
As the title indicates, this is not a general, gentle introduction, but a focused, sophisticated treatment of pioneering French scholars with a quantitative bent, a treatment that explores how their worldviews influenced their questions, methods, and answers. As Mary Morgan explains in the foreword, “Le Gall’s most significant conceptual contribution to the literature on the history of econometrics is his designation of the ‘natural econometrician,’ an economist of the nineteenth century whose worldview was that of a natural scientist” (xiv).

Le Gall begins by tackling the conventional history head on: “It is generally taken for granted that econometrics took-off at the end of the 1920s and early 1930s, with Ragnar Frisch in the cockpit” (1). Frisch coined and defined econometrics as the unification of statistics, economic theory, and mathematics. The Econometric Society and its journal, Econometrica, were founded at this time, along with the Cowles Commission. Le Gall grants that this is a reasonable take on the institutional rise of econometrics, but with respect to econometric ideas, roots stretch much farther back. “The aim of this book is precisely to show that this standard, canonical starting point of econometrics is just the tip of the iceberg” (3). That is an interesting claim and one that I looked forward to learning about. Could it be that forgotten French authors discussed the Probabilistic Revolution a hundred years before Trygve Haavelmo?

The short answer is no. As Le Gall introduced nineteenth-century Frenchmen Antoine-Augustin Cournot, Jean-Edmond Briaune, and Jules Regnault as natural econometricians, I realized that the word econometrician meant something different to him than to me. Le Gall’s story would be clearer if he explained that there are at least three senses of the term. Originally, as defined by Frisch, an econometrician combined statistical techniques with what we now call economic theory (and used to call mathematical economics). Thus, an econometrician was as much a theorist as a statistician. Over time, the math econ aspect evolved into its own specialization, and econometrician acquired its modern definition—someone who uses statistics in
economics, either in theoretical areas (e.g., discovering new methods or testing estimators) or applied work. Le Gall implicitly uses a third and much broader definition of econometrician: anyone who uses quantitative methods, ranging from mathematics to statistics.

This explains why Cournot can be an econometrician in Le Gall’s eyes while never doing any empirical work at all. Claude Ménard (1980, 533) argues that “this was not for lack of data—these existed—but because Cournot’s ambition, his idea of the science to be created and of its object, led him to seek solutions in rational economics (in the sense of ‘rational mechanics’).” Le Gall disagrees, explaining that Cournot could not acquire the data he needed because, quoting Cournot, France “has not yet reached a practically stationary condition” (44). Only at the end of history can Cournot’s ideal in science, the combination of mathematics and statistics, be implemented. Cournot’s contribution is to indicate potential applications and ideas. Even though Cournot did not practice what he preached, for Le Gall, the idea that a function could be found—in principle—for the law of demand and used to compute an optimal price makes Cournot an econometrician.

While I find Le Gall’s definition of econometrician overly broad and a likely source of confusion for many readers, the book makes a strong contribution when attention turns to the idea of the natural econometrician. Le Gall does a good job of explaining that Cournot, Briaune, and Regnault shared a worldview that stressed a divine, unified, and deterministic order based on simple laws waiting to be discovered via mathematics and statistics. As David Berlinski (1995, xii) poetically puts it, “If the calculus comes to vibrant life in celestial mechanics, as it surely does, then this is evidence that the stars in the sheltering sky have a secret mathematical identity, an aspect of themselves that like some tremulous night flower they reveal only when the mathematician whispers.” I especially enjoyed the presentation of Regnault’s Calcul des chances, which Le Gall sees as the apogee of natural econometrics. For Regnault, the average is an instrument that can detect the hidden, secret order that underlies the chaos at the surface. Le Gall convinced me that the natural econometrician can be spotted and identified.

Le Gall then brings the story into the twentieth century, explaining how a two-step process led to the emergence of a new worldview. First, Lucien March abandoned the determinism that characterized natural econometrics. Marcel Lenoir took the second step, building artificial worlds to deal with the overwhelming complexity of the real world and in so doing, moved econometrics from nature to models.

Having devoted individual chapters to Cournot, Briaune, Regnault, March, and Lenoir (including translating passages) and tracing the evolution of natural econometrics into more modern, model-based work, Le Gall offers a concluding chapter. Le Gall argues that the natural econometricians in France were not exceptional and examples can be found elsewhere. Individual sections are devoted to Roger Babson, William Stanley Jevons, and Henry Ludwell Moore, dubbed the “last hero” of natural econometrics.

I believe Le Gall should have explicitly defined econometrics and how the meaning of this word has changed over time. As I read the book, I was operating under the assumption that the worldview of a modern econometrician is that of estimating coef-
cient from a data-generating function, such as $y = \beta x + \varepsilon$. I was expecting to find an explanation of how natural econometricians (both in France and the rest of the world) handled randomness and paved the way to the modern worldview. This story was never told and because of this omission, I remain a bit skeptical about the fundamental message of the book, that natural econometrics is the root of econometrics.

This is not an easy book to read. It presumes detailed knowledge of the history of econometrics. It is stimulating, however, and presents the work of lesser known pioneers of quantitative economics. Morgan’s foreword gets it exactly right: “Le Gall’s account provides a specialized companion to more broad ranging histories of statistics in the economic and related realms provided by Theodore Porter and others” (xiv).

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References


Gordon Fletcher, lecturer in economics at the University of Liverpool Management School, is the author of Understanding Dennis Robertson (Edward Elgar, 2000) and is the announced author of the future volume on Robertson in Palgrave Macmillan’s series titled Great Thinkers in Economics, so one approaches this collection of his essays on Robertson, written for quite varied audiences at different times, with the suspicion that it might be a superfluous, and less well integrated, supplement to the works on Robertson that Fletcher has or intends to write as unified books. But the suspicion is unfounded. Despite the miscellaneous origins of the essays, they fit together well, providing an engaging, sympathetic, and insightful discussion that should be read by everyone interested in Sir Dennis Robertson—which should be everyone interested in the interwar emergence of macroeconomics from monetary and business cycle analysis and in the background of postwar macroeconomics, both Keynesian and non-Keynesian. Readers will not be moved to mount barricades for or against the Keynesian revolution, but they will enjoy watching a craftsman at work on a subject that he knows thoroughly and finds most congenial.

Three chapters—chapter 1, “In Search of Dennis Robertson: Through the Looking Glass and What I Found There”; chapter 6, “Regulating the Role of Money in Robertsonian Economics”; and chapter 8, “The Keynesian Revolution and the Role of Money: Keynes’s Lasting Contribution” (the latter, despite its title, is at least as much about Robertson as about Keynes)—are lectures Fletcher gave at Nihon University in Tokyo, while chapter 9, “Dennis Robertson and the Economics of the Short
Period,” was read at the workshop on the Cambridge School at Hitotsubashi University. Three other chapters were appendices to Fletcher’s 2000 doctoral dissertation “Understanding Dennis Robertson” that did not make it into his book of the same title. These chapters examine opinion on Robertson’s Study of Industrial Fluctuation (1915), opinion on his Banking Policy and the Price Level (1926), and “The Centrality of the Rate of Interest.” One chapter, on Robertson as a literary economist, is from Fletcher’s 2000 book, and another, “Shall I Compare Thee to a Wild Duck?” is from Fletcher’s 1987 book The Keynesian Revolution and Its Critics. The book is rounded out with Fletcher’s entry on Robertson in The Biographical Dictionary of British Economists, a conference paper titled “Money and the Management of the Short Period: Dennis Robertson’s ‘. . . odd little book,’” and a concluding chapter (issued as a discussion paper in 1996) on Robertson, Keynes, and the Keynesian revolution. Assembling these chapters from different venues results in the inconsistency that, although there is a consolidated bibliography at the end of the book, each individual chapter may end with a “bibliography” or with “references” or with neither. Apart from that, however, the chapters go together well.

The key to the book is Fletcher’s claim (contrary to George Stigler’s stand on the limited role of biography in scientific exegesis) that “Robertson’s approach to economics, like that of every other economist, must to some extent have been shaped by his personality. This, in Robertson’s case, could perhaps account for his fastidious overemphasis on detail at the expense of the whole, his scrupulous concern with the apportionment of credit and, as we shall see, a step-by-step ‘realistic’ approach to economic analysis” (150). Drawing on Robertson’s private papers in the Wren Library at Trinity College, Cambridge, Fletcher challenges “the view of there being a simple dichotomy between the positive and negative phases of Robertson’s life, with the Keynesian Revolution as the cause of the change. . . . the reality was much more complex. . . . the actual division was between his public self and his private self” (13). Fletcher explores this complex reality with sensitivity and insight, giving due emphasis to Robertson’s fascination with Alice in Wonderland and his failed attempt to escape from academic life to the stage—not a sentence that one could write about most books in economics! Dennis Robertson was complicated and fascinating both as an economist and as a person. Gordon Fletcher’s essays give vivid glimpses of that brilliant and troubled man.

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Carl Menger: Entre Aristote et Hayek; Aux sources de l’économie moderne.

This book is one of the outcomes of the meticulous archival work that Gilles Campagnolo is carrying out. With this book, Campagnolo offers us a synthesis in French of the current knowledge on the life and work of Carl Menger, enriched with the
considerations he has drawn from Menger’s archives. The Hitotsubashi University in Japan stores about twenty thousand volumes of Menger’s personal library, most of them annotated by hand; there is also the annotated version of his own *Grundsätze der Volkswirtschaftslehre*, of which Menger was planning a new edition that was ultimately completed by his son.

According to Campagnolo, only archival work can give us an appropriate knowledge of Menger, our understanding of whom comes mostly from the secondary literature or from the unsatisfactory (in Campagnolo’s view) English translations of his books and articles. Of course, the author is aware of the limits of archival work: finding the annotated copy of Say’s *Traité* in Menger’s library does not inform us of the extent to which Say may have influenced Menger; the book may have been read after the conception of the *Grundsätze* or, if we can assert that he had effectively read Say’s book before he wrote his own, nothing may help us to understand if Say had been a source of inspiration or if his book simply confirmed Menger’s preestablished intuitions. In other words, archival work may turn into a highly speculative exercise if not rigorously carried out. I am not sure, however, that Campagnolo entirely averts this pitfall. Indeed, the author hardly quotes from Menger’s manuscript notes, basing his arguments on his private reading of the notes. Under these circumstances, I am ready to credit his interpretations only when they fit my own. When his reading of Menger contradicts my knowledge with, as sole argument, the discovery of an “outraged” exclamation mark in the margin of a volume, I don’t think I should reconsider my position: it is only an exclamation mark, and we cannot know the meaning of it—dismissal? excitation? disappointment? something else? The impression I get is that Campagnolo did not make full use of the potential interpretative resources of the material he analyzed, distilling the quotations to the minimum and leaving the reader somewhat frustrated.

There are, however, a few fortunate exceptions, such as the analysis of the influence of Gossen on Menger, where Campagnolo used the archival material to confirm or invalidate the interpretations of the historian of thought. Consider also his analysis of Menger’s attitude toward the use of mathematics; access to Menger’s comments in the margins of the books of Gossen and of Auspitz and Lieben allows Campagnolo to affirm that Menger’s resistance to the use of mathematics in economic theory cannot be reduced to a question of form, but stems from his rejection of the equilibrium Walrasian framework. I regret once again, however, that, except quoting from Menger’s comments on Gossen’s book, Campagnolo does not quote any of Menger’s annotations on this theme.

If this aspect leaves us a little bit disappointed, there is another aspect of the book that quite deserves the attention of those historians of economic thought who consider their practice as mainly an exercise in full immersion into the historical context of the author they study. From this point of view, Campagnolo’s book is undoubtedly a mine of information. If one believes in the relativism of economic theories, she will find here an accurate analysis of the cultural, political, and academic contexts that shaped Menger’s reflections. Campagnolo offers us a detailed portrait of the Austrian founder, whom Campagnolo ultimately depicts as a “man of the Enlightenment.”
The priority given to the comprehension of the historical context over the theoretical content is flagrant, and it is not in this book that one should look for a linear presentation of Mengerian or Austrian economic theories. Consider for instance the presentation of the first and second generations of Austrian authors: Campagnolo sets the reader straight about the academic relationships between Menger, Wieser, and Böhm-Bawerk, about the difficulties met by Mises and Hayek during their American exile, about the role of Rothbard in forming a libertarian American academic lobby, and so on; but few words if any are spent describing Wieser’s theory of imputation, Böhm-Bawerk’s theory of capital, Lachmann’s theory of institutions, and the like. As a matter of fact, Campagnolo defines the Austrian tradition as a set of antitheses. There are a few exceptions, such as the reflections—the rapid reflections—on the theoretical connections between Menger and Weber and, more surprisingly—and even more rapidly—between Menger and Karl Polanyi. Campagnolo suggests here that Weber and Polanyi grasped what constitutes the essence of the Mengerian message and elaborated their own views starting precisely from these Mengerian aspects neglected by his official followers. These original interpretations of Campagnolo remain, however, superficial and are diluted into the relativist approach he privileged.

Throughout the book, there is a theme that is particularly well analyzed and dealt with, in a better way than all the rest, thanks precisely to the methodological choice of the author: the Methodenstreit. It is very well documented by Campagnolo and is perhaps the most detailed analytical subject of the book to which the author puts his knowledge of the intellectual Austrian context to good use for an analytical comprehension of the controversy. This successful example of retrospective analysis represents a middle ground between pure relativism and pure absolutism.

The author is currently preparing a translation in French of the Grundsätze, one of the Untersuchungen, and also a translation of the notes that Carl Menger prepared for the second edition of his Grundsätze. No doubt the access to these notes will inject new enthusiasm and revitalize studies on Menger. This book represents the appetizer of a meal that holds a few pleasant surprises in store.

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Historians of economics frequently lament that more and more our colleagues fail to see history as essential to an economics education. We frequently lampoon the student who believes that everything worth knowing was written no more than five years before he began his PhD program. It is bad enough that a cultivated amnesia means that an economist such as Don Patinkin, who shaped modern monetary economics and who is not long dead, is unknown to young stars of the profession; but it is even worse that vastly important figures can easily live long enough that only historians...
recall why they are vastly important. Yet, if we were given the chance, how would we actually use history in the teaching of *mainstream* economics? And what resources would be available to us? Snowdon and Vane have provided such a resource: a comprehensive history of macroeconomics since Keynes. I know of no other book of similar scope and range.

*Modern Macroeconomics* is literally “thick” history, weighing in at more than 800 pages in dense type. Snowdon and Vane seem to have read and cited every primary source. In the historian’s sense, it is not thick, but internalist—mainly sticking to the published record. The major exception is a Snowdon and Vane trademark—rich and insightful interviews with key players: Milton Friedman, James Tobin, Robert Lucas, Robert Solow, Paul Romer, Gregory Mankiw, Edward Prescott, and Alberto Alesina, as well as Keynes’s biographer and historian, Robert Skidelsky.

I am not one to complain about internalist history. It remains highly valuable for many purposes, especially for educating the insiders. There is something deeply unaesthetic about pervasive ignorance of the bare facts of history. Something is wrong when students are unsure whether the American Civil War came before or after World War II or, equally, whether Adam Smith ever taught at Chicago. A mastery of the internalist history of macroeconomics would not only reduce the number of times that we must cringe, but would provide a foundation on which wider contexts and implications can be hung. And it is the kind of history that has a fighting chance of being included in mainstream courses.

Still, I am skeptical of Snowdon and Vane’s targeting the book at intermediate macroeconomics courses. It is too hefty to serve as a supplement for such a course. And I doubt that history is the best way to teach the foundational course in macroeconomics. Students need a good grasp of fundamentals from a particular point of view before they are able to appreciate the development of a field or to take a critical perspective. Teaching macroeconomics in the first instance as a series of conflicts among various historical “schools” tends to promote a sort of cheap relativism in which history is merely a menu: “I’m a conservative, so I’ll plump for monetarism”—whether Friedman is right or wrong. Snowdon and Vane also assume too much background knowledge for the typical American student starting an intermediate macroeconomics course. For instance, it would be hard to learn the mechanics of the IS/LM model for the first time from their discussions of Hicks and his successors. Those reservations to one side, I have successfully assigned *Modern Macroeconomics* as a supplement for an undergraduate history-of-macroeconomics course in which the original articles are the primary sources and core macroeconomics courses are prerequisite.

*Modern Macroeconomics* is a history of mainstream macroeconomics that merely bows to the heterodox. The sequence runs: Keynes and the classics; orthodox Keynesianism; monetarism; the new classical school; real business cycle models; and the new Keynesian school. Economic growth is addressed in the last substantial chapter, which is a little jarring, since growth theory 1939–70 really does belong to the history of Keynesianism, while more recent growth theory is strongly conditioned by new classical developments. The peripheral quality of their treatment of heterodox views is underlined by the fact that Snowdon and Vane have subcontracted chapters on the Post
Keynesian school to Paul Davidson and on the Austrian school to Roger Garrison—both partisans for their approaches. Omitting those chapters—in effect, conceding that this is really a history of mainstream macroeconomics—would have strengthened the coherence of the volume. The authors also include a chapter on the new political economy, which is obviously closer to their own core interests. It is valuable in its own right, though it is not tightly integrated into the main story line.

Snowdon and Vane offer conventional readings. There is no hidden history here nor any surprising new interpretations. It is largely history as the mainstream profession would see it if they bothered to read the older work at all. This is not necessarily a negative, since, to a large extent, readings are conventional precisely because these interpretations framed the development of the field in the minds of the practitioners. Keynes, for instance, knew that he perpetrated a solecism in bracketing everyone from Ricardo to Pigou as “classicals.” Yet, Snowdon and Vane start from Keynes’s position. They acknowledge the richness of the macroeconomic debates of the 1920s and 1930s—citing Laidler’s *Fabricating the Keynesian Revolution*, albeit without engaging its interpretive argument.

In general, Snowdon and Vane make little use of historical scholarship and focus instead on a comprehensive review of published papers. The strategy reminds me of mapping the human genome. When I asked a biologist, just after the announcement of the completion of the project, exactly what had been accomplished, he likened it to having constructed an aerial map of all the buildings of a city without knowing just what purpose each one served. Such a map is invaluable as a stepping stone to a deeper understanding, but naturally there is much more to be done. Snowdon and Vane’s book is a great accomplishment—I would not know where to look for another such comprehensive map. It should serve as a stepping stone to a rich history of modern macroeconomics.

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Scholastic economic writings in sixteenth-century Spain and Portugal, sometimes referred to as the school of Salamanca, mark an important stage in the transition from medieval to early modern thought. Very little of this material is available in English. In the present *Sourcebook* three typical bits of text are judiciously selected by the general editor and presented in translation, each with a special introduction by an international authority. Monetary theory was a prime concern in the Iberian peninsula at a time that witnessed, among other major upheavals, unprecedented changes in the value of money and prices due to the influx of precious metal from the New World.
In the case of Martín de Azpilcueta (1492–1586), the text presented is one of four appendices to his *Manual de confesores y penitentes*, a celebrated contribution to an old genre. The price revolution in Castile in particular precipitated his occupation with the subject of money. The problem discussed is how to estimate a just price of money in various kinds of economic contracts. The author’s suggestion that money be treated as any other commodity is fair enough as far as it goes, but the reference to rules laid down by Thomas Aquinas on buying and selling in the *Summa theologiae* is unhelpful because the question of the value of money is not raised there. Rodrigo Muñoz de Juana, in his excellent introduction, states Azpilcueta’s metalist conception in simple terms: “If goods got costlier because of the great need for them and their scarcity, that is, because of supply and demand, money was not an exception to this rule” (11).

Luis de Molina (1535–1600), the eminent jurist, philosopher, and theologian, was one of the key figures in Spanish late scholasticism. He is represented in the present volume with a number of excerpts from his huge *De iustitia et iure*. The introduction by Francisco Gómez Camacho, S.J., is probably alone worth the price of the volume; partly because he is the undisputed authority on the Salamancans, particularly Molina, partly because his introduction with its analyses of the texts is of greater immediate value to most readers than their translations. Molina deals with money-changing in a variety of forms: different currencies, different times and locations, “that is, by means of bills of exchange,” or “the rental of services and the necessary activity to transport the money from one place to another,” as when a “banker anticipates the money in one place to recover it in a different place” (181–85). The fundamental principle is the requirement of equality between what is given and received in exchange: “Just as with the contract of sale and all other commutative contracts in which it is necessary to abide by the equality between what is given and what is received, so in the exchange in which money is given for money should there be equality, because this is the law that rules over commutative contracts, among which is the exchange” (155). However much the late scholastics can be held to have anticipated modern economics, Aristotelian justice as equality was still being forcefully maintained in late-sixteenth-century Spain.

The final installment in the *Sourcebook* is *De monetae mutatione* by Juan de Mariana, S.J. (1536–1624), briefly introduced by Alejandro A. Chafuen. This work is less readily available than those of Azpilcueta and Molina, and its translation is most welcome. Criticizing the practice of debasing the currency could be a risky undertaking. Mariana nevertheless insisted that debasement might be both unjust and unethical. The king is not the owner of the private possessions of his subjects. Taxes, monopolies, and the like require their consultation and approval. Likewise, the king cannot normally debase money by changing its weight and quality without consulting his subjects. Money has a twofold value; one is intrinsic and natural, the other is extrinsic and legal inasmuch as it has been established by the king. These two values must be diligently and accurately kept equal, particularly in the case of silver money, for silver is the backbone of commerce, its ultimate foundation. If it is altered, everything resting upon it will collapse.
A possible objection to this volume is the enormous number of references, layer upon layer of previous authorities, supporting or opposing the views expressed by the authors. The competent scholar will need no translation but will work with the original and find the references there. On the other hand, these references may be an excellent demonstration of what is awaiting anyone tempted to initiate research in scholastic economics: a vast and promising and largely unexplored field of learning.

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This book gathers contributions to a conference organized by the Cournot Centre for Economic Studies. Its goal is “to assess the relevance of [Cournot’s] heritage, and therefore his topicality, on the epistemological, methodological and doctrinal levels” (4).

In chapter 1 (“Cournot as Economist: 200 Years of Relevance,” by Jean Magnan de Bornier) we find a list of “the most important elements that an economist can learn from the Recherches” (8) (about the use of mathematics, the analysis of monopoly, competition, and taxation, or game theory). The chapter concludes that “the case of Cournot as an inventor in the field of economics is most particular, if not unique” (18). Chapter 2 (“Cournot’s Probabilistic Epistemology,” by Thierry Martin) analyzes various dimensions of Cournot’s probabilistic thought: the objective versus subjective nature of probability, and “philosophical probability,” which characterizes “all the judgements that cannot be formally demonstrated, primarily those by which we attempt to account for reality with the help of explanatory hypotheses” (25). Philosophical probabilities, Martin explains, include criteria “allowing us to confirm the conformity of a given representation with the phenomena it describes, but they cannot be expressed numerically” (25). In chapter 3 (“The Functions of Economic Models”), Bernard Walliser identifies “fundamental functions” (41) that models fulfill (“iconic,” “syllogistic,” “empirical,” “heuristic,” “pragmatic,” and “rhetorical”). He compares the way they apply to the social and to the natural sciences, and presents in each case Cournot’s thinking. Chapter 4 (“From Cournot’s Principle to Market Efficiency,” by Glenn Shafer) offers an analysis of Cournot’s principle (“an event of small or zero probability singled out in advance will not happen” [55]). After an explanation of its “rise and fall,” Shafer reconstructs it in a game-theoretic form and then applies it to the efficient markets hypothesis—more generally, he explains how economics and finance could benefit from it. Robert J. Aumann’s Nobel Prize lecture is reprinted in chapter 5 (“War and Peace”). In it he presents his analysis of strategic equilibriums in repeated games, which leads to his “economic analysis of war” (98)—war would be “rational”—and reading of the Talmud. In chapter 6 (“Cournot and the Social Income”),
Robert Solow explains that although he is fascinated by the Mathematical Principles, Cournot’s macroeconomic work would be based on a “mistake” (108), relative to the definition of social income. “I hoped to find that Cournot had made more headway towards modern macroeconomics or towards modern welfare economics than he was actually able to do,” Solow laments (114–15). Finally, in chapter 7 (“Comparing the Incomparable: The Sociology of Statistics”), Alain Desrosières focuses on the construction of statistical nomenclatures; they presuppose the definition of a space of equivalence from which various objects become comparable, and belong to “conventions that are negotiated and inscribed in stable texts” (139).

We can learn much from some of these chapters. Martin offers a careful analysis of some philosophical foundations of Cournot’s thought; and Shafer’s treatment of Cournot’s principle, based on an accurate historical investigation and a robust attempt to adapt it to contemporary issues, is highly stimulating. One may also praise other contributors for reviving some of the richness of Cournot’s thought—a fascinating nineteenth-century polymath (comparable to Jevons, as Maas 2005 portrays the latter) who developed strong interactions between history, philosophy, and the natural and the social sciences. At first sight, such an interdisciplinary path underlies this book—in it the reader will find economic, methodological, philosophical, statistical, and sociological studies. But Augustin Cournot: Modelling Economics disappoints, since the various chapters remain self-sufficient and no cross-fertilization emerges from the diversity of the perspectives followed. This is not surprising once we discover that in some chapters Cournot in his context is the hero, whereas in others the targets are current issues; that some chapters treat Cournot in an instrumental (and sometimes Whiggish) way; or that the sole link between chapter 5 and Cournot is a preliminary note stipulating that “according to Aumann, if Augustin Cournot had still been alive, he could have won the [Nobel] prize on at least three different occasions” (96).

Scholars already familiar with Cournot and with existing studies such as Ménard 1978 and Martin 1996 will make few discoveries here. The raison d’être of this book rather lies in an analysis of what we can learn today from Cournot, but this collection of papers remains exploratory and a sufficient assessment of Cournot’s legacy is still to be written.

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References

As I read this book in October 2008, I wondered why it was published. The best answer I could accept was that Keynes recognized in 1930 that the Great Depression that was then underway would end. Growth would resume at about past rates. Real incomes would rise in the next hundred years by a factor of at least 4 and as much as 8 times 1930 income. That contrasted with the now daily bombardment of claims and speculations that current financial and economic problems would end capitalism and bring back the Great Depression. Keynes would have properly said “poppycock,” or even “idiocy.”

But the answer was not available to the eighteen authors who wrote the fourteen essays in this book, and none mention it. Almost all praise Keynes for getting his hundred-year forecast about right. None recognize that a few years later Joseph Schumpeter made a similar forecast about growth in his epitaph for capitalism. Schumpeter’s unstated concern was to dismiss the stagnationist views of Alvin Hansen, views similar to Keynes’s. Unlike Keynes, he predicted the arrival of some form of democratic socialism. Of course, events proved him wrong, just as they did Keynes. Western capitalist countries accepted the welfare state but avoided the confiscation of productive property by the state.

Keynes predicted a decline in work and a rise in leisure. He gave fifteen hours a week as his guess as to the average amount of time that people would devote to work. We are three-fourths of the way to 2030 with no sign that his guess will prove right. None of the authors defends his claim. Almost all point out that he seemed to rely on the income effect and neglect the substitution effect of higher income. Gary Becker and Luis Rayo point out that in the same year that Keynes published his essay, Lionel Robbins published an analysis of income and substitution effects. Becker and Rayo then point out that, at the time Keynes wrote, economists did not recognize human capital as a part of wealth. They note that in the oil-producing Middle East, where wealth is all in physical form, male workweeks are similar to Keynes’s guess about 2030. They say that when wealth consists of knowledge or human capital, people work harder. Perhaps they recognize that new knowledge can reduce the value of what they know, thus the value of their capital. Of course, new knowledge can make the oil fields lose value, but that may be less likely.

Richard Freeman points out that Keynes missed the effect of economic incentives to induce high earners to work more (137). Hours worked increase with incomes. Factory workers and men with incomes about average retire early. On the other hand, many women consider work outside the home as “liberating.”

Jean-Paul Fitoussi disputes Keynes’s neglect of income distribution. If all incomes increase eightfold in a century, the gap between high and low incomes increases commensurately. If the gap between the highest and lowest quintiles was $10,000 dollars in 1930, it would be $80,000 in 2030. Fitoussi finds this implausible and politically or socially unacceptable; I agree.

There is considerable overlap in the essays. In addition to praising Keynes’s forecast for 2030 and rejecting the guess about hours of work and leisure, most of the
essays criticize Keynes’s beliefs about absolute and relative needs and what he called the end of the economic problem of scarcity. The editors point out that he would have avoided his principal errors if he had accepted his teacher’s (Alfred Marshall’s) message that technical change gives rise not just to higher income but also to new wants or demands (14–15). Keynes is perceptive for his time about the role of technical change and capital accumulation in bringing growth but fails to recognize that the same process brings new goods and services that people choose to buy. He argues that absolute wants will be satiated by 2030, and he dismisses relative wants as not very important. The essayists almost all disagree.

Most writers also dismiss Keynes’s moral prejudices that led him to dismiss and scorn the pursuit of money (income?), material possessions, and the British wealthy who did not share Bloomsbury values. Benjamin Friedman says that “most people never regard their economic problem as solved” (129). He and others point to rising demands for healthcare and education. In a consumer society like the United States, Keynes’s conjecture about satiation seems weird.

None of the essayists mention that Keynes’s beliefs about material wealth, money, avarice, and greed were standard beliefs based to a considerable extent on what he learned from the philosopher G. E. Moore during his student years at Cambridge. In his essay “My Early Beliefs,” Keynes refers to the “religion and morals” that he learned from Moore. The religion was self-centered. The morals were directed toward the outside world. The objects of life were, or should be, love, truth, beauty, timeless contemplation, and the pursuit of knowledge (Meltzer 1988, 35). In many of his writings, especially his popular writings, Keynes emphasizes the ideal, to pursue higher values. He rejected the Benthamite utilitarian calculation, with its emphasis on gains and losses in utility. It was a less attractive guide to action than the pursuit of beauty, truth, etc. Keynes joined Moore’s philosophy to the so-called “prepositions of Harvey Road” that he learned from his parents. These proposed that an intellectual aristocracy would guide society toward the ideal.

Keynes still had these beliefs when he wrote to Hayek, praising The Road to Serfdom. But he then wrote that if the right people were in charge of policy, Hayek’s concerns would not be realized. It should be no surprise that he held these views when he thought about how people would use their abundant leisure in 2030. They should pursue truth, beauty, and the other virtues that Bloomsbury liked. Keynes’s scorn for the British upper-class was nothing new.

In “Economic Possibilities” Keynes wrote that the pace at which economic bliss would be achieved depends on four things: controlling population, preventing wars, trusting science to solve scientific questions, and continuing the accumulation of capital. Once bliss is achieved, old ideas will have to be replaced by “the most sure and certain principles of religion and traditional virtue—that avarice is vice, that exaction of usury is a misdemeanor, and the love of money is detestable... We shall once more value ends above means and prefer the good to the useful.” For Keynes, Moore’s philosophy lived.

Most of the papers in this volume are brief, often no more than six pages. Joseph Stiglitz wrote forty-five pages. Baumol and Becker wrote six. The average is less than
fourteen pages, including the reprint of Keynes’s paper. Should we read anything into the short responses?

Keynes was a brilliant economist. As editor of the Economic Journal, he wrote brief responses to submissions that went to the central issue. His many essays and commentaries on current events, starting with the Economic Consequences of the Peace, are insightful and stimulating. His philosophical views seem, to me, to be misguided and wrong. Why reprint them now?

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References


The epigraph to the introduction of this book clarifies its rather obscure title—the “compass of society” is, according to Noël-Antoine Pluche writing in the mid-eighteenth century, the skillful merchant, esteemed by his countrymen for the resourcefulness he displays in procuring what they desire and what they need (ix, 100). This perspective upon the merchant represents, Clark argues, “a distinctively French conception of the modernizing and commercializing monarchy” (101) that did not long outlast the death of Louis XIV. Instead, what human desires and needs might be, and how they made their different contributions to a social and moral order, became an issue debated with vigor and quite varied arguments throughout the eighteenth century until they came together in the writings of Sièyes. This is one level of the book. Another is an implicit reevaluation of Tocqueville’s conception of the ancien régime, for Clark weaves together a close reading of contemporary writing with an extensive grasp of modern historical commentary upon the social, political, and economic forces that culminated in the French Revolution. This is therefore an ambitious project; and if, ultimately, it seeks to cover too much ground and do too much, at the very least the trajectory that he describes, reaching from the mid-seventeenth century to the early nineteenth, is very clearly the one appropriate to any understanding of, for example, quite how Mirabeau and Quesnay invented “physiocracy,” or into what kind of debates Smith stumbled on his visit to France—each of these being the more familiar perspectives on this material on the part of historians of economics.

The broad chronological scope allows Clark to show how the question of monarchies, commerce, welfare, and luxury emerged from the different experiences of Holland, Britain, and France in the later seventeenth century, laying emphasis, however, on the central role of Fénelon’s sequel to the Odyssey, Telemachus, a work which
offered a blueprint for the reform of monarchies and was directed against the policies of Louis XIV in general, and Colbert in particular. This critique of the role of luxury, and hence commerce, as a key factor in the moral corruption of monarch and people alike remained popular throughout the century, but was displaced by the publication of Montesquieu’s *Spirit of the Laws* in 1748, which provided both a new framework for the appraisal of constitutions and a comparative framework that could displace the usual practice of contrasting Holland with France, or France with Britain. Pluche’s conception of the merchant as the “compass of society” was articulated at about the same time, and so this immediately became an obsolete idea related to the commercial monarchy of the seventeenth century which, by the later 1740s, was the problem, not the solution. It is in this context that Clark introduces first the work of de Gournay and his circle, the emergence and subsequent development of physiocratic analysis (especially their conception of legal despotism), and also of course the writings and activity of Turgot. The commercial republicanism of Turgot in turn gives way to Siéyès’s arguments concerning representative government based upon a conception of social action, the interaction between human needs and the means that humans could muster to meet these needs if able freely to do so.

Stated in this way it is clear that Clark eschews “liberalism” as a category with any real bearing on this period, demonstrating for example the manner in which “manners” were closely connected to “wealth,” so that it remained a matter of dispute the degree to which the pursuit of wealth would inevitably undermine the very fabric upon which it depended. Clark resorts occasionally instead to more modern social science conceptions such as “trust,” “information,” and “social capital,” but such conceptions ring hollow. Midway through the book Clark offers another gloss on the title: Terray, Turgot’s predecessor as controller-general, compared the system of market prices to a compass “that gyrates wildly in response to hopes, fears, sales, purchases.” In place of this fluctuating compass Terray offered instead the image of a balance distributing fairly the legitimate interests of farmer and consumer. This image might have permitted Clark to build a link to contemporary ideas of political equilibrium, an avenue that he does not explore; but which does suggest how lightweight modern social science writing is when compared to that on commerce, liberty, and manners in the eighteenth century.

This book forcefully, bewilderingly, conveys the range of argument and writing relevant to an adequate understanding of the emergence, out of the French Revolution, of new conceptions of welfare, government, and liberty. It demonstrates quite decisively the primitive and very rudimentary nature of most accounts of the early development of political economy. That it ultimately overreaches its grasp is unsurprising; by opening the door upon all this Clark has shown quite clearly how much work there is yet to do if we are to understand how conceptions of social, political, and economic action formed and then came to be related in the way that they are today. And, in understanding how this came about, to fully understand for the first time the language that we often so casually and unreflectively employ.

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