourt (1977a) would manage one more time, even if with limited success, to bring together economists from distinct streams to discuss the micro-foundations of macroeconomics, a topic indirectly related to the capital debates and indeed close to his research agenda ever since his days at Melbourne University in the early 1950s.

Despite the highly technical terms of the debates surveyed by Harcourt (1969a, 1972, 1976) – dominated by formal discussions of reswitching, capital reversal and capital measurement issues – they were, he insisted, essentially about the *meaning* of capital, which involved different visions and ideologies about the working of the economic system. Neoclassical economists reacted to Harcourt’s survey by arguing that the Cambridge (UK) mix of ideology and analysis rendered Cambridge economics “methodologically corrupt and unscientific” (see Davis 1997: 447). The outcast of Cambridge post-Keynesian economics from top journals and most textbooks followed up. Cambridge status as the respected home of the Keynesian tradition, well recognized up to the 1960s, was lost in the process, at least from the perspective of the mainstream. In that sense, one might say that the capital surveys backfired, to Harcourt’s disappointment.

At the same time, post-Keynesian economics emerged and became institutionalized, initially in the United States, largely as a result of those developments (see the “manifesto” by Eichner and Kregel 1975; and the historical accounts by King 2002 chapter 4, and Mata 2004). However, it soon became clear that the outcome, lessons and relevance of the Cambridge capital controversies were not seen the same way by distinct and often conflicting strands of post-Keynesian economics (see further below), as perceived by Harcourt in his influential new series of surveys, this time about post-Keynesianism (Harcourt [1982] 1985; Hamouda & Harcourt [1988] 1992; Harcourt 2001c). The Hamouda-Harcourt ([1988] 1992) survey was close to Harcourt’s (1987) entry on post-Keynesian economics in the first edition of the *New Palgrave*. Two of the editors of the *New Palgrave* (John Eatwell and Murray Milgate) were Cambridge post-Keynesian Sraffian economists, which helps to explain the relatively large number of entries in that *Dictionary* on topics related to the capital debates. However, puzzling enough, none of those entries was

written by Harcourt, which might reflect the fact that the conclusions he drew from the capital controversies were not quite the same as the Sraffians’.

Harcourt belonged to what he dubbed the “class of 1955-58” of Cambridge graduate students. It included, among others, Pierangelo Garegnani, Luigi Pasinetti, Amartya Sen, John Whitaker, Charles Feinstein and Athanasios (Tom) Asimakopulos – who in fact arrived before 1955 and stayed until 1956, and would eventually become Harcourt’s close friend. 1956 was the year of publication of Joan Robinson’s *Accumulation of Capital*, which she taught in her Cambridge lectures at the time. Robinson’s book and lectures had a “profound effect” on Harcourt by providing an analytical framework that would form the basis of his theoretical work from then on (Harcourt 1992b: 3). Indeed, his accounts of post-Keynesian economics did not hide his preference for the Robinsonian-Kaleckian strand. Robinson was a key figure in his surveys of the capital debates, with endorsement of her claim that the central issue was the search for an appropriate method of analysis of the *processes* of accumulation, distribution and growth, as opposed to the comparative statics of *differences* in steady-state equilibrium positions. Nevertheless, as discussed below, Harcourt ([1991] 2001b: 211) protested against the notion that he was a “*militant* Robinsonian”.

Robinson never managed to formally analyse such disequilibrium processes, something that, as Harcourt acknowledged, was also missing from the models discussed in his surveys. He had attempted – in a paper he often described as his favourite one ever – to deal with economic dynamics as a succession of short periods (Harcourt 1965a; see Harcourt 2001a: 13). Presented successfully at a Cambridge seminar in 1964, it was published in the leading Australian journal *Economic Record*. As recalled by Harcourt (2001b: 13), the paper “made explicit the structure of my approach to understand the processes at work in modern economics from then on.” However, he would seldom mention the 1965 *ER* article, apart from references in his autobiographical pieces, probably because its impact on the literature was rather disappointing. We shall see that some key ideas from the 1965a paper are often implicit in much of what Harcourt wrote afterwards about economic dynamics and historical time.

Harcourt (1965a) grew out of his formal criticism of the full-employment assumption of Nicholas Kaldor’s (1955-56, 1957) theory of distribution and growth (Harcourt 1963a). Moreover, the 1965 *ER* paper was an effort to “avoid the obstacles



surrounding the theory of accumulation and capital emanating from Joan Robinson's and Piero Sraffa's writings" (Harcourt 2019a: 101). Neither Kaldor nor Robinson deployed mathematics to convey their economic concepts. Harcourt (1963b) was an attempted contribution to formalize essential aspects of Robinson's (1956) complex volume, along the lines suggested by Harry Johnson (1963), who taught at Cambridge from 1949-1956; it was further developed in Harcourt's version of Asimakopulos's (1969) Robinsonian model (see Harcourt 1972: 232-40). Sraffa (1960), the other Cambridge economist Harcourt carefully discussed at the time, on the other hand, used plenty of mathematics in his difficult book. It took Harcourt intensive joint effort with Vincent Massaro – who was visiting Cambridge in 1963 – to get on the top of it, particularly Sraffa's innovative notion of subsystems (Harcourt & Massaro 1964a, b; Harcourt 1972, appendix to chapter 4).

Harcourt was impressed by Sraffa's analytical restatement of the classical concept of economic surplus (see e.g. Harcourt 1986). He would define economics as the "study of the processes whereby surpluses are created in economies, how they are extracted, who gets them and what they do with them ... Capitalism does it in a particular way ... I would like to help to create a society where the surplus is extracted and used in a way quite different from that of a capitalist society" (Harcourt 1992b: 9). Despite Harcourt's admiration for Sraffa's analytical achievements, he never regarded himself a Sraffian (or neo-Ricardian) economist, mainly because Sraffa (1960) did not deal with historical time, as discussed further below.<sup>7</sup>

When Harcourt returned to Cambridge in 1982, his days as model-builder were largely gone. Instead, his main project was to document and write the intellectual history of what he called "Joan Robinson and her circle", formed by J.M. Keynes's Cambridge pupils, immediate colleagues and their followers – Austin Robinson, Richard F. Kahn, P. Sraffa, N. Kaldor, Richard Goodwin and L. Pasinetti (sometimes David G. Champernowne was added to the list) – and by Michal Kalecki, who spent a few years in Cambridge in the late 1930s and influenced Joan Robinson

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<sup>7</sup> Sraffian economist Edward Nell (1983) was puzzled by the fact that, having mastered Sraffa's concept of subsystems, Harcourt, unlike Pasinetti or I. Steedman, never used it in his own models. Nell was critical of what he perceived as Harcourt's contradiction between theoretical emphasis on short-run models and methodological attention to Sraffian surplus analysis in economies with a uniform rate of profit. However, from Harcourt's perspective there was no contradiction (see e.g. Harcourt 1981).

deeply from then on. Harcourt (1998: 12) “passionately” believed there was an internal coherence to the Robinson circle’s version of the Cambridge tradition that should be preserved.

Harcourt’s perception of Joan Robinson’s central role reflected his own trajectory as an economist (see Davis 1997: 452-53). Indeed, he was as much part of the “circle” as, e.g., his colleague Pasinetti. During his last, long Cambridge period (1982-2010), Harcourt became increasingly attracted to the history of modern economic thought, often in the form of intellectual biographical portraits and oral histories (see Harcourt 1993). That period culminated with his 2006 volume about post-Keynesian economics as done mostly by the Cambridge “pioneers” from 1950s-70s, followed by his biography of Robinson joint with Prue Kerr (Harcourt & Kerr 2009).

The intellectual history of the Robinson circle was deemed a “vital project” in order to “salvage the wisdom” contained in the writings of Keynes, Kalecki and their Cambridge post-Keynesian pupils and colleagues (Harcourt 2001a: 20-21). Few years after Harcourt’s return to Cambridge, Joan Robinson, Kahn, Sraffa and Kaldor passed away, which added relevance to the project – even more so as they had failed to establish successors with chairs and power at the Faculty of Economics. The downfall of the Cambridge tradition – as illustrated by the fact that Harcourt never got a personal chair – was evident enough by the time he retired in 1998, which led him to discuss the issue as part of papers and interviews, usually in bitter terms. The contrast between Cambridge in the 1950s-60s on one hand and after the mid 1980s on the other was all too clear: “You will not find [in Cambridge nowadays] what I found, first as a research student there in 1955 and then a young lecturer there in the 1960s, which was the most exciting time in our life and, Joan<sup>8</sup> and I always say, possibly the happiest time as well” (Harcourt 2012b: 23).

However, that did not mean he regarded the post-Keynesian research program as dead-end. Indeed, the *Handbook of Post-Keynesian Economics*, formed by invited contributions – edited in two volumes by Harcourt and his former student Peter Kriesler a couple of years after Harcourt moved back to Australia as Honorary Professor at the School of Economics at the University of New South Wales (Sydney)

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<sup>8</sup> Geoff and Joan Harcourt (née Bartrop) got married in Melbourne in August 1955, shortly before leaving for his doctorate in Cambridge.

– was intended to show otherwise (see Harcourt and Kriesler 2013a). He saw that *Handbook* as the “culmination of my life’s work” (Harcourt 2019a: 111).

## 2. History of economics and economists’ lives

Around the time he arrived for a permanent position in Cambridge, Harcourt (1983) summed up in the *Who’s Who in Economics* his main contributions so far, divided into three areas. The first was capital theory, as he tried to make clear “in a fair and good-humoured way”, to students and colleagues alike, the issues of the Cambridge controversies. His contributions to “post-Keynesian theory” came next, particularly to the theory of pricing and investment decisions, and to the theory of income distribution and level of activity in the short run. Finally, he referred to his biographical sketches of some leading economists as an attempt to “make the subject more alive and humane.”

That was also the time when Harcourt put together his first collection of previously published essays, which would be followed by other volumes approximately every 5 or 6 years until 2012. Some of his papers written after that were collected in *Post-Keynesian Essays From Down Under*, jointly with other Australian economists (Halevi, Harcourt, Kriesler and Neville 2016). By the time of his entry in the *Who’s Who*, he had written two books: Harcourt (1972)<sup>9</sup> and an introduction to Keynesian macroeconomics (Harcourt, Karmel and Wallace 1968)<sup>10</sup> with colleagues from the University of Adelaide. He would write two other, close to the end of his 1982-2010 Cambridge period, as mentioned above (Harcourt 2006 and Harcourt & Kerr 2009). His books on *Economic Activity* (1968) and *Post-Keynesian Economics* (2006) reflected his teaching experiences over the years in Adelaide and Cambridge, respectively (see Harcourt 2001d).

Upon his arrival in Cambridge in 1982, Harcourt’s output as historian of economics became increasingly conspicuous over the years, partly as by-product of his research project on Robinson and her circle. As a result, apart from biographical

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<sup>9</sup> Translated into Italian, Polish, Spanish and Japanese between 1973 and 1980.

<sup>10</sup> Translated into Italian in 1969.

essays, he published historical pieces in journals such as the *Economic Journal* (Harcourt 1986), *History of Political Economy* (Harcourt 1984, 1990; Gram and Harcourt 2017), *History of Economic Ideas* (Harcourt 1991a 2001e, 2009), *Journal of the History of Economic Thought* (Araujo and Harcourt 1993), *Cambridge Journal of Economics* (Harcourt 1994b), *History of Economics Review* (Harcourt 1991b) and *Eastern Economic Journal* (Harcourt 1981, which should be read against the background of his [1982] 1985 survey of post-Keynesianism).

Moreover, he produced broad historical accounts of economics at Cambridge and of growth theory – beyond the Robinson circle – as book chapters (Harcourt [1998] 2001b, Harcourt & Kerr 2003; Harcourt 2016, chapter 7 on “Theories of growth: from Adam Smith to ‘modern’ endogenous growth theory”, and appendix I on “Biographical sketches of the pioneers”). Finally, Harcourt’s collaboration with Avi Cohen sharpened his historical take on the capital debates that took place not just during the Cambridge controversies, but also before that (Cohen and Harcourt 2003, 2005a). Indeed, Harcourt’s (1969a, 1972) surveys may be read as a genre of contemporary history of economic thought (see Davis 1997).<sup>11</sup>

Most of Harcourt’s HET pieces are about or closely related to Cambridge, including Alfred Marshall and disputes over his theoretical heritage. He identified two major routes that came out from the Cambridge Marshallian tradition: one goes through Keynes and macroeconomics – further subdivided into the post-Keynesian group’s essentially theoretical work and the Cambridge Department of Applied Economics (DAE) led by Richard Stone – and the other through Pigou and economic welfare, represented mainly by Amartya Sen and James Meade (who extended both branches). Moreover, Dennis H. Robertson, the quintessential Marshallian economist, developed an approach of his own, particularly in monetary macroeconomics, which led to a split of Cambridge into two camps witnessed by Harcourt as a graduate student in the 1950s: the Keynesians, led by J. Robinson and R.F. Kahn, and the people around Robertson (Harcourt 2012b).

That split had its origins in the 1930s, when Keynes and Robertson parted company following the publication of the *General Theory*, having collaborated

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<sup>11</sup> The History of Economics Society, the European Society for the History of Economic Thought and the History of Economic Thought Society of Australia have all elected Harcourt as a Distinguished Fellow, in recognition of his work as a historian of economics.

extensively before that – an event Harcourt ([1998] 2001b: 343) described as “both a personal tragedy and a professional one for the development of economic theory.” Harcourt ([2000] 2001b: 203) eventually put Robertson in the company of Kalecki and Goodwin as three of the “most perceptive” students of capitalist processes who showed that economic growth is inevitably cyclical. Hence, he eventually sided with Robertson’s view of the interaction of the real and monetary sectors, against Keynes’s claim that macroeconomic stabilization should try to remove cyclical developments altogether (Harcourt & Kerr 2003: 345; see also Boianovsky 2018).

Harcourt’s thesis supervisor, Ronald Henderson – after a disastrous attempt to be supervised by Kaldor – belonged to the Robertsonian segment. That did not interfere with Robinson’s overwhelming influence on Harcourt, as his PhD thesis, unlike Garegnani’s, Sen’s and Pasinetti’s theses, did not address Cambridge theoretical themes, but a topic of applied economics reflecting his strong Australian accounting background (Harcourt 1992b: 3; Harcourt 1960; see section 3 below). Harcourt attended Robertson’s lectures in 1955, but “could not make heads or tails of them” until he read the three volumes of Robertson’s published *Lectures* for his 1991 article on Marshall’s centenary (see Boianovsky 2003: 783).

Harcourt (1990) is remarkable for bringing out Joan Robinson’s early pioneer approach to tractability issues in economics. His 2001 *HEI* article provided a first general account of the phases in Robinson’s intellectual trajectory, from the perspective of the influences of her teachers and, later, colleagues, including Oxford economist Roy Harrod. As Harcourt (2001e: 66; Harcourt & King 1995: 48) pointed out, there were also clashes in the 1950s and 1960s within the Cambridge post-Keynesian group. J. Robinson and Kaldor disputed priority over the generalization of the *General Theory* to deal with growth and distribution issues in the long run. Harcourt’s accounts of those and other events in the history of Cambridge economics are often coloured by inside perspectives, as in his remark that “Nicky [Kaldor] thought that Joan [Robinson] was pinching his ideas, Joan tried to calm him down but Richard [Kahn], a rather naughty man in some ways, stirred them up, just when there was a terrible fight between the Robertsonians and the Keynesians in Cambridge” (Harcourt 2012b: 20).

Harcourt’s 1984 *HOPE* article – originally delivered as the 1982 G.L. Wood Memorial Lecture at Melbourne University in June 1982 – is one of the exceptions to the overall dominance of Cambridge topics in his HET agenda. It consisted of a

pioneer attempt to tell the history of modern economics from the perspective of economic Nobel prizes awarded from their start in 1969 up to the early 1980s. Harcourt (1984: 489) regretted the “scandalous oversight” that had kept Joan Robinson out of the awards.<sup>12</sup> Apart from writing to most Nobel Prize winners and some other well-known economists (e.g. R. Solow, J. Tobin, R.C.O Matthews, W. Baumol) with questions about their background and training, Harcourt read their Nobel lectures, Nobel memorials etc. That was part of his broad interest on how economists’ backgrounds and personalities affect their approaches. It linked up with his exercises in oral history displayed in a number of biographical essays informed by chats with his subjects, started with a sequence published in the *Journal of Post Keynesian Economics* (on G. Shackle, K. Boulding, L. Tarshis and R. Goodwin; see Harcourt 1993, part II). That was followed years later by conversation with and intellectual biography of Richard Stone (Harcourt 1995d; Pesaran and Harcourt 2000), among the most comprehensive in Harcourt’s series of oral histories.

Harcourt (1984: 490) concluded from his perusal that Alfred Marshall’s “cool heads and warm hearts” were well represented among the Nobel laureates up to 1984. That was his favourite Marshall phrase.<sup>13</sup> Most of the recipients saw economics as a

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<sup>12</sup> Meade was the only Cambridge-connected economist awarded the Nobel Prize in the period covered by Harcourt (1984); Stone would receive it in 1984, Mirrlees in 1996 and Sen in 1998. Harcourt (1988: 167) saw Kaldor’s absence from the awards as yet another (apart from Robinson and Kahn) “scandal” and “victim of the, perhaps unconscious, boycott by the Electors of those unorthodox creative minds who were too close to Keynes and to England.” He speculated that that was a belated reaction by the Swedes to the way “the Brits treated Wicksell” (Harcourt & King 1995: 58). Interestingly enough, Harcourt reported a dinner with Assar Lindbeck (then chairman of the committee for the economics Nobel Prize) in Melbourne just before the economics Nobel Prizes started, when Lindbeck revealed that he was going to give it to E. Chamberlin and J. Robinson for their contributions to the theory of monopolistic competition. Harcourt then reacted by informing Lindbeck that Chamberlain had died two years before, and that Robinson had repudiated her 1933 *Economics of Imperfect Competition*. Lindbeck responded, “‘Well, she’s not done anything else’. So we argued for ages on that” (Harcourt & King 1995: 59). It is worth mentioning that Sraffa had been awarded the Söderström Gold Medal by the Royal Swedish Academy of Sciences in 1961, which was equivalent to receiving the Nobel Prize, largely thanks to Gunnar Myrdal (see Arthmar and McLure 2019; and Harcourt 2004, one of his best short biographical pieces).

<sup>13</sup> Marshall (1885: 57; italics added) vowed in the last paragraph of his Cambridge inaugural lecture that “It will be my most cherished ambition to increase the numbers of those whom Cambridge ... sends out to the world with *cool heads and warm hearts*, willing to give some at least of their best power to grappling with the social suffering around them.” At the same time, Marshall wanted to avoid his students

means to analysing economic problems (such as poverty, unemployment, discrimination and inflation) in order to improve the “lot of ordinary men and women” (ibid). Harcourt (1992b: 9) would later define his own goal as an economist along those lines: “to make the world a better place for ordinary men and women, to produce a more just and equitable society.” Interestingly enough, he included in that set Nobel winners who, like M. Friedman, F. Hayek and G. Stigler, opposed discretionary government intervention, as they based their case on the damage to welfare that they believe “will ensue and has ensued” – although he rejected their critical interpretations of Keynes’s macroeconomics as over-simplifications (Harcourt 1984: 490-91; 500). Harcourt was particularly positive about the concern with economic development issues displayed by some Nobel recipients – as in models of dual economies designed by Arthur Lewis in the 1950s, whose framework was based on the classical concept of the economic surplus, along the lines Sraffa (1960) would develop later for rather different purposes, a connection seldom acknowledged by Cambridge economists apart from Harcourt.

“I still believe in heroes and heroines and I like to know what makes them tick”, Harcourt ([1996] 2001b: 330) stated while explaining his attraction to intellectual biographies and their role in allowing to see the links between the individuals’ “historical settings, their class, educational, philosophical and religious backgrounds” and the economic issues they worked on. That was also how he taught HET, under the influence of R. Heilbroner’s 1953 classic *Wordly Philosophers*, which came out when Harcourt was an undergraduate in Melbourne (see Harcourt 2019a: 114). The formulation of economic theories is driven by the economist’s goals and conditioned by the economist’s view of the world and his or her life trajectory, which explains Harcourt’s concern with investigating the economist’s values and personality as a first necessary step before interpreting his or her economics (see Dow 2004: F175). This is distinct from J. Schumpeter’s well-known notion of “vision”, since, in Harcourt’s approach – as in some others like G. Myrdal’s or M. Dobb’s – the

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being soft headed, as he felt the socialists to be (Harcourt 1972: 165). Harcourt (ibid) worried that emphasis on mathematical rigor and elegance could turn economics barren, away from Marshall’s dictum. In another paper, written in 1980 but published 16 years later, Harcourt ([1996-97] 2001b: 290-91) expressed his concern that the heads of economists belonging to New Classical macroeconomics “are cool, but their hearts are cold; we, all of us, are on our supply curves where what we do is what we wish to do”, away from the “enlightened policies” of the past.

economist's world-view and ideology do not tend to gradually recede into the background as "scientific progress" takes place, but remain permanently intertwined with his or her theoretical framework.

Harcourt's biographical essays – collected in Harcourt (1993) and, for the material written after that, spread over journals and other collections of his papers<sup>14</sup> – have been compared to J.M. Keynes's (1933) *Essays in biography* (Dow 2004: F175; Backhouse 2007: 62). Like Keynes, Harcourt focused on individuals he knew personally; both men generally wrote biographical essays out of affection and admiration for their subjects. Again, in both cases Cambridge (and its associates) received much of the attention. As Harcourt (2019b) pointed out, Keynes explored his subjects' family antecedents and psychology, as well as their religious and philosophical beliefs, with a capacity to penetrate and evaluate people's personalities. In view of the passages quoted above, these look like qualities that Harcourt tried to emulate in his own biographical pieces. However, Harcourt ascribed part of Keynes's interest on psychological aspects – in the *Essays* and, in a different context, in the *General Theory* as well – to the popularity of Freudian ideas in Keynes's circle at the time, which Harcourt (2001a: 21) believed not to be the case anymore in his own time.

As noticed by reviewers of Harcourt & Kerr's 2009 *Joan Robinson*, details about Robinson's life take up a relatively small portion of the book (as part of the introductory chapter), which is mainly an intellectual biography along the lines of most of Harcourt's biographical essays. Harcourt & Kerr (2009: 1) offer "some glimpses" into Robinson's life, such as her background, of her forebears, upbringing and other information that provide insights into her intellectual life. As Harcourt (1998: 13) stated, "I am not a fan of the modern 'tell it all' biography". Accordingly, his use of correspondence etc. from archival material was selective. The dominant focus was the development of ideas; in the case of Robinson, as in many others,

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<sup>14</sup> See e.g. Harcourt 2001b, parts II and III on "Intellectual Biographies" and "Tributes", which include essays on Joan Robinson (previously published in the *EJ* 1995), Austin Robinson (originally published in *Proceedings of the British Academy*, vol. 94, 1996) and L. Tarshis (originally in the *EJ* 1995), among others. Harcourt was the obituary editor for the *Economic Journal* (1990-98) and after 2010 for the Australian *Economic and Labour Relations Review*, when he wrote brief tributes to K. Arrow, H. Uzawa and J. Mirrlees, among others. Of course, his major biographical work was the volume on J. Robinson, approached as an extended intellectual biography (Harcourt and Kerr 2009).



Harcourt purposely “left the discussion of personal, as apposed to intellectual interactions, to a minimum” (Harcourt & Kerr 2009: 203). Hence, for instance, although Harcourt ([1990] 1992: 300) described the correspondence between J. Robinson and Sraffa as an “intimate discussion of close friends who share a common language and who enjoy gossip”, containing as well “some fascinating debates from which Sraffa invariably emerges the winner”, he refrained from using that material.<sup>15</sup>

Again, Harcourt (2001e: 59-60) (see also Harcourt & Kerr 2009: 1-2) suggested that J. Robinson’s childhood was affected by the “infamous Maurice debates” that took place in the House of Commons in 1918, involving her father Major General Frederick Maurice’s criticism of Lloyd George’s military strategy during the War. Harcourt reported that J. Robinson had told him that, until she was 15, she was known as Maurice’s daughter, “her life in her mind was more real to her than life in reality”, when she did a switch (ibid). Harcourt (2001e: 59-60) conjectured that “her childhood fantasy life may be one clue as to why she was such a powerful theorist and remorselessly logical writer.” However, instead of following that up, Harcourt (2001e: 60) downplayed it by remarking: “But enough of speculative psycho-babble”.

Harcourt’s approach to the history of Keynesian and post-Keynesian economics in general, and of J. Robinson and her circle in particular, has been sometimes criticized for its allegedly uncritical tone and attempt to vindicate heterodox economics. J. Lodewijks (2003: 660) has claimed that “it is quite a challenge” to find any critical remarks about Cambridge, the “circle” and Keynes in Harcourt (2001b). However, a careful reading indicates otherwise. A case in point is Harcourt’s ([1992] 2001b: 36) discussion of what he perceived as Keynes’s failed attempt at being formal about investment theory in chapter 11 of *The General Theory*. Keynes’s “makes one hack of a mess of it”, which, Harcourt suggested controversially, was caused by the fact that “he probably had forgotten all his maths by the time he was in his late forties, so there are a lot of errors running through the

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<sup>15</sup> Harcourt & Kerr (2009: 101) did reproduce a letter of October 1936 from Sraffa to Robinson, which as Harcourt pointed out a couple of times, anticipated aspects of capital measurement problems Sraffa (1960) would elaborate in detail in print later. Another example of Harcourt’s use of *professional* correspondence is his careful discussion of exchanges between J. Robinson, M. Dobb and G. Shove on accumulation and profits (Araujo and Harcourt 1993).

maths of *The General Theory*.”<sup>16</sup> Again, referring to the history of the Cambridge Faculty of Economics and Politics since Marshall, Harcourt ([1998] 2001b: 335) observed critically: “We have always had a propensity to produce bantam cocks who fight with great gusto on a dunghill for their positions.” Keynes (1933) glassed over Marshall’s “manipulative and scheming ways” in Cambridge politics, which had set up that pattern, Harcourt (2019b: 177) charged.

Harcourt was as fond of his autobiographical memoir essays as he was of the biographical pieces he produced about other economists. His recollections are especially rich as sources about his family and psychological backgrounds. As he put it, “political and especially religious problems were, from very early on, stark and frightening experiences for me” (Harcourt 1992b: 1). He was born as the younger of twin sons of Marjorie and Kenneth Harcourt, into a middle-class “assimilationist, agnostic Jewish household with right-wing political views.” He recalled Melbourne then as a “stuffy, snobby place”, marked by sectarian fights between Catholics and Protestants, who were nevertheless united in their anti-Semitism against a large Jewish community (ibid). Anti-Semitism, especially at school, was a “fundamental landmark on [his] psychological and philosophical development”, which made him conscious of being an “outsider” (Harcourt 1998: 4).<sup>17</sup> His parents’ assimilationist position was partly ascribed to their attempt to protect the children from anti-Semitism, to no avail. Geoff Harcourt would eventually disagree with his parents’ political and, especially, religious stances, which, he regretted, denied him of a rich “birth-right” (see Macfarlane 2007).

By the time he enrolled Melbourne University in 1950 for a B. Com. (Commerce) degree, with its atmosphere of enlightenment and tolerance so distinct from his childhood, Harcourt (1992b: 1) became involved in the search for a political philosophy and a religious belief. As he recalled, “the two pressing issues for

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<sup>16</sup> Arrow (1999) recalled, from his visits to Cambridge, Harcourt’s interest in HET and its methodology, as illustrated by an episode when Harcourt “introduced and defended a young scholar at a seminar, one who was advocating a method of ‘rational reconstruction’, very far removed from the ‘unmasking’ approach so common in Cambridge at that time.” As pointed out by Harcourt (2016a: 559), Arrow had in mind Stanley Wong’s PhD thesis on P. Samuelson’s revealed preference theory, supervised by Harcourt at Cambridge in its final stages in the early 1970s (see Wong 1978).

<sup>17</sup> His father’s parents came from Eastern Europe, and his mother’s from Germany and England. The original family name was Harcowitz, changed by his father – who was a leather merchant – to Harcourt in the 1920s (see Harcourt 1998).

thoughtful students in those days were stances on political and belief or not in God” (Harcourt 2011a: 119). The lectures on economic geography in the first year quickly persuaded Harcourt that economic systems were often organized on irrational and unjust foundations, which led him to abandon the right-wing views of his parents, embrace “democratic socialism” and join the Australian Labour Party (ibid).

The journey to religious belief took a bit longer, as he was puzzled by the divergence between the beliefs and declared moral values of Christians on one hand, and their actual behaviour on the other, especially in capitalist societies (Harcourt [1999] 2012a: 13-14). By 1953, after discussions with theologians at his university college (Queen’s College, a Methodist institution), Harcourt made up his mind and moved to conversion to Christianity. By 1958, upon returning from Cambridge and taking up a position in Adelaide, he called himself the only “Jewish Methodist in that fair city” (Harcourt 2011a: 110). Commenting on Harcourt’s religious conversion, Perlman (1997: 464, n.1) observed that he saw human improvement as associated “not to the individualism strand in our heritage but to the communitarian one”.<sup>18</sup>

### 3. Those capital debates

The turning point in Harcourt’s career, represented by his 1969 *JEL* survey, came by accident, as he recalled a few times. Mark Perlman was visiting Melbourne from the United States in August 1968 – as Perlman did regularly since the early 1950s, when he carried out research for his PhD thesis about almost unique Australian centralized wage-fixing institutions (see Perlman 1954) – when he mentioned to Wilfred Prest that J. Hirshleifer, whom he had commissioned to contribute a survey on capital theory for *JEL*, had declined the invitation a few weeks before (see King 1995: 176; Mata 2004: 245, n. 9). Prest, who had taught Harcourt capital theory at Melbourne University back in the early 1950s – Hayek, Boulding and Hicks were on the reading

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<sup>18</sup> Harcourt (1994a: 207-09) rejected F. Knight’s view that religion is related to individual instead of group behaviour, defined as a social community to which “*both* religious beliefs *and* moral actions are attached.” He sided with P. Wicksteed’s – one of his “heroes” during his undergraduate years in Melbourne – belief system of indissoluble integration of religion with economics, the basis of Wicksteed’s socialism (see also Harcourt [1999] 2012a: 14-15).

list (see Harcourt ([1999] 2012a: 20) – suggested Perlman to approach Harcourt in Adelaide, as he was “good on capital theory” (King 1995: 176). Moreover, as recalled by Perlman (1995: ix), Prest told him of Harcourt’s “unusual ability” in getting each side’s position in economic debates straight and sympathetically stated. Perlman then asked Harcourt to “explain to Americans the Cambridge-on-the-Cam side of the capital debate between Joan Robinson ... and Solow/Samuelson” (Perlman 1995: ix). Harcourt’s ([1999] 2012a: 26) recollection was that Perlman wanted him “to do a ‘Hahn-Matthews’ for capital theory”, referring to Cambridge economists F. Hahn and R.C.O. Matthews’s 1964 famous survey of growth economics.

Indeed, Harcourt did more than explaining Robinson’s complaints about capital measurement to American economists, as initially conveyed in her 1953-54 paper. The survey was successful in clarifying the terms of the debate and its main technical result – reached in the 1966 *Quarterly Journal of Economics* symposium on capital theory – that, due to reswitching and capital-reversal phenomena, the  $K$  of the production function can only be used, for both theoretical and empirical work, if there is just a single homogenous capital good. Under more general circumstances, capital-intensity is not uniquely related to relative factor prices.<sup>19</sup>

However, the interpretation of those results and of their implications for distribution and growth theories were not the same on the two sides involved, which led to criticism, by some neoclassical economists, of the *JEL* survey and its 1972 book sequel, followed by Harcourt’s (1976) counter-reaction and attempted further clarification. As put by Nell (1983: 1595), although Harcourt (1969a, 1972) was not neutral, the role he adopted in the Two-Cambridge battlefield was of a “war correspondent, rather than a combatant” as he tried (successfully) to report objectively.

Harcourt’s Cambridge (UK) credentials were made explicit in Harcourt (1971) – prepared for a well-known book of readings co-edited with his Adelaide colleague N. Laing, who played for the “neoclassical” side<sup>20</sup> – and in Harcourt (1976), which rejected general equilibrium as a way out of the difficulties faced by neoclassical aggregative distribution theory, and further elaborated J. Robinson’s methodological

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<sup>19</sup> Cambridge (UK) overall concern with conceptual and measurement precision went back to J.M. Keynes, as discussed in comprehensive fashion by Bradford and Harcourt (1997).

<sup>20</sup> Harcourt (1971: 9, n. 1) stated that he was “proud to wear the all-red colour of the neo-Keynesian club.”

critique concerning changes versus differences. Unlike Harcourt's other papers (and 1972 book) on the capital controversies, Harcourt (1976) was not commissioned. It was initially submitted to the *American Economic Review* but rejected under unpleasant circumstances that, according to Harcourt ([1995c] 2012a: 264), indicated a fore-taste of the "narrowness and ruthlessness of the mainstream as they recovered confidence in the 1970s and decided (at least tacitly) to purge the profession of dissent."

Harcourt had not written about capital theoretical debates before embarking on the production of his 1969 survey. However, related conceptual and measurement problems were conspicuous in his PhD thesis, a few papers and a co-edited collection of *Readings in the Concept & Measurement of Income* (Parker and Harcourt 1969), reflecting the accounting foundations of the study of economics in Australia at the time (see Groenewegen & Macfarlane 1990: 197). "The accountant in a golden age" (Harcourt 1965b), his best-known paper next to the *JEL* survey, investigated whether accountant methods may be used to find out what the economic rates of profits are. Since the model could not be solved analytically, Harcourt used simulations on the then pioneer Cambridge computers to establish that the accounting and economic profit rates (in J. Robinson's "Golden Age" with fulfilled expectations) differed mainly because of the depreciation methods deployed.

The accounting background is also clear in Harcourt's (1960) PhD thesis, an empirical study for British firms – inspired by some recent investigations for Australia – of the implications of using historical cost accounting procedures to measure income for dividend and tax purposes and to set prices in inflation periods. Harcourt (1958), instrumental in his application for a position in Adelaide, was part of the dissertation.<sup>21</sup> It was part of Harcourt's broad interest in the links between accounting practices and economic performance at the micro and macro levels.

By the time Harcourt was asked to do the capital survey essay, his research agenda was dominated by practical and policy-oriented material about rates of return, investment incentives and methodological criticism of econometric estimations of CES production functions (see e.g. Harcourt 1965b, 1966, 1968). His joint work with Massaro on Sraffa was – together with Harcourt (1965a) – an exception to that. Indeed, when he decided to spend an academic year in Cambridge in 1963-64 on

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<sup>21</sup> On the relation between economics and accounting in Harcourt's framework see also Velupillai (2017: 1008-10).

leave from Adelaide, his priority was to get on top of Sraffa (1960), described as “the hardest intellectual task of my life” (Harcourt 1992b: 4). Harcourt & Massaro (1964a, b) reflected their systematic interaction with Sraffa at Cambridge. Although the effort to grasp Sraffa (1960) would prove helpful for his survey (see Harcourt 1972, appendix to chapter 4), Harcourt & Massaro missed – like many others at the time (see Birner 2002: 68) – Sraffa’s discussion of reswitching in part III of *Production of Commodities* (King 1995: 175).

By 1965, when Samuelson’s pupil D. Levhari (1965) claimed to have shown that reswitching and capital-reversal could occur in an industry but not in the economy as a whole, the concept was well established. Harcourt ([1999] 2012a: p. 45, n. 9) recalled how he was the “first person in Cambridge” to read Levhari’s article and to report to Sraffa that “some chap” in the *QJE* “said his [Sraffa’s] result was wrong.” Sraffa reacted: “He’s wrong – and you show it”. Harcourt replied: “I can’t do matrix algebra.” “Neither can I”, retorted Sraffa. So Sraffa asked Pasinetti, who proved Levhari (and Samuelson) wrong (Pasinetti 1966). This is Harcourt’s ([1999] 2012a: 33) recollection of how “I missed my moment of glory” because he could not do matrix algebra. It was not the only time his mathematics would let him down (see section 4 below).

While Harcourt was “floating round the capital theory area” (King 1995: 176) around 1968, he had not yet mastered the main issues of the capital debates when Perlman visited him in Adelaide. At first, Harcourt was so overwhelmed by the huge and difficult literature that he “despaired” until he decided to circulate working papers on parts of the debates to several friends and readers, both for and against the Cambridge (UK) position (Harcourt 2019a: 105). The first – and most difficult one – was titled “A child’s guide to the double-switching debate” (see Harcourt 1969a: 386-96; Harcourt 1972, chapter 4). Readers included, among others, Joan Robinson, J. Mirrlees, L. Pasinetti, R. Solow, E. Nell, M. Dobb, J. Stiglitz and C. Bliss, as Harcourt (1969a: 369) acknowledged.<sup>22</sup> He was especially thankful to a mathematician named Pippa Simpson for expert advice that saved him from getting (even more) lost in the “jungle of squiggles that characterizes most modern economic theory.”

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<sup>22</sup> Harcourt (1972: viii) added A. Asimakopulos, P. Davidson, D. Newbery and W. Nordhaus, among others, as well as his Japanese hosts at Keio University, where he wrote the first draft of the book in the second semester of 1969.

The main pieces of mathematical formalism used by Harcourt in his survey came from neoclassical economics, particularly Samuelson's work on linear programming, non-substitutions theorems and the trade-offs curves between wage and interest (w-r curve, or factor-price frontier). Harcourt (1972: viii) observed the irony that while his survey was mainly critical of neoclassical equilibrium analysis, much of the formal part is expressed in those terms. Harcourt (1969a, 1972, 1976) explained, mathematically and through several influential diagrams, that, if the w-r trade-off curves are not straight lines (as Samuelson 1962 assumed they were under the strong assumption of uniform capital-labour ratios throughout) but convex or concave to the origin, they may intersect twice or more, which is the end of the neoclassical parable that the economy works as if it consisted of a single sector. As put by Harcourt (1969a: 387) in his unique style, "the neoclassical tradition, like the Christian, believes that profound truths can be told by way of parable".<sup>23</sup>

Reswitching and capital reversal, as Harcourt (1972: 124-25) recounted, were first noticed by J. Robinson and D. Champernowne in the 1950s, and by Sraffa in 1960. Champernowne provided an early formal analysis, but it was only after Samuelson (1962) and the 1966 *QJE* symposium that such "perverse" and "curious" phenomena (as Robinson called them) and their implications became well known. Robinson described them, but she neither realized nor spelt out their implications at the time (Harcourt 1972: 125). Indeed, Robinson would focus instead on the methodological problems involved in the comparisons of long-run equilibrium states as poor guides as to what ultimately may happen during the process – even if the factor price frontier is a straight line, a change in the rate of accumulation will bring about a "transition-out-of-equilibrium process" in historical time as long-run equilibrium and the expectations associated to it are disturbed (Harcourt 1976: 150).

Interestingly enough, Harcourt often pointed out that Samuelson and some other neoclassical economists were not oblivious to Robinson's warning, even if they did not always acknowledge her priority in that regard (see also Birner 2002: 23-24). Already in the 1969 *JEL* survey he observed that "modern writers" have been careful to clarify that their analysis deal with comparisons of different equilibrium positions,

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<sup>23</sup> J. Robinson, who in general approved of Harcourt's surveys on capital, was not fond of the jokes he often used to convey concepts and ideas (see Harcourt 1992b: 6). His overall goal was to restore senses of "proportion, balance and good humour" in the debates (Harcourt 1972: 250).

not processes of actual change. In the book version he called attention to recent growth models in which disequilibrium processes were analysed, “often (but not exclusively) by neoclassical economists equipped with the appropriate techniques to do so” (Harcourt 1969a: 387; 1972: 29; see also Harcourt 1976: 150, and 163, n. 38 on correspondence with Samuelson)<sup>24</sup>. This may be understood as supporting Harcourt’s ([1991] 2001b: 21) later claim that he “was never a *militant* Robinsonian”, against Andreu Mas-Colell’s characterization of him as such. “My role in the capital theory debates (as it was in the anti-war movement, when Australia and New Zealand were America’s only respectable allies in Vietnam) was always that of a moderate, something Robinson upbraided me for”, while she herself clearly was a militant.

Cambridge UK economists – except for Kaldor, who used his distribution mechanism to ensure the equality between (Harrod’s) natural and warranted growth rates – acknowledged that their growth models remained mostly within the “confines” of steady-state (Golden Age) growth analysis. As put by R. Kahn, they were “learning to walk before they ran” (quoted by Harcourt 2001c: 273). In fact, Harcourt (1969a, 1972) followed their lead and was taken to task by Stiglitz (1974) for restricting his survey to stationary-state comparisons of long-period equilibrium positions. Harcourt ([1996] 2001b: 327) would defend the method of analysis deployed in his surveys as suitable to test for the robustness in heterogeneous capital good models of the conclusions of one all-purpose commodity models. Robinson, however, felt increasingly pessimistic about the difficulty to deal analytically with disequilibrium processes. As put by Harcourt, she “felt that she couldn’t get anywhere with analysing processes. That’s what she set out do, and she decided she hadn’t been able to do it” (Harcourt & King 1995: 56), which led her to a nearly “nihilistic” stance at the end of her life (Harcourt 2001e: 66; Harcourt & Kerr 2009: 223).

However – pushed by Harvey Gram’s (2010) criticism that neither Harcourt (1969a, 1972) nor Harcourt & Kerr (2009) had offered an overview of the basic structure of the MIT approach to capital theoretic questions as contained particularly in chapter 12 of Dorfman, Samuelson & Solow (1958), and of Robinson’s (1959) charge that neoclassical economics did not provide a theoretical mechanism to keep the economy in equilibrium – Harcourt would revise his account, as explained in his

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<sup>24</sup> He referred to Hahn (1966) and Cass & Stiglitz (1969), among others.



final assessment of the Cambridge capital controversies, when he joined forces with Gram (Gram and Harcourt 2017).<sup>25</sup>

Of course, by then, nearly 50 years after Harcourt (1969a), and in fact from the 1980s on – as Harcourt was painfully aware – the topic essentially belonged to the history of economics, upon being regarded by most of the profession as a “waste of time” and a “tempest in a teapot” (see Cohen & Harcourt 2003, published in the “retrospectives” section of *JEP*).<sup>26</sup> Gram & Harcourt (2017) set out to argue that Robinson’s (1959) problem of getting into equilibrium came to be perceived as insurmountable by modern treatments of how to achieve equilibrium in dynamic models with saddle-path stable solutions. Gram & Harcourt claimed that Robinson’s point, concerning the reconciliation of past, present and future capital valuations in order to achieve equilibrium, was not solved by DOSSO. Chapter 12 of that famous book, about dynamic optimization, makes clear that “vision at a distance” is embodied in the transversality conditions necessary to pick out the convergent branch of a saddle path with changing capital stocks and prices. Formally expressed, Robinson’s main, intuitive question was how to get on to the convergent path, which has never been answered.

Harcourt’s role in the emergence of post-Keynesian economics – defined, along Robinson’s line, as a theoretical framework which takes into account the differences between the future and the past (Harcourt 2006: 2) – was closely related to the capital debates, as discussed next.

#### **4. Post-Keynesian model-builder**

Harcourt’s (1969a, 1972) narrative of the Cambridge controversies, by bringing out in clear fashion fundamental problems of the neoclassical theory of distribution and growth, played a role in the institutional emergence of post-Keynesianism as part of heterodox economics in the 1970s. However, it soon became clear – to Harcourt

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<sup>25</sup> Chapter 12 of DOSSO (as Hicks named Dorfman, Samuleson and Solow 1958), on “Efficient programs of capital accumulation”, was, nevertheless, reproduced in Harcourt & Laing (1971: 348-68). See also Backhouse (2014), which prompted Harcourt & Gram’s (2017) reaction.

<sup>26</sup> Even authors who did mention the capital debates – like Piketty (2014: 230-31) – often missed the point completely, as Harcourt (2015) complained.

anyway – that there was a difference between the overall negative critical work carried out in the capital debates and the positive task of elaborating an alternative framework. Harcourt (1976: 141-42) warned against the failure to distinguish between “the critical and positive tasks that the Cambridge (England) school have set themselves.” It often happened that the apparatus used for a “destruction job within the enemy’s frame of reference” was interpreted as the “tool with which an alternative paradigm” to neoclassical economics could be constructed. Sraffa’s 1960 book was a case in point. Just because Sraffa chose to investigate the working of economic systems from which changes are excluded, it did not imply that, e.g., prices do not depend on demand, the level and composition of output in actual economies, in the absence of constant returns to scale (Harcourt 1976: 142; 1972: 195).<sup>27</sup>

Eichner & Kregel (1975) had argued for post-Keynesianism as a “new paradigm” in economics. Eichner & Kregel (1975: 1294) acknowledged that Harcourt’s (1969a, 1972) surveys contributed to make some elements of post-Keynesian theory better known in the US through the criticisms coming from Cambridge (UK). However, in the main they regarded the highly abstract and formal Cambridge controversies as a “diversion” that had left the misleading impression that post-Keynesians had only “negative criticism” to offer. Indeed, after some initial enthusiasm for the impact of the Cambridge (UK) capital critique on mainstream economics, leading American post-Keynesians quickly moved away from those issues. Sidney Weintraub (1973: 249) praised Harcourt (1972) for reporting in a good mood the eruption of a “real analytical rumpus of major importance for economics”, although he doubted it could be absorbed by undergraduate students as Harcourt (1972: viii) said to be the case in Adelaide – they “must be the brightest and best trained in the world”, he remarked. However, capital theory never played a significant role in S. Weintraub’s contributions to post-Keynesian economics started in the 1950s, some of them surveyed by Hamouda & Harcourt ([1988] 1992: 213-14).<sup>28</sup>

Paul Davidson, whose post-Keynesian economics, like S. Weintraub’s, remained apart from the Cambridge debates, eventually concluded that the capital debates were “barren controversies” which had drained “energy and intellectual

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<sup>27</sup> Blaug (1975: 82) quoted that passage, but distorted its meaning completely.

<sup>28</sup> As pointed out by Roy Weintraub (2014: 39, n. 10), “Sidney [Weintraub] never could follow Robinson’s argument about capital theory.”

resources” (Davidson 1994: 138).<sup>29</sup> Hyman Minsky (1985: 28 and 54, n. 4) acknowledged the relevance of the Cambridge controversies, but charged Harcourt (1972) for not making clear their outcome, which, in Minsky’s view, was about capital accumulation in a monetary economy. From Minsky’s (ibid) standpoint, the capital controversies led to the conclusion that “the proposition, that an investing economy with money and capital assets, generates a growth equilibrium rests upon a prior assumption that investment goods and capital-asset prices are always equal.” Harcourt was, of course, aware of the cold reception of his capital surveys by some influential American post-Keynesians. As he told Mongiovi (2001: 515), whenever he talked to Minsky about the capital theory controversies, “we never understood one another” (see also Harcourt [2000] 2001b: 198 on how Minsky’s interpretation of the capital controversies baffled him).

From Harcourt’s perspective, his divergence with Sidney Weintraub, Paul Davidson and Hyman Minsky concerning the meaning and implications of the capital debates came from the fact that they did not organize their frameworks around the (classical-Sraffian) notion of economic surplus, with the distinction between potential surplus on the sphere of production and its realization on the sphere of exchange and distribution. Whether the surplus is realized or not “depends upon the Kaleckian-Keynesian forces of effective demand and on the distribution of income; so you have to take a position on pricing. That’s my approach precisely” (Harcourt in Mongiovi 2001: 515; see also Harcourt 2001c: 264-65).

Due to his new reputation from the capital debates, Harcourt was asked – under John Hicks’s suggestion – to chair the 1975 International Economic Association conference on the *Microeconomic Foundations of Macroeconomics* (held in S’Agaro, Spain) and edit the conference volume (Harcourt 1977a). Harcourt invited (mostly) Keynesian economists of distinct persuasions, including post-Keynesians such as P. Davidson, Tom Asimakopulos, P. Garegnani, E. Nell and M. Nuti, and others involved in the then fashionable research on disequilibrium micro-foundations (see Backhouse and Boianovsky 2013: 127-29). As Harcourt (1992b: 7) recalled, writing the introductory chapter to the conference volume was “one of the hardest

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<sup>29</sup> Mata’s (2004) otherwise useful paper, about Harcourt’s (1969a, 1972) influence on the construction of post-Keynesian identity, misses the point about the ambiguous role eventually ascribed to the capital debates by some leading American post-Keynesians.

tasks I have ever undertaken”. At the same time, he was “over the moon” to be asked to organize the conference, as micro-foundations had been his most sustained interest ever since his undergraduate dissertation (King 1995: 182; Harcourt 2016).<sup>30</sup>

Harcourt and other post-Keynesians at the conference tried, with no success, to reaffirm the Cambridge (UK) capital critique (see Harcourt 1977b: 9; King 1995: 183-84). He stressed, however, in the reported discussions at the conference, that in the capital debates one should distinguish between *two* groups antagonistic to neoclassical economics, which had also been antagonistic to each other (Harcourt 1977a: 175). The Sraffians, led by Garegnani, argued that precise theoretical results could only be reached if based on comparisons of long-period equilibrium positions, whereas the Robinsonians tried to analyse “out-of-equilibrium processes occurring in actual time” (ibid). Hence, there was the “paradox” of some common ground between neoclassical economists like Samuelson and Solow on one side and the Robinsonians on the other concerning process analysis, together with an internal clash between the two sets of Cambridge economists (Tran-Nam and Harcourt 2017: 502-03; Harcourt 1976: 156-57).

When, under invitation of the Federal Reserve Bank of Australia, Harcourt gave a seminar in Sydney in October 1981 about post-Keynesianism, his survey pointed to three different strands – plus Kaldor as a “group in himself” – instead of Eichner and Kregel’s (1975) claim of a post-Keynesian “paradigm” (the tripartite division was further elaborated in Harcourt 1987 and Hamouda & Harcourt 1988). The study of the monetary dimension of capitalist economies, often stressed by Keynes, was associated to American post-Keynesians, especially Davidson and Minsky, whose ideas Harcourt ([1982] 1985: 139-41) discussed in some detail – as suitable for a seminar at the Australian Fed.<sup>31</sup>

Harcourt’s preference for what he called the “third strand” of post-Keynesian theory was already clear in his 1976 *OEP* article. In view of the results of the capital debates, Harcourt (1976: 156) concluded that “we therefore have to start on a

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<sup>30</sup> Harcourt’s (1953) undergraduate dissertation investigated the systemic effects of introducing Rothschild’s (1947) oligopolists in a Keynesian framework. He used a sample of balance sheets and profit and loss accounts of Australian companies during the depression years in order to examine their reserve practices (see Harcourt 2016). That certainly was not a typical undergraduate piece of work.

<sup>31</sup> Harcourt had been invited in 1974 for the position of Governor of the Australian Fed, which he declined by replying that he was a “real man, not a money man” (Harcourt 2001a: 13).

different tack.” One possibility was “the ‘here and now’ of the Keynesian-Kaleckian short period and its developments which have been neglected in the literature on the theory of growth.” That was very close to the goals of his 1965 *ER* paper. Harcourt (1965a: 83) had criticized prevailing growth models for overlooking pricing decision under imperfect competition, sector specific (instead of malleable) capital goods, the dynamic determination of money-wages, and the study of the economic process through a succession of short periods (as opposed to steady states).

Harcourt’s ambitious 1965a model showed how traditional Keynesian short-period analysis could be adapted to deal with price-making and choice of techniques decisions in order to determine the distribution and level of income (see also Harcourt 2001a: 13). However, he did not manage to extend the analysis in historical time through a period-by-period treatment, which was mathematically demanding. Harcourt asked J. Mirrlees, and then J. Stiglitz, who was visiting Cambridge at the time, to collaborate with him on an extension of the 1965 paper, but both declined (see Harcourt 1998: 8; [1996] 2012b: 331). The paper was well received at a Cambridge seminar – granting Harcourt an invitation to take up a position as lecturer at the Faculty of Economics during 1964-66 – but, after that, to his disappointment, “it vanished, virtually without a trace” (Harcourt 1992b: 4).<sup>32</sup>

He felt that, had he been able to extend his 1965a model, it would have come close to tracing a process of cyclical growth, along the lines of R. Goodwin’s mathematical models and Kalecki’s later suggestions that the trend (growth) and the cycle were indissolubly mixed, not determined by separate, independent factors (Harcourt 2006: 121-22). When asked what was the way forward for post-Keynesian economists, Harcourt (2011b: 11) replied that they should develop Goodwin-Kalecki cyclical growth models as *the* proper way for “understanding capitalism and for putting policy around it”, which would also incorporate Kaldor’s later notion – a further elaboration of Myrdal’s ideas – of “cumulative causation” (see also Harcourt and Kriesler 2013b: 2). Interestingly enough, Harcourt (2015b: 1571) regarded P. Le Corbeiller’s mathematical theory of oscillations – a main ingredient of Goodwin’s approach – as “crucial” for understanding cyclical growth, which pointed to a key role of mathematics in business cycle theorizing (see also Harcourt 1995e: 211).

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<sup>32</sup> Harcourt (1965a) has been occasionally acknowledged as a pioneer contribution to post-Keynesian economics (see e.g. Eichner & Kregel 1975: 1307, n. 35).

During his student days at Melbourne University, Harcourt (1998: 8) aimed at becoming a mathematical theoretical economist, but that sometimes turned into a source of frustration. As Harcourt (1995e: 201-02) recalled, his mathematical shortcomings could be partly traced back to the fact that, during his childhood, he had a “bright twin brother who was a good mathematician” and helped him with his homework, which contributed to his lack of confidence in his mathematical abilities. However, as Harcourt (1995e: 202) often remarked he was never a mathematical “Luddite” in economics. The most use he made of mathematics was in his 1968 *EJ* paper on the choice of technique. He regarded the paper, from a methodological standpoint, as his most post-Keynesian piece of work, as it compared the investment-labour ratio that would be predicted by the (neoclassical) axiomatic approach with those predicted by assuming “real world” investment decision rules in a vintage capital model à la Salter (1960) (see Harcourt [1996] 2012b: 329). Harcourt would analyse investment decisions and appraisal again – adding insights from behavioural economics – in his very last paper, joint with his former student Michelle Baddeley (see Baddeley & Harcourt 2021).

Harcourt’s (2006) *Structure of Post-Keynesian Economics* reflected how he lectured the subject at Cambridge over 1982-1998.<sup>33</sup> It showed how he valued economic models both as pedagogical tools and as engines for the design of theories. The 2006 volume displayed most of the formal post-Keynesian models Harcourt had contributed from 1963 to 1976 – especially Harcourt 1963a, b, and Harcourt & Kenyon 1976, but not Harcourt 1965a – together with formulations by J. Robinson, N. Kaldor, S. Marglin (as re-elaborated by Harcourt) and Kalecki – who may be regarded as the central character in the book – among others. Much of that may be also found in several formal post-Keynesian models featured in Harcourt (1972), including his own.

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<sup>33</sup> Chapter 8, on “Applications to Policy”, was as a partial exception, as it resulted mostly from Harcourt’s experience in Australia and the influence of his Australian mentors W. Salter and especially Eric Russell, who was Harcourt’s colleague in Adelaide from 1958 until his death in 1977 – which was one of the reasons Harcourt decided to move to Cambridge (see also essays collected in Harcourt 2001f). Salter’s (1960) book on technical change and non-malleable capital – based on Salter’s 1955 Cambridge PhD thesis – made a great impression on Harcourt (see Harcourt 1962, the first of more than a hundred book reviews he wrote over his life). As put by Hatch and Rogers (1997: 97), the “real world” for Harcourt meant essentially Australia.

Harcourt's admiration for Kalecki increased after he, as co-editor of the *Australian Economic Papers*, decided to have a translation of Kalecki's 1936 Polish review of Keynes's *General Theory* published in that journal (Targetti and Kinda-Hass 1982). Harcourt (2006) pointed out how Kalecki's review corrected some mistakes in Keynes's approach to investment and offered new micro-foundations – leading to an early macroeconomic theory of distribution – partly inspired by Marxian reproduction schemes. In the process, Kalecki showed that market structures were qualitatively unimportant in establishing the main aggregative results, and firmly established his claim of independent discovery of some of the main propositions of the *General Theory* (see Harcourt 2006: 21-25).<sup>34</sup>

Harcourt (2006) discussed the Cambridge capital controversies in an appendix only, as he wanted to focus on the positive contributions instead of the negative criticism by Cambridge post-Keynesians – even if the capital critique provided the “conceptual core” of the post-Keynesian discontent with orthodox theories of value, distribution and growth (Harcourt 2006, appendix II). Moreover, apart from biographical appendix I, and some references in a careful discussion of Pasinetti's contributions, Sraffa and Sraffian economics are absent from the book. As Harcourt (2006: 73) asserted, the neo-Ricardian long-period method is “at odds with those who stress the need for analysis of process in historical time”. There was no history – in the sense of an on-going process – in Sraffa's (1960) economics, since the relationships described related to one year only, with no connection between different periods (Harcourt & Massaro 1964a: 716). That passage in Harcourt & Massaro (1964a), as Harcourt (1981, n. 11) recalled, was inserted “at Sraffa's insistence.”

Harcourt (2006: 43-50) discussed in some detail his 1976 paper with his former student Pater Kenyon – probably Harcourt's most influential contribution to post-Keynesian economic modelling. Like Wood (1975), it belonged to the set of post-Keynesian models of the determination of the size of the mark up. However, whereas Wood's model was set in logical time in a Golden Age, Harcourt & Kenyon (1976) set the same problem in historical time, relating pricing, investment and the choice of technique decisions to successive short periods. When post-Keynesian economics was criticized for lacking solid microeconomic basis at a session at the American Economic Association meetings – the only time the AEA dedicated a

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<sup>34</sup> Harcourt (2006: 163) regarded Kalecki as the “greatest all-round economist of the 20<sup>th</sup> century” for his contributions in many areas, including development economics.

session to post-Keynesian themes – Harcourt (1980) claimed that his 1976 pricing model filled the gap.

Harcourt's (1963a) critique of Kaldor – which Robinson regarded as the first to “really nail” the weak spot of early Kaldorian models – contributed to Kaldor's perception that Harcourt belonged in Robinson's camp and to the ensuing cool relations between the two men for about 10 years (Harcourt & King 1995: 48; Harcourt 2001e: 66). Harcourt would never give up his detailed piece of criticism, which marked the beginning of his career as a post-Keynesian model-builder<sup>35</sup> – not least because of an unpleasant episode involving his presentation of the 1963 paper at Kaldor's research seminar in 1964, when (according to Harcourt's 2002 recollection) Kaldor misbehaved and tried to shout him down and change the subject whenever Harcourt got near a telling point. Harcourt (1963a) charged Kaldor (1955-56; 1957) for ignoring the peculiar pricing behaviour that Kaldor's assumption of full employment implied for the different sectors in order for Kaldor's distributive mechanism to work.

Harcourt (2006) was an attempt to systematize what he perceived as the fundamental intellectual heritage of the “Robinson circle” and the Cambridge post-Keynesian tradition. He then refrained from addressing how that tradition had all but disappeared from the Faculty of Economics. That was a main subject of Pasinetti (2007), published the year after and reviewed by Harcourt (2009). Harcourt discussed the matter for the first time as he replied to a question by Gary Mongiovi (2001: 617-18) on the erosion of the Cambridge tradition. Harcourt ascribed that mainly to Cambridge politics: “Joan [Robinson] and Richard [Kahn] were hopeless politicians, and Nicky [Kaldor] was even worse. They just made very bad decisions, then they lost their power and there was a huge reaction to the way they had used their power when they had it.” They alienated J. Meade and R. Matthews, who joined F. Hahn as they came to dominate the Faculty Board. Austin Robinson tried with little success to “keep the peace”, whereas Sraffa remained “aloof” from the fights.

Harcourt (2009: 207) partly agreed with Pasinetti about the inability of Keynes's pupils to establish successors in the Faculty, but stressed that the failure was

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<sup>35</sup> Harcourt's taste for economic modelling is visible as well in his collective introductory volume (Harcourt, Karmel and Wallace 1967) – which he finished writing in Cambridge – and in his 1969b paper on a modified version of the IS-LM model, written in Japan.



mainly about securing appointments at chair level, as they did place post-Keynesian in posts as lecturers, as was the case with Harcourt and Pasinetti, among others (see also King 2017: 148-52 on the decline of the Cambridge tradition since the mid 1970s). Teaching post-Keynesian economics at Cambridge in the 1980s-90s, when the topic was increasingly marginalized, was not an easy task. Harcourt (2001d: 104) recalled his public debates with Frank Hahn, usually at the Marshall Society, when Harcourt sometimes “lost [his] cool”. When he retired in September 1998 nearly all his courses were discontinued, in contrast with the interest that his seminars and invited lectures still commanded at many other universities.

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In July 1996, a couple of years before retiring, Geoff Harcourt ([1998] 2001b) gave a lecture about the Cambridge contributions to economics in the Lady Mitchell Hall at Cambridge. He ended the lecture with a compelling positive reflection about his trajectory as a Cambridge economist from down under:

I am a fortunate person because I have been a student of this tradition. I have taught it, most of the people about whom I have written ... were my teachers and then my colleagues and my friends. So, it has been my good fortune also to work in this tradition. I have brought to this peculiarly Australian contributions as well, because of the mentors I had in Australia, most of whom came out of the Cambridge tradition. Therefore, you see, you have before you a fulfilled person who has tried to do something to preserve the Cambridge tradition and who is going to play cricket for the Jesus College Long Vacation High Table side this afternoon. On a glorious summer day, what more could anybody ask for? (Harcourt [1998] 2001b: 351)

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