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Book Reviews

Editor's Note: Guidelines for Selecting Books to Review

Occasionally, we receive questions regarding the selection of books reviewed in the *Journal of Economic Literature*. A statement of our guidelines for book selection might therefore be useful.

The general purpose of our book reviews is to help keep members of the American Economic Association informed of significant English-language publications in economics research. We also review significant books in related social sciences that might be of special interest to economists. On occasion, we review books that are written for the public at large if these books speak to issues that are of interest to economists. Finally, we review some reports or publications that have significant policy impact. Annotations are published for all books received. However, we receive many more books than we are able to review so choices must be made in selecting books for review.

We try to identify for review scholarly, well-researched books that embody serious and original research on a particular topic. We do not review textbooks. Other things being equal, we avoid volumes of collected papers such as festschriften and conference volumes. Often such volumes pose difficult problems for the reviewer who may find herself having to describe and evaluate many different contributions. Among such volumes, we prefer those on a single, well-defined theme that a typical reviewer may develop in his review.

We avoid volumes that collect previously published papers unless there is some material value added from bringing the papers together. Also, we refrain from reviewing second or revised editions unless the revisions of the original edition are really substantial.

Our policy is not to accept offers to review (and unsolicited reviews of) particular books. Coauthorship of reviews is not forbidden but it is unusual and we ask our invited reviewers to discuss with us first any changes in the authorship or assigned length of a review.

A General Economics and Teaching

Secrets of Economics Editors. Edited by Michael Szenberg and Lall Ramrattan. Forward by Robert M. Solow. Cambridge and London: MIT Press, 2014. Pp. xvii, 389. $35.00, cloth. ISBN 978–0–262–52546–6, pbk.

JEL 2014–0405

This volume is based on a request by the editors to current or former editors of over twenty journals to write short essays on their "experiences as an editor." That's pretty open-ended, so how the different authors responded varies tremendously. Most of them are rather chatty and autobiographical, often including a history of the journal and their involvement with it. Many discuss the circumstances that led them to accept an offer of editorship, with much retrospective ambivalence and even painful remembrance. Some of them venture into personal issues (e.g., what my spouse thought about my becoming an editor).

Some of the editors who contributed are older members of the profession, and their editors' terms often took place in the 1980s. For those readers (like this reviewer) who are of a certain age, reading the histories of the various economists involved, many of whom are no longer with us, brought back old memories of their personalities and reputations. For those interested in the history of our profession, there is some decent

material here for archival research. But this is pretty inside baseball, and I'm not sure anyone under thirty would find those discussions all that interesting.

Most of the chapters discuss, of course, the business of being an editor, and perhaps this is the source of the humorous title of the volume (there are no secrets revealed). Editorships are time-consuming jobs requiring daily difficult judgements, attempts to minimize Type I and II errors, and attempts to balance one's own personal preferences with those of the broader profession. There is much talk in the essays of the perennial problem of choice of appropriate referees—something that should, in fact, occupy a large fraction of the time devoted to any single manuscript—and authors' responses to referee reports ("The essential mystery of editing is why the reports I receive as an editor are so much better than the reports that I receive as an author," p. 59). A subject in most of the essays is how to get referees to respond in a timely fashion, and attempts to reduce decision times is a common theme in the chapters. And, of course, many are full of war stories, recounts of old battles, favorite author and referee tales, and the like. I got many chuckles out of these anecdotes, even the ones I had heard before but had forgotten (like the rejection letter, "We have received your manuscript '_____.' You will be pleased to learn that we find it both novel and interesting. Unfortunately, the part that is interesting is not novel, and the part that is novel is not interesting." p. 216).

Many of the essays also have "tips for authors," the kind of practical advice that most editors freely dispense whenever asked, or dispense even without being asked. For the more experienced members of the profession, this material will be old news, because they have already figured it out. But such advice is often useful for graduate students and young economists just starting off, and can be of value.[1] Many of the essays also have "tips for editors," in case some editor-of-the-future is reading their essay and wants to know what to do (although I'm not sure many economists have editorship as one of their conscious ambitions).

[1] I have indulged in this myself (Moffitt 2011).

My favorite essay was near the very end, written by Timothy Taylor, the profession's National Treasure who is the managing editor of the *Journal of Economic Perspectives*. Well-written, thoughtful, and full of useful perspectives—all of which is not surprising, for those of us who know him—the essay is a delightful read. It was also nice to hear some of his history as a graduate student at Stanford and why he chose not to become a professional research economist ("I ran across a paper . . . called 'I Just Ran Two Million Regressions.' . . . I thought to myself: 'That's one reason I wasn't suited for graduate school. Let *him* run the two million regressions.'" p. 338).

Substantively, however, I do not feel that the essays very often discussed what I regard as the major issues facing economic journals today. The one that is neglected the most, with the exception of the essay by John Pencavel, concerns the structure of the journals market in economics. As I see it, at least in the market composed of the leading journals, most journals have specialized in publishing papers with particular styles, subjects, or methodologies and this tendency has increased over time. This implies that, contrary to what many economists often assert, there is not as much competition in the market as might first appear. An author who is rejected at one good journal might not see a good alternative where her paper will be welcomed. The common statement by editors to authors that they will get a different random draw of referees at a different journal isn't really correct much of the time. One could argue that the specialization of leading journals is just a reflection of specialization within the discipline itself, with many groups in the profession no longer talking to one another, but I think it works the other way around as well—the preferences of journals for certain types of papers exerts an influence on the types of papers that get written and the types of research that get conducted.

Now, product differentiation is common in many markets other than that of economics journals. However, unlike most private markets, there is no real market test for academic journals the way there is for private firms, where profits provide the bottom-line signal, and profits reflect

consumer demand.[2] In academic research, even in economics where the criterion should be which research has the biggest contribution to understanding the economy and policy, the popularity of different types of research is heavily influenced by differing perceptions of what likely constitutes such a contribution. Indeed, the leading journals in economics are published by professional associations or individual economics departments and not by for-profit publishers. Add to this the barriers to entry created by the enormous advantage of incumbency in journals. The reputation of a journal, and hence the kinds of papers that get submitted to it, is a function of its past publication record, and this is why the leading journals in the profession are about the same as they were fifty years ago. While there are some notable examples of entry successes at the second rank (e.g., the *American Economic Journals*, the *Journal of the European Economic Association*, and *Quantitative Economics*), they usually require the backing of an important association that can subsidize them for long enough to establish a reputation and/or put leading figures in the profession in charge.[3]

A second issue that would have been nice to see more insights into is the discussion of reduction of decision times. Everyone agrees that reasonably short decision times are desirable, but simply saying that they should be as short as possible, as many of these essays do, ignores effects on the quality of referee reports and ignores the general equilibrium nature of the problem. In most cases, higher quality referees take longer to do their reports, hence editors will make higher quality decisions, on average, if they wait for those reports. Journals that insist on short decision times often lead editors to make decisions themselves on the basis of just one report, for example, which has to lead to inferior average decisions. The general equilibrium nature of the problem is that if all journals simultaneously reduce the time in which they force referees to do their reports, then rejected manuscripts will get sent to the next journal more rapidly. This will result in an increase in total, overall submissions within any given time frame. This creates an increased demand for referee reports and, if editors go back to the same pool of referees, and if those referees do not increase the total time they spend refereeing, the editors will experience a higher rate of refusals to review. Editor journals will consequently have to move down the quality distribution of referees to obtain reports in the new, shorter time frame. My perception is, indeed, that both the volume of submissions and of refusals-to-review have been rising at many journals, and this could be one reason for that trend.[4]

An issue that is touched upon in the volume is that of data availability and replication, a topic in which I have a long-standing interest.[5] The most direct discussion of the issue is provided in the essay by Dewald and Anderson on the famous replication experiment at the *Journal of Money, Credit, and Banking* (*JMCB*) in the 1980s. The editors began requesting the data and programs for all accepted papers, with an astonishing rate of authors either not keeping those data and programs or supplying them in unusable or incomplete form. Further, even for those papers where usable data and programs were furnished, the published results could not be replicated for a small but surprising fraction of the papers (Dewald, Thursby, and Anderson 1986).

The *JMCB* experiment was aimed solely at the narrow issue of replicability and whether

[2] This may be less true in the market for lower-ranked field journals, where private for-profit publishers appear to have found lucrative avenues for expansion, perhaps because of the inelastic demand curves of university libraries (Bergstrom 2001).

[3] Incumbency advantage does increase the social value of a journal, however, because the profession benefits from knowing, based on past knowledge, that a journal publishes good papers of a certain kind. The welfare effects of incumbency advantage, like those of network effects, are not completely clear.

[4] At the present time, not all journals have reduced their decision times. This means, further, that those journals that have not done so will experience even longer waits for reports, as referees do reports for the "fast" journals first.

[5] At my first chief editorship at the *Journal of Human Resources*, I attempted to create a forum for replication articles. My efforts were a dismal failure. At the *American Economic Review*, I was more successful, at least in encouraging simple data availability. Taking Ben Bernanke's initial efforts in 2004 as a starting point, I put enforcement procedures in place, codified the requirements, hired staff to check submitted materials for completeness, and established formal web posting of the materials. This resulted in a major jump in compliance.

published results contained errors. The larger and more important goal is testing the robustness of results to alternative assumptions and treatments of the data and reconciling the results of different papers that obtain different results from either the same or different data sets.[6] The *JMCB* was ahead of its time because more journals today request, and obtain, the data and programs for their published papers. It is much easier to do today than it was in the 1980s, when data were typically stored on nine-track magnetic tapes. But, despite this, there are very few papers published conducting this type of work despite the repeated attempts of various editors at different journals to encourage it. Part of the problem is that journals have no incentive to publish positive replications, and hence authors have no incentive to conduct them. Even when more substantive robustness checking is performed, journals prefer not to waste journal space on such papers compared to publishing articles with fresh new results. This would seem to be a case where the private incentives of both authors and journals are not well aligned with social value, and therefore where there should be a role of professional associations or the public sector (e.g., the National Science Foundation) to intervene.

The authors of the essays in this volume were not asked to address most of these larger issues. The material they did provide is often interesting and entertaining and worth a perusal on a leisurely Sunday afternoon.[7]

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[6] I generally call the entire enterprise the quest for the Three R's: replication, robustness, and reconciliation.

[7] I would like to thank Joe Harrington for comments on this review.

B History of Economic Thought, Methodology, and Heterodox Approaches

Documents Related to John Maynard Keynes, Institutionalism at Chicago & Frank H. Knight. Edited by Ross B. Emmett. Research in the History of Economic Thought and Methodology, vol. 31-B. Bingley, U.K.: Emerald; distributed by Turpin Distribution, Biggleswade, U.K., 2013. Pp. vii, 256. ISBN 978–1–78350–060–4.
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The second volume of this research annual contains some useful and provocative material for the historian of economic thought, particularly for exegetes of John Maynard Keynes and scholars of Frank H. Knight. The volume publishes four separate archival sources, each introduced by a helpful interpretive essay. It includes the original version of the lecture on "National Self-Sufficiency" that Keynes delivered in Dublin in 1933; a term paper that Arthur Bloomfield wrote on Thorstein Veblen in 1937 for Frank Knight's course on institutional economics; a previously unpublished essay on classical economics and economic method written by Knight, most likely shortly after 1927; and finally the correspondence between Knight and his undergraduate mentor and lifelong friend Frank Kershner that transpired between 1915 and 1951. Each piece, with accompanying introduction, stands alone. Casual observers of the title would be misled to think there is a connection drawn between Keynes and Knight in this volume, although the relationship between Knight and institutionalism is readily explored.

The version of Keynes's "National Self-Sufficiency" lecture republished here is longer than the one included in *The Collected Writings of John Maynard Keynes* (volume XXI). This version reproduces the handwritten manuscript of his lecture, and was itself published in 1933 in *Studies: An Irish Quarterly Review*. The lecture contains passages relevant to the Irish political situation that were omitted from more general versions that also appeared in print. In his introductory essay, Mark Nolan points out that this omitted material makes the original intent of the essay more transparent. Many interpreters of Keynes, on the right and the left, read the *Collected Works* version of "National

Self-Sufficiency" as a rejection of free trade. But Nolan argues persuasively that Keynes's Dublin lecture was politically motivated to forestall Irish protectionist policies and the pursuit of an "economic war" between Ireland and Britain, out of which the Irish leadership hoped to gain greater independence. Thus, Nolan urges that Keynes's sympathetic remarks towards national policy experiments should be understood to be tempered by Keynes's final message to his Irish audience that "[n]o one has the right to gamble with the resources of a people by going blindly into technical changes imperfectly understood" (Keynes, p. 53). This warning, along with other remarks critical of protectionism and indicative of the political context of the lecture, did not survive the edits in the more general and well-known versions of the essay.

Arthur Bloomfield's University of Chicago term paper on "Thorstein Veblen and his Analysis of Business Enterprise" is a clearly written, useful, and insightful introduction to Veblen's thought, particularly his belief that there is a distinction in modern capitalism between "business," meaning the pursuit of profit, and "industry," the task of efficiently making goods (Veblen is a predecessor of the contemporary critique of the "financialization" of capitalism). In his paper for Knight, Bloomfield opines "that Veblen's general conclusions as to the nature of Business Enterprise are substantially true" (Bloomfield, p. 109). Clearly there were open minds at the University of Chicago in 1937. In their introductory essay, Pier Francesco Asso and Luca Fiorito note the value of Bloomfield's paper, not only for its insightful reading of Veblen, but also for its display of the kind of education Chicago students received in the 1930s. "It . . . provides evidence on the role that Frank Knight, together with Viner and other senior economists, played in fostering pluralism in methods and the selection of topics among their most promising graduate students" (Asso and Fiorito, p. 58–59).

In his introduction to Knight's previously unpublished paper on "Institutional History and the Classical Economics," which was probably written during or immediately after 1927, Ross Emmett affirms that good social science, for Knight, requires "a pluralist approach, which combines the study of economics with a comparative history of a society's economic, social, and political institutions and ethical consideration of what humans do, and should, value" (Emmett, pp. 122–23). Knight's essay, however, is an argument to define economic theory as the analysis of how people pursue their interests subject to their constraints. Knight's pluralism does not apparently countenance a variety of economic methods. He maintains that classical economics did not develop an adequate theory of economic behavior and economic life, but instead provided only a theory of economic policy, or rather a theory about what policies not to follow (e.g., mercantilism and regulation of exchange). Good economic theory, for Knight, must be based on the presumption that "human beings have *known* desires which they *strive* to realize. Conduct is viewed as a problem of the *administration of means* in the achievement of ends which are not problematic" (Knight, p. 145). So, while Knight plainly believes that discussion of ethics, values, and institutions is important, these discussions are not part of economic theory properly conceived.

The last part of the volume publishes the content of a series of letters between Knight and Kershner, a preacher in Disciples of Christ churches, academic administrator, and undergraduate instructor of Knight. The letters are pertinent to historians of economic thought for their discussion of the relationship between religion and social philosophy. They are also of potential interest to Knight scholars for displaying his lifelong struggle with Christianity and his ultimate skepticism regarding the compatibility of Christian ethics with "the impersonality of the market system" (Emmett, p. 164). As Knight puts it in a 1942 letter, "in virtually everything it stands for in the way of a social program, the Church is a sentimental ass" (Knight, p. 234). These letters contribute to understanding why he reaches this conclusion.

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Advanced Introduction to the Austrian School of Economics. By Randall G. Holcombe. Elgar Advanced Introductions. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2014. Pp. xi, 126. $99.95, cloth; $27.95, pbk. ISBN 978–1–78195–573–4, cloth; 978–1–78195–574–1, pbk.; 978–1–78195–575–8, e-book. *JEL 2014–1141*

The title phrase "Advanced Introduction" suggests that the intended readers of this slim volume are those who are well-grounded in economics at the undergraduate level—and even at the graduate level—but are not familiar with the Austrian school of economics. Edward Elgar offers a series of advanced introductions in various areas of the social sciences. This genre seems particularly appropriate for the economics that emerged as Carl Menger's *Principles of Economics* (1950) and then took root during his three-decade teaching career (1873–1903) at the University of Vienna.

During the decades before and just after the turn of the century, Eugen von Böhm-Bawerk developed a theory of capital that he took to be implicit in Menger's *Principles*. The time element in the economy's production processes took the form of "maturity classes" of capital goods—the more developed the economy, the greater the number of maturity classes. Ludwig von Mises, in his *Theory of Money and Credit* (1953), combined Austrian capital theory with the Swedish distinction between bank rates and natural rates of interest to produce a theory of business cycles—or, more pointedly, a theory of the unsustainable credit-induced boom. Friedrich A. Hayek, in his *Prices and Production* (1935), transformed Böhm-Bawerk's "maturity classes" into a sequence of "stages of production" and then developed Mises's theory to demonstrate that the seeds of the cyclical downturn are sown in the form of systemic credit-market distortions that give rise to a protracted but ultimately unsustainable boom. This reckoning, which pitted the Austrians' identification of boom-time distortions against the Keynesians' near-exclusive attention to post-boom uncertainties and demand deficiencies, marked the heyday of the Austrian school—and was the basis for Hayek's being awarded a Nobel Prize in 1974. To contrast the methods of these two schools of thought, it can be said that the Austrian focus was on the key causal element that foretold the cyclical downturn, while the Keynesian focus was on remediation of the downturn's most salient negative features.

When Keynes and his policy prescriptions triumphed over Hayek (arguably on political grounds, rather than on theoretical grounds), the Austrian theory went into eclipse. And capital theory (in the sense of a modifiable temporal sequence of stages of production) dropped out of macroeconomic theorizing. The Keynesian avalanche, as it is sometimes called, buried the Austrian theory for decades—and for longer in the halls of academe than in the more earthbound financial press. This is the circumstance that well justifies an advanced introduction to the Austrian school.

Holcombe's treatment of the Austrian views on money, banking and business cycles comes late in his exposition—in section 4 of the book's five sections. However, the earlier sections lay the groundwork that gives credibility to the Austrians' macroeconomic theorizing. The market is conceived as a discovery process in which, ultimately, both the value of consumption goods and their costs of production are understood to be "subjective." Menger challenged head-on the classical cost-of-production theory of value on the basis of a thorough-going subjectivist perspective. It is the anticipated value of the consumable outputs that determines the value of the inputs. Even the Marshallian supply-and-demand reckonings of markets for particular goods are ripe for an Austrian reworking. The supply curves, which reflect "costs," are not, in the final analysis, objective but rather are reflections of the subjective values of other consumable goods that could be produced with those inputs.

In the microeconomic sphere, Holcombe gives credibility to the Austrian perspective by, for example, comparing standard textbook treatments of product differentiation (using static equilibrium analysis) with the Austrian treatment (based on the conception of the market as a process). The textbook treatments focus on the higher-than-minimum average total costs that product differentiation entails. It is as if the optimal features of planned undifferentiated products are somehow already known before any actual production takes place. The Austrian view is that those higher production costs are not deadweight losses, but rather are the necessary costs of determining just what product characteristics are most highly valued by consumers. Holcombe associates product differentiation with progress rather than with excessive costs.

The general focus for Menger and modern Austrians is on the entrepreneurial give-and-take of the market process, rather than on some eventual equilibrium. Entrepreneurial decisions are based as much, if not more, on anticipated relative-price changes than on some measure of overall inflation. This perspective does not mesh well with some modern-day trends in academic economics. Stochastic dynamic general equilibrium analysis crowds the warm-blooded entrepreneurs out of the picture. And econometric studies using *pro forma* reduced-form equations, while possibly yielding publishable correlations, must, by construction, remain silent about the particular forces that are actually driving—or possibly distorting—the economy.

The dynamics of academic research have led to an obvious disconnect between the views expressed by the academy and those expressed in the more enlightened reaches of the financial press. On macroeconomic issues as they pertained to the last several boom–bust episodes and especially to the 2007–09 recession and anemic recovery, the financial press is much more inclined to adopt an Austrianesque view. Entrepreneurial judgments colored by policy-induced low interest rates gave rise to an unsustainable boom, most visibly in housing markets; the misallocation of resources during the boom eventually led to liquidations of unfinished projects, and hence a rise in idled capital and its labor complement; and perverse policy (demand management) aimed at rekindling the ill-fated boom forestalled the recovery. This understanding is fully reflective of the Mises–Hayek theory of the business cycle.

Holcombe's sympathetic "advanced introduction" may well serve as an enticing bridge between the thinking of the modern mainstream and the insights of the Austrian school.

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C Mathematical and Quantitative Methods

The Index Number Problem: Construction Theorems. By Sydney Afriat. Oxford and New York: Oxford University Press, 2014. Pp. xii, 220. $95.00. ISBN 978–0–19–967058–1.

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This happens to be the second book that is entitled *The Index Number Problem*. The first was written by Gerhard Stuvel, published in 1989, and concerns statistical price and quantity indices. The second, now under review, is concerned with economic price and quantity indices. Both authors claim to have solved "the index number problem" (actually, the full title of Stuvel's book was *The Index–Number Problem and its Solution*). Unfortunately, both claims are false.

The index–number problem can be formulated as the problem of decomposing a value ratio, usually comparing a certain period to an earlier period, into a price index and a quantity index. Both indices should exhibit appropriate behavior; that is, formulated mathematically, they should obey certain functional equations. As far as statistical indices are concerned, there appear to be multiple solutions, but for every solution there is at least one important functional equation that is not obeyed. The problem, however, is not so much that, until now, economists and statisticians have been unable to discover the ultimate solution; the sad fact of life is that one can mathematically prove that such a solution is impossible. All this has been documented extensively by Balk (2008).

Turning to economic indices, and restricting ourselves to consumer behavior, the problem is stated in terms of cost-of-living and standard-of-living indices. There is a solution here, but only in the (very) restrictive case where consumer preferences can be represented by a linearly homogeneous utility function. All this is well-known, and the young Afriat has contributed much to our understanding of this situation. For a three-page summary I might refer to Balk (2010). When there are only two data points, one obtains the familiar result that the cost-of-living index is included in the Laspeyres–Paasche interval. When more data points are available—the most interesting case being an international comparison—then this interval can be reduced by employing the

transitivity of the cost-of-living index (see Balk 2009).

Thus, Afriat's "solution" or "new formula"—"Any point in the Paasche–Laspeyres interval, if any"—holds only if one is prepared to assume that a linearly homogeneous utility function is realistic. But even then, there are as many of those intervals as there are consumers. One would have wished that Afriat had provided some guidelines for making a reasoned choice here.

The book under review goes back to a special session at the 2011 conference of the Royal Economic Society, devoted to "Foundations of Revealed Preference." The papers presented in this session, by Sydney Afriat, Erwin Diewert, and Hal Varian, together with an introduction by Frederic Vermeulen, were published in a special issue (numbered 560) of the *Economic Journal* 122 (2012). Afriat's article, entitled "Afriat's Theorem and the Index Number Problem," essentially corresponds to section I of part I of the book. The remainder of part I expands the basic theory, all in the classic Afriat style. Part II contains revised versions of a number of hitherto unpublished manuscripts and informally published articles, spanning the years 1963–99. Thus, this book can be considered as a sort of collected works. Though much of the material is pretty out of date, there is, I think, still much to enjoy for both theorists and historians who have sufficient patience to work their way through the rather idiosyncratically formulated texts.

As indicated, this is a book about theory. The basic variables are prices and quantities. In practice, however, prices and quantities are usually not available. Instead, for example in the consumer case, at a certain level of commodity aggregation expenditures and price indices based on samples of prices, outlets, and time periods are available. Theory then is applied not to prices and quantities, but to price index numbers and deflated expenditures. One of the consequences is that most studies of substitution effects appear to be restricted to so-called upper level substitution; that is, substitution between expenditure groups. In practice, however, lower level substitution, that is, substitution between brands, types, outlets, and timing of purchase, appears to be a much more spectacular and difficult-to-handle problem.

Put otherwise, published statistical data contains already dozens of decisions, some reflected, some unreflected. To appreciate all this, the reader is advised to consult the manuals on consumer price indices, producer price indices, export and import price indices, and residential property price indices, which have been published over the last ten years by a number of international organizations and can be downloaded from some of their websites (Eurostat, IMF, United Nations ECE, ILO, OECD, World Bank). These manuals serve to show that and how official statistical practice is firmly anchored in economic theory.

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D Microeconomics

The Economics of Conflict: Theory and Empirical Evidence. Edited by Karl Wärneryd. CESifo Seminar series. Cambridge and London: MIT Press, 2014. Pp. xviii, 285. $40.00. ISBN 978–0–262–02689–5. *JEL 2014–0816*

Economics studies allocation of limited resources to people with unlimited wants. Many allocations mechanisms, such as price posting, bargaining and auctions, allow the use of prices. Some others, such as matching and contests, typically do not (allocation of school slots and kidney transplants among people, medical interns among hospitals, allocation of parliament seats, contested territories among countries, a

promotion among some employees). While neither contests nor matching typically allow prices, the former involves noncooperative mechanisms, while the latter mostly relies on some rules or algorithms that may have some desirable normative properties. Consequently, while some allocation mechanisms such as bargaining, contests, and matching have extensive subliteratures using (normative) axiomatic approach, some, such as auctions and price posting, never needed any axiomatic analysis.

The analysis of wars and contests over territory and tournaments of all kinds in real life have the contest theory at the center stage as a prominent way of modeling such conflict situations. However, real life is replete with hybrid mechanisms as well, some of which may involve contests and other allocation mechanisms. Given their army and other strengths, two countries that are to fight over a territory may eventually end up bargaining over that territory (see Anbarci, Skaperdas, and Syropoulos 2002). All-pay auctions are at the boundary of auction theory and contests (see Baye, Kovenock, and de Vries 1996, for a full characterization of the complete-information case). In some elementary schools, the best student of the week gets to take the pet of the classroom home during the weekend; along those lines, hypothetically, one can imagine students in a university being assigned their spots in the hierarchy to choose dorm rooms according to their GPAs, which implicitly has a contest behind it, since many grades are curved by instructors.

Thus, using several dimensions, papers in contest theory can be axiomatic or noncooperative, theoretical or empirical, without involving any prices, and may have some overlap with other allocation mechanisms at times. See Corchón (2007) for a recent comprehensive survey of the theory of contests.

The papers in this book, though their choice by the editor Karl Wärneryd is naturally subjective, also reflect some of the above-mentioned diversity of contest theory currently, as well as its relatively recent direction to some extent. The first four papers are theoretical and the remaining five are predominantly empirical, the latter focusing on civil war and insurgency rather than on international conflict.

Konrad's chapter on "Alliance Formation in Conflicts" tries to solve the alliance puzzle, namely why alliances even form, given that they weaken their members compared to their stand-alone positions given the free-rider and hold-up problems. Konrad identifies effects and environments that potentially more than offset the two disadvantages of alliances, which range from effort supermodularity of, and information transfers between, members to tightness of budget constraints, internal conflict threats, multiple fronts, and in-group favoritism versus out-group spite.

Cubel's "Conflict under Fiscal Federalism" shows that the degree of costly political conflict between regions depends on how much regional budgets can contribute to public goods, full proportional equalization being immune to conflict with full public goods, while conflict being more probable with lesser public goods.

Fiaschi's paper on "Dynamic Model of Growth and Conflict over Resources" establishes a link between growth and conflict over relatively abundant natural resources, where the conflict can lead to stagnation with an initially low per capita income, a high population growth but low life expectancy, high social fractionalization and weak institutions coupled with low investment rates in the presence of high depletion of natural resources.

Jia and Liang's "Decentralization and Stability of Government" examines a Machiavelli conjecture on decentralization and stability of regimes, using a sequential contest model, with the possibility of bargaining. It is harder to defeat a centralized government, but easier to hold on to power afterward, while the opposite holds for a decentralized government.

In Caruso, Costa, and Ricciuti's paper on "The Probability of Military Rule in Africa, 1970 to 2007," income, ethnic fractionalization, and lagged external threats explain the probability of military coups in Africa.

Evia, Laserna, and Skaperdas's "Sociopolitical Conflict and Economic Performance in Bolivia" empirically studies externally induced growth versus internal positive investment-induced growth and their respective positive and negative correlations with conflict in Bolivia by also utilizing a theoretical model along those lines.

Bove and Elia, in their chapter on "Occupational Choices and Insurgency in Afghanistan's Provinces," examine occupational opportunities affected by antigovernment (or illegal) activities in Afghanistan, where conflict causes an increase in supply of but a decrease in demand for opium, causing lower prices.

Stroup and Zissimos's paper "Social Unrest in the Wake of IMF Structural Adjustment Programs" (SAP), uses cross-country data to show that SAP brings trade liberalization with a decrease in government unemployment, leading to social conflict (whereas in the absence of SAP, the landowning elites would allow government employment to swell as a concession to the masses).

Cassar, Grosjean, and Whitt's paper, "Social Preferences of Ex-combatants: Survey and Experimental Evidence from Postwar Tajikistan," which is the last chapter of the book, has a major overlap with Cassar, Grosjean, and Whitt (2013)—with several paragraphs in both papers being even verbatim without any quotation marks in either—and the readers who have access to the latter paper should not expect to take away anything else significant from this chapter.

The theoretical chapters provide a fresh breath and nice balance in terms of how related they are to the standard tools of contest theory and its current applications, as well as how they can potentially advance them. The empirical chapters cover a wide variety of real-world examples and applications. Thus, although its chapters would be much more accessible to literati in political economy (i.e., to advanced Ph.D. students and academics who are equipped with relatively advanced theoretical and empirical tools), there is something for everyone in the book, certainly for both theorists and empirical researchers in both economics and political science.[1]

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[1] I am grateful to Nick Feltovich and Stergios Skaperdas for their invaluable comments and suggestions on this review and to Hao Jia with whom I had discussions on the book, contest theory, and this review.

G Financial Economics

Insurance and Behavioral Economics: Improving Decisions in the Most Misunderstood Industry. By Howard C. Kunreuther, Mark V. Pauly, and Stacey McMorrow. Cambridge and New York: Cambridge University Press, 2013. Pp. xii, 329. $29.99, paper. ISBN 978–0–521–84572–4, cloth; 978–0–521–60826–8, pbk.

JEL 2014–0110

Insurance and Behavioral Economics is a thorough exploration of the workings of insurance markets through the lens of behavioral economics. It offers a practical viewpoint from authors well-versed in both the institutional details of these markets and the theoretical models used to describe them. Citing a wide range of examples across numerous insurance markets, it documents patterns of behavior inconsistent with the canonical expected utility model of insurance, and more consistent with behavioral models.

The book begins with a discussion of the canonical expected utility model of insurance. Along the way, it notes that some insurance markets, such as automobile collision insurance, are seemingly well described by the canonical model. Most car owners have collision coverage and purchase from a competitive market offering prices with relatively low markups. The book then discusses a range of alternative models, some closer to the canonical model (e.g., asymmetric information) and others further away (e.g., prospect theory). It explores the predictive power of these theories, to the extent that they help us understand behavior in a range of insurance settings

On the demand side, the book argues that individual purchase decisions are often consistent with models of inertia, loss aversion, and high search costs, potentially reflecting complexity

and imperfect information. The purchase of flight insurance, rental car insurance, and cancer insurance are shown to be difficult to rationalize within the expected-utility framework, yet consistent with some of these alternative models. In longer-term markets like life insurance, the book illustrates how individuals may have a difficult time making accurate rate-of-return calculations, which can lead to a demand of whole life insurance over term life insurance.

On the supply side, one might have thought the discipline of the market would prevent many behavioral anomalies. But here, the examples are even more striking: Florida insurers stop selling property insurance after major hurricanes, California insurers stop selling property insurance after major earthquakes, and even the market for insurance against terrorism evaporated in the wake of 9/11. Not only are these examples inconsistent with standard models, but they also led to significant government involvement in the insurance marketplace. Hence, understanding these anomalies is of first-order importance for both policymakers and academics.

In its analysis of the supply-side anomalies, the book is exceptional and highlights the deep institutional knowledge of the authors. It discusses real-world issues, such as agency problems between owners (and reinsurers) and managers, ambiguity aversion by managers, and probability biases arising from overweighting recent events that are more salient in memory. Although it provides convincing evidence that the decisions of insurers to stop selling insurance after large losses from earthquakes, hurricanes, and terrorism events were irrational, it also raises the important question: Why doesn't another firm enter the market and profit from the biases of the existing firms? This is given a plausible answer: these large-scale property loss markets have relatively high entry costs and, on average, deliver average or less than average return on assets in the long run. This prevents short-run market entrants from correcting the behavioral bias of the relatively few market participants after these extreme events.

The book concludes with a discussion of strategies and recommendations to deal with these anomalies. On the demand side, it discusses a range of issues such as the importance of providing information to purchasers to help reduce bias. On the supply side, it recommends greater adoption of information forecasting tools to deal with probability biases. The book also discusses at length the importance of promoting multiyear contracts, and it provides the concrete suggestion that National Flood Insurance Program policies should be multiyear policies tied to the property, as opposed to single-year policies tied to the individual.

The clear contribution of this book is to conduct a sweeping overview of a wide range of markets and explore the extent to which behavioral and canonical models fit observed behavior. In doing so, the book does a tremendous service to the literature by synthesizing behavior in a wide range of settings. However, by attempting such a broad endeavor in one book, it opens itself up to some potential criticism that readers of the book should keep in mind.

First, the book attempts to construct a binary classification of markets into those best described by behavioral and rational models. Because models of rational and irrational decision making are at the individual level, the quest to classify behavior at the level of the market leads to some seemingly dubious classifications. For example, the health insurance market would seemingly be classified as best fitting the rational model, and readers searching for detailed descriptions of health insurance market anomalies might be disappointed. This is arguably not because of lack of evidence: Handel (2013) and Bhargava, Loewenstein, and Sydnor (2014) show that individuals make strictly dominated health insurance choices.

Second, the book largely ignores some of the anomalous behaviors by insurers that are at the center of policy debates, such as rejections and rescission. For example, in health insurance, one in seven applications to the four largest nongroup insurers were rejected between 2008 and 2010; insurers chose not to offer applicants a policy at any price.[1] The lack of discussion of this

[1] See the congressional investigation by the Committee on Energy and Commerce, which requested and received this information from Aetna, Humana, UnitedHealth Group, and Well-Point. The congressional report was released on October 12, 2010; the report used to be made available to the public at http://energycommerce.house.gov/Press_111/20101012/Memo.Pre-existing.Condition.Denials.Individual.Market.2010.10.12.pdf, but has since been taken down after the change in party control of the House.

phenomenon is surprising, because many of the supply-side anomalies discussed in the book are cases where insurers chose to not offer coverage. The book would have perhaps benefited from a discussion of these anomalies.[2]

Overall, this book is a very nice contribution with superb insight into the workings of many insurance markets through the lens of behavioral economics. I highly recommended it for anyone interested in how insurance markets work in the real world.

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I Health, Education, and Welfare

Legacies of the War on Poverty. Edited by Martha J. Bailey and Sheldon Danziger. National Poverty Center Series on Poverty and Public Policy. New York: Russell Sage Foundation, 2013. Pp. xii, 309. $39.95, paper. ISBN 978–0–87154–007–2, pbk.; 978–1–61044–806–2, e-book. *JEL 2014–0155*

Legacies of the War on Poverty is an ambitious examination of President Lyndon Johnson's War on Poverty programs and their long-run economic and policy impacts. The book is organized by antipoverty initiative, with four chapters devoted to education and workforce development, three to safety net programs including housing, and two to health care. More cohesive than many edited volumes, the book weaves literature review and new policy analysis into an accessible and compelling historical narrative.

The book's exploration elucidates several key themes. First, the War on Poverty was inextricably linked to the Civil Rights struggles of the time. Redistribution and human-capital investment targeted at the poor population disproportionately benefited Southern blacks. Further, program design ensured that blacks had access to the programs. As noted in the chapter by Chloe Gibbs, Jens Ludwig, and Douglas Miller, civil rights concerns likely motivated the decision for the federal government to directly support Head Start providers, rather than running funding through the states, for example. Similarly, as discussed in the chapter by Bridget Terry Long on higher education, the focus of massive growth in federal support was grants and loans for individual students, rather than transfers to states or universities.

In addition, though ending the unfair treatment of the impoverished black population within Southern states was not the primary stated goal of the program, the War on Poverty indirectly increased federal pressure to desegregate by making funding contingent on compliance with the Civil Rights Act of 1964. One example is illustrated in the chapter on K–12 education by Sarah Reber and Elizabeth Cascio. Title I of the Elementary and Secondary Act of 1965 provided federal funds to support academic programs for poor children, thereby creating a program that remains the largest federal contribution to K–12 education. Title I was historic not only in the size of the federal involvement in education, but also in that it provided more funds to poorer districts and thereby helped equalize spending across districts. Further, because Title I funds were contingent on school desegregation, the program represented a new era in federal enforcement of civil rights. In their chapter on housing, Edgar O. Olsen and Jens Ludwig make a similar (but more difficult) argument for Johnson's housing initiatives. Katherine Swartz also notes that federal pressure from Medicare dollars allowed for the surprisingly rapid desegregation of hospitals.

[2] Rejections are also common in other health-related nongroup insurance markets, such as life, long term care, and disability insurance (Hendren 2013). Readers might wish to draw an analogy from rejections to the decision of insurers to stop selling insurance after earthquakes or hurricanes. However, the model provided in the book to explain the decision of insurers to stop selling insurance relies on correlated risk and large aggregate losses, so that providing insurance changes the aggregate balance sheet of the insurer. While this fits the risk profile of property and casualty risk from hurricanes and earthquakes, it seemingly does not fit the more idiosyncratic risk profile in health-related insurance markets.

A related point is that the War on Poverty significantly (and perhaps permanently) altered the balance between the state and federal roles in redistribution. Federal human capital, safety net, and health programs facilitated redistribution across states and shrank differentials in antipoverty efforts across states. Indeed, it was exactly this expansion in federal power that facilitated the desegregation in otherwise recalcitrant Southern states. But, as illustrated by the Title I example, it also ushered in an era of contingent federal spending that exerted federal control more broadly in education policy (No Child Left Behind, for example) and beyond (minimum legal drinking ages tied to highway funds, for example). In this way, the War on Poverty precipitated a centralization of social policy to the federal level.

The War on Poverty created a federal mandate for major redistributive and social insurance programs that form the core of the current American safety net. The chapter by Jane Waldfogel on the safety net for families highlights the development of nutrition programs and notes Johnson's aversion to cash welfare. The introduction of Medicare and Medicaid—now representing more than a fifth of the federal budget—are also a key part of Johnson's legacy. Thus, War on Poverty initiatives, including the failure to significantly expand cash welfare as part of the War on Poverty program, can be seen as antecedents to the current structure of the federal safety net. The safety net today is characterized by heavy reliance on in-kind assistance like food and health insurance and almost no cash support for able-bodied adults. A similar foreshadowing of Section 8 vouchers can be seen in Johnson's preference for subsidizing housing in the private market, as noted in the chapter on housing by Edgar O. Olsen and Jens Ludwig.

In the editors' introduction, they note that the War on Poverty is often judged a failure. Economists at the time, unaware of the upcoming economic transformation that would threaten middle-class manufacturing jobs and generate rising inequality, were optimistic that poverty could be eliminated within one generation. Clearly, despite substantial real per capita income growth, poverty persists.

There are challenges to identifying the causal effect of a large set of federal programs implemented around the same time. Indeed, much of the evidence amassed in the book stems from studies of policies similar to individual War on Poverty programs, rather than the War on Poverty itself. There are measurable, and perhaps unsurprising, short-term poverty reductions from expansions of Social Security and the introduction of the Food Stamp program. It is harder to measure benefits from the "teach a man to fish" elements of the War on Poverty. Nevertheless, although the test score gains to Head Start appear to fade by early elementary school, evidence suggests there are long-run benefits to adults that may be mediated by pathways other than academic success.

There are also less encouraging results. For instance, despite a rapid tenfold expansion in grants and loans for higher education, the gaps in college attendance by race and family income persist. In her chapter, Long argues that this disappointing reality may not reflect a failure of the War on Poverty higher education programs, but instead to an inadequate commitment to them in recent decades, which allowed support to lag far behind the rapidly growing costs of college. Similarly, large public housing projects are generally deemed a failure because concentrated poverty leads to undesirable social outcomes. Nevertheless, Olsen and Ludwig note that these programs do alleviate financial constraints for the roughly 10 percent of families that participate in them.

Perhaps surprisingly for a book edited and authored by economists, the most powerful legacies of the War on Poverty are shown to be social and political. It hastened desegregation and equality of opportunity, and created a federal mandate for major redistributive and social insurance programs that form the core of the current American safety net. This book is a must-read for scholars and students wishing to understand poverty policy in the United States today and what can be done to reduce poverty in the future.

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The Rise and Decline of Faculty Governance: Professionalization and the Modern American University. By Larry G. Gerber. Baltimore: Johns Hopkins University Press, 2014. Pp. x, 250. $59.95, cloth; $29.95, paper; $29.95,

e-book. ISBN 978–1–4214–1462–7, cloth; 978–1–4214–1463–8, pbk.; 978–1–4214–1464–5, e-book. *JEL 2014–1283*

The Rise and Decline of Faculty Governance describes the evolution of modern higher education institutions since 1876 as a story about the gradual sharing of decision making between faculty, administrators, and governing boards shaped within the context of ever-changing social and economic forces.

The book's thesis is that the rise of America's colleges and universities to international prominence since World War II cannot be separated from the evolution and strengthening of three critical organizational elements for research universities, four-year liberal arts colleges, and their counterparts in four-year engineering institutes. Those prongs are institutional commitments to shared governance between the faculty, administration, and governing boards; strong protections for academic freedom; and increasingly professionalized faculties. A faculty's claim to expertise provides the basis for its control over curricular and academic matters, as well as faculty appointments and dismissals. Gerber goes to some length to document the uneven growth in these three interacting foundations for higher education governance since 1876. He includes discussions about the struggles for expanding these principles to governance in two-year colleges and for-profit colleges and universities.

Present-day critics of higher education contend, in part, that colleges and universities need governance reforms that emphasize incentive structures in order to increase efficiency, control costs, and better satisfy student preferences for courses and amenities. Distance education and more attention to offerings promoting job market skills are examples where incentives might change current curricular structures. Responsibility Centered Management (at my home institution) is one example. It offers Responsibility Centers (which may or may not be departments) financial incentives to improve the matching of student demand and faculty supply of classes compared to the system in place prior to adoption of that business model.

Gerber sees these business management models as threats to the hard-won acceptance of shared governance over the last century. For example, he argues that before the rise of business models since the middle 1970s, universities and colleges could and did consider broader goals, such as preparing students to be informed citizens and allocating institutional resources, taking into account some broad notion of common good. Gerber does not address why the market models must seemingly fail at those broader tasks. For example, he does not discuss the impact of business models on general education curriculums present in many traditional four-year colleges and research institutions. General education is usually determined by an institution's faculty. It is not a sure outcome that business models destroy the cohesiveness of a general-education curriculum. Gerber's evidence against the business model includes the observation that humanities faculties have shrunk since the middle 1970s and contingent faculty headcounts have grown rapidly in that period.

The current rise in the market model's importance in traditional universities and colleges is not an entirely new tension in higher education. Gerber points out that business models have been under discussion at least since the early 1900s. Early forms of the discussion concern, in part, the appropriateness of Taylorism—or the application of scientific management principles—to obtaining efficiencies in the achievement of educational objectives.

The interwar years saw important expansions in academic freedom and shared governance and a growing recognition of the professionalization of the faculty. Gerber documents the increasing number of Ph.D.-trained faculty taking tenure and tenure-track positions, as well as growth in the numbers of earned doctorates in American universities instead of European ones. This manifestation of a growing professionalized faculty is seen by Gerber as the basis for faculty empowerment over curricular matters and the attendant necessity for guarantees of academic freedom.

Gerber's historical sweep includes the founding of the American Association of University Professors (AAUP) in 1915 and its continuous development of principles for governing universities and promoting academic freedom. He also discusses the role played by professional associations such as the American Economic Association (founded in the 1880s) in pushing forward the

importance of academic freedom as a means to improve the educational and research missions in universities. Gerber recalls (p. 46) Richard T. Ely's argument for economists as the most knowledgeable experts about the economy in asserting the need for protecting academic freedom. Ely had faced dismissal charges at the University of Wisconsin for taking controversial positions in the 1890s, and this played a role in shaping his arguments for the right to speak out in public policy debates.

The so-called "Golden Age" of American higher education receives extended treatment, as Gerber walks his readers through the postwar growth of U.S. higher education in combination with unprecedented growth in the economy and the flood of G.I. Bill–funded students seeking college degrees following World War II. This Golden Age also saw the first stirrings of faculty unionization as one means to achieve more faculty input to institutional governance, as well as obtain better working conditions and pay. Changes in the determination of faculty voting rights also evolved in this era. Extension of the vote from full professors to untenured, contingent, and in some cases students, came under consideration.

The Golden Age ends with stagflation starting in the mid-1970s. Various cases illustrate the subsequent slowing of union growth and other retrenchments in shared governance principles as market or business models come to the fore in the more resource-constrained post–Golden Age environment. The rapid expansion of for-profit institutions in this new period contributes to Gerber's sense that the faculty's professionalism is in decline, at least in some quarters of the industry. Contingent faculty, whether full- or part-time, are seen as another threat to the professionalization prong upholding the shared governance model. Gerber sees another problem in the use of nontenure-track faculty who are employed to teach courses based on preselected materials not controlled by the faculty directly, but by administrators instead, as a potential threat to shared governance principles. It is important to note that this problem cuts across for-profit and not-for-profit institutions.

Gerber reports and critiques a series of surveys, mostly conducted by the AAUP, over many decades to provide a sense in how the faculty, or at least some faculty, see the rise of their capacity to consult the administration on budgetary matters, appointments of deans, chairs, and heads, as well as control outright the curriculum. These surveys are used to buttress Gerber's argument (p. 164) that the ideal of shared governance is still alive. Although his readers might also conclude from his book that this ideal is on life support.

The book's overall conclusion is that the shared governance model is necessary for the rise of the American system of higher education and the esteem in which it is held by the international community. He argues for a reinvigoration of that system to counter the trend towards business models as the preferred mode of governance and resource allocation in higher education.

The audience for this book clearly includes faculty interested in governance matters, along with deans, department chairs and heads, as well as individuals serving on governing boards. I certainly wish I could have read it before I went into administration! New faculty can also profit by reading this book. It is easy to take for granted institutions protecting academic freedom. Newly appointed faculty colleagues might be interested to know that these protections did not always exist, nor were they necessarily easily obtained by faculties at large. The threats to faculty governance, potentially to academic freedom, and the professionalization of the faculty, are all issues that confront the newest generation of faculty. They would be well served to gain a background in these matters by reading Gerber's book.

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J Labor and Demographic Economics

Trade Unions in Western Europe: Hard Times, Hard Choices. By Rebecca Gumbrell-McCormick and Richard Hyman. Oxford and New York: Oxford University Press, 2013. Pp. xx, 242. ISBN 978–0–19–964441–4. *JEL 2014–0581*

Trade Unions in Western Europe: Hard Times, Hard Choices explores the experiences of labor unions in ten European countries—Denmark and Sweden representing the Nordic case; Germany, Austria, the Netherlands, and Belgium representing the center of West European

capitalism; France and Italy representing southern Europe; and Great Britain and Ireland representing European liberal market economies—in a socio-politico-economic environment that has created hard times for organized labor over the past couple of decades. Following the traditions of European industrial relations scholarship, the full breadth of labor union relationships are emphasized—not only the traditional economic relationship with employers that economists study, but also internal relationships with union members and potential members, external relationships with policymakers in the political arena, and participation in civil society.

Rather than being organized country-by-country, the book is organized thematically, with key chapters focusing on renewing power resources through recruitment and mobilization; restructuring unions through mergers and other changes; bargaining in times of adversity; battling for ideas and alliances in national political systems; and seeking economic and political influence transnationally, especially within the European Union. Moreover, the book does not focus on specific sectors, but ambitiously draws examples and patterns from across the sectors and unions of the countries studied. There are few scholars who could be successful in this. But the subject matter is clearly in expert hands. Richard Hyman, Emeritus Professor of Industrial Relations at the London School of Economics, is one of the most influential British industrial relations scholars of his generation, and author of the important book *Understanding European Trade Unionism: Between Market, Class and Society* (2001), while Rebecca Gumbrell-McCormick is a respected comparative industrial relations scholar and former international union official.

The challenge for the reader is trying to digest the incredible breadth of knowledge that Gumbrell-McCormick and Hyman present. Their command of such a diverse set of countries across the full range of union roles in society is remarkable. Some economists might worry about Ronald Coase's (1984, p. 230) critique of the old institutionalists' "mass of descriptive material waiting for a theory, or a fire." This would be a mistake. There are many insights and lessons relating to the continued importance of national-level institutions, the persistence of history and other path-dependent factors, and the prospects for labor union revitalization, to name just a few. Underlying the book is also a rich model of union power that encompasses multiple elements—structural, associational, organizational, institutional, moral, collaborative, and strategic (pp. 30–31). This goes far beyond the conceptualization of union power typically embedded in economic models, and should prompt rethinking of this approach. The range of examples provided throughout the book, then, should be seen as a rich presentation of the importance of context that shapes power and provides the opportunities for and constraints on what unions are able to do.

By the authors' own characterization, the book "range[s] widely over time, place, and issues, charting complex and contradictory dynamics which will continue to develop in unexpected ways" (p. 205). This can make it challenging for some readers. Industrial relations scholars and others interested in the future of workers' organizations and worker's rights, as well as others interested in countermovements to neoliberal forces, can benefit from the entire book. Others might be more selective. Labor economists might get the most out of discussions of power and bargaining; behavioral economists can find connections with framing, communication, and other influences on decision making; the material on labor union restructuring and strategic unionism should be stimulating for economists studying comparative organizations and the dynamics of leadership. On a policy level, there is much to be learned about reactions against macroeconomic responses to the global financial crisis, and the varied responses by labor unions should prompt thinking about filling in some of the holes left by Thomas Picketty's glossing over of labor unions as a counterweight to inequality in his instantly famous *Capital in the Twenty First Century* (2014).

Lastly, at the risk of oversimplifying nuanced scholarship in both disciplines, one could argue that economic theorizing emphasizes the generalizable, while industrial relations emphasizes the contingent. *Trade Unions in Western Europe: Hard Times, Hard Choices* makes a powerful case for the contingent. The ball is now back in the economists' court to refine our understanding of the generalizable nature of labor unions in our economies and societies.

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