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The Failure of Historians to Engage with Recent Economic Thought: The Case of Millmow

Gregory C. G. Moore[1]

Abstract: The narrative that follows is a review essay of Alex Millmow's *A History of Australasian Economic Thought* (2017). The chief finding is that Millmow fails to devote sufficient attention to the history of economic theory in the last decades of the twentieth century because of his preoccupation with the history of economic policy. This is demonstrated, in part, by considering the extent Millmow appraises the work of those prominent modern economists who are identified in an informal survey of ten respondents.

1. Introduction.

Historians of economic thought often defend the value of their sub-discipline by pointing to the way the past giants of economic theory—from Jacob Viner to Milton Friedman to Paul Samuelson—took a keen interest in the history of their subject matter. Samuelson (1970), for one, famously stated at the banquet to celebrate his Nobel Prize that one of five necessary conditions for success as an economist is that a practitioner "must read the works of the great masters". Historians of economic thought should, however, pause to consider just how much of the historical research undertaken by these greats entailed historicizing the recent secondary literature that they needed to master before they engaged with the prevailing knowledge on the frontier. After all, Leon Walras had been dead for only two years when Joseph Schumpeter celebrated the Walrasian frame in his first history of economics in 1912; and Alfred Marshall had been dead for just 25 years when Friedman drew upon his notes from Viner's theory course to render Marshallian demand theory more rigorous in 1949 (Schumpeter ([1912] 1954, Friedman 1949). Even though this research often entailed pressing an amateur historiography into service to provide historical narratives of the recent literature, the intellectual product was invariably more impressive and fruitful than the a-historical 'literature surveys' seen in most modern doctoral dissertations and the unread journal articles made to type. The product was also usually less controversial than their accounts of doctrines from more distant times, which were sometimes driven by a whig agenda that induced rational reconstructions. Historians of economic thought should, conversely, pause to consider just how little of their own research is devoted to recent economic thought and how they sometimes botch such research when they do engage with it. With the obvious exceptions to the rule and the usual invocation of *ceteris paribus* assumptions, it is as if there is a mirror inverse, whereby the greats who have engaged with the history of their discipline become poorer historians as the doctrines they consider become more distant, and the specialist historians of thought become poorer historians as the doctrines they consider become more recent.[2]

The central argument advanced in this essay is that Alex Millmow's *A History of Australasian Economic Thought*, though worthy in many ways, is less than perfect in the way just predicted; namely, as a specialist historian he has failed to consider adequately Australasian economic theory in the last decades of the twentieth century and he has slightly botched such research when he has engaged with it. I state bluntly that Millmow's book is of great worth and all good libraries should hold a copy, but this failing must be emphasized for all this. In any event, there are many possible *and understandable* reasons why specialist historians as a species handle recent history of economic thought (HET) less than perfectly or, more usually, not at all. They are less well read in the high theory of the last twenty-five years since they have exploited their comparative advantage to publish lettered-journal articles on the history of a now archaic conceptual framework from the distant past. They are, and let us be frank on this point, often historians of economics by default because they are beholden to a conceptual framework, not necessarily irrelevant or erroneous, that was prominent in the distant past and which is now fundamentally at odds with modern orthodoxy. They are, as good historians should be, aware of a larger narrative arc in which the present is just a coda of a *grand pas de deux*, and hence they are often occupied with the steps in the distant past, with some even claiming that anything published in their lifetime is merely the current dance before them rather than history itself. They work within the balkanized, intolerant, "publish or perish' world of the modern economics department where members of the different sub-disciplines no longer cross-fertilize in a common room. This list of causal processes, which could be extended, is certainly relevant to any account of why Milmow slightly undercooks recent Australasian economic thought. As it happens, however, they predominantly operate in a channeled fashion through Millmow's singular fascination with economic policy formation. It is a case of policy trumping theory.

The review essay is in three main sections. In section two I review the scope, method and design of Millmow's history to provide context for my central criticism that his account of recent Australasian economic theory, which for convenience is defined as the issue from 1970 to 2000, is less than perfect largely because he is preoccupied with the history of policy formation. Problems with the narrative relating to the pre-1970 contributions by Australasian economists are considered lightly, if at all, on the grounds that these will most likely be tackled by other reviewers in the symposium devoted to Millmow's book in this journal issue. In section three I present the findings of an exploratory survey of ten senior Australasian economists, many approaching their dotage years, to determine what they perceive to be the most important contributions in their discipline from 1970 to 2000. The contributions so listed, which are of minor value for the historical record, are compared with the account of this work in the book under review. In section four, I conclude by re-emphasizing the outstanding features of Millmow's history and proposing that more historians should bring their historiographical skills to bear on recent economic theory.

2. Millmow's History and Contributions 1925-70.

Millmow sets himself the task of writing a history of Australian and New Zealand economic thought from 1925 to 2000. These bookends were chosen because 1925 is commonly accepted as the beginning of the professionalization of economics in the Antipodes due to the founding of the *Economic Record* and the Economic Society of Australia and New Zealand in that year, while 2000 is, well, the 75th anniversary of this journal and society, and hence it is a convenient terminus date. The extension of the project into the final decades of the twentieth century, however, amounts to the authorial equivalent of imperial over-reach, since, as outlined in my introduction and as further elaborated in the next section, Millmow does not appear to have the stamina, the interest or (perhaps?) an adequate word limit to fully explore the intellectual output of these decades. Indeed, the most startling characteristic of the book is that only 75 of its 240 pages are devoted to the economic theory published in the 30 years from 1970 to 2000, even though more than treble the number of individuals were undertaking economic research in these years compared to the 45 years from 1925 to 1970.[3] The decision to consider the intellectual issue in economics from both sides of the Tasman places further strains on Millmow's narrative. The cultural similarities between New Zealand and Australia, together with the mobility between the two sets of economic communities, render sensible this narrative strategy. It nonetheless more than doubles the composition (and research) workload given the complexity of weaving the two stories together; and does so in a way that would test the literary skills of even a Joseph Schumpeter or a Donald Winch. I put the extent of Millmow's success in this direction to one side, however, partly because Tony Endres, a leading New Zealand historian, is reviewing this history in this journal issue and partly because my knowledge of New Zealand is largely confined to the vexing fact that we rarely beat that country's rugby team. What follows, in short, is largely confined to Australian thought.

Millmow's justification of his enterprise is also troubling, but in minor ways. To make space for his narrative in the market place for ideas (and presumably to make it acceptable to the publishers and benefactors) he is obliged to justify it in the face of the existing general histories. These include John La Nauze's *Political Economy of Australia* (1949), Craufurd Goodwin's *Economic Enquiry in Australia* (1966) and Peter Groenewegen and Bruce McFarlane's *A History of Australian Economic Thought* (1990). La Nauze's history is a slim volume of essays devoted to the key figures of the nineteenth century, but, as is apparent to anyone who visits the La Nauze archives, it is rich with many research hours backing each sentence. Goodwin's majestic book, which is far grander at 650 pages and with copious notes, extends the historical narrative into the 1920s. Groenewegen and McFarlane's book has the broadest canvas, tracking from the First Fleet to the 1980s, and, further, has the authority that accompanies the imprimatur of the first of these authors. It is only the last narrative that overlaps Millmow's time-span, and hence it is only this book that needs to be distinguished from his own. Indeed, I suspect Millmow extended his analysis to 2000 and across the Tasman to demarcate it from Groenewegen and McFarlane's history, especially since both are published in the Routledge series devoted to country-specific histories of economic thought. The only difference in the titles is, after all, the attributive adjective "Australasian" rather than "Australian". In any event, Millmow could have been more charitable in his quest for space. He grants that Groenewegen and McFarlane did an "adequate" job for the early years and that they could not be "comprehensive" due to word limitations, before adding that they were culpable for "striking omissions" and criticized at the time of publication for their biographical approach (5-6). This is unnecessary, since the pressing need to integrate recent research into the received narrative is sufficient justification for Millmow's enterprise. The research undertaken on Australian HET, much of it authored or co-authored by Millmow himself, has certainly been impressive in the years since the publication of Groenewegen and McFarlane in 1990. One thinks of the oral histories by John Lodewijks and others; the prize-winning *Giblin's Platoon* of 2006 by William Coleman *et.al.*; the conferences run by the local history of economic thought society, HETSA, and the numerous publications in that society's journal, the *History of Economics Review*; and John King's *Biographical Dictionary* of 2007. Millmow needed only to point out that Groenewegen and McFarlane did a decent job on a near *tabula rasa* (with apologies to Neville Cain and others), but it was now the opportune time to add depth.

If anything, Millmow has failed to properly salute recent research output in an organized review of the troops. Groenewegen and McFarlane carefully detailed the existing literature in 1990 in a tight introductory chapter, and, reflecting the then paucity of existing research, quaintly acknowledged their debt to the *Australian Dictionary of Biography*, most entries of which are, by the by, outstanding. Millmow, by contrast, disperses his acknowledgments. He refers to some of the general histories on p.3, then again on p.5, before returning to the issue on p.12, where he also considers a handful of encyclopedic surveys, the most important of which is perhaps Coleman's (2015) idiosyncratic, contestable and brutally honest account entitled "A Young Tree Dead". No wider review is undertaken, even if due recognition of the literature is always given in the narrative itself when relevant. The true grandeur of recent research is only caught in the meticulous bibliographies that Millmow presents at the end of each chapter, which, for me, are worth the price of the book alone.[4] Frankly, I did not realize how rich was the HETSA research output relating to Australasian thought, nor how much outstanding research Millmow himself has already undertaken. This issue is even more impressive when it is recognized that it was conceived over a period when the sub-discipline of HET was increasingly starved of resources, if not publicly and ritualistically belittled. History units were eliminated from the curriculum; HET practitioners were put out to pasture early; and doctoral students were dissuaded (at least by me) and, those brave enough to proceed, are now largely unemployable. This process of decline culminated in what is known as the 'savage attack' of 2013, which systematically reduced the soviet-style rankings of the main HET journals such that no career advancement is now possible in this sub-discipline 'down under'. This is a statement of fact and not too much should be made of it—it is not, after all, the death of cricket, and what now passes for civilization will proceed without us. A statement of account is nonetheless now in order. I suggest that Millmow and an elderly HETSA member—a Lodewijks, a King or an Endres—write a survey of the research on Australasian economics for the recently resurrected survey section of the *Economic Record*, which, for the benefit of readers from abroad, is the voice of orthodox Australian economics. It would act as a wonderful eulogy at the funeral pyre for the society that is HETSA.[5]

A slightly more important problem relates to the historiography that Millmow deploys, or at least what may, with difficulty, be discerned as the historiography he deploys. He makes a stab at explaining his approach in the introductory chapters, which are written with some verve, but, as suggested by my comments on his literature review, lack structure. In a strange and lurching way, Millmow describes what the book is about immediately before a section entitled "What this book is about" (3). He states that he wishes to "examine some of the contributions to knowledge by Australian and New Zealand economists in the context of their own economy"

(3). He adds that he seeks to consider how politicians and journalists influenced this thought directly and as disseminators (3). In the actual section devoted to what the book is about, he states that he wishes to look at the multiple causality between events, policy and theory, and, as a sub-theme, provide an account of the development of the economics profession in both countries (3-4). Millmow does not refer to any seminal historiography publication to further clarify this line of advance. This is not unusual in HET, since most of its practitioners are trained economists who treat historiography as an afterthought or, more usually, simply ignore it. Still, from Millmow's less than precise introduction, and from reading the book itself, it is apparent that placing the "contributions" of Australasian economists "in the context of their economy" entails tracking how emerging economic problems prompted members of an evolving economics profession to derive innovative theories to support policy solutions that suited local conditions, and how these solutions were then either checked or promoted by a policy elite. This is all very good. It is an author's prerogative to choose a driving theme and, as someone who wears his contextualist historiography on his sleeve, I readily accept that policy demands are part of the context for the development of ideas. The result of this choice, however, is that Millmow's narrative is in no way a general history of Australasian economic thought. First, there is more context shaping theory than those historical particulars relating to policy demands, with even the old 'internalist' sequence of theories begetting theories, paradoxically, part of the context. The sub-theme of tracing the evolution of the Australasian economics professions could have prompted the scrutiny of further context via some sort of sociology-of-knowledge framework, but no such framework is deployed. Second, the policy focus has reduced the space available for exploring the theoretical aspects of the contributions and, more disturbingly, those contributions less relevant to policy are given short shrift or simply ignored. This is even more dramatic for the post-1970 period. The saving grace is that Millmow's restricted mission is executed with élan and the policy-driven product is of value.

Millmow's preoccupation with what I hereafter refer to as a policy-theory nexus has also been emphasized in a recording of a symposium devoted to this book for the 2017 Australian Economics Conference. This symposium, which I viewed very late in the piece, includes speeches by Millmow, Lodewijks and Coleman (Oslington *et.al.* 2017). Lodewijks there levels some further reasons for the lop-sidedness in Millmow's narrative that I did not initially detect, and which are true enough, such as a bias for those contributions emanating from Melbourne and a preoccupation with macroeconomic policy. Coleman also raises a related issue with which I was occupied, but with greater power than my initial effort, and hence I adjust my narrative in response. He proposes that Millmow's book is incidentally devoted to unravelling what is distinct about Australasian economic thought and, by implication, that Millmow could have done more to highlight this. This proposition indirectly relates to the policy-theory nexus because theories that are distinctly Antipodean are often those designed to support policies to resolve problems arising in local conditions. One key strand in this fashion is, in Coleman's (and my) view, the dependent economy model of a domain with a large non-tradeable sector that is reliant on non-manufactured exports and dependent upon capital inflows. The trajectory of the versions of the dependent economy model—the rough outlines of which most Australasian historians know well enough—has recently been fleshed out with skill by Metaxas and Weber (2016). The usual suspects rounded up for their contributions on this score include, amongst others, Roland Wilson, Trevor Swan, Wilf Salter, Max Corden and Bob Gregory. To a large extent, the many variations of this model involve the realization that, for a small open economy, export and import prices are sufficiently set in world markets that the terms-of-trade are independent of domestic conditions and hence a single price for tradeable goods can be identified. This, in turn, means that a "second" terms-of-trade can be identified between an exchange-adjusted tradeable good price and a non-tradeable good price. Such models are used to explain how large shocks (say via net exports or capital inflows) to the external and internal balances induce not only changes in Keynesian-style aggregate variables, but also changes in the exchange adjusted relative price—through either the visible hand of the state or the invisible hand of the market—that lead to Marshallian-style switching between the tradeable and non-tradeable sectors. And, of course, Australian history is replete with these shocks, from the cessation of capital inflows in the 1930s to the mineral booms of more recent times.

I wholeheartedly agree with Coleman that the dependent economy model amounts to a special case of a general theory that warrants the accolade of being "distinctly Australasian" and encourage the reader to listen to his speech on You-Tube (Oslington *et.al.* 2017). After all, the phrase 'special case' is the hedge to prevent any suggestion that there is an Australasian economic 'science' which is distinct from a North Atlantic economic 'science'. I can also imagine other candidates for a pantheon devoted to "distinctly Australasian" contributions, especially any model arising from the singular institutions of this region, such as the fixing of many prices by tribunal fiat following Federation in 1901. Millmow himself makes this point (27-8). *I do, however, disagree with Coleman's separate claim* that a narrative arc driven by an analysis of "distinctly Australasian" economics is required because a general history of "economics in Australasia" would amount to a mere almanac or compendium. Strong themes, whether they be a policy-theory nexus or a "distinctly Australasian" economics, are important within a general history because they enable the overwhelming number of historical particulars to be mustered. Those contributions that are not part of this arc should nonetheless be considered along the way, if only to explain why they are not part of the said arc, and the explication of theory should certainly not be sacrificed on its altar. This is the reader's expectation when picking up a book with a title such as *A History of Australasian Economic Thought*. Too many readers will be asking where is professor X and what of theory Y? Millmow himself states that his secondary theme of tracing the development of the economics profession entails surveying its key theorists, innovators, expositors and builders (4). I am also troubled by Coleman's gracious suggestion that this alternative narrative arc of what is "distinctly Australasian" sits, in a suppressed way, alongside Millmow's main policy-theory nexus arc. This is plausible enough due to the reasons provided earlier, namely, a focus on policy leads to a greater emphasis on "distinctly Australasian" theoretical innovations. Such a suggestion can, however, be accepted with qualifications only. First, Millmow briefly touches on the issue of whether an Australasian economics exists (6-7) and provides a worthy account of the dependent economy model (56, 134-6), but does not dwell on the theories that may be regarded as "distinctly Australasian" to a degree that warrants the conclusion that this is a main theme in his history.[6] Second, Millmow considers an array of policies and theories that cannot be regarded as "distinctly Australasian", but which sit neatly within his main arc of tracing the policy-theory nexus in this region.

Still another bothersome characteristic of Millmow's narrative is the disproportionate space devoted to economists with whom he is particularly enamored. The abnormal amount of attention given to Colin Clark, for example, is a perfectly understandable authorial choice given Clark's outstanding contributions to economics, but it may raise an eyebrow once it is known that Millmow is writing Clark's biography (which we all look forward to reading) and that other economists of note are given short shrift. It is similarly odd that Roland Wilson, whose *Capital Imports and Terms of Trade* of 1931 is of great importance for the development of the aforementioned "second" terms of trade, receives no more than a few paragraphs (56), while four pages are devoted to the admittedly important ("distinctly Australasian") Brigden Report (52-5). Ed Shann is referred to in a random way without any serious analysis (5, 40-1, 80-1), even though his *An Economic History of Australia* of 1930 is regarded as the first economic history of worth in this country. Indeed, economic history and most of the sub-disciplines, other than agricultural economics, that are not part of the orthodox core are largely overlooked in this text. This is presumably because of their smaller impact on policy formation. The Butlin brothers (S. J. and N.G.), for example, are cited a few times for their commentaries on the economics profession, but there is not a single reference to any of their monumental economic histories (which weighed in at 400 pages at a time) and the distinctly Australian economic history that they promoted. Similarly, Geoffrey Blainey—who did have an important impact on policy, if only via his aphorisms—rates only one mention, which is an unexplained reference to *The Tyranny of Distance* of 1966 in Millmow's review of the HET literature (12). There is also clearly a disposition to dwell on any connection between Australasian economists and Cambridge dons. This is understandable given the Cambridge influence in the Antipodes via the parade of Australasian students who sought education at that place and, further, the strength of the Marshallian, Pigovian and Keynesian research programs in the twentieth century in most countries. At the same time, interesting anecdotes relating to this Cambridge connection are, in my view, bordering on that guilty pleasure which I call Cambridge porn. This is dramatically seen in the decision to allocate three pages to the Cambridge experiences of those who gained employment at Canterbury University College (32-4), while allocating a single short paragraph to UWA, in a general review of the Australasian universities.

I also have one or two niggles about outright omissions from Millmow's history in the pre-1970 world. Some leniency should be shown with respect to unexplainable omissions due to the subjective nature of determining what constitutes a major contribution in economics and the binding nature of word limits. Some omissions are, however, sufficiently perplexing to deserve passing comment. For want of space (and reflecting my own subjective preference), I draw just two illustrations from Shann's world and hence the only archival research I have undertaken on twentieth century Australian economics. Shann's close friend A. C. Davidson is nowhere mentioned, yet it was late night conversations between these two at UWA, prior to the elevation of the latter to the head of the Bank of New South Wales (BNSW), that gave rise to *Central Reserve Banking* in 1929. This slim volume is one of the earliest Australasian proposals for an independent central bank that should possess Bagehotean lender of last resort responsibilities, but not the ability to allocate scarce means to alternative ends via the credit creation process. Nor is there any reference to the way Davidson established the largest economics research department in Australasia in the 1930s at the BNSW, with a complement equal to any university department of that time. It was in the early days of this venture that Davidson and Shann engineered one of the most important policy responses to Australia's second Great Depression by controversially depreciating the Australian pound in 1931. The department, which subsequently became a minor kingdom overseen by Davidson, both undertook research to prevent capital flight at a time when new political movements threatened property rights and funded economic research in the broadest sense, including the work of many economists who were earlier involved in the Premiers' Plan. It was justified in Davidson's mind because the BNSW and its customer base were so large that the long-term interests of the BNSW shareholders and Australia were one and the same thing.[7] These contributions, moreover, merit comment since, at least outside HETSA circles, they are perhaps the least known in Australian economic thought and because they fit neatly within the policy-theory nexus that sits at the heart of Millmow's narrative. It is also an odd omission given that Millmow has written effectively on other aspects of Shann's life and the debates of the 1930s generally. I accept, for all this, that both he and the readers of this review might reasonably shrug off this quibble with the argument that it is a subjective call. The post-1970 omissions, which are a different matter, are considered in the next section.

The publishers should also be a little concerned with the number of grammatical errors and inelegancies that were not picked up in the final proofs. These perhaps strike the eye only because the narrative is, for the most part, well written. Indeed, I suspect many external parties have dutifully undertaken editorial sweeps. The number of semi-colons in some sections is certainly a tell-tale sign that an effort has been made to mend mangled sentences. Some chapters have, furthermore, clearly been worked over to a far greater extent, with the result that they read far more easily than others. I nonetheless noticed an astounding number of stylistic slips while lazily reading the book without ill intent. Consider the following as an absent-minded sample. The definite article is missing in the following: "marked a singularly Australian contribution to trade theory; later known as [sic] dependent economy model" (56). It is inelegant to use two colons in the same sentence (61). There are two aheads in the following: "Jumping ahead a few years ahead" (132). There is an unwanted comma in: "Whitehead, had earlier persuaded…" (133). The following requires addressed or addresses: "Philpott address the related problems" (145). The following reads oddly: "recast the shaping of the Australian economics profession" (149). Shortly after we have: "those with a professional training basis in price theory" (149). It is not standard practice to use a comma to separate a title from a name when the former is used as an adjective, as in the following mouthful: "He referred to Deputy Prime Minister, Jim Cairns's suggestion" (175). The noun "commitment" is usually followed by a preposition: "a commitment promoting collective bargaining" (177). The mangled nature of the following is self-evident: "It was so successful that…Willis adamantly declared that it was their creation, not economists" (189). The following passage requires "who identified" or, if the publication is to be anthropomorphized into an active agent, "which identified": "His thesis was fortified by Snape (1977), identifying what would become known as..." (195). The logical syntax of the following should be "funded partly by A and partly by B": "The IMP model was part-funded by the university and partly by business corporations along with various state and welfare bodies" (207).

This really is a case of an irresponsible child throwing stones in his own glasshouse given my own inability to write error-free copy and, as the reader can readily see, many of the stylistic errors are very minor. I nonetheless felt obliged to comment. I add that although most chapters read extraordinarily well—at least by the standards of the dour world of economics—some pages are bereft of a narrative rhythm. Perhaps in a quest to finish the text in haste or due to the intervention of an external proof reader or because of the transgression of a word limit, quite a few passages entail a series of short sentences that are not placed in an elegant arc (such as the top of 170 and bottom of 144). I also suspect that the uneven nature of the text will be magnified for readers who are not Australian economists, since quite a few of the protagonists in Millmow's tale are not placed in context. An educated Australian, for instance, would know that Paul Kelly and Geoffrey Blainey are two of our most important public intellectuals, but most international readers will not know them from Adam. Australians who are not economists of a certain age will also find it difficult to weigh the importance of some anecdotes, claims or contributions, since sometimes the subject of study is introduced with a short biographical sketch and sometimes he or she is not. Similarly, sometimes the protagonist's first name is given and sometimes it is not, and sometimes the theoretical contribution is explained in detail and sometimes it is not. Millmow's history is, in short, written for local economists who know the stage, are familiar with the actors and are conversant with the plays of this vintage. This, of course, may not be a problem if such economists are the target audience. Indeed, given its readability, I strongly suspect the elderly in our profession will readily turn to the narrative itself after sheepishly looking up their own names in the index.[8] The referencing could have also been better, with the citations for some quotations short of the required page number and some claims being moored at a sufficient distance from the citations that one cannot determine their anchorage. In Millmow's defence, managing the number of *personae dramatis* in this work would have tested the skills of a literary historian of the highest order. A form of revealed preference is also exhibited by the fact that I read it to the end almost in one sitting.

3. Leading Australian Contributions 1970-2000.

The exercise of identifying Australasian economists who made important contributions in the last decades of the twentieth century is even more subjective and fraught than in the earlier period, since most candidates are in some way or another still alive. Insufficient time might have passed to judge the worth of their contributions, offence may be taken or favor curried, and judgements may be driven by random associations between historian and subject. I certainly do not want to present myself as a cultural commissar who, standing above the community, solemnly announces the ten or twenty best economists in recent times. Thus, to remove myself from the exercise somewhat, I have surveyed ten Australasian economists, all of whom gained professorial rank, to identify those Australasian economists who they believe made the greatest contributions to economics from 1970 to 2000. I encourage readers to treat this exercise with skepticism, since I have found in the modern age that much faux objectivity is conveyed when traditional narratives are transformed into lists, matrices and numbered bullet points. (And I do provide a list below!). Such an exercise also drifts dangerously close to the type of phenomenology studies that now plague the business disciplines, whereby transcribing what people say strangely renders it factual. The survey was, furthermore, executed in an informal manner via casual correspondence and in a way that allowed respondents to report in their own opinionated fashion. Thus, to repeat in order not to mislead, all I have done here is to replace my own subjective assessment with the subjective assessments of ten other, admittedly more senior, economists. Millmow's book is then judged on the extent to which some of the economists so identified are considered in his narrative.

The individuals chosen as respondents in the survey have certain characteristics. First, many were chosen because of their own contributions to economics, as reflected in fellowship of the Academy of the Social Sciences in Australia, the ESA Distinguished Fellow Award, the ESA Honorary Fellow Award, the ESA Distinguished Public Fellow Award, the Order of Australia, and so on. Indeed, one or two of the respondents have themselves been identified in this survey as making a substantial contribution between 1970 and 2000. Several other respondents were added to the mix because they are integrated sufficiently into the economics community that they could weigh the contributions of their near contemporaries. Second, to ensure that all sub-disciplines, schools of thought and regions are represented, I have sought individuals from a range of backgrounds, including two senior heterodox economists, one New Zealander and, in a vain attempt to shift the view beyond the local fishbowl, one economist who has worked predominantly outside Australasia. A major failing of the exercise is that no senior econometrician (as opposed to applied economist) was approached due to the simple fact that I do not know any and was unwilling to cold call busy people whom I do not know. Some econometricians have nonetheless been identified by the other respondents. Third, except for one economist aged in his fifties, the respondents are in semi-retirement or approaching semi-retirement, and hence have less skin in the game. Fourth, to reduce the role of individual authority and to encourage a free response, the respondents remain anonymous.

The ten respondents were asked to nominate ten economists who made the most important contributions to economics in any domain—whether it be theory, policy or text-book writing—while predominantly residing in the Antipodes. In keeping with my *Centre Pompidou* approach of clearly displaying the (sometimes flawed) inner workings of this exercise, I stress that those who were asked to participate responded in a range of ways. Some nominated contributions in an informal narrative, while others presented their findings in a neat tabulated form. Some described the contributions in a general way, while some accompanied their description with specific publications (but invariably with the rider that this was not easy because each contribution was "more compelling than a single article or book"). Some of the elderly, no doubt busy in their post-teaching life of publishing without perishing, provided fewer than ten nominations. Indeed, two provided no more than a handful of nominations, with one of these explaining he was in ill-health.[9] Most, however, presented a full list of ten, and one or two could not resist the temptation to add one more. Finally, reflecting the importance of drawing upon economists from a range of backgrounds, some respondents dwelt on those contributions in their area of interest, such that trade theorists predominantly nominated trade theorists and heterodox economists predominantly nominated heterodox economists (and this is returned to below). More complex survey approaches could have mitigated these problems. I particularly contemplated deploying the multi-round Delphi method, whereby in the first round somebody jarringly called a change agent (i.e. me) collects the anonymous responses from an expert panel, and in the second round, panel members revise their responses, without the noxious influence of authority, in light of the first-round results. Such a technique may have both prompted the memory of those who provided only eight or nine nominations and encouraged respondents to consider contributions outside their area of interest. In the end, however, I suspected that the respondents would not have the patience for this, especially as I specifically stated that this was for a review rather than a full-length history. In any event, to further reduce the subjective nature of the exercise, two nominations were required to make the list, which is shown below.

Economist	Contribution	Votes
Brennan, Geoffrey	Public Choice Theory.	3
Chapman, Bruce	Public Policy, particularly income-contingent loans (HECS).	4
Corden, Max	International Economics, particularly the general equilibrium analysis of "Dutch Disease".	6
Dixon, Peter	CGE modelling.	3
Groenewegen, Peter	History of Economic Thought.	3
Gregory, Bob	"Gregory Thesis", Labour Economics, Public policy.	7
Garnaut, Ross (& Anthony Clunies-Ross)	Taxation of Resource Rents, Public Policy.	3
Gruen, Fred	Public Policy, Welfare Economics.	2
Harcourt, Geoff	History of Economic Theory, Post Keynesian Economics, and Cambridge Controversy.	3
Hughes, Helen	Development Economics, Indigenous Education.	2
Kemp, Murray	International Trade.	2
Kriesler, Peter	Post-Keynesian Economics, particularly Kaleckian Economics.	2
Lloyd, Peter	International Trade, particularly intra-industry trade and the analysis of protection.	2
Nevile, John	Macroeconomics, Unemployment, Fiscal policy, Income Distribution.	2
Pagan, Adrian	Time-series Econometrics.	4
Pitchford, John	Pitchford Thesis.	2
Quiggin, John	Micro-theorist, Critic of Reform Movement.	3
Snape, Richard	International Trade and Teaching.	2

One attribute of this list immediately stands out. The votes were dispersed over so many candidates that, except for Corden and Gregory (with six and seven nominations respectively), most of those listed received no more than three nominations. The subjective nature of the process is further reflected in the great variance in the accompanying justifications for these nominations. There was also an extraordinary number of economists who were proposed once only. Space limitations prevent me from commenting on these singletons, so I restrict myself to listing a handful who were not mentioned in Millmow's book. These include Paul Miller for his work in applied economics; Bill Griffiths for his work in Bayesian econometrics; Noel Butlin for his work on the economic history of pre-colonial Australia, with specific reference to *Economics and the Dreamtime: A Hypothetical History* of 1994; Boris Schedvin for his work on the economic history of the 1930s depression, with specific reference to *Australia* *and the Great Depression* of 1970; and Peter Earl for his work on "psychological economics", with specific reference to his 1990 survey article in the *Economic Journal*. There were many more highly praised singletons who did earn guernseys in Millmow's history but were given no more than perfunctory run on parts. I now consider the way Millmow treated a sample of the economists who were nominated twice or more to demonstrate how his interests, particularly his preoccupation with policy, have caused him to undervalue or ignore some notable contributions. It would have been interesting to consider every economist so listed, but the handful chosen is sufficient to make my point, especially as space is short.

(i) Geoffrey Brennan

Brennan was nominated thrice for his contributions to public choice theory (PCT). Reference was made to his co-authored books with the Nobel Laureate James Buchanan (1980, 1985); his theory of expressive voting; and his 1976 *Econometrica* article "integrating the spending and taxing sides of the fiscal problem". I make no attempt to provide an account of Brennan's *oeuvre* simply because of the sheer number and intellectual weight of his publications. Their range is such that I cannot even find something representative to dwell upon. At most, broad divisions may be delineated. First, he contributed to that part of the Virginia public choice tradition now called constitutional economics, which amounts to modelling the way agents agree to rules that constrain their future collective choices. The publications along these lines that were co-authored with Buchanan were referred to in the latter's Nobel citation. Second, and again related to PCT, he played an important role in the development of the theory of expressive voting in several books with the senior presses (some co-authored) and numerous articles. Third, he contributed to virtue economics, the economics of esteem and the economics of religion. Brennan's ESA fellowship citation, which also provides a decent synopsis of his work, concludes that he was "a key figure in the emergence of public choice theory in the later part of the twentieth century, although critical of the reductionist approach of much of it" (Anon. 2013). It is, indeed, worth emphasizing that Brennan contributed to the tail end of the development of the Virginia school, one of the most important intellectual revolutions of the last century, by embedding himself in Virginia Tech (VPI) in the late 1970s (Brennan 2004). He was described by Buchanan himself as that "golden-voiced 'wild colonial boy' from Down Under" (Brennan and Buchanan [1980] 2000, xix). The scandalous way this school was pushed out of the University of Virginia following one of the most clearly identified acts of intellectual discrimination in the history of economics, and subsequently moved to VPI and then to George Mason University, has bred attractive anecdotes that now compete with Cambridge Porn. It does not take much prompting for Brennan, an opera singer of some worth, to sing the troubadour tale in which this long march is traced.

There is not a single reference to Brennan in Millmow's book. This cannot be explained by Millmow's requirement that an economist's career predominantly transpired in Australia because Brennan's stint at VPI was not long (1978, 1979-83). If anything, Brennan was firmly embedded in the Australian scene via an ANU chair and a period as editor of the *Economic Record*.[10] An alternative, more speculative, explanation for this omission is that Millmow has relied on J. J. Pincus' (2014) argument that PCT had little impact on Australian public policy, which Millmow duly cites (221-2). Pincus may be right, but, as I keep emphasizing, although the policy-theory nexus should be welcomed as a driving theme, non-policy related contributions must be acknowledged along the way, if only in asides. Pincus' argument is also a little more nuanced than Millmow suggests. He contends that although PCT did not play a role in the shift towards a pro-market sentiment in the post-1970 world, it may have had a perverse and minor role via the Bentley-Schattschneider tradition within PCT that entails modelling pressure group activity. Specifically, the modelling of the nefarious influence of interest groups on political decisions may have contributed to the establishment of the many regulatory bodies in Australia, outside the politician's reach, the members of which now act as Walrasian planners who impose imagined market outcomes in incomplete markets. I suspect that this development is disconcerting for both the men of 68 and the men and women of 86. It is also worth mentioning that Pincus himself had stints at VPI and made contributions to this Bentley-Schattschneider tradition by fusing it with Mancur Olsen's modelling of groups to measure the extent pressure activity shaped US tariffs (1975, 1977).[11] Millmow largely ignores this, with a lone reference to Pincus' doctorate (171), and also overlooks the way Pincus's pioneering use of cliometrics to explain political outcomes has now become a standard exercise in those blue-ribbon journals that rank statistical findings over originality. He instead focuses on the admittedly important anecdote that Pincus, rather than Arthur Laffer, is responsible for the Laffer curve because he drew the "pregnant belly" curve on a board for his fellow Stanford classmates, Laffer and Michael Porter (171). This story has been doing the traps for some time (and since acknowledged by Laffer), but it is good that someone has finally put it in print. It is still odd that Millmow awards Pincus four references and Brennan none. It also must be admitted that PCT is now inexplicably in a parlous state in Australia. Nancy Maclean's (2016) recent denigration (if not poisonous account) of the origins of PCT in *Democracy in Chains*, which is distorted history at its very worst, may not help matters.

(ii) John Quiggin

Quiggin was also identified three times. Reference was made to his contribution as a microeconomic theorist, but with the delightful backhander of "when he is not trying to save the world"; work on the theory of anticipated utility; and *Great Expectations: Microeconomic Reform and Australia* (1996). In the latter publication Quiggin drew upon a disturbingly large number of technical articles that he had published in ranked journals. (Consult the line-entries between 35 and 113 of the 213 articles in his 2011 CV). It is therefore representative of a fair share of Quiggin's research intent prior to the millennium, but, admittedly, it is not related to his widely cited articles on either contingent valuation methods to determine values of non-market goods or the theory of anticipated utility. *Great Expectations* made a mark because it challenged the reform movement within the boundaries of established economic theory rather than by delivering a hectoring and wayward critique of something called "economic rationalism", which was then a popular sport amongst sociologists, such as Michael Pusey. Quiggin's key proposition was that reform was driven less by economic theory and more by members of an ideologically charged "policy elite" who had an almost emotional objection to the policies of the 1950s and 1960s (1996, ix, 5). Modern orthodox economics, after all, could not be blamed for the reformist zeal since it was devoted to both market failure and beauty. It must be granted that a few of Quiggin's arguments are overstated and he never resolves the problems associated with quantifying the effects of reform. He nonetheless makes many telling points. The one that caught my eye is that the redistribution which many presume follows reform—say via transforming a potential into an actual Pareto improvement or achieving an egalitarian goal after enlarging the pie—creates distortions that need to be considered in any efficiency calculation (1996, 45). This insight topples the traditional partition between the efficiency and equity issues. Quiggin surprisingly concludes in an uncontroversial fashion by claiming that the gains from reform were over-stated, even if they had a "positive" impact "in many, perhaps most, cases"; and that more, not less, economic theory should be deployed to challenge the unthinking formulae of an over-zealous elite (1996, 222-3).

Millmow acknowledges Quiggin by referring to the way he challenged Olsen's thesis that governments are beholden to special interest groups (222, but without reference to a specific publication); emphasized the potential losses from privatization (235); and claimed reform was driven by dogmatism rather than analysis (236). Quiggin and his numerous publications are, however, not placed in any context. This is in keeping with the irregularity with which

Millmow provides contextual background for his players. The result is that readers marginal to the local economics community would have no idea about the importance of Quiggin, for good or ill, in the economics debates prior to 2000 (and, of course, since). There is certainly no reference to the way that Quiggin emerged as a public intellectual in Australasia in the early 1990s. His publications now number over 1500 once non-academic items are counted; he seems to be a fellow of very nearly every society (including the ESA); and he has over 5000 Twitter followers. It is also strange that Millmow considers the economic rationalism (ch.10) and microeconomic reform (ch.11) debates in different chapters even though they are intimately connected. Perhaps Quiggin would have loomed larger if they had been linked. Some contested issues could have also been resolved if the two debates were considered jointly. For example, Millmow's fair-minded account of the debate over 'economic rationalism' entails questioning Pusey's 'straw man' argument that the economics discipline was to blame for the rise of this contested entity, while his account of the debate over 'reform' leaves unquestioned Quiggin's argument that an ideologically charged 'policy elite' implemented reform without regard for economic analysis. The two contentions are quite different. Fred Gruen (1997), for one, claimed within a civil review of *Great Expectations* that reformers, like himself, were responding to economic crises in the best and most rational way they could. Millmow also fails to explore the many theoretical arguments that underpin Quiggin's critique of reform. Nor, for that matter, does he provide any analysis of this author's technical papers on anticipated utility and valuing non-market goods, let alone any historical context for their emergence. These omissions are again in keeping with Millmow's preference for policy over theory. In this case, however, there is even less justification for this strategy. This is because those economists with 'cool minds and cool hearts' would have simply dismissed Quiggin's policy interventions as the unreasoned contentions of a bleeding-heart if it were not for the credentials earned for his technical articles in blue-ribbon journals. Quiggin is, in fact, a fine representative of that late-twentieth century figure who may be called the orthodox 'heterodox' economist.

(iii) Peter Groenewegen

Groenewegen was nominated by three respondents for his contributions to HET, with all specifically mentioning the importance of his biography of Alfred Marshall, *A Soaring Eagle* (1995). I have little to add to the several appreciations of Groenewegen's career, especially Dollery (2002) and Aspromourgos and Lodewijks (2004). His 18 books and 140 plus chapters and articles by the time of his retirement from Sydney University in 2002 also speak for themselves. I remind the reader only that Groenewegen's celebrated work on late Victorian proto-neoclassical economics, especially in the form of his biography of Marshall, came quite late in his career. He began his calling as an historian in the 1960s as a specialist in eighteenth-century economic thought and devoted many years in the 1970s to public finance, which led to the publication of the distinctly Australian textbook, *Public Finance in Australia*, in 1979. Perhaps the best comment to make about Groenewegen's research is that his pre-occupation with the historical particulars is such that one cannot detect his ideological position by consulting his texts, with the surplus or the marginalist or other vision looming large depending on the era under examination. His only religious commitment seems to be to scholarship, which, and this must be stated, he occasionally defends in an unnecessarily brutal manner. At the same time, Groenewegen's devotion to scholarly rigor has been imitated by many younger Australasian historians—if only from fear of being caught out—in a way that has made them better historians. It has also been accompanied by a type of mentoring of the young that is no longer rewarded in Australian universities. Indeed, it should not be forgotten that Groenewegen has had an equally important career as a teacher of a historically-inclined economics to generations of Sydney University students. I never audited these classes, but numerous students, including Glenn Stevens, the ex-governor of the RBA, testify to their erudition.[12] I believe, however, that a more valuable signal of their worth is how they shaped the world view of lesser mortals. I recall some years ago coming across a young entrepreneur in a public bar who was carting bird seed between Sydney and Melbourne and who, on hearing I was an economist, promptly relayed an elegant account of Ricardian rent he had derived from Groenewegen's class. I am sure it helped him in his endeavors.

Now, though Millmow may cite Groenewegen's publications in the process of commenting on an historical issue, he does not make a single reference to the way this scholar's body of work acts as an independent contribution to Australian economics. This omission is even more astonishing given Millmow is the president of HETSA, which was founded in 1981 and, until recently, dominated by Groenewegen through the force of his personality and the number of 'encyclopedic' questions he asked from the floor at the annual HETSA conference. Given that many economists no longer recognize HET as economics, this omission is tantamount to Millmow accepting that HET, and thereby his own research agenda, is not an integral part of economics. Maybe it isn't. I have never had strong views on the matter and, in any event, although I regard myself as an economist, I feel there may be more kudos derived from the appellation of historian. Still, nearly all HET specialists disagree with me on this matter, and hence Groenewegen's absence from Millmow's history will raise an eyebrow amongst HETSA members. Indeed, given the way Australasians punched above their weight in HET in the post-1970 world, it is also odd that there is no reference to the contributions made by the senior figures in this field, such as Barry Gordon, John Pullen, John Creedy, Michael White, Michael Schneider, Tony Aspromourgos, Rod O'Donnell, Tony Endres and John King.[13] The absence of Groenewegen is, furthermore, odd even if we accept the incorrect proposition that a policy-oriented history of ideas entails the remit to ignore theory that does not have policy relevance. This is because Groenewegen contributed in the policy domain through his call to rationalize the Australian tax structure by replacing the bewildering array of levies with a wealth tax, a consumption tax and a less onerous income tax. This call manifested itself in contributions to the *Economic Record* (1971, 1984), newspaper columns, attendance at the 1985 National Tax Summit (where Paul Keating proposed a consumption tax), a lecture to the Liberal party (where John Howard was possibly influenced) and a Shann Memorial lecture (Dollery 2002, 147-8). As it happens, the only reform to materialize was a consumption tax in the form of the GST in 2000, which has induced some to refer to Groenewegen as the "grandfather of the GST" (Aspromourgos and Lodewijks 2002, 3). Given Millmow's fascination with policy, it is interesting that he did not consider the way economists shaped these tax reform debates.

(iv). Kriesler, Nevile, Harcourt and Heterodox Economists

As has already been mentioned, two senior heterodox economists were recruited as respondents for the survey to ensure that all schools of thought were represented. Unfortunately, I suspect that these respondents incorrectly took my brief to mean that they should select the most important heterodox contributions rather than the most important contributions. This is the only explanation for the predominantly heterodox lists that were submitted by these famously fair-minded economists. The possibility of anomalies arising from such a misunderstanding is, however, of little consequence compared to the gains that ensue from diversity. This possibility is also, to some extent, mitigated by the requirement that an economist needs to be nominated twice to be included in the table. Still, two worthy heterodox names made the list via the votes of these respondents. Nevile was nominated for his work on fiscal policy, unemployment and macroeconomics, with reference being made to a large body of work rather than a single article or book, much of it completed in his semi-retirement. Kriesler was nominated for his work on post-Keynesian economics, with specific reference being made to *Kalecki's Microanalysis* (1988), which is devoted to the way Kaleckian mark-up pricing in an imperfectly competitive environment determines distribution. It also should be noted that one respondent itemized an array of articles by both these authors from the bibliography of Dalziel and Nevile's (2013) survey of Australian post-Keynesian economics. Harcourt, by contrast, was nominated by four respondents for his analysis of the capital controversy, which is fair enough, since although his famous *JEL* article on this subject appeared in 1969 (a year before the cut-off date for this exercise), the extended narrative appeared in 1972 as *Some Cambridge Controversies in the Theory of Capital*. Harcourt's analysis of these debates certainly had a larger impact in Australia than in most countries due to his influence within the local economics community and, until recently, the number of Australians who had some sort of Cambridge connection. Two respondents also referred to Harcourt's contributions to the history of political economy and post-Keynesian economics in the last decades of the century. The most important of the latter publications may, again, be found in Dalziel and Nevile's (2013) bibliography.

Millmow naturally dwells on Harcourt's work on the capital controversy (169-71), considers his involvement in the 1974 Adelaide Plan to reduce inflation (182), and mentions the way his policy influence was marginalized by the late 1970s (188, 190). Millmow also dutifully traces Nevile's contributions prior to 1970, particularly his role in developing one of the earliest econometric models of the Australian economy (even if he repeats the story in different contexts without cross referencing: 140, 168). He also refers to Nevile's support for more expansionary fiscal policy within the framework of an incomes policy in the early 1980s (but again in two different locations: 187, 191). There is, however, no account of the voluminous amount of research undertaken by Kriesler, Nevile, Harcourt and the army of heterodox economists of Post-Keynesian, Institutionalist and Marxist hues after the mid-1980s. The single paragraph devoted to this assembly of the marginalized turns on the demise of their influence rather than their research contributions (228). Indeed, John King's voluminous work in this dimension earns one citation via a reference to the way a reviewer stated that King's annotated bibliography of Post-Keynesian economics marked the gravestone of that school (228). (As an aside, King would have gained two votes in the polling exercise if one historically-minded economist had not conveyed that he was in two minds whether King's HET research was economics proper.) More also could have been made of the importance of the UNSW as a nesting place for the aging members of this endangered species (indirectly considered on 187), and of the way that many more have been forced into marginal universities or out the campus front gate (194, 228). The post-1980 output of the retreating army of radical political economists who subscribe to the *Journal of Australian Political Economy* is also not commented upon, while the account of their activities in the 1970s is largely confined to the institutional battle arising from their quest to establish a political economy department at

Sydney rather than their publications (178-9). The upshot is that, again, Millmow's pre-occupation with the policy-theory nexus means that the theoretical work of a set of economists receives short shrift if its champions have no policy clout or, in this case, when their policy influence suddenly settles dead due to the shifting Lakatosian currents.

(v.) Some Last Comments on the Usual Suspects: Dixon, Chapman, Corden, Gregory, Garnaut, Gruen, Hughes, Kemp, Lloyd, Pagan, Pitchford and Snape.

As already mentioned, a case-by-case account of the way Millmow treats every economist who gained two or more nominations is not possible because of word limitations. The small sample provided fortunately makes my main point. I also accept that Millmow devotes more space to the remaining economists on the list because of the obvious policy implications of their research. I do, however, add the rider that his policy focus invariably means that insufficient attention is given to the etiology of their theoretical innovations. Consider a few examples. (i) Australasian econometricians will be bemused that their post-1970 contributions are presented in a solitary paragraph and, further, that the last third of this paragraph is devoted to Pagan's policy role as an RBA board member rather than fleshing out the nature of their innovations (234). (ii) Kemp, one of the most important trade theorists Australia has produced, is allocated a single perfunctory paragraph (50-1), presumably because the technical and terse nature of his contributions to general equilibrium trade analysis have fewer direct links to local policy issues. (iii) Garnaut is well served with a biographical introduction and dutiful references to his early publications with Clunies-Ross on resource rent tax (190), but there is no discussion of the source of this tax theory, nor its relative originality. It is also strange that Millmow's preoccupation with policy does not induce him to comment on the subsequent publications in this domain with an ANU pedigree, nor how this research line led to the Petroleum Resource Rent Tax in 1987. (iv) Chapman's income-contingent scheme is placed in limited context with two citations to earlier works, but there is no reference to the complex trajectory of this idea via England and the US in the 1960s and 1970s. (v) The "Pitchford line" or "consenting adult" proposition that foreign debt is a product of mutually beneficial exchanges, and hence the current account should not be targeted, is well handled, but there is no reference to the way it was anticipated in the late 1970s and 1980s (Corden 1991, Belkar *et. al.* 2007). Indeed, like the Gregory Thesis (or Dutch Disease), this proposition is known by a different title in England (i.e. the Lawson Doctrine).[14] There is also no reference to the way Pitchford's key insight—that international trade entails traffic across both boundaries and time periods—helped popularize formal intertemporal modelling in this country. This list could go on, but I am the reviewer rather than the author of a history of Australasian economics.

5. Conclusion

Millmow's history is a good book which could have been even better if the author had devoted another six months to polishing the narrative, crystallizing the historiography in a tighter introduction and exploring the non-policy context of each theoretical contribution. My late emphasis on its qualities while dwelling on its obvious flaws should not induce the reader to presume I am that insidious critic described by Alexander Pope who damns "with faint praise", assents "with civil leer", and "without sneering teach the rest to sneer".[15] Rather than a "timorous foe" or a "suspicious friend", I am a reviewer who subscribes to the maxim advanced by the economist Paolo Sylos Labini that "the person who cares for someone also criticizes that person" (Dollery 2002, 138), but with the caveat that this criticism should always be respectful and constructive. The book is of sufficient worth for me to invest the time to write this review and, in the process, I have been repeatedly prompted by Millmow to reflect on recent Australasian economics in a productive manner. The high ratio of gentle criticism to praise in this review should certainly not dissuade anyone from reading the book. The poorer passages, omissions and imbalances stand out so dramatically since they sit cheek by jowl with great swathes of narrative that are informative and quite gripping. Millmow may baulk at engaging with the theory of any given contribution, but there is no doubt that he knows more about the historical particulars of Australian economics than any living historian and, further, than any future historian for some time. He has done the hard yards in the archives.

I finish, as promised in the introduction, with the proposals that historians should engage with more recent theoretical advances and that economists, at least in their literature surveys, should occasionally tackle their subject matter from an historical perspective. There is low hanging fruit in this research field and such contextual interventions, critical or disinterested, would add value to contemporary economic debates. Needless to add I am not suggesting that historians abandon their important work of contextualizing and reconstructing the conceptual frameworks from more distant times. We are historians after all. The many additional historiographical problems that the historian confronts when investigating recent history should also be squarely confronted. Just one danger of celebrating the contributions of the living is that one may be perceived as presenting unctuous commendations to curry favor with the influential for professional advancement, whether it be in the form of kindly journal referee reports or success in grant applications. This is less of a problem in the case of Australia because the leading economists who made a lasting contribution are nearly all elderly scholars who are shortly going to die (and hence "to stand naked in the wind and to melt into the sun").[16] The next generation of mostly foreign-trained economists have also taken the reigns of decision making to an extent that the elderly so scrutinized no longer have a voice in the way the prizes are distributed. I further suspect that my own gentle mocking of the hubris of some of my subjects will alienate as many as it will win over. I do admit, however, that such contextual interventions may remind contemporary economists who look down on HET that someone will eventually judge their contributions. When an ageing Gustav Cassell suggested that a brash Gunnar Myrdal should pay greater respect to the elders who governed his promotion prospects, Myrdal replied with: "Yes, but it is we who will write your obituaries" (Balabkins 1988, 99).

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[2] Although as a contextualist I cannot accept that a whig-oriented rational reconstruction is history proper, the sub-discipline of history of economic thought is a broad church and this approach is often productive if practitioners are conscious of what they are doing. See Creedy (2001), O'Brien (2007), and Medema and Waterman (2015) for sympathetic accounts. I certainly do not think the hectoring anti-whig, pro-contextualist special issue for the *Cambridge Journal of Economics* (May 2014) helps, especially as few of the contributors meaningfully engage with the associated literature. The chief obligation of a historian is to reflect on his or her craft, however interpreted, before rushing at the subject matter.

[3] I am not aware of any publication that traces the number of economists practicing in Australia through time, but, if anything, my conjecture that this number trebled after 1970 is an underestimate. After all, in the last third of the twentieth century the number of universities quadrupled, the number of students and professors in higher education more than quadrupled, and the public service and financial houses dramatically expanded their economic and financial research departments. For data on student numbers in Australian higher education, see DEEWR (2001).

[4] Also note that, almost as an afterthought, Millmow states on the last page of his narrative: "Australian libraries, archives and universities, along with historians of Australian economic thought, have done an excellent job in preserving the legacy of their forebears" (237). On this point, it is a shame that Millmow did not add a preface describing the various archives he exploited for this narrative, since this would have alerted the reader to one of his unsung contributions over the last two decades; namely, to alert other researchers that these archives exist and to yield protocol statements from these archives for others to use in different contexts.

[5] The reference in the text to HETSA's funeral should be interpreted as a statement of fact and not, as one referee feared, a possible attack on that society. One can only hope for a Twain twist, but without youth, all societies die, and, in the modern academic arena in which promotion is based on a publication count, there will be no influx of youth into a society without a sufficient number of A-journals in the sub-discipline associated with that society. Also note that there will still be another thirty-five years of quality conferences and good journal copy before its current membership is extinguished.

[6] Also note that although Millmow presents a worthy account of the dependent economy model—which, again, is the most important example of a "distinctly Australasian" economics—he disperses his analysis (at one point considering it under the heading of "Growth Theory") in a way that makes Metaxas and Weber's (2016) survey superior in clarity and depth.

[7] Any number of the voluminous files in the *Westpac Group Archives (Sydney)* could be consulted for the BNSW economics department, but a flavour of its operations is caught in the correspondence of C. V. Janes, T. Hytten, Shann, and Davidson, (a53, a54), as well as a 1940 formal review of its affairs (N2 197). The department itself was effectively closed in the early 1940s when the directors challenged the costs involved and soon after the directors arranged for Davidson's departure.

[8] But note that Millmow's index is not always reliable. John King, for example, gains one index listing for his biographical dictionary (13), but is also mentioned on p.228 and p.233.

[9] It also should be noted that two senior economists failed to complete the survey and were replaced with others. One respondent did not receive the request due to an email malfunction, but later stated that he would have liked to contribute his views. Another respondent stated that he would get back to me after the busy Christmas season, but must have forgotten. I did not know him well enough to badger him further.

[10] The origins of some of Brennan's co-authored works with Buchanan could also be traced, in part, to the Australian context. Pincus recalls sharing a Canberra office with Brennan in the mid-1970s when the latter seized on the idea that "became the core of his first book with Buchanan" while on the staff of the Asprey commission of inquiry into the tax system (2014, 84).

[11] Other Australians who visited VPI around this time included Michael Brooks, Kwang Ng, Ross Parish, Sue Richardson and Cliff Walsh (Pincus 2014, 84).

[12] Glen Stevens testified to their erudition in a speech at the 2011 HETSA conference.

[13] Neil de Marchi is excluded due to his expatriate status.

[14] Nigel Lawson, then Britain's chancellor of the exchequer, argued along these lines in the 1980s. The less obvious intellectual context for the debate has been partly outlined by Corden (1991). The shift in policy sentiment was, in part, a result of a shift *from* aggregated, single-period, fixed-price models of the Mundell-Fleming marque *to* inter-temporal, flexi-price models of the 1970s onwards. Also note that the above narrative should in no way be taken to diminish Pitchford's contributions, nor the contributions of others in the list. My point is that ideas do not come from thin air and the historian's job is, in part, to explain some aspect of the etiology.

[15] These lines are from "Epistle to Dr. Arbuthnot, Prologue to the Satires". See any collection containing Pope's complete works.

[16] This line is from Kahil Gibran's "On Death". See any collection containing Gibran's complete works.