1. Introduction

This book does several things, although certainly not in equal amounts. It is primarily an economic, as opposed to a personal, biography of an influential English economist, Arthur Joseph (A.J.) Brown. In this case, though, the influence was neither on the development of high theory nor on the knee-jerk reaction of public opinion that often determines subjects for biography. Rather, it examines the importance and changes in application of applied economics in public policy creation. Added to this, it is an account of how economics can usefully be applied to examining important problems of the day in a rigorous and structured way given the data and technical constraints in place. Linked to this, it implicitly examines the commonality of many economic problems across time and space. It also looks at how university-level economics education in Britain changed in the 1950s and 1960s, and the challenges of building a successful department, if not from scratch, then at least from a relative small scale.

Most academics are people of their times. Arthur Brown fits very much into that mold. He taught and researched economics from the 1930s through the 1980s at a time when many transitions in the subject were taking place; at a time when there were major shifts in the way that the role of higher education, especially in the UK, was perceived; and at a time when there were significant changes in the world economy. In particular, the period from the Great Depression to the 1960s saw the push to operationalize the ideas found in John Maynard Keynes’s “book on economic theory which will largely revolutionize—not, I suppose, at once but in the course of the next ten years—the way the world thinks about economic problem.” But, within economics, it also saw the emergence of systematic macroeconomic data collection, the intellectual technical tools to model from it, and the “electro-engineering equipment” to analysis it.

Brown was an economist who in a way straddled this world. He went to Oxford in the mid-1930s to study “science” (these days it would be called physics), graduated in politics, philosophy, and economics (PPE), and went on to become an academic in a provisional English university and to provide a range of public services making use of largely Keynesian economic ideas but within an older tradition of applied economics that can be traced back at least to the nineteenth century. In this context, he
interacted with many of the leading economic thinkers of his time, such as Roy Harrod, James Meade, Charles Hitch, Jacob Marschak, John Hicks, Henry Phelps Brown, and later Bill Phillips, Wassily Leontief, and Oscar Lange.

The challenges of economics, at least most of that which has any real-world use, was famously summed up by Keynes (1924: 333n), whom most would consider one of the more accomplished practitioners, even if not always agreeing with all his views:

Professor [Max] Planck, of Berlin, the famous originator of the Quantum Theory, once remarked to me that in early life he had thought of studying economics, but had found it too difficult! Professor Planck could easily master the whole corpus of mathematic economics in a few days. He did not mean that! But the amalgam of logic and intuition and the wide knowledge of facts, most of which are not precise, which is required for economic interpretation in its highest form is, quite truly, overwhelmingly difficult for those whose gift mainly consists in the power to imagine and pursue to their furthest points the implications and prior conditions of comparatively simple facts which are known with a high degree of precision.

THE SHIFTING SANDS OF ECONOMICS

Defining economics beyond Jacob Viner’s quip that “Economics is what economists do” or Kenneth Galbraith’s more caustic “Economics is extremely useful as a form of employment for economists” is not easy. Even Joseph Schumpeter (1954: 3), in his seminal work on the history of economic analysis, does not attempt a definition. Rather, he talks about being concerned with “the history of the intellectual efforts men have made to in order to understand economic phenomena or, which comes to the same thing, the history of the analytic or scientific aspects of economic thought” (italics in original). But he never defines what “economic thought” is. Today’s students are often left in similar murkiness surrounding more experienced scholars. Many modern textbooks do not even try to provide a definition, but simply update Viner and Galbraith’s vagueness by dancing around, providing a definition using phrases such as “We let the subject matter speak for itself.” In this age of dissolving disciplines and interdisciplinary analysis this is, however, perhaps less of a cop-out that it may initially appear.

This vagueness is perhaps more reasonably justified in the changing nature of economics. The modern idea of economics—compared to say chemistry or physics on the hard-physical science side, or distinct notions of literature and sculpture on the arts side—is relatively new and, importantly, far from settled.
Although Schumpeter’s work on the history of economic thought extends back to classical times, economics as we now think of it is generally traced back to the second part of the eighteenth century. In the days of Adam Smith, economics was largely seen as part of a wider moral philosophy. Smith’s (1776) work clearly takes this line, with his *An Inquiry into the Nature and Causes of the Wealth of Nations* dealing with what he calls “familial rights”—now generally considered the core of economics. His earlier work, *The Theory of Moral Sentiments* (1759), sets this within a broader range of subjects that also included ethics and virtue, private rights and natural liberty, and state and individual rights (what might be considered political science today). Smith’s subsequent definition of economics, as the title of his famed 1776 tome highlights, places it squarely as the study of the nature and causes of nations’ wealth, or simply as the study of wealth.

How this is to be done, how wealth is to be defined, or indeed what is a ‘nation’ in the modern world, keeps economic philosophers occupied. The subject has also increasingly been expanded in scope as these debates have taken place and a range of sub-areas, partly as a function of the need to make analysis manageable, have emerged. The long listing of “Subject Descriptors” found in the *Journal of Economic Literature* is clear testament to this.

Our interest here is in a microcosm of this world of work, and particularly in one person’s contribution to our understanding of how economies function. The backdrop is not the heady days of turmoil in economic thinking at the end of the eighteenth century, not the early Industrial Revolution, not the days of Empire, and not those of the Great Depression—although the latter marginally comes into the story, when many of the better-known economists came to the fore. Rather, it is a period of consolidation of existing economic ideas and of quantification and public policy adjustments. This was the period from the very late 1930s to the late 1970s. And, while the focus is largely on the United Kingdom and economics therein, there is much wider geographical relevance to the work involved.

The period immediately after World War II saw considerable shifts in emphasis in the economics discipline. The traditional approach, with its focus on exposition and prose, was being transformed by the likes of Jan Tinbergen, pushing forward more explicit quantification, and of Paul Samuelson, bringing the language of mathematics to the core of the subject; in the pre-Samuelson era, 70 percent of articles in the *American Economic Review* in 1940 contained no mathematics. None of these changes were entirely new in themselves—Stanley Jevons, Léon Walras, and others had been involved in encouraging more explicit formalization—but the early 1950s saw a major shift in the mainstream.
As the study of the aggregate gained ground, Keynesian economics—
with its novel concepts such as the consumption function, liquidity
preference, and marginal efficiency of investment—cried out for more for-
malization and quantification. The inflations of the immediate post-World
War II period highlighted a specific area where Keynesian theory was
proving deficient: understanding the forces that cause rises in aggregate
price levels. To help resolve this type of problem, electronic calculators,
and later mainframe computers, began to provide the necessary powers of
estimation, econometrics (the link between data and theory), and math-
ematics (the tools for formal hypothesis setting). The political desire to
have more control, or at least influence, over national economies provided
the stimulus for systematic data collection. Institutionally, this led to a
period of shifting “power” within the academy, with an overall rise in the
perceived importance of economic research and education resulting in
new departments being formed, existing ones being expanded, and some
established ones losing influence.

Against this background, the interests here are several. One is the nature
of the transition process in economic methodology, and examining just
how messy it was. It was neither a rapid change nor a very clean one, and
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thrown up was useful. Against this background, the interests here are
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gy, and examining just how messy it was. It was neither a rapid change
nor a very clean one, and certainly there was questioning of the speed at
which some of the results thrown up were useful. In this context, the role
of the economist changed in terms of how and what economic advice was
given to government and industry, and the sorts of information that was
given. Coupled with this was the matter of what education and training
economists should receive in universities, and what types of career paths
would emerge. These were matters that Brown was as much engaged in as
in the actual changes in economic thinking themselves. While he produced
important theoretical papers Brown’s main contributions were, however,
on the application of economics.

THE ROLE OF THE APPLIED ECONOMIST

A reading of Adam Smith, and indeed of most subsequent works of the
neo-classical economists of the nineteenth century, reveals considerable
knowledge of and interest in the economic conductions of their time.
Their authors did not see economics as a sort of intellectual exercise based
entirely on inductive methods, but rather as one that had relevance and an
understanding of actual relationships that would allow the development of economic policy. When cogitating in front of his students over the justification for his 1979/80 and 1980/81 London School of Economics (LSE) course on the History of Economic thought, Lionel Robbins observed that, “It’s a subject which, strange as it may seem, has the property of being described—to use the fashionable word—as relevant to your interest in the world around us” (in Robbins et al., 1998: 6).

So, what are the general skills required of an economist? Here Keynes provides the answer:

The study of economics does not seem to require any specialized gifts of an unusually high order. Is it not, intellectually regarded, a very easy subject compared with the higher branches of philosophy or pure science? An easy subject at which few excel! The paradox finds its explanation, perhaps, in that the master-economist must possess a rare combination of gifts. He must be mathematician, historian, statesman, philosopher—in some degree. He must understand symbols and speak in words. He must contemplate the particular in terms of the general and touch abstract and concrete in the same flight of thought. He must study the past in the light of the present for the purposes of the future. No part of man's nature or his institutions must lie entirely outside his regard. He must be purposeful and disinterested in a simultaneous mood; as aloof and incorruptible as an artist, yet sometimes as near to earth as a politician. (Keynes 1924: 321–2)

But, given this, the discipline of economics has fragmented over the years into schools of thought, methods of analysis, subject content, and degree of interface with other disciplines such as sociology and psychology or topic areas such as policy studies. Where economists work depends on their interests and, given notions of comparative advantage, their skills portfolio. But perhaps more importantly and, like in many other subjects, there has been something of a split between the theoretical and more abstract side of the discipline and the applied side. As with physics and other disciplines where this also features, the boundaries are not firm and individuals often cross them in their work. Indeed, in the latter account of Brown’s work, while much of this would fall into what most would consider the applied side of the divide, he also worked on the theoretical.

The contents of a symposium published in the History of Political Economy offer more detail of how applied economics has developed. In that work, Roger Backhouse and Jeff Biddle traced the history and meaning of the concept of applied economics, suggesting that “at least by the turn of the century [1900], and probably earlier, it had become common for economists to talk and think in terms of applying applied political economy/economics.” They add: “In the twentieth century, the idea that there is a distinct set of activities that can be labelled applied
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economics has come to be reflected in the institutional structure of the discipline” (2000: 5, 9; italics in original).

At that time, however, there was not necessarily agreement as to what the term “applied” meant, and views on precise definitions and boundaries differed and have subsequently evolved over time. Nevertheless, Backhouse and Biddle argue, this conception of applied economics “involves a strict distinction between building theory and applying theory, with the former activity being regarded as more important from a scientific standpoint. . . . Related to this is a belief that applied economics is more ephemeral than theory” (p. 12). This was reflected in some quarters in the late nineteenth century and slightly later, when the more general use of the term applied economics was used to indicate its practical side: that is, it had some use for business or policy making.3 This is the way John Neville Keynes (1917) defines it in The Scope and Method of Political Economy.

Backhouse and Biddle also suggest that any history of economic thought that focused on applied economics could lead to a very different outcome from one that did not. Moreover, it is necessary to understand the issues that are important at any time: “to neglect them is to distort the history” (2000: 13) Hence, “the notion of applied economics itself is an object of historical investigation,” including “the perceived relationship between applied and non-applied economics,” which has “changed significantly over the past century.” Of relevance, here, it is maintained that: “It is when one reaches the 1950s that any failure to focus explicitly on applied economics can lead to a seriously distorted picture in which the bulk of the discipline is neglected” (p. 14).4

From different perspectives, various other economists have expressed related concerns about the focus of economics in the late twentieth century onwards. For example, Ronald Coase (2006), when considering a widely debated case of the economic implications of a hold-up of the take-over of coachbuilder Fisher Body by General Motors (GM), observed that mainstream economics is strong on theory but weak on facts, a position more recently argued by Stephen Littlechild (2008) in a lecture at Cambridge. In his view, this imbalance has led economics into error. To quote Coase (2006: 276) on the general situation:

As I see it, progress in understanding the working of the economic system will come from an interplay between theory and empirical work. The theory suggests what empirical work might be fruitful, the subsequent empirical work suggests what modification in the theory or rethinking is needed, which in turn leads to new empirical work. If rightly done, scientific research is a never-ending process, but one that leads to greater understanding at each stage. In scientific research, we may win battles but not the war.
Another major British economist of this period, although one that stayed in the United Kingdom for his academic career, Maurice Peston (2006: 81), also bemoaned the lack of realism in much of modern analysis:

Economics journals continue to be full of weird and wonderful theories, which are for the most part rehashes of earlier weird and wonderful theories. What has changed is that it is now absolutely necessary to formulate everything in high-powered mathematical terms. I know I should be accused of churlishness if I were to add that this is intended largely to disguise the essential triviality of what is set forth. Occasionally, something of intellectual interest emerges, and there is even the odd article that throws light on the real world. Econometrics too gets more and more sophisticated, but rarely is anything of interest discovered.

There have been concerns that the shift towards more formalization of economics from the 1950s, despite the resistance of such names as J.K. Galbraith, the move away from the historical school that had been generally dominant in Continental economic thinking, and the institutionalism of the likes of Thorstein Veblen, John R. Commons, Wesley Mitchell, and Clarence Ayres (with its pedigree largely in the US) were also distracting economists from the real world. However, the emergence of cliometrics and the New Institutional Economics—and Nobel Prizes being awarded to Robert Fogel and Douglass C. North in the former context and to Oliver Williamson and others in the latter—suggests that this situation has subsequently been at least partially reversed. Brown was certainly aware of the debates over these topics but, that at least in the 1950s, institutions were still adequately being handled in applied economic analysis, although his views on economics education at the time were somewhat different:

It is doubtful if economists engaged in research sin very greatly by simplifying or generalizing the institutional background. It might be claimed that simplification of this kind is essential to the business of perfecting instruments of analysis and that economists are usually aware of what they are doing in this direction. But it can hardly be denied that economics is usually taught in such a way as to under emphasize the importance of the institutional background in determining what kind of theoretical analysis is applicable. (Brown, 1951c: 489)

WHERE DOES A.J. BROWN FIT INTO THIS?

By inclination, Brown was an applied economist, as can be seen from his subsequent economics textbooks (Brown, 1948; 1959a), seeing his role as applying economics to assist in the formulation and assessment of policy. The elements of his applied economics were the development of an
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appropriate theory for the problem and the manipulation of available statistical data to get a “feel” for the quantitative significance of the variables. Bowers (2003) outlines Brown’s approach: “At meetings, having explained the economic theory, Brown would produce a small slide-rule that he carried in his top pocket and carry out rapid calculations to give some quantitative basis for argument. In his hands this simple tool kit could be devastatingly effective.” By modern standards this approach would likely be considered ad hoc, but it was used at a time before computers and associated software were widely available, and when even electronic calculators were rare (Renfro, 2004). In practice, it provided a powerful tool for policy makers of the day.

Brown did significant work in international trade, economic development, inflation, and regional economics. His 1959 book, *Introduction to the World Economy*, was a standard text for many years, and *The Great Inflation, 1939–1951* (1955) lays claim to have presented the underlying concept of the Phillips Curve relating wage changes to unemployment some four years before Phillips himself.

Brown was the archetypical high-flying academic of his time. He graduated in 1936 with first-class honors in PPE from Oxford University and within three years, and whilst working in a Harrods research group on prices and interest (Besomi, 1998), he had published the first empirical analyses of liquidity-preference, making the first use of confluence analysis in the United Kingdom (Brown, 1938, 1939a). Meantime, he beat the future British Prime Minister, Harold Wilson, and the historian and future principal of the University College of Buckingham, Max Beloff, to a Fellowship at Oxford’s All Souls College. Brown had a distinguished war service in the Foreign Research and Press Service, the Research Department of the Foreign Office, and in the Economic Section of the Cabinet Office, where he worked on German re-armament and on the creation of the Government Statistical Service. He moved to Leeds University in 1947 as Professor of Economics and head of the department at the age of 33, the youngest professor of his generation in the subject. He subsequently declined professorships at both Oxford and Cambridge.

His extensive administrative acumen, allowed Brown to build up a major academic department at the University of Leeds and to later serve as its pro-vice-chancellor. Brown was also not averse to econometrics, and was far from non-technical, having initially intended to read science at Oxford, and appointed leading quantitative economists to his department. Indeed, his DPhil supervisor was Jacob Marschak, sometimes called “the father of econometrics.” Brown’s contribution to public service in economics was recognized by his being President of Section F of the British Association for the Advancement of Science, an elected Fellow of the British Academy, being made a Commander of the Order of the British Empire (CBE),
and receiving honorary doctorates from four universities. He was on the council of the Royal Economic Society twice and was its President from 1976 to 1978.

But Brown also had another talent, and one that was often drawn upon outside of academia. Although competent as a mathematician (and many of his papers have mathematical derivations in them), he was not in the league of John Hicks whom he thought was a mathematician in the way he thought. Most of Brown’s writings were largely (and often exclusively) in prose, with a sprinkling of charts and tables. But underlying this was a solid and rigorous basis to his arguments. Indeed, it was very much of the style of Alfred Marshall, who said of presenting economics:

[I had] a growing feeling in the later years of my work at the subject that a good mathematical theorem dealing with economic hypotheses was very unlikely to be good economics; and I went more and more on the rules—(1) Use mathematics as a shorthand language, rather than an engine of inquiry. (2) Keep to them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real-life. (5) Burn the mathematics. (6) If you can’t succeed in (4), burn (3). This last I did often.5

Brown’s applied skills, drafting abilities, and inherent common sense made him a popular economist for government and international agencies, and whilst at Leeds he continued in part-time public service. In the 1960s he was a member of both the East Africa Economic and Fiscal Commission and the Secretary of State’s Advisory Group on Central Africa (a constituent of the UN’s Consultative Group that looked at the economic and social consequences of disarmament), and a member of the University Grants Committee that allocated public monies to universities. This was in addition to serving on committees responsible for establishing the Universities of Kent and Bradford. When the 1966 Wilson government launched an active regional policy, Brown was appointed to the committee considering the Intermediate Areas. He was already directing a major project on regional policy based at the National Institute of Economic and Social Research (NIESR) and thus was well prepared for the task.

What Brown was not was a “public economist” in the modern sense. Despite drafting very accessible articles during World War II, most of his research output was aimed at academics and policy makers. When it came to spreading his findings, or offering his views, he was no Maynard Keynes who wrote extensively for the Manchester Guardian and other newspapers and magazines between the wars, or the American economist Irvine Fisher who frequently wrote newspaper articles and circulars over the same period. Brown certainly did not produce regular contributions of the kind that Paul Samuelson and Milton Friedman did for
alternative issues of *Newsweek* in the 1960s through the 1980s, or regular opinion editorials and blogs that Paul Krugman (*New York Times*) and Lawrence Summers (*Financial Times*) currently do for major newspapers; and it seems unlikely he would have become an addicted blogger even if born thirty years later! His media involved more conventional academic channels.

**IS THERE STILL ROOM FOR APPLIED ECONOMICS?**

The trend in economics since the mid-1960s or earlier has been one of focus on theory and abstraction, with many subject areas that were traditionally part of the economics taught at universities hived off into departments such as business schools or engineering. Indeed, the School of Economics and Commerce that Brown built up at Leeds in the 1950s is now within the Leeds Business School at the university. This trend is perhaps understandable as the market for students has shifted, with rising demand for graduates with specific skill sets rather than a more general background. Also, employers are seldom fully conversant with what is taught as part of specific degree programs and, at least in shortlisting, degree titles have relevance.

On the research side, pressure on academics has also shifted, with the “publish or perish” philosophy becoming even more pronounced, and added to this has been the widespread use of “impact factors” in assessing the quality of research. There is a degree of logic in this approach. Refereed journals (at least the more established ones), while their assessments processes are not perfect, do offer feedback from peers. The imperfections lie largely in selecting appropriate reviewers, but also in the inherent inertia in the way that peers often think.6

Regarding the impact factor, this is more problematic, and especially so for policy-oriented researchers. First, the medium of publication generally changes as an economist gains experience and as a wider array of opportunities emerges. As George Borts (1981: 458) put it when retiring from editing the *American Economic Review*: “Most of our authors are younger members of the profession, and most of the papers we print are distillations from doctoral theses. The older, well-established authors have many opportunities to publish outside of the realm of highly competitive refereed journals.”

In Brown’s case—and reflective of the publishing technologies of the time, when blogs and the like did not exist—the dissemination of ideas shifted, albeit not exclusively, to refereed articles from pamphlets, books, invited lectures, and contributions to official reports. This shift, besides
the career development element, also reflected Brown’s interest in economic policy; and, again referring to Borts (1981: 458), the refereeing and publication processes of journals are not ideal for this:

There were few articles on policy issues and few expository papers. This was hardly surprising, since economists are notoriously reluctant to invest time in writing serious policy papers that will be submitted for refereeing and subject to outright rejection. For one thing, the delays required for refereeing can reduce the timeliness of a policy paper.

Second, there is the matter of the “language of economics,” which has become increasingly mathematics. This has advantages of ensuring logic, although, as Thomas Piketty, author of the influential *Capital in the Twenty-First Century*, said in a 2015 *Financial Times* article: “Too often, economists build very complex mathematical models to look scientific and impress people. I have nothing against mathematics—I initially trained as a mathematician — but it’s usually to hide a lack of ideas.” Equally concerned is Paul Romer (who trained as an engineer) who, just prior to his appointment as Chief Economist at the World Bank, upbraided his fellow economists for their growing taste for obscure prose and “mathiness.” Basically, he argues that the excessive use of arcane equations in economics papers makes them so hard to follow that they allow authors to avoid scrutiny. If economics is to benefit society, he continues, researchers need to make arguments which are clear enough to be understood, tested, and disproved.

From a publication perspective, and here again we defer to the experience of George Borts (1981: 458): “The increased use of mathematics has made life more difficult for the editor and referees.” The trouble is that mathematics, while some would say it is the *lingua franca* of economists, is not the language of many of those who want to use economics in policy and other forms of decision-making. Modern technology has a way around this, and many journals have authors put their mathematics in electronic attachments, requiring a clear account of their intuition as the main text. In Brown’s day, as highlighted above in the quote by Alfred Marshall, the mathematics had to essentially be there; but its translation into English was more demanding, in part because there were no electronic annexes in which to locate the mathematics, but also because the primary intended readers were often not professional economists. Economists, of course with exceptions, want to shape public and private sector decisions rather than enhance their impact factors by gaining more citations for their papers from those producing equally obscure publications.

This focus on theory can easily be tested by looking at the ways economist view their journals: the evidence is that they see many more brownie
points in publishing in theoretically oriented than in applied journals and in general journals as opposed to more specialized, subject-directed journals. The h-index and others confirm this, or at least show it to be a self-fulfilling prophecy. While much of recent interest has involved factors such as citation counts, Button and Pearce (1977) surveyed UK economists and found a clear view that kudos in publication was highly correlated with outlets being perceived as more theoretically oriented.7

THE BOOK

At some point, it is generally useful in an Introduction to a book to provide some guidance as to its structure and approach. This book looks at the work of an important English economist from the mid-twentieth century, and does so in the context of a challenging and evolving political and social order and dynamic intellectual environment. It is not easy bringing together the work of an applied economist because of the diversity of topics that are often covered in a career. Someone involved in developing a school of thought (such as J.M. Keynes) or in stimulating a significant shift in methodology (such as Paul Samuelson, Ragnar Frisch, and Jan Tinbergen) is much easier to discuss because there is less need to provide context. Indeed, as Keynes once famously quipped in the General Theory (1936: 383–4):

> The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed, the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist.

As we have seen, the applied economist essentially acts as the conduit between those generating the ideas and economic theories and their application. This involves testing and modifying concepts and, often, translating them for “practical men” when relevant to allow for public policies or private actions. It also involves debunking them in not a few cases.

Here we use the economic contributions of a major UK applied economist who was active in both academic life and as a public servant in the second half of the twentieth century to explore the types of issues that can be usefully addressed by applying appropriate economic thinking and data. The book also provides insights on how that differs from much of what is done in economics these days, where, to adopt one of the criteria of figure skating, virtually all evaluation is put on technical merit and the
execution of methodology. There is inevitably judgment involved in economics, a subject that, as Keynes highlighted, often depends as much on animal spirit as it does on rational behavior. It also, despite the tendency towards abstraction, requires real-world data that models can be evaluated against. Basically, the cycle described by Coase.

Brown worked on both macroeconomics and on what may be considered mesoeconomic subjects, but his approach was essentially the same: Use the best data available and supplement it where possible; set this data in the appropriate theoretical analytical framework and use the analytical technique (quantitative assessment methodology) that fits the context. He did this in his academic papers and books, as well as in his advice to government and other official bodies. The idea of modeling for the sake of it or adopting the latest econometric technique for trendiness was never a criterion, and there seems no good reason why it ever should be.

To set things in context, the book contains a certain amount of biographical detail regarding Arthur Brown’s life, which has also been supplemented in the reminiscences of two of his sons in the Foreword. After all, his wider experiences must have inevitably shaped his ideas and interests. There are also brief accounts of the lives and economic works of some of those who worked with him in a variety of ways during his life. But this is neither a strict personal biography of Brown nor that of others in the story. The book also provides quite a lot of background material about the way economics evolved over Brown’s life-time, the changing questions that economists addressed, and the institutional surroundings in which they worked. This is done by way of contrasting the context of the second part of the twentieth century with the situations economists find themselves in or, and perhaps partly more accurately, have put themselves in, during the first part of the twenty-first century. In terms of exposition, the material is partly chronological, but at times is grouped together by context—people’s lives and work are not always linear.

NOTES

2. Keynes’ father (John Neville) seemed, however, to believe a division of labor was also needed: “theoretical and practical enquiries should not be systematically combined” (Keynes, 1917: 54).
3. Although one could question how practical economics is, as the Cambridge economist Arthur Pigou implicitly did when supposedly asking the rhetorical question: “Who would ever think of employing an economist to run a brewery?”
4. Again, in the symposium we find Christopher Dow et al. (2000) putting the case that not all economics followed this trend: “Scottish economics represented a tradition in applied economics that totally rejected such a [mainstream] view. In this tradition, the essence of
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... economics, as synonymous with applied economics, is remaining in close touch with the real world.”


6. For example, both the *American Economic Review* and the *Review of Economic Studies* rejected George Akerlof’s 1970 paper “The market for lemons”—which essentially resulted in his being awarded the Nobel Prize in 2001—on the grounds of “triviality,” while the *Journal of Political Economy* rejected it as incorrect, arguing that, if it were correct, then no goods could be traded. Only on the fourth attempt did the paper get published in *Quarterly Journal of Economics*.

7. Not only did they find, when asked how much kudos they felt publishing in particular journals conferred on them, that the academic economists sampled ranked the more theoretically oriented ones more highly, but also, when confronted with fictional titles in the list presented, many who claimed to recognize them gave a higher ranking to the fictional, theoretical-sounding ones.