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Book Review: Defending the History of Economic Thought. By Steven Kates. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2013. Pp. x, 140. $99.95. ISBN 978–1–84844–820–9 — Source link

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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

Mechanism and Causality in Biology and Economics. Edited by Hsiang-Ke Chao, Szu-Ting Chen, and Roberta L. Millstein. History, Philosophy and Theory of the Life Sciences, vol. 3. New York and Heidelberg: Springer, 2013. Pp. x, 256. ISBN 978–94–007–2453–2.
JEL 2013–0962

The volume under review collects the papers given at a 2011 conference, held in Taiwan, on philosophical issues arising in economics and biology. The focus on economics and biology is a welcome change from the usual practice in philosophical investigations, which is to base discussions of causation on the physical sciences or analysis of household appliances. The editors explain that they focus on economics and biology because these are fertile fields that involve new and significant philosophical issues.

As the book title indicates, the focus is on mechanisms and causation. Mechanisms have become fashionable in philosophical discussions of biology (according to Till Grüne-Yanoff), and they appear in economics in such terms as market mechanism and mechanism design. The editors express the view that causation is a topic under active development, particularly in economics, which seems a questionable contention. Most of the authors of these papers are primarily oriented toward philosophy, and have a secondary interest in economics or biology (or both, or neither). The book is therefore likely to be of more interest to philosophers than to practicing economists or biologists, who are not drawn to the philosophical mindset.

Philosophers are well aware that the modern treatment of causation, insofar as there is one, originated in the economics literature with the Cowles Commission economists. The focus is on criteria for causation in formal models, rather than in the world. Herbert Simon's classic 1953 paper is the major example, with related work by Trygve Haavelmo, Ragnar Frisch, and Nikolaas Tinbergen. The last of these is extensively discussed in Marcel Boumans's paper in this volume. However, analysis of causation has virtually disappeared from the economics literature (except in the guise of Granger causation, with the meaning of causation morphed into predictability).

It is worth inquiring why formal analysis of causation has dropped out of economics. Unfortunately, the papers in the volume under review do not address this question. The reason appears to be that the Cowles treatment of causation was based on structural models. These have undergone drastic revision in current economic analysis. For Simon a (linear) structural model was one in which internal variables are linear functions of other internal variables and external variables. Questions of causation were analyzed in terms of interventions—hypothetical alterations—on the coefficients of the structural equations. This procedure implicitly redefined the coefficients as external variables, rather than constants. Under this treatment, structural models in effect have two types of external variables: the additive errors that represent the routine operation of the model and the coefficients that represent interventions. As a result, structural models are properly regarded as bilinear rather than linear.[1]

Contemporary practice in economic model building is very different: coefficients of linear equations are viewed as shallow parameters; functions of the deep parameters of tastes and technology. Shallow and deep parameters are linked by cross-equation restrictions. The Lucas critique, at least in one version, consisted of the reminder that the coefficients of linearized models, being causal consequences of the deep parameters, cannot be treated as variation free, as was explicitly assumed in the Cowles treatment of causation. Thus the Cowles conception of structural models, which underlies most theorizing on causation by noneconomists, has largely disappeared from mainstream practice by economists. With it, we lost the Cowles analysis of causation.

Economists reading these essays will be reminded how different economics and philosophy are. The questions being addressed, the meanings of words, and the common conceptual backgrounds that authors draw on are all different. As a result, in varying degrees, these essays are difficult for economists to read and interpret. Take, for example, Boumans's essay "The Regrettable Loss of Mathematical Molding in Econometrics." The argument here is that the tradition of mathematical molding that had characterized business cycle analysis from the 1920s through the 1940s was lost with (or after; it is not clear) the Probabilistic Revolution in econometrics. The problem is that I am not familiar with the term mathematical molding. At first I thought molding was a typographical error for modeling, but this possibility is contradicted by several sentences that contain both words. Outsiders like me would have benefited from a definition of mathematical molding, but it is difficult to find one. The reader is left to infer a definition from passages like this (p. 62):

> Mathematical molding disappeared in the changeover from mechanisms to specify causal mechnisms of business cycles to methods to identify economic structures, that is, invariant relationships underlying the workings of an economy. . . . When the econometric program shifted its focus from mechanisms explaining phenomena to uncovering structural relationships, direct feedback from the phenomenon to the mechanism was lost, and the role of mathematical molding ceased to exist.

I do not know what any of this means.

Tinbergen, whose work is discussed in detail in this essay, is identified with the mathematical

[1] These ideas are discussed further in LeRoy 2014. That paper presents a discussion of causality that avoids reference to an underlying model that is structural in the sense of the Cowles economists.

molding tradition, but Boumans does not tell us which authors led the movement away from mathematical molding. Tinbergen was concerned with formulating models that produce cyclical behavior, but there is no way to argue that this research question has been abandoned in the more recent literature: a major concern in the real business cycle literature has been to produce models that produce significant business-cycle dynamics. I cannot find any discussion of why the move away from mathematical molding is regrettable, as advertised in the title of the paper.

Several of the essays make very worthwhile reading. Grüne-Yanoff compares the use of models incorporating replicator dynamics in evolutionary game theory in economics with their counterparts in biology. He concludes that the differences between the two are great enough to lead him to reject viewing replicator dynamics as a single mechanism that subsumes both social and biological applications. Also, Ruey-Lin Chen's discussion of discovery generated by experimentation (as distinguished from theory-based discovery, such as that of Einstein) in the context of Gregor Mendel and his predecessors in genetics is interesting.

One is gratified that philosophers are finding rich source material in substantive disciplines like economics, as opposed to basing their disquisitions exclusively on the mechanics of toasters and the like. Nevertheless, it is not likely that books like this will induce economists or biologists to pay substantially more attention to the philosophy literature than they have in the past.

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B History of Economic Thought, Methodology, and Heterodox Approaches

Defending the History of Economic Thought. By Steven Kates. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2013. Pp. x, 140. $99.95. ISBN 978–1–84844–820–9.
JEL 2014–0007

Steven Kates's book *Defending the History of Economic Thought* comes at just the right time. As the history of economic thought (HET) often regains importance in times after economic crises, it is also since the financial crisis of 2008 that parts of the economics profession have become more self-reflexive, acknowledging to a certain extent the shortcomings of economic models in predicting what happened (see e.g., Hoover 2010). The fact that some economists turned towards the history of their subject matter in substantiating such arguments naturally provokes questions about the role that HET could and should play in economics.

As for historians of economics, they have been rethinking their identity for some time now. Being under constant exposure to a persisting decline of the field, questions about the ideal intellectual home, the appropriate methodology and the subject matter of HET have been continuously addressed in the last decades (see e.g., Medema and Samuels 2001, Moscati 2008, Schabas 1992, Weintraub 1989 and 2002). Thus, the concrete arguments that Kates provides as to *what* historians of economics should do and *how* they can contribute to economics is a relevant topic.

However, I doubt that Kates's book offers what is needed for fruitfully advancing the debate. It fails to provide convincing arguments for the importance of HET as a self-contained field that is valuable in itself, and as a pluralistic enterprise that might produce knowledge of interest for mainstream economics, but also beyond.

The main message of Kates's book is that HET is a constitutive part of economics, which is why HET should be preserved as constitutive of the economics profession. To support his position, Kates develops a multilayered argument for why HET would significantly improve teaching, research, and theory application in economics, and why it should therefore be considered a core element of the discipline. Like many contributions debating the role of HET, Kates's project is marked by normative undertones. But the book aspires to provide more than a general proposal. It is structured as a user's guide, giving concrete advice for defending HET against serious threats to remove it as a subfield from economics. The target audience of the book is broad, spanning researchers and students of economics, professional economists outside of academia, the economic policy maker, communicators of economic concepts to the layman, and the everyday individual having an interest in economics. It is the interests of those groups for Kates that suggest the relevance of HET for current economic affairs.

Kates lays out his arguments in five short chapters. After sketching his motivation and general position in chapter one, Kates presents various arguments for why studying HET would make someone a better economist in chapter two. "Better" thereby refers to improving a person's capability and skills to solve today's economic problems. Much of this argumentation implicitly rests upon a "presentist" view of history, that is, a view that considers understanding history not as an important enterprise in its own right, but instead interprets the past in terms of present-day interests and theories of economists (e.g., Medema and Samuels 2001). On this view, HET is to be consulted as a "storehouse of [potentially relevant] economic ideas"; as engaging in a "conversation with economists of the past" that can illuminate debates in the present; as educating economists to write about and communicate technical concepts and mathematical arguments in everyday language; and as a way to better understand the scope of application of a specific theory or concept (p. 16 ff.). In turn, "[a]n economist without the kinds of knowledge that HET provides, so far as their role as economists is concerned, is no better than a tradesmen. They may be able to run a regression but they are not scholars as it was once understood" (p. 41).

The third chapter discusses the main positions brought forward in the debates about the relationship between HET and mainstream economics. Kates identifies at least two main strategies in the literature to defending HET against "hostility from the mainstream" (p. 43). The first strategy is to ensure that HET does not automatically result in criticism of (present-day) economics; the second strategy is to outsource HET to the history and philosophy of science. Kates himself repudiates both strategies. Because a part of his proposal rests on the idea that HET allows for testing mainstream economic theories, Kates rejects the first strategy by arguing that studying the history of theories must inevitably be accompanied by a critical stance; and it also should. He rejects the second strategy because it commits to the general view that history of science should not primarily aim at making a difference to the science itself, a view that is common in the history of science but that Kates disagrees with. Rather, the main purpose of studying HET according to Kates is to improve current economic theory. Thereby, the historian of economics is effectively a mediator who translates old ideas and concepts into modern language, illustrates the continued relevance for mainstream economics today (p. 69), and directly contributes to how economics is currently practiced.

This idea of improving present mainstream economics through the study of HET has obvious implications for the relevance of HET in teaching economics. In the fourth chapter, Kates argues that understanding the historical origins of theories is of critical importance in the social sciences. Unlike the natural sciences, where new theories irreversibly replace older ones, past economic theories can regain relevance if and as they inform current discussion and illuminate contemporary problems. Consequently, it is crucial to teach economic theories not as ultimate truths but as provisional conclusions, thereby retaining openness to their possible return as well as to alternative ways of thinking. Furthermore, by helping us distance ourselves from our own time and place, HET makes us understand the purpose that a theory was originally developed to fulfill and the boundaries between useful and misguided applications. As the denial of such history can mislead us in our applications, the student of economics without a background in HET is "less well equipped [. . .] than one who does have such knowledge" (p. 21). To function as a kind of user's guide to theory application, teaching HET is to consist of studying—in chronological order—the original textbooks that were once considered part of the canon. Containing the major concepts and theories that were used in a given period of time, Kates considers such textbooks as representative for the theoretical toolbox available to economists and, as such, as providing the "context" necessary for better understanding how theories have been, and are to be, changed and applied (p. 101). However, as for Kates only those past theories are relevant that help us understand current ones, the textbooks to be studied should be those that have had considerable influence on mainstream economic theories. Marxist economics and Austrian economics, for example, should thus be excluded from teaching HET on the grounds that those traditions never had a profound impact (p. 95), a claim that to my mind is the least convincing part of the book.

In the final chapter, Kates discusses two recent attempts by the Australian Bureau of Statistics and the European Research Council to remove HET from the economics research classification scheme. Successful exclusion would surely have far-reaching implications for the HET community, its location, and its research orientation, which is why this chapter provides interesting and relevant insights into the actual policy dimension of science and the production of knowledge. Drawing upon his personal engagement in these "classification wars" (p. 107), Kates presents a proposal for taking action against such reclassification attempts. This proposal is the most persuasive part of the book. Kates analyses the practical implications of such a reclassification for the status of HET as a field of study and supports his analysis with personal anecdotes and correspondence with respective authorities. He furthermore puts together a twelve-step action plan in order to convince economists (and administrative authorities) of the benefits of preserving HET as a core field of enquiry that should remain within the economics profession.

Most historians of economics would probably agree that studying HET should, for various reasons, be essential for modern economics. However, Kates's position has implications for the type of knowledge to be produced by historians of economics, the methodology to be applied in HET, and the status of HET as a discipline. To emphasize the primary concern of HET to have an impact on current economics, Kates's proposal diminishes the importance of HET as a self-contained field of study and downplays the idea that scholarship undertaken by historians of economics has value in its own right.

First, adopting Kates's view of HET would severely limit the scope of enquiry of the historian of economics. Kates commits to a particular view of what economics is, what historians of economics do, and what both fields *should* be concerned with. Having undergone a professional career in Australian politics (p. 106), Kates takes a pragmatic stance. He defines economics as an "amalgam of theories and techniques that can be used to make sense of the ways in which we provide for our material wellbeing" (2013, viii). As for Kates, economics is primarily a policy science (p. 20), it follows that a large number of people involved in economics as an academic profession will ultimately work in practical fields where those theories and techniques become applied to concrete policy problems. From this view of economics and of the task of an economist follows a specific and rather narrow conception of history and view of what a historian of economics should do. Understood primarily as a constitutive part of economics, HET has itself to be "a form of technique" (p. 15). Studying HET as such a technique enables the student to broaden her thinking beyond an accepted paradigm, to acknowledge the potential depth of economic analysis, to accept the contingent truth of economic theories, and to better understand the scope of application of a particular theory. Yet maintaining this link between HET and contemporary economics defined as a policy science comes at the price of confining HET to only studying the history of canonical economic ideas.

Such an understanding of HET as *Dogmengeschichte* or the *history of ideas* is not only a rather outdated view of how the history of a science should be studied. It also ignores the various historiographical developments that HET has recently undergone and that contribute to its evolving into a pluralistic field of study. Historians of economics increasingly use a variety of new techniques and exciting sources that provoke new questions for further study. The availability of archival material and the possibility of drawing upon papers from some of the most prominent economists of the twentieth century have made the study of HET not only a fascinating, but also an extremely worthwhile enterprise in its own right.[1] Such sources furthermore require innovative methods that allow for placing economic ideas into their social, cultural, and epistemic contexts. Pursuing HET in the service of modern economics only and focusing on

[1] One example is the Economists' Papers Project, initiated in the 1980s by the Center for the History of Political Economy at Duke University. The archive comprises the correspondence, manuscripts, drafts, memoirs, interviews, diaries, documents from administrative duties and political involvements, teaching documents, etc. of more than forty distinguished economists that have mostly lived during the twentieth century (see Weintraub et al. 1998).

the study of past textbooks, in contrast, leads to the exclusion of various ways in which HET has been practiced and flourished in recent decades. Furthermore, by precluding noneconomists from the field (p. 50), Kates's proposal discounts the value of many excellent projects currently undertaken by noneconomists within HET and thereby prevents HET from progressing as an innovative and self-contained field of study.

Second, Kates's arguments also fail to identify the distinctive value of HET, its usefulness for mainstream economics, and beyond. Apart from being rather ad hoc and unsupported by empirical evidence, Kates's arguments hinge on the idea that studying HET will provide specific skills to the student that get lost in a discipline that focuses more on mathematical tools and statistical techniques than on interpretation. While indeed, convincing arguments can be made in favor of a more pluralistic and diverse education as being desirable in economics, it is not only HET that would incorporate a level of reflection and diversity into the curriculum. As Kates's arguments often sound as though primarily formulating a critical view about the extensive use of mathematics and statistics in economics, various courses on nonmathematical subjects or courses from a humanistic education that would broaden the narrow education of economists could do the job. For example, a similar argument could be made for including mandatory courses in economic methodology or philosophy of economics into the economics curriculum. It is not the unique nature of HET that allows for it to play the role in economics that Kates envisions as justifying its pursuit.

Two questions that seem to be crucial for advancing the debate about the role of HET for current economics are what the community of HET ultimately wishes to accomplish and what economists are ultimately interested in when consulting HET. Much of recent work in HET suggests that its development towards becoming a self-contained discipline with a broader historiographical focus is desirable. Such a development would not necessarily exclude the history of ideas from its agenda. But the history of ideas would be complemented by high-quality historical and methodological work that rests upon employing the appropriate historical and philosophical tools. Such scholarship, I would argue, might have more chances of arousing the interest of economists than any attempt by a nonspecialist to change some economic concept or a theory, which is itself part of a highly specialized subfield and requires the respective skill. Rather, the historian of economics could be consulted as a specialist in her field of expertise, thereby mastering the methods and tools from the history of science that she is expected to apply. Such a step would allow HET to become a self-contained field of study that could produce knowledge accepted by, useful and relevant not only for, mainstream economics but also beyond.

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F. A. Hayek and the Modern Economy: Economic Organization and Activity. Edited by Sandra J. Peart and David M. Levy. Jepson Studies in Leadership series. New York: St. Martin's Press, Palgrave Macmillan, 2013. Pp. xviii, 247. ISBN 978–1–137–35958–2.

JEL 2014–0424

This volume grew out of a conference on "Hayek and the Modern World" held in April of 2013 at the University of Richmond. All but one of the chapters were presented there in some form.

Because Friedrich Hayek's work is so wide-ranging and because of the growth in Hayek scholarship in the last couple of decades, the major challenge facing any new collection of essays on Hayek is finding a theme or providing enough new insight to provide value added for readers. Although the volume lacks a clearly defined theme, many of the ten substantive chapters address areas left underexplored by previous scholarship, or offer new insights on well-established areas of Hayek's thought.

Peter McNamara's opening chapter "On Hayek's Unsentimental Liberalism" explores Hayek's relationship with Adam Smith by noting that, unlike Smith's emphasis on sympathy in *The Theory of Moral Sentiments*, Hayek offers no account of human nature in his work. McNamara argues that Smith's account is actually consistent with modern work on neuroscience and sociobiology/evolutionary psychology, giving it an advantage over Hayek. McNamara may be correct about Smith, but he ignores Hayek's book *The Sensory Order*, in which Hayek does provide an account of human cognition that is consistent with modern work. Hayek's argument in that book is especially interesting when read in conjunction with both modern neuroscience and Smith on the moral sentiments.

Three of the chapters address Hayek's activities and influence in the decade of the 1940s. Editors Sandra Peart and David Levy's contribution looks at Hayek's relationship with a particular group of American "individualists" in the years leading up to the creation of the Mont Pelerin Society in 1947. That group was, despite their claimed commitment to laissez-faire, deeply influenced by eugenic and anti-Semitic thinking. Peart and Levy argue that Hayek's belief in the importance of population growth, and what they call the "analytical egalitarianism" of classical liberalism, distanced him from the more odious forms of laissez-faire at play in the 1940s.

Ekkehard Kohler and Stefan Kolev argue in their essay that there are deep similarities among the reform programs put forward in the 1930s and 1940s by the "Old Chicago" school of Henry Simons, the Freiburg School of Walter Eucken, and Hayek's ideas. What they shared was a rejection of the old idea of "laissez-faire," understood as a complete absence of government action. Instead, all three groups had a "positive program" for reform that emphasized the need for governments to more actively create the "rules of the game" or framework within which competition and the market order unfolded. Getting those rules right, for example deciding whether different monetary institutions are needed, was the best way to understand government's role in reform. The authors make a compelling case for the overlap of these research programs, and their continued joint relevance can be seen in contemporary work that draws on a mixture of public choice theory, new institutionalism, and Austrian economics to offer similar analyses of the necessity of the right rules.

The third of the 1940s chapters is Andrew Farrant and Nicola Tynan's discussion of Hayek, Clement Atlee, and the "Control of Engagement Order" passed in Britain in the fall of 1947. A number of conservative British politicians, including a young Margaret Thatcher, had argued that this law and its purported attempt to allocate labor from the top down was evidence of what they saw as Hayek's thesis in *The Road to Serfdom*: that economic planning would eventually lead to some form of totalitarianism. Hayek also made similar claims. Farrant and Tynan offer archival evidence that their concerns were overblown and that the actual scope and effects of the order were limited. To the degree that the order was being used as evidence in support of Hayek's thesis, the authors argue that was a mistake. This chapter offers a nice example of the way archival work can help us better understand the influence of Hayek's ideas.

Two other chapters worth noting are Gerald Gaus's attempt to expand our understanding of Hayek's evolutionary account of morality by the use of "evolutionary adaptive landscapes." Gaus's argument enables theorists to account for the evolutionary processes that produce social morality while still having a standpoint from which to judge any given outcome without falling for the Nirvana Fallacy. This chapter is an important technical contribution to Hayekian thought.

Christopher Martin's chapter suggests that Hayek's understanding of Athenian democracy used to construct his ideal constitution in the third volume of *Law, Legislation, and Liberty* is historically suspect. Martin argues that Hayek's attempt to limit the role of political faction would have "benefited from more appreciation of the later Athenian constitution and its use of randomly chosen juries to decide constitutional questions" (p. 148). Martin opens up an intriguing line of research in connecting Hayek more accurately to the ancient Greeks than Hayek did himself.

Hayek scholars will learn much from this volume, though it is not suitable as an introduction to his work. Historians of economic thought and historians of the discipline will find value here as well, particularly in the chapters on Hayek's influences and his role in the 1940s. Several of these chapters open up fascinating new avenues for research on Hayek, and those engaged in such work will want to explore what it has to offer.

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C Mathematical and Quantitative Methods

Game-Changer: Game Theory and the Art of Transforming Strategic Situations. By David McAdams. New York and London: W. W. Norton, 2014. Pp. 303. $26.95. ISBN 978–0–393–23967–6. *JEL 2014–0437*

The premise of David McAdams's new book *Game-Changer* is that in order to change the outcome of a strategic situation, one first has to understand the incentives faced by the individuals who are making decisions. Only then is it possible to design a policy that realigns private goals with those of the policy maker. McAdams brings this perspective to a broad audience with a nontechnical introduction to basic game theory. Through a variety of interesting and colorful examples, he argues that game theoretic concepts can be used to clarify the relationship between incentives and behavior. Such an analysis can reveal opportunities to transform the strategic situation, and thereby solve practical problems in business and public policy.

The book is split into two parts, the first of which deals with methodology and the second gives applications. Part one focuses on the strategic transformations one can use to change the game. Interspersed throughout are three "Game Theory Focuses" that describe various types of games and solution concepts, and they even touch on more advanced concepts such as evolutionary stability. The theory is explained with a bare minimum of formalism, which is essentially limited to graphical depictions of games as payoff matrices and trees. This half of the book hums along with plenty of anecdotes for illustration, and to break up some of the drier material. Regulation of cigarette advertising, the cartelization of charitable fundraising, and the Battle of the Bismarck Sea are among the many examples that make appearances, as well as McAdams's hypothetical adventures convincing his children to eat their vegetables.

Game-Changer develops a somewhat distinctive style and terminology that will be evocative for the uninitiated, but will require some adjustment for those already familiar with the material. Some terms, such as "rollback equilibrium," used in lieu of subgame perfect equilibrium, are simply a relabeling of familiar concepts. Others, such as "commitment moves" for actions that commit one to a particular behavior at a later date, draw distinctions that are not usually made in standard treatments. These idiosyncrasies will create some adjustment cost for the reader who plans to delve further into the subject.

The majority of the analysis is focused on the prisoners' dilemma as a canonical model of inefficient self-interest. The transformative techniques are geared towards moving the players in that game to the efficient outcome through government regulation (chapter 2), merger (chapter 3), and the addition of various kinds of dynamic incentives (chapters 4, 5, and 6). For the most part, these techniques correspond to viewing the prisoners' dilemma as being embedded within a broader game, in which the putative game-changer has actions that shift payoffs in a manner that heads off social inefficiency. Whether or not one views these transformations as changes to the game or as actions in an expanded model, McAdams's point is that the minimalist prisoners' dilemma is rarely a complete description of the strategic options. To realize all routes to improving the outcome, one should try to take an expansive view of the mechanisms that shape incentives.

Most of the extensions to the prisoners' dilemma have readily identifiable inspirations in the academic literature, such as classical regulation and merger analysis for chapters 2 and 3 and repeated games for chapter 6. For others, it is less obvious what formal model lies behind the prose. Chapter 5 discusses "trust" as a way to avoid inefficient outcomes in a sequential move prisoners' dilemma. At times, McAdams suggests that rational agents can instill trust in others through impassioned and sincere speeches, which presumably constitute a kind of costly signaling about one's true preferences. Elsewhere, trust seems to reflect a concern for long-term reputation on the part of a "trustworthy" second-mover. This explanation overlaps with McAdams's later reference to reputation effects in leveraging relationships, and it is not entirely clear how the two approaches differ.

Though other games make occasional appearances, the primary explanatory tool throughout the exposition is the prisoners' dilemma. The reader is thus left with a somewhat narrow view of the range of phenomena that can be understood using the game-theoretic approach. Coordination games are only mentioned in passing, and the role of incomplete information is hardly touched on at all. Those who are interested will have to look elsewhere for an introduction to these important topics.

The second half of the book applies the transformation techniques in a series of case studies. These "Game-Changer Files" describe social inefficiencies in situations ranging from fisheries regulation to real estate agency to online marketplace reputation. For each file, the author argues that there exist feasible policies that would modify incentives and increase social efficiency. Some of the suggestions seem more practically useful than others. One can certainly envision random sampling of fishing catches, with no penalties attached, to accurately estimate stocks and calibrate an industry-wide quota. It is less easy to see how game theoretic insights will transform the fight against antibiotic resistant bacteria without significant breakthroughs in medical technology. Nonetheless, these case studies serve to illustrate the core message of the book, which is the practical usefulness of game theory for policy analysis. More than any specific prescription or case study, the value of the game theoretic approach is as a framework for understanding strategic interactions.

Ultimately, does *Game-Changer* provide a "radically new . . . way to outstrategize your rivals," as promised by the dust jacket? For those who are new to the subject, the book provides an accessible and broad introduction to the possibilities of game theory, which will facilitate new ways of thinking outside the box about strategic situations. Academic economists, on the other hand, will appreciate the trove of well-chosen and colorful examples. While the material is unlikely to be radically new for the latter audience, *Game-Changer* is nonetheless an enjoyable read.

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E Macroeconomics and Monetary Economics

Beyond GDP: Measuring Welfare and Assessing Sustainability. By Marc Fleurbaey and Didier Blanchet. Oxford and New York: Oxford University Press, 2013. Pp. xvi, 306. ISBN 978–0–19–976719–9. *JEL 2013–0708*

What are good indicators of social performance? How to measure whether societies progress? Which societies are better, as evaluated by the interests of the individuals living in those societies?

During the last decades, per capita gross domestic product (GDP) has been used as an indicator of countries' success by economists and others. Simultaneously, GDP has been severely criticized, leading to a plethora of alternative indicators. In this book, Fleurbaey and Blanchet present an impressive comprehensive study of alternatives to GDP and their foundations. It discusses different classes of indicators in a thorough and thought-provoking manner, with the motivation of contributing to better practical measures of welfare and sustainability.

Chapter 2 is devoted to the role of sustainability indicators, arguing for the need to differentiate between a dynamic measure for the long-run sustainability of the current situation and a static measure for the goodness of the current situation. The remainder of the volume is devoted to the latter challenge of developing such a static measure of current social well-being. It identifies and discusses four alternative approaches in chapters 1, 3, 4, 5, and 6.

Chapter 1 presents the first approach where various indicators of social performance are gathered into a hybrid, composite index. The Human Development Index (HDI) is the most well-known example. The chapter discusses the problems associated with such ad hoc indices. In particular, it is pointed out how, in the HDI, the valuation of longevity depends in an arbitrary manner on the methodology being applied.

Chapters 3 and 4 develop two alternative versions of the monetary approach, where the monetary metric of the GDP is kept, but its content is changed. One version is based on uniform pricing; the other is based on individual willingness-to-pay and "equivalent income." A central argument is developed, namely that the concept of "equivalent income" is a monetary indicator that respects the diversity of individual preferences in a context where the sources of human well-being extend beyond the vector of marketable commodities.

Chapter 5 discusses a third approach, where "happiness" as a subjective measure of well-being is obtained directly through questionnaires. The authors argue forcefully that it is difficult to use such data for ethically sound comparisons between individuals with different preferences about various aspects of life (e.g., as expressed through different aspiration levels).

Chapter 6 deals with the fourth and final approach, namely Amartya Sen's capability approach (CA), ending with the following claim: If the CA is developed in a way that respects the diverse individual values—which is a route that exerts strong appeal—then the "equivalent-income" approach can be proposed as a possible methodology for the application of the CA.

The book's concluding chapter summarizes the lessons learned in the discussions of the sustainability and the various approaches to measuring current social well-being, ending with concrete and pragmatic advice. Finally, two appendices present a theory of the reference for equivalent incomes and include proofs of various results.

By giving a comprehensive treatment of the technical and ethical problems associated with the challenge of measuring social well-being, this book is a very important contribution. The authors evaluate, in detail, the theoretical foundations of each approach to measuring current social well-being, while keeping in mind the pragmatic goal of contributing to better practically implementable measures. The book is accessible to readers with an interest in, and familiarity with, the main concepts and issues and no more than an undergraduate background in economics is necessary for most of the text. Figures provide useful empirical evidence, ensuring that the book is not a theoretical discussion without empirical underpinnings.

Arrow's impossibility theorem of social choice has discouraged many economists from attempting to measure social well-being, since this theorem is usually interpreted as implying that there is no reasonable social evaluation methodology that respects individual ordinal preferences and does not rely on extra utility information. The "equivalent-income" approach illustrates this problem, since it respects unanimous individual preferences without assigning a dictatorial role to one particular individual. However, it violates Arrow independence (as the social ranking of any pair of alternatives depends not only on how the individuals rank these two alternatives). With a reference to the theory of fair allocation (in particular, as developed into a theory of "fair social choice" by Fleurbaey and Maniquet 2011), the authors argue in chapter 4 that Arrow independence is not compelling. Therefore, the "equivalent income" approach is not flawed, but allows comparisons of the well-being of different individuals, while respecting their preferences and taking into account that their situations differ.

The interesting treatment of "happiness" indicators in chapter 5 could have been complemented by a discussion of an evolutionary theory of happiness, put forward by Samuelson (2004) and Rayo and Becker (2007). Their main argument is that the subjective feeling of happiness is a relative scale that works as an optimal incentive scheme for the purpose of promoting effort. Information about the success of others indicates available opportunities, and a low subjective feeling of happiness of a disadvantaged but capable individual has the effect of motivating the individual to take advantage of those opportunities. In contrast, paraplegic individuals might not, in the long run, feel unhappy about their inability to take part in useful and enjoyable physical activities, as increased motivation in this direction is not productive. This evolutionary theory of happiness further undermines the normative appeal of "happiness" indicators, and reinforces the conclusions that the authors reach in this chapter.

The book treats the question of measuring sustainability as a separate issue from the issue of measuring static well-being. The former question relates to whether the current opportunities are used in a manner that reduces, in a relevant sense, the opportunities for future generations. If not, these opportunities are utilized in a sustainable manner. However, as is evident from the discussion of the capability approach, the available opportunities also matter at an individual level, not only how these opportunities are utilized. A key example is that fasting is different from starving, because it involves a possibility of greater nutritional achievement that is absent from the starvation situation. Moreover, one might argue that there are a number of challenges that must be confronted both at an individual and social level, in addition to the issue of relating the current set of opportunities to the current level of well-being.

One such issue is how societies and individuals should rank the elements in their opportunity sets (or their current vector of "consumption," in a wide sense) from a normative perspective. A separate but related issue is to what extent societies and individuals are able to implement good choices, when taking into account government failure at a social level and problems of self-reflection and self-control at an individual level. Even though these issues are discussed throughout the book, their relevance at both a social and individual level is not applied consistently as an integrating device.

It would also have been very interesting if the issue of assessing sustainability (which is one of two issues mentioned in the subtitle of the book) were revisited at the end of the book. In my opinion, an appropriate measure of static well-being is a prerequisite for a relevant measure of sustainability. At an abstract level, the concept of green (or comprehensive) NNP can be understood as a measure of opportunities in the same metric as the appropriate measure of static well-being. Denoting green NNP by y and static well-being by u, the development of green NNP might be governed the following differential equation, $dy/dt = r(y - u)$, where r is a real rate of interest that depends on the metric being used. Hence, for the purpose of indicating sustainability, it is not the difference between conventional GDP and an adjusted measure of green NNP that matters, but whether the latter concept signals prudent behavior by having a positive growth rate, or equivalently, by exceeding the appropriate measure of static well-being.

This points to a more general insight: That in order to answer the questions posed at the beginning of this review—how to measure whether societies progress; which societies are better, as evaluated by the interests of the individuals living in the society—we are not interested in the absolute size of the appropriate measures, nor how they compare to conventional measures. We are interested in how the relevant measures develop through time, and how such measures vary across different societies. The book by Fleurbaey and Blanchet is essential reading for economists who want a critical and wide-ranging assessment of the different approaches that have been suggested as such measures of social performance.

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G Financial Economics

Asset Price Response to New Information: The Effects of Conservatism Bias and Representativeness Heuristic. By Guo Ying Luo. Springer Briefs in Finance. New York and Heidelberg: Springer, 2014. Pp. vii, 70. $54.99, paper. ISBN 978–1–4614–9368–6, pbk.; 978–1–4614–9369–3, electronic.

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How asset prices react to new information has been a topic of long-standing interest in finance. In this book, Dr. Guo Ying Luo theoretically studies this topic by incorporating two well-known behavioral biases into the standard Grossman and Stiglitz (1980) framework and the Kyle (1985) model.

This book is relatively short and is extremely well-organized. In chapter 1, Luo reviews the empirical evidence on underreaction and overreaction in asset prices, and briefly surveys leading behavioral models of underreaction and overreaction. Chapter 2 shows that in an extended Grossman and Stiglitz (1980) model, conservatism bias can generate both asset price overreaction and underreaction to new information in a perfectly competitive market with noise traders. Chapter 3 illustrates that in an extended Kyle (1985) model, conservatism bias can generate asset price overreaction and underreaction in a market allowing for strategic interaction among rational and conservatism traders. Chapters 4 and 5 basically repeat the analysis in chapters 2 and 3 by replacing conservatism bias with representativeness heuristic. Luo shows that representativeness heuristic can also generate both asset price overreaction and underreaction to new information either in a perfectly competitive market or in a market allowing for strategic interaction among rational and representativeness traders. Finally, chapter 6 incorporates both conservatism and representativeness into a standard static Kyle (1985) model to explain the phenomena of asset price overreaction and underreaction. As we can see, this short book offers new theoretical models of underreaction and overreaction that are different from leading studies such as Barberis, Shleifer, and Vishny (1998), Daniel, Hirshleifer, and Subrahmanyam (1998), and Hong and Stein (1999). In this sense, this book is more of an extended research paper.

The usual criticism of behavioral studies is that there is too much freedom in choosing behavioral biases. In addition, different behavioral biases are sometimes required to account for different asset pricing phenomena. In this book, Dr. Luo tries to avoid this criticism by focusing on two well-documented biases: conservatism and representativeness heuristic. These two biases have been replicated by many psychological studies and have already been extensively applied to the finance literature. More important, Dr. Luo shows that one single behavioral bias is enough to account for both underreaction and overreaction.

Typically, researchers think that conservatism bias is responsible for underreaction, whereas representativeness heuristic is responsible for overreaction (see, e.g., Barberis, Shleifer, and Vishny 1998). Surprisingly, Luo shows that after introducing noise traders (whose demand is exogenously given), conservatism bias alone can lead to both underreaction and overreaction. Similarly, representativeness bias alone can also result in both underreaction and overreaction, under different parameterizations. I find that this result is quite surprising. Specifically, Luo forcefully illustrates that these two biases are very flexible in producing both underreaction and overreaction in asset prices.

Given this flexibility in producing underreaction and overreaction, I believe that it is especially important to provide more refutable implications of the models and test these implications with real data as well. For example, the book shows that under some parameterizations, asset prices overreact to good news. However, these conditions are a bit abstract. Thus, it would be useful to translate these abstract parameterizations to more directly testable implications, so that empiricists can easily test these new implications. For instance, it would be nice to identify conditions under which the momentum effect or the reversal effect will be stronger or weaker. New empirical tests could validate/refute the importance of those biases in asset prices determination. Since the current book is more about the theoretical possibility to generate both under- and overreaction with a single behavioral bias, it (understandably) lacks the empirical analysis. On the theory side, it would also be interesting to extend the static setting to a dynamic setting. In a dynamic setting, one may derive richer asset return dynamics based on these biases and probably provide more testable implications as well. Alternatively, it might also be interesting to introduce multiple risky assets into the current settings. This way, one can study cross-sectional anomalies more realistically.

Given that it is theory oriented, this book might be too technical for general audiences such as professional students in finance and economics. The ideal readers of this book would be PhD students and professors in finance and economics, especially those who have a special interest in behavioral economics/finance. However, readers should note that many of the theorems in the book are not empirically tested yet. Thus, some of the conclusions might not be supported by the real data. Again, the book is mostly about theoretical explorations and readers (especially PhD students) might find it fruitful to test some claims in the book.

In sum, the book clearly shows that the two well-documented behavior biases are quite powerful in producing both underreaction and overreaction in asset markets. What the paper lacks, in my view, is a clear delineation of the empirical implications of the model, so that the model can be easily taken to the data by empiricists. Overall, I find that this book provides a very nice theoretical contribution on how a simple behavioral bias such as conservatism or representativeness could result in both underreaction and overreaction simultaneously in a stylized two-period setting.

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The Global Debt Crisis: Haunting U. S. and European Federalism. Edited by Paul E. Peterson and Daniel J. Nadler. Washington, D.C.: Brookings Institution Press, 2014. Pp. viii, 241. $29.95, paper. ISBN 978–0–8157–0487–4, pbk. *JEL 2014–0513*

The Great Recession increased the pressure on national and subnational budgets and often increased the level of public indebtedness. The common theme of this book, a collection of articles edited by Paul E. Peterson and Daniel Nadler, is how the increasing level of public debt affected the relationships between different levels of governments, and how preserving and reinforcing competitive federalism may offer a solution to the current "Debt Crisis."

All of the chapters, with one exception, are focused on subnational debt. State and local public debt has been growing in importance both quantitatively and in the academic debate, and a book dealing with subnational debt after the Great Recession was long overdue. The first chapter introduces the book, the next five chapters deal with U.S. states, the seventh chapter deals with the European Union, the eighth chapter with Germany, the ninth and tenth chapters with Spain, and the last chapter with Canada.

A key strength of the book is its comparative perspective. The credibility revolution in empirical economics increased the academic studies focused on a single country because it's typically easier to find exogenous variations in policies in a within-country setting. As a result, the ability to draw broader conclusions by looking at a single piece of research was diminished. This book aims at filling this gap, by offering a unified framework to interpret many case studies and providing much "food for thought" for academic economists in the fields of public finance, political economy, and macroeconomics.

Subnational public finance has its own peculiarities. It is well known, both in developing and developed economies, that local governments' policies are typically less countercyclical than national ones (Gavin and Perotti 1997; Hines 2010). Assuming that the ability to implement countercyclical fiscal policy is one of the rationales for allowing governments to run debts, this implies that subnational public debt may be, all else equal, more distortionary than national debt and, therefore, deserving of distinct academic investigation. Additionally, Peterson and Nadler are well aware that political distortions can explain the persistence and increase of public debt over time and underline in chapter 1 how the analysis of public debt can't ignore political economy considerations.

Consistent with this attention to political economy, several of the authors analyzed policies that were adopted by various states as a consequence of the increasing levels of subnational debts and default risks across the globe. The most common policy response has been to move towards centralization and away from federalism. This centralization response is especially frequent in Europe, both in the European Union and in specific countries (for instance, Germany and Spain). In contrast to this "centralization trend," the main policy proposal of the book for the current fiscal instability of many local governments around the world is to preserve and reinforce competitive federalism. This latter term is mainly defined in the second and third chapters of the book as a federalist system characterized by fiscally disciplinary, market-based forces combined with a commitment from the upper layers of governments not to bail out lower layers.

While the policy proposal is plausible, there are many challenges in its practical implementation, such as political and legal ones. From the political perspective, it may obviously be hard for national governments to not bail out subnational governments ex post, no matter how credible the commitment is. From the legal perspective, as noted by Rodden in the third chapter, the rules of subnational defaults are often not clearly specified for market actors, hence, the risk of default increases even more the uncertainty of public creditors.

Another challenge for competitive federalism is the potential lack of budget transparency. This is the main topic of the fourth and fifth chapters, which deal with public pension plans, and of the sixth chapter, which deals with high-risk investments by subnational governments. Unfunded pension and health care obligations have had an important role in the recent bankruptcies of several U.S. municipalities, and the size of these unfunded liabilities is highly dependent on the assumptions made when estimating them. In the sixth chapter, Shoag emphasizes how U.S. subnational governments rely on risky investments to finance themselves and how this can increase the frequency of bankruptcies in the future. Engaging in risky financial investments is not peculiar to U.S. subnational governments: for instance, several Italian cities signed complex derivatives contracts, whose effect on public accounts was delayed over time and often generated lawsuits between local governments and investment banks (Bloomberg 2009).

Two important topics that would have probably deserved more attention are individual mobility and fiscal rules. First, making individual mobility easier may be, in my opinion, a policy that may increase competitive pressure on subnational governments. Consistent with this point, I believe the lack of individual mobility within the European Union is one of the reasons that competitive federalism may be difficult to quickly achieve among the EU countries. Second, fiscal rules are laws designed to increase the incentives for fiscal discipline and are increasingly common across the globe, especially when imposed by national governments on subnational ones.

In spite of all of the aforementioned challenges, competitive federalism has survived for many decades in the United States, as Rodden argues in the third chapter, and still survives in Canada, as Simeon, Pearce, and Nugent argue in the last chapter of the book. In this last chapter, the authors try to identify the reasons that explain the success of a competitive federalist system. Although such an identification process is hard, the authors argue that the strength of legal and political institutions can have an important role, by making coordination among different layers of government easy and by facilitating compromises across different preferences on the level of government interventions in the economy.

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H Public Economics

Tax Systems. By Joel Slemrod and Christian Gillitzer. Zeuthen Lecture Book Series. Cambridge and London: MIT Press, 2014. Pp. x, 223. $30.00. ISBN 978–0–262–02672–7.

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Tax policy has been central to economists' raison d'être since they first evolved from moral philosophers. Our core theories of taxation—pioneered by Frank P. Ramsey, Arnold C. Harberger, James Mirrlees, and others—provide deep economic insights involving incidence, excess burden, and optimal rate structures, which balance objectives of equity and efficiency. Yet, if one were to ask most ordinary citizens what they think of the "excess burden" of taxes, they would evoke painful tax forms and audits, and look puzzlingly at our odd triangles. If asked how they might respond to taxes, many would bring up various avoidance "loopholes," rather than how much to work and save. Economists' core theories also do little to enlighten tax experts in other fields—such as law, accounting, or administration—interested in issues like how to define tax bases, relay tax information, and enforce compliance. Moreover, the core theories do not predict existing tax structures with features such as low, flat-rate taxes on consumption. They do even worse with past structures, which included particular taxes on salt, chimneys, and windows.

Unbeknownst to many, economists have extended economic reasoning to a much wider array of tax issues for decades. Perhaps best known is the work of Michael Allingham and Agnar Sandmo, who applied Becker's theory of amoral criminal behavior to tax avoidance, pointing out that imperfect enforcement adds a hidden welfare cost to taxes through uncertainty. Yet, not until *Tax Systems* have hard-won insights on these broader issues been synthesized succinctly and rigorously in a far-reaching monograph. There is no more qualified author than Joel Slemrod. He has spent over thirty-five years working on these issues, trained legions of tax economists, gained tremendous respect from tax experts across disciplines, and personally authored or coauthored over fifty of the 350 articles and books covered in *Tax Systems*. The notes of his intellectually rich graduate course on taxation provided the backbone of this book. Faced with many other pressing duties, Slemrod enlisted one of his best students, Christian Gillitzer, to coauthor this text.

After explaining its purpose, the book briefly reviews a succinct form of optimal tax theory, helpful for later chapters. It nicely generalizes how differentiated optimal tax rates equally balance "behavioral" and "mechanical" (i.e., revenue-raising) responses across rates.

The second section discusses less standard components of tax systems, starting with alternative behavioral responses. Taxpayers may illegally evade taxes by underreporting income and risking punishment, or legally avoid taxes through costly sheltering efforts that otherwise do not affect consumption (except through income effects). The next chapter explains administrative and compliance costs, responsible for an entire industry of tax preparers and much of the public's distaste for the tax code. In a discursive but informative chapter, the authors consider multiple nonstandard tax instruments, such as withholding, and information collection and distribution. A take-away from this section is that the things governments tax may be rather different from the things taxpayers ultimately care about. One consequence is that workers may respond to a tax cut very differently than to a wage hike of the same nominal value, violating a common restriction made in theoretical and applied work.

The third section puts these nonstandard instruments and elements together into a generalized "optimal tax systems" model. The upshot is that nonstandard instruments, however difficult they may be to quantify, should balance behavioral and mechanical responses in the same way as tax rates. These formulae must also factor in marginal changes in administrative and compliance costs, with the former being more onerous, as they are paid out of tax revenues. The rest of the section covers optimal tax bases and endogenous elasticities. An important point for optimal taxation is that those who appear poor on their tax returns may just have better avoidance opportunities, or be prone to measurement problems, blunting the case for redistributive taxation.

The authors dispel numerous myths about tax systems, including the so-called "remittance invariance theorem." In real-world tax systems, how tax payments (and information) are remitted does influence revenue collection and the after-tax prices faced by buyers and sellers. When England first made firms remit income taxes during the Napoleonic Wars, it doubled revenues, possibly changing the course of world history.

Milton Friedman expressed regret for irreversibly "ratcheting" up the size of the U.S. government by setting up its remittance infrastructure during World War II. Remittance and third-party reporting also explains why self-employed workers report spending over a third more of their ostensible incomes on food and charity than other workers with the same purported income; it has little to do with hunger or charity. These and many other issues should help the reader to realize that the general theory of tax systems is not small potatoes.

The book's coverage is broad yet concise, its writing is well-crafted and edited, and its discussions are lively and informed. We learn on page 106 that informative mailings "with a simplified layout and less repetition" get better responses from readers. The same is true for books, and this one is not completely free of first-edition imperfections. The book could be made even more readable through some minor reorganization and streamlining, particularly with the formal models. I thoroughly enjoyed the chapter on the tax base elasticity, which explains how both standard and nonstandard responses to taxes contribute equally on the margin to the excess burden of taxation. Yet, I was unsure why this chapter was sandwiched in the second section, as the third of four chapters. It contained formal results on tax-evasion engendered risk that might have belonged two chapters earlier.

The book is not short on mathematical models, and is best read by those with formal graduate training in economics. Not much background in public finance is required. It could be used as a standalone textbook, although if I were to teach a semester-long course on taxation, I would complement it with Bernard Salanie's *Economics of Taxation*.

Every public economist should learn the lessons covered by *Tax Systems*. These lessons would also be very useful to development economists and economic historians trying to make sense of the public sector, or to any economist trying to make sense of noneconomists' reactions to Tax Day. Reading Slemrod and Gillitzer's book may just be the most efficient, and possibly equitable, way to learn those lessons.

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K Law and Economics

Reflections on Judging. By Richard A. Posner. Cambridge and London: Harvard University Press, 2013. Pp. ix, 380. $29.95. ISBN 978–0–674–72508–9. *JEL 2014–0188*

Among economists, Richard Posner is best known as a member of the Chicago School and a founding father of Law and Economics. Posner is a prolific scholar. He has written or edited more than forty books and authored more than four hundred articles. He is the most cited legal scholar of all time (Shapiro 2000b), and his seminal treatise *Economic Analysis of Law*, first published in 1973 and now in its ninth edition (Posner 2014), is among the most cited legal texts published in recent decades (Shapiro 2000a).

Posner has amazingly done much of his academic writing "on the side." Since 1981, his day job has been as a federal judge on the U.S. Court of Appeals for the Seventh Circuit. Posner has been equally productive as a federal appellate judge. By his own account, Posner has heard oral argument in more than six thousand cases, read many more than fifteen thousand briefs, and written more than 2,800 published judicial opinions (p. 2).

In his most recent book, *Reflections on Judging*, Posner reflects on his experience as a federal appellate judge. Posner offers his (strong) opinions on a litany of subjects, including how appellate judges should decide cases, write opinions, and manage their staffs; how appellate lawyers should brief and argue cases; how to improve the trial process in the federal courts; and how federal judges should be selected and trained.

For the most part, Posner's reflections amount to a critique of the federal judiciary. Posner's main concern is that federal judges are not coping well with the increasing complexity of federal cases, the primary sources of which are external to the law and the legal system (e.g., scientific and technological progress). Rather than taking a pragmatic, realist approach to judging and grappling head-on with the challenge of rising external complexity, Posner bewails, many judges retreat to legal formalism and obfuscate their avoidance by needlessly complicating the law and the legal process. "They escape from [external] complexity into [internal] complexity" (p. 14).

Many of the particulars of Posner's critique are mundane (though oftentimes colorful). For instance, he complains that the "staff allocation in the judiciary is proportional not to need but to rank" (pp. 51–2). He denounces the 511-page *Bluebook,* the leading manual of legal citation, as a hypertrophic absurdity and decries judges' sheepish adherence to its byzantine rules. He laments judicial timidity about conducting Internet research, relying on visual material (diagrams, graphs, maps, photographs, and the like), and utilizing court-appointed expert witnesses in adjudicating cases. He criticizes the widespread practice of delegating opinion writing to law clerks, arguing that it is inefficient and engenders formalist opinions. And he bemoans the paucity and content of judicial training, both initial and continuing, warning that judges are "falling behind" (p. 351) and urging the Federal Judicial Center (the education and research agency for the federal courts) and the legal academy to do more to better equip federal judges to manage and adjudicate increasingly complex matters.

Other aspects of Posner's critique are more high-minded. For example, he ponders the complexities of federal criminal sentencing, concluding that it should be "evidence-based, not emotion-based or intuitive" (p. 70) and chastising the federal judiciary for "ignoring scientific and social scientific research" on the social benefits and costs of imprisonment (though he acknowledges an absence of consensus, both normative and empirical, on many aspects of the social welfare analysis) (pp. 67–8). He thoughtfully considers the role of the jury in federal trials and the ways that judges instruct them, fretting once more about judicial ignorance of the "immense social-scientific literature" on the subject (p. 301). He eruditely discusses at great length the jurisprudential subjects of judicial self-restraint and statutory interpretation, dismissing most forms of the former as formalist escapes from dealing with complexity (the exception being James Bradley Thayer's theory of constitutional constraint, which Posner extols as realist) and excoriating two theories of the latter—Justice Antonin Scalia and Bryan Garner's version of textual originalism, again as a formalist escape route, and Akhil Amar's theory of the unwritten Constitution, as an unprincipled (albeit not formalist) escape from the strictures of empirical reality. And he skewers civil recourse theory, a noninstrumental account of tort law expounded by John Goldberg and Benjamin Zipursky, as "antimodern" and "antirealist" (p. 358) and an illustration of a kind of "hermetic legal scholarship that cannot help lawyers and judges to master the complexity of modernity as it impinges on the law" (p. 366).

Although Posner discusses a number of modest proposals for reform, he fails to address long-standing calls for the imposition of a mandatory retirement age (e.g., Major 1966) or term limits (e.g., Cramton and Carrington 2006) for federal judges. This is a glaring omission in light of two facts. First, Posner acknowledges that part of the problem is that the federal judiciary is aging (due to the combination of life tenure and increasing life expectancy). Elaborating on his statement that judges are falling behind, Posner explains: "The problem is not caseload; it is case content. Judges aren't coping well with the increased complexity, mainly but not only scientific and technological, of modern society. . . . [W]e judges are not inhabiting this new world comfortably" (p. 351). Why? In part, it is because "many federal judges serve into their seventies and eighties and occasionally beyond, and elderly judges are likely to find today's rapid pace of technological advance particularly difficult to keep up with" (p. 346). Second, Posner has written previously on the subjects of aging judges (Posner 1995) and term limits (Posner 2008), and has plainly rejected the argument that the former justifies the latter. Yet in these prior writings, Posner did not consider the challenge of rising complexity. And so we are left wondering how or whether Posner would reconcile or change his position.[1]

[1] In fact, several parts of the book are repurposed material from Posner's prior extrajudicial writings. Although I agree with other reviewers that the stitching together of this material is not "uniformly successful" (Walsh 2014) and that the "seams occasionally do show" (Orthofer 2013), I also agree that the way this prior material is integrated into the book is valuable in that it "reveals Posner's understanding of the overall coherence of these extrajudicial writings" (Walsh 2013) in terms of the challenge of rising complexity. There would have been an equivalent benefit to Posner revisiting his prior extrajudicial writings on aging judges and term limits in the context of the book's unifying theme of complexity.

All in all, *Reflections on Judging* is a worthwhile read for economists with an interest in the law and the federal judiciary. I also recommend it to economists with an interest in Richard Posner in his capacity as a federal judge. Indeed, the book is quite personal—Posner frequently draws on his own experience as a judge, and the book opens (after an introduction) with an autobiographical chapter in which Posner recounts his path to the federal bench, including his higher education (Yale, then Harvard Law School), his early career as a lawyer (Supreme Court clerk to Justice Brennan, then stints at the Federal Trade Commission, Solicitor General's Office, and President's Task Force on Communications Policy) and as a law professor (Stanford for one year, then Chicago for life), and his appointment by President Reagan and Senate confirmation to the Seventh Circuit. However, I would not recommend the book to economists who are interested in Richard Posner solely as a law and economics scholar. *Reflections* is a book about the practice of judging, not about the economics of law.

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The Economics of Information Security and Privacy. Edited by Rainer Böhme. New York and Heidelberg: Springer, 2013. Pp. xiii, 321. ISBN 978–3–642–39497–3, cloth; 978–3–642–39498–0, electronic.

JEL 2014–0606

If all the solutions to online security flaws were laid end to end, they would add up to a complete removal of privacy. Of course, an extreme approach will not be adopted in most countries, and so online security remains one of the most vexing issues in electronic commerce today. And the need for solutions is urgent. Security issues infest almost every essential online activity today—electronic mail, video entertainment, electronic commerce, delivery of advertising, and more.

If there are no straightforward economic principles that determine all choices about privacy and security, what should an economist think? It is time to think hard. Fortunately, that is true of this book. It contains thirteen chapters that survey the landscape of economic understanding, and provide a taste of the directions taken by applied researchers on the frontiers of this issue.

To be clear, not very much unites these chapters except a broad interest in economic approaches to these topics. Each chapter can be read independently of the others. A few chapters present grounded empirical research, while others present simulations or behavioral theory. Every chapter displays careful application of theory within the context of institutional constraints, and thoughtful measurement of statistical evidence.

Chapter 12, "Measuring the Cost of Cybercrime," was my favorite chapter. An eight-author team builds on a study done for the UK government and estimates the losses from cybercrime. This estimate goes over broad territory—phishing, malware, copyright infringement, identity theft, online payment fraud, and all the defensive expenses affiliated with cleaning up this mess. The goal seems problematic from the outset: how could they possibly estimate a specific number with any reliability? Yet, the exercise requires and displays a lot of judgment (and courage), as it tries to get the order of magnitude in the right ballpark. Sure, there is room for improvement, but now every researcher has a framework to follow, as well as a baseline estimate.

Another favorite was the study by Neuhaus and Plattner of security fixes in several large software programs, such as Apache httpd. They frame a research agenda at the intersection of models and phenomenon. For example, as with any other investment, they expect diminishing returns to investing in security flaws. Yet, they do not find evidence of it. They also look for evidence that security software improves as part of engaging in an arms race, and, again, do not find what they expect. This is analytical social science at an early moment in an industry. Is there room for more conversation between modeling and statistical testing? Sure, but once again, the chapter lays out a good baseline from which to start.

The conversation with models also gets an interesting test in the chapter by Kelley and Camp. They examine a deceptively simple policy question: How can some security issues, such as viruses, be handled with limited resources? Using modified epidemiological models, their simulations suggest a public-goods approach to containing computer viruses, with targeted investment on some subpopulations. Once again, the chapter illustrates how modeling can play a useful role in framing policy options.

Three of the privacy papers are largely behavioral in approach, designing experiments to further understand user attitudes and responses to different privacy choices. It is a rich topic, because there seems to be a remarkable amount of variance in human attitudes about different dimensions of privacy. At the same time, it must be said that the literature on the economics of privacy is much bigger than the landscape surveyed by these three papers. Of the three, I best liked the experiments examining user willingness or reluctance to provide deeply personal information, such as a death in the family. As the experiment shows, many people resist talking about such topics, but some will do it if prompted in the right way.

Readers with a theoretical bent also have a few papers to choose from. My favorite was the paper by Demetz and Bachlechner, which examines principles for organizations making investments in security, which is a foundational question. It harkens back to a familiar theme; the absence of any sweeping principle from which to address all questions. They show the range of approaches taken by researchers, and identify the strengths and weaknesses of each. This is as close to a survey as any of the chapters get, albeit, the emphasis is on a question that has not been settled.

The book has more chapters than reviewed here, and different chapters will appeal to different readers. That is indicative of the state of economic research about security and privacy today. It is too soon to declare victory over many fundamental questions. This is not a topic that has yet refined its way to an accepted set of general lessons, so there is plenty of opportunity for more research. In that sense, this book is a good start for a researcher with an interest in economic approaches to online security issues.

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American Railroads: Decline and Renaissance in the Twentieth Century. By Robert E. Gallamore and John R. Meyer. Cambridge and London: Harvard University Press, 2014. Pp. xiii, 506. $55.00. ISBN 978–0674–72564–5.
JEL 2014–0981

The bankrupt Pennsylvania Railroad merged with the New York Central Railroad on February 1, 1968 to form Penn Central. In 872 days, Penn Central filed for bankruptcy protection. How did this happen? What was the impact of government regulation of the railroad industry, which started in 1887 with the establishment of the Interstate Commerce Commission? Why was the railroad industry in dire financial straits through most of the twentieth century, but thriving today? Why was the railroad industry eager to turn over passenger service to the newly formed AMTRAK? How did the railroad industry get where it is today and where is it headed the future? The answers to these and other questions are contained in the book *American Railroads: Decline and Renaissance in the Twentieth Century* by Robert Gallamore and John Meyer.

John Meyer was a long time Harvard professor who passed away before the book was completed. He was Gallamore's Ph.D. advisor at Harvard. Gallamore served in a variety of positions in the railroad industry, government, and academia. The idea for the book came from Meyer, who realized there are books about the nineteenth-century railroads, but none about the twentieth. This book is designed to fill that gap. It is a mixture of railroad history, the economics of government regulation, public policy analysis, and rail costing and pricing. It is not only about railroads, but about the economic history of the United States. They note in the first chapter, "[a] central theme of this book is that railroads, throughout their history, were so important to the U.S. economy that politicians could not leave them alone, and when governments did intervene in transportation markets, they usually made a mess of things" (p. 17).

The book begins with a primer on the economics of transportation, government regulation, and pricing. This chapter is written so as to make economics accessible to noneconomists. This includes a discussion of natural monopoly, cost-based and demand-based rates, value of service ratemaking, elasticity of demand, Ramsey pricing, and the authors' ten principles of transportation economics.

Public policy in the first half of the twentieth century is the subject of chapter three. The early part of the century featured attempts to combine railroads into more efficient and effective enterprises. These combinations were opposed by President Theodore Roosevelt in the trust busting era, and found illegal by the Supreme Court. Towards the end of the century, these same mergers were implemented and resulted in the creation of the BNSF and UPSP systems.

The railroad industry was taken over by the federal government during World War I. After the war, the railroads were given back to private hands through the Transportation Act of 1920. However, stringent regulation of the railroads continued. There were attempts to consolidate strong railroads with weak ones in the hopes of rate equalization and other goals. But, most of these attempts resulted in failure. Gallamore and Meyer show how the lessons from yesteryear can be applied to issues today, such as the debate between consolidation and regulation versus competition in pricing.

At the beginning of the twentieth century, railroads held a monopoly over long distance passenger service, with few exceptions. By 1970, passenger service was a not only losing money, but had deteriorated to such an extent that it was no more the elegant transportation mode as it once was. No more were the Hollywood stars long distance rail passengers. No more movies like "North by Northwest," which featured the New York Central's Twentieth Century Limited service from New York to Chicago. The book highlights the factors causing the decline of private rail passenger service and the creation of AMTRAK. The authors cite ICC regulation, the growth in alternative modes, which were heavily subsidized, the mix of freight and passenger service on the same lines, and public policy, which favored the airline industry.

Parallel versus end-to-end mergers are the subject of chapter 6. In the middle of the century, the ICC was more willing to approve parallel than end-to-end mergers. The authors argue that parallel mergers sought to achieve substantial cost economies, while end-to-end mergers were designed to facilitate improved service. The Penn Central disaster was a parallel merger that ended up in the nationalization of rail service in the Northeast and the creation of Conrail out of the ashes. Many factors contributed to the demise of the Penn Central and other Northeast railroads. The book goes into detail explaining each of these. In addition, there are a variety of case studies of railroad mergers in the mid-twentieth century with detailed descriptions of the genealogy of each. How did public policy react to the rail bankruptcies of the 1970s? What role did the federal government play in helping or impeding change? The book addresses many of the public policy issues and how sometimes government got it right, and most of the time did not.

One public policy that government got right is deregulation. This started with the 3R Act, then the 4R Act and then the Staggers Rail Act of 1980, which had a massive impact on the industry. Deregulation culminated in the ICC Elimination Act, in which the ICC was replaced by the Surface Transportation Board—or STB—with substantially diminished regulatory power. Gallamore worked in government when much of this legislation was passed and gives a firsthand account of the debates that took place in Congressional hearings and the discussions in and out of government on the merits of deregulation.

In the concluding chapter of the over 500-page book, entitled "Decline and Renaissance of American Railroads in the Twentieth Century" the authors provide a summary of the history of the railroads and the lessons for public policy in the future. This chapter is such a great summary, that the reader may be best off starting with it, before reading the book. But don't forget the afterword, which provides the authors' recommendations for future U.S. policies for the railroads. It is a very insightful chapter.

It is difficult to be critical of such an excellent work, which is very through and impeccably referenced throughout. The errors are ones of omission rather than commission. For example, Gabriel Kolko has developed a very different thesis of government regulation from the authors' public interest gone amuck theory. They briefly mention Kolko in two chapters of the book, but never discuss why they feel Kolko is wrong. Second, there is no discussion of short line and regional railroads which grew in importance after deregulation. It would be helpful to read the authors' analysis as to why these railroads are doing well, while the Class I railroads could not make money on these routes. Third, there seems to be a growing interest in high-speed rail in the United States. Can such service ever break even? What are the economics of high speed rail? Short lines and high-speed rail would make an excellent sequel to *American Railroads*. I hope Bob Gallamore will consider coming out of retirement to write such a book.

American Railroads should be on the reading list of economists interested in transportation and logistics, economic historians, government officials, and rail fans who would like to know more about the history of the railroads in the twentieth century, and are interested in understanding the economics of the industry and the problems of government regulation. Gallamore and Meyer, at the end of the book, sum up why it should be read:

> This book's authors love railroads because they have a great history, fascinating operations, intriguing technology and untold opportunity for the future, but we also love them because no other enterprises illustrate elegant economic principles quite so well (p. 435).

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Baseball on Trial: The Origin of Baseball's Antitrust Exemption. By Nathaniel Grow. Urbana and Chicago: University of Illinois Press, 2014. Pp. 282. $95.00, cloth; $35.00, paper. ISBN 978–0–252–03819–8, cloth; 978–0–252–07975–7, pbk.; 978–0–252–09599–3, e-book. *JEL 2014–0975*

Baseball on Trial is the first full-length treatment of the antitrust suits brought by the Federal League and the Baltimore Terrapins against Organized Baseball (the former name of Major League Baseball) during 1915–22. Grow's narrative is detailed, entertaining, and smart. I am not sure, however, that he has unlocked the secret to judicial missteps over the last twenty years, or that he truly vindicated the Taft Supreme Court of 1922, as he purports to have done.

The basic story is as follows. The Federal League (FL) was founded in 1913 as a minor league. At the end of its first season, the FL announced that it would seek major-league stars and expand play in eastern cities. That is, it would attempt to become a competitive rival to Organized Baseball. Over the next two years, thanks to higher salaries, the FL induced eighty-one major leaguers to jump to the fledgling league. Labor market competition led to a spike in the average salary of major leaguers from $3,800 in 1913 to $7,300 in 1915. The salary wars led to losses in both leagues, with larger losses in the FL.

Meanwhile, Organized Baseball was threatening those players who jumped to the FL with blacklisting. It also sought injunctions in court to prevent players from jumping. The floundering FL's only hope was to bring antitrust litigation against Organized Baseball, which it did in January 1915. The case was in Chicago in the courtroom of Kenesaw Mountain Landis, baseball's subsequent commissioner. Landis sat on the case for a year, issuing no decision and leading to a settlement between the two parties. The settlement paid the FL owners $600,000 and allowed two FL owners to buy into National or American League clubs.

The FL's Baltimore Terrapins, however, were virtually excluded from the benefits of the settlement. Organized Baseball eventually offered Terrapins' owners a $50,000 buyout, but they rejected it and filed an antitrust suit in 1916. The Terrapins won an $80,000 award (tripled to $240,000) plus expenses in trial court, but lost on appeal before the District of Columbia Court of Appeals in April 1921. In May 1922, the U.S. Supreme Court, headed by former president William Howard Taft, in a decision written by Oliver Wendell Holmes Jr., upheld the appeals court decision, holding that baseball was "a purely state affair," neither involved in commerce, nor interstate in its basic nature. Hence, Organized Baseball was immune to both the Sherman and Clayton Antitrust Acts.

Many books, of course, have told this story. *Baseball on Trial* distinguishes itself by taking the reader inside the courtroom and giving a blow-by-blow account of the legal strategies, the arguments, and the characters involved. Grow's account is not only engrossing, it is edifying.

Grow emphasizes the gravamen that ultimately formed the basis of both the DC Court of Appeals and the Supreme Court—that baseball did not constitute interstate commerce. Grow claims that previous baseball historians have been unfair to the Taft court. He asserts that the conception of interstate commerce at the beginning of the twentieth century was much narrower than it is today. To wit, Grow claims, as have others, that the conception of commerce in 1920 included only tangible goods, not services, and he points to a few court cases, such as *Metropolitan Opera Co. v. Hammerstein*, that sustain this view.

While others, including myself, have acknowledged this narrower and more ambiguous conception of interstate commerce, Grow goes a step further and vindicates the Taft court on this basis. I think here Grow overstates the case. First, there were other cases where the interpretation pointed to a broader conception of interstate commerce; and second, Grow's position minimizes an enduring tendency of the Supreme Court (and other courts) to find legal precedent to justify a decision that it wanted to make for political, philosophical, or personal reasons.

In the case of William Howard Taft, there was good reason to believe that his participation in the decision was heavily compromised: Taft's half brother was the former owner of the Chicago Cubs, who sold the team as part of the settlement with the FL; Taft was approached by Organized Baseball in 1918 to see if he would be interested in becoming the head of the sport's governing body; the American and National Leagues each sent the chief justice a season's pass to all games for the 1922 season, just weeks before the Terrapin's appeal was heard; and Taft was a longtime associate of George Pepper, who was baseball's lead counsel, and whom Taft had offered a seat on the Third Circuit Court of Appeals while he was president. Taft did not recuse himself from the case. Other justices had similar prior leanings.

Even for 1922, there was plenty of evidence and precedent to establish the business of baseball as participating in interstate commerce. Organized baseball received payments from equipment manufacturers to use their products. It received payments from Western Union for the right to transmit game scores over the wires. The 1921 World Series was broadcast over a radio relay from the Polo Grounds in Manhattan via KDKA (Pittsburgh), WBZ (Springfield), and WJZ (Newark). Even if the games, which involved half the players crossing state lines, were "purely state affairs," the business of baseball involved tangible goods that were sold on an interstate basis.

To be sure, Grow is a sophisticated analyst and he gives play to these arguments. He just seems to discount them in backing the Taft court. I have other differences with Grow's analysis, but they are minor.

The book should be read by those seeking to understand the origin and dimensions of baseball's antitrust immunity, and by any jurists who may be deliberating on this issue in the future.

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The People's Network: The Political Economy of the Telephone in the Gilded Age. By Robert MacDougall. Philadelphia: University of Pennsylvania Press, 2014. Pp. 332. $55.00. ISBN 978–0–8122–4569–1.

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In *The People's Network: The Political Economy of the Telephone in the Gilded Age*, Robert MacDougall argues that AT&T's bounteous corporate archives have led scholars to tell the history of the telephone from an AT&T-centric perspective. MacDougall's history of the Independents, a movement that provided a robust alternative to AT&T in the late nineteenth and early twentieth centuries, aims to widen the range of analysis in the literature. This is not a book for those interested in a comprehensive examination of the industrial organization of the Independents, nor does it provide a systematic overview of the changing regulatory environment in which the telephone operated. Instead, MacDougall weaves a narrative of the cultural, political, and commercial forces that shaped the development of the industry in the United States and Canada. MacDougall dismisses the idea that technology primarily determined the development of the telephone industry, instead arguing that its organization was "politically constructed," with "political institutions" providing "the environments in which individual actors had to fight for their interests and plans" (p. 175). The organization of the telephone network raised questions of "independence, interconnection, and scale" that reflected "arguments about the way the country ought to be organized" (p. 17).

MacDougall presents three alternative visions of how and by whom the telephone should be used, each of which was held by an early leader at Bell Telephone. Gardiner Hubbard, Bell's first president and subsequent advocate for reform, proposed a "telephone for the people" that was simple to use and locally owned. This vision helped to shape "a decentralized network of local and regional systems" that used Bell's technologies (p. 71). Hubbard mistrusted Western Union's size and political influence and thought the telephone would provide a "democratic good" to offset it. William Forbes, Bell's second president, had a conservative vision of the telephone that focused on rich customers and steady profits. His vision pushed Bell to provide high-quality service to a limited customer base. The third vision of "One System, One Policy, Universal Service" was advocated (somewhat retrospectively) by Theodore Vail, AT&T's first president. This vision of a single national network later became an important part of AT&T's public relations campaign to win back business and influence legislation that would reestablish its national monopoly.

At the heart of MacDougall's book is a comparison of the telephone networks in the United States and Canada. He uses two cities, Muncie, Indiana, and Kingston, Ontario, to illustrate differences in regulation, industrial organization, and telephone culture. The sociologists Robert and Helen Lynd studied Muncie in the 1920s and 1930s, dubbing it "Middletown" to reflect its ordinariness. Although Muncie and Kingston were similarly ordinary in many respects, telephone usage was not. In 1905, near the height of the Independent Telephone Movement, Indiana had one telephone for every twelve people, while Ontario only had one telephone for every ninety people. Indiana had over 1,000 independent telephone systems in 1907, while Bell Canada was a monopolist in every major urban center in Ontario in 1910. Deep differences in telephone culture also existed. In Ontario, the telephone was used primarily by upper class men doing business, while the independent telephone in Indiana was also used by women and children and for social purposes. "A Muncie farm girl in the early 1900s might have giggled for hours on the telephone, then got off the line to let the neighbors hear her father play his banjo for a spell" (p. 264). Bell Canada and AT&T charged for measured service while Independents charged flat rates. These differences reflected differences in regional cultures and regulation.

The history of the telephone in both Canada and the United States takes many unexpected turns. When Bell Canada's patent monopoly expired in 1885, it successfully sidestepped competition by petitioning the national government to amend its charter to declare Bell "a work for the general advantage of Canada," giving "the company virtual immunity from both private competition and municipal reform" (p. 177). Yet regional politics and discontent with Bell Canada's service led to "the bumblebee of communications," seven regional monopolies that interconnected with local operating systems to create a national network. It should not have been able to fly, claimed Bell engineers, but it did, offering an alternative to the centralized AT&T system. In the United States, a Bell monopoly turned into a competitive landscape with "over thirty-two thousand active telephone companies outside of Bell's control" (p. 110). Regulation rose from the municipal to the state and finally to the federal level, and as it did, the vision of the "telegraph for the people" lost ground while that of a national network gained. By World War I, "natural monopoly" rhetoric was gospel. A brief stint of nationalization served to solidify AT&T's position, and the once important Independents became a "protective fringe" around AT&T's monopoly position.

Was it "economies of scale" or "politics of scale" that ultimately determined the organization of the telephone industries in the United States and Canada? MacDougall contends that "Telephone monopoly in Canada was never truly 'natural'; telephone competition in the Midwest was never truly 'free.' Both were political outcomes, established and maintained by regulation and litigation" (p. 130). He makes a convincing case that the three distinct visions of the telephone—as a social and democratic tool for the people, as catering to business, or as a unified national network—played differently in different political cultures, helping to shape the organization of the industry and the uses to which the telephone was put.

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The Creation of Inequality: How Our Prehistoric Ancestors Set the Stage for Monarchy, Slavery, and Empire. By Kent Flannery and Joyce Marcus. Cambridge and London: Harvard University Press, 2012. Pp. xiii, 631. ISBN 978–0–674–06469–0. *JEL 2014–0648*

In contrast to most social science research, anthropology considers the full range of human social systems, and looks deep in time, using archaeology to probe social and economic processes beyond the reach of historical records. In light of the increased focus on the political economy of inequality in academic, policy, and activist circles, an examination of the origins of, and cross-cultural variation in, inequality is timely.

Kent Flannery and Joyce Marcus (hereafter, F&M) are both eminent archaeologists, each with a lifetime of research in many settings; here, they are clearly writing for the general reader. The volume ranges globally, and over the full span of our species' history (nearly 200,000 years), but with an emphasis on the last few millennia, when most social complexity and inequality arose and spread. The prose is clear, even folksy at times, and generally free of specialist terminology. There are many line drawings (of archaeological sites and artifacts), but no tables or graphs, let alone equations, and the few numbers are mostly dates, population sizes, or site dimensions. The bibliographic notes are copious and indicate wide familiarity with archaeological and ethnographic literature on particular societies; citations to the theoretical or analytical literature are much more selective, even idiosyncratic (on which more, below).

F&M aim to take stock of what we have learned since Jean-Jacques Rousseau penned his famous *Discourse on the origin of inequality* in 1753. They draw on the two main branches of anthropology, ethnography and archaeology, utilizing their relative strengths—archaeology for long-term change and material evidence, ethnography for detailed descriptions of functioning societies, including their ideologies. Like Rousseau, F&M prefer to focus on flesh-and-blood actors struggling to impose or resist systems of inequality. The book title itself heralds this focus: "creation" rather than "emergence" or "evolution," implying the priority of agency (conscious change) in explaining the origins or causes of inequality. This focus is both a strength and a limitation.

As F&M note, "our earliest ancestors were all born equal, but the Ice Age had barely thawed when some of them began surrendering bits of equality" (p. 547). Any explanation of this recurrent dynamic must address two striking puzzles:

1) Why do the simplest human societies, small-scale foragers (hunter-gatherers) and horticulturalists (low-density, low-capital farmers) lack systematic inequality based on dominance, given that the latter is found in all social vertebrates, including our primate relatives?

2) How did such egalitarian societies persist for at least 180,000 years and spread all

> over the globe, yet become unstable or vulnerable enough to evolve into or be replaced by the hierarchical systems that have prevailed in recent millennia?

F&M raise these questions, but do not satisfactorily address them. This is due to their dismissive view of theory (beginning with their preface, where they compare theory to perfume that should just dabbed on lightly), as well as their view that changes in norms and ideologies—what F&M term "social logic"—are the main drivers in the emergence of systems of inequality. The result is an account that is full of rich details about *how* past societies were organized and changed over time, but very little attention to *why* this variation was patterned over time and space.

Noting correctly that the relatively recent and rapid emergence of systems of inequality "did not require genetic evolution," they argue that this resulted from a change in "social logic" that can be analyzed using social anthropology. By implication, evolutionary biology is irrelevant, and even theory and findings of other social sciences (economics, political science, sociology, game theory) are given almost no attention. Their approach is essentially inductive: describe multiple similar societies, summarize their commonalities, and derive a set of propositions that describe the "social logic" of these cases. Then do the same for another set of societies with increased inequality, and extract the changes in social logic needed to get from set A to set B.

As one example of a primordial social logic, F&M repeatedly cite the maxim "we were here first," used by many groups to justify priority in land rights, kin group rankings, and the like. But they simply note it, attempting no explanation, and make no mention of the economic literature on property rights, nor evolutionary game theory models indicating that a convention of first-come first-served can outcompete either open access or fighting for every resource patch encountered, occupied or not (Maynard Smith and Parker 1976; Gintis 2007). This is one of many examples of how F&M's disciplinary provincialism and aversion to theorizing limit their ability to explain the patterns they so richly describe.

However, F&M cannot resist offering explanations for some regularities, and reinvent various theoretical wheels in doing so. For example, they chide unnamed scholars for characterizing hunter–gatherer land-sharing arrangements as "altruism" that might be genetically evolved, proposing instead that it might be "self-serving investment, a way of obligating their guests to host them in the future" in situations of locally fluctuating resources (p. 34). But the latter is precisely what various analyses have proposed (e.g., Smith and Boyd 1990), drawing on the Trivers (1971) model of "reciprocal altruism"—not really altruism at all, but rather delayed reciprocity, an iterated prisoners' dilemma.

If there is a dominant explanatory thread in the book (beyond the question-begging appeals to shifts in social logic), it is the "dynamics of competitive interactions" (p. 473) as a driver of increased social complexity and inequality. In this, F&M stand with many who have researched the topic. According to F&M, the first steps towards social inequality arose when certain hunter–gatherers developed clan systems (social groupings larger and more permanent than extended families, and based on real or fictive common descent). These clan systems created cooperative units that could outcompete less organized societies, either directly in warfare or indirectly in sharing food and information. F&M describe similar increases in scale (and resulting inequality) for later stages in social evolution (e.g., chiefdoms, states, empires).

F&M note that "societies with agriculture and animal husbandry do seem to create more opportunities for inequality" (p. 67) but offer only cursory attempts to explain how and why. Research showing that material wealth such as land and livestock is both more important to such societies, and more readily transmitted to offspring (Borgerhoff Mulder et al. 2009; Bowles, Smith, and Borgerhoff Mulder 2010) is perhaps overlooked because of its recency, but older research addressing this question also goes unmentioned. F&M do provide cogent summaries of how certain hunter–gatherer groups in California and the Northwest Coast of America were able to build systems of inequality without agriculture, through control of trade or rich fishing grounds by a subset of the population. Yet even here they seem to privilege ideological factors over material ones, for example noting that Northwest Coast foragers like the Nootka exhibited a "surprising...

level of inequality" compared to other foragers, but "many principles of Nootka inequality could have been created out of the preexisting principles of egalitarian foraging society. All that would have been required were appropriate changes in social logic" (p. 74). Taking this statement at face value would imply that a Nootka-like system of hereditary aristocracy, commoners, and slaves with elaborate systems of property rights for the titleholders was within the reach of any foraging society that chose to make the requisite changes in social logic, a most dubious idea, given the antiquity and stability of egalitarian hunter–gatherer systems. When F&M do consider material wealth flows among Northwest Coast foragers, they favor an argument involving enslavement of debtors that is both illogical (why would debtors be more common in a setting they characterize as resource-abundant?) and at odds with known facts: as F&M note, slaves among Northwest Coast societies were war captives or offspring of same, not debtors. (They were also, we might add, important sources of labor power for surplus production, as well as goods that could be traded or given as presents, or even destroyed in displays of conspicuous consumption.)

F&M are also quite dismissive of environmental drivers of social change. A striking example of this occurs in their discussion of Pueblo societies of the American Southwest; while noting the paleoclimatic evidence for drought coinciding with the abandonment of many major Pueblo sites, they downplay its importance, speculating that "a long-standing desire for equal treatment... overcame hereditary privilege" (p. 160). But there is a mass of evidence that the century-long drought in question had region-wide effects on Pueblo abandonment, migration, warfare, and trade contraction; one need not be an environmental determinist to give this factor its due. In another passage F&M argue that Peru is "a graveyard for theories of environmental determinism," and cite the example of the Chavin chiefdom that inhabited a high-altitude, agriculturally unproductive valley—but then note in passing that this polity was strategically located "along a trade route linking the Pacific coast, the Andes, and the Amazon basin" and was thus "the midpoint for the long-distance movement of goods among three major cultural provinces" (pp. 246–47).

In sum, F&M provide a very readable and well-informed summary of ethnographic and archaeological evidence on the variation in systems of inequality found in our species. The volume promises to address the key puzzle of why systems of inequality arose and spread only in the last 5–10 percent of our species's history, but largely fails to do so. Fortunately, other researchers are making significant progress in this important task; no doubt they will find the empirical information summarized and categorized in this volume very useful.

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The House of Rothschild in Spain, 1812–1941. By Miguel A. López-Morell. Translated by Stephen P. Hasler. Studies in Banking and Financial History. Farnham, U.K. and Burlington, Vt.: Ashgate, 2013. Pp. xviii, 449. $144.95. ISBN 978–0–7546–6800–8.

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López-Morell's book is a very detailed, fine-grained narrative of the workings and withdrawal of one of the most prominent international financial houses in Spain during a crucial and often misunderstood period in the country's history. The house arrived in Spain in the wake of Napoleon's overthrowing of the Bourbon King in 1808, which triggered the institutional, political, fiscal, and monetary turmoil that reigned in the country until 1856, at least. As the book shows well, the hundred-or-so years in which Rothschild was vested in Spain were consistently a time of protracted political and macroeconomic volatility. From the 1890s, the house started declining; it first lost its assets in railways with the nationalist drive of the 1920s and Primo de Rivera dictatorship—though the mining investments in Rio Tinto and Peñarroya survived well until the Great Depression.The fatal blow was the failure of its agent in Madrid in 1931—at a significant loss—in the onset of the disturbances leading to the Civil War. By 1941, the Spanish branch of Rothschild was greatly diminished and hurried to leave the country, sharing the fate of the larger family inflicted by rising anti-Semitism in other European countries. Nevertheless, Rothschild managed to liquidate the remainder of their businesses without complete losses under Franco's regime.

The archival work behind the book is truly painstakingly done and the wealth of empirical information collected transpires in every page. There is a significant amount of relevant data in tables and graphs: although their presentation is far from ideal, in particular for purposes of comparison over time and across sectors (e.g., various currencies, unclear indication of current or constant values, profits over the long run could be expressed as a proportion of GDP, etc). The translator has deftly dealt with the nuances in the grammar of Spanish language, and the book is quite readable despite the myriad of details, names, and events. Thus, a narrative too close to the domestic political developments may appear unnecessarily complicated for the unacquainted reader; instead a more general background on the fiscal and financial situation, the position of the treasury and the broad macroeconomic circumstances at different points would have shed a brighter light on the decisions that public officials and the house made. This would have made the arguments much more intelligible. Otherwise, claims such as those for the end of the businesses: the "failure in the generational take over" and "the rise of nationalism in Spain after World War I" had a somewhat ad hoc flavor, for instance.

The first three chapters build on secondary literature and offer a broader, more coherent historical context than the rest of the book—admittedly more abundant scholarship published on the period 1811–1855 has assisted the author in the task. The following seven chapters are the empirical core of the book; organized chronologically, they survey in minute detail the actual involvement of the house in the financing of government in times of war—for which Rothschild secured the monopoly over quicksilver from the Almaden mines early in the 1820s—the investment on railway construction, the continuous short-term lending to liberal governments in the hectic environment of the 1860s, the more direct "industrial" investments after the 1870s in the mining of pyrite (a critical input to produce sulphur), copper, and lead, and even the management of the royal family's financial portfolio. Sometimes there is far more on the participants than on policies, but introducing too many individuals and idiosyncratic ideological labels does not conceal almost identical fiscal and monetary policies among the different administrations, all of which characterizes the chronic public finance and currency messes of nineteenth-century Spain. However, these ill–defined features are the legacy of a political historiography that Spanish economic historians have so far been reluctant to take on.

The last two chapters have a more comprehensive analytical approach and reveal more on the Rothschild than on the *politicos*. Chapter 11 sums up the "fundamentals" of the house activities, although the analysis of the structure of decision making and of changes over time in the relative importance of different assets and investments in the Rothschild portfolio is a bit shallow and disconnected from changes in the economy, the businesses of the other branches in Europe, and the overall international economic environment. Some of these fundamentals deserved more attention, e.g., the control of the supply of quicksilver, vital for smelting gold and silver, ought to have helped the house to enjoy the upper hand in the arbitrage in bullion trade in European markets throughout the nineteeth century; similarly, the timing for the decisions to pull out from state financing and to move to direct investment in the 1870s could have helped to distinguish changes and opportunities in the domestic environment. Chapter 12 does a long-run balance of the Rothschild years; in a very condensed fashion (pp. 386–404), it surveys the aggregate effects in the Spanish economy, the contribution to domestic growth, to sectors and markets, and the "cost of investments." The treatment of these issues was what the book was missing all along. The briefer "final reflections" sum up the role of foreign capital for the development of the Spanish economy. This was a topic of controversy, in the 1970s, about the roots of Spain's relative backwardness, in which some of the pioneers economic historians of Spain intervened. The book makes several timid references to this literature, but López-Morell is unfortunately not conclusive about it.

Yet, the book will be valuable reading for those economic historians of Spain concerned with the public finances of the various political experiments of the century, the monetary consequences of expansionary fiscal policies, the fiscal and financial impact of the Spanish American War, and the overall fiscal capacity of the Spanish modern state. Although the discussion of the economic sectors in which Rothschild was invested is a bit too general, scholars will find a multitude of leads to explore further—particularly the relations of Catalonian and Basque vested interests with the protectionist efforts of the so-called nationalist governments of the 1920s. More general business and financial historians will be able to gather an immense amount of empirical details from this book. They should be intrigued by the house propensity to seek a monopolistic position in each of the ventures they pursued and the relative lack of adaptability when facing an adverse political climate or outright competition. López-Morell is admittedly ambiguous about how Rothschild organized and articulated their various investments (pp. 378–381). He has engaged little with other popular histories of the house, which probably appeared after the author did the original research; however, López-Morell could have served more from them to flag his own contribution and the differences of the Spanish branch of Rothschild for this English edition.

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O Economic Development, Innovation, Technological Change, and Growth

Of Medicines and Markets: Intellectual Property and Human Rights in the Free Trade Era. By Angelina Snodgrass Godoy. Stanford Studies in Human Rights. Stanford: Stanford University Press, 2013. Pp. xiv, 183. Paper. ISBN 978–0–8047–8560–0, cloth; 978–0–8047–8561–7, pbk. *JEL 2014–0669*

Lawyers and economists have recently paid a great deal of attention to the globalization of intellectual property rights. The 1994 Agreement on Trade-Related Aspects of Intellectual Property Rights, (the TRIPS agreement), established minimum standards for intellectual property protection in all member states of the World Trade Organization.

Nations outside the OECD accepted the agreement on the understanding that they would in turn receive improved access to OECD markets, especially in agriculture and textiles. They also recognized that failure of the agreement would expose dissenting nations to serious bilateral trade sanctions from the United States. In fact, the promised agricultural trade benefits have not been delivered. The post-TRIPS Doha meeting occurred at a time when an abortive lawsuit to prevent use of generics to fight AIDS in Africa was making international headlines, and was further complicated by the anthrax emergency in the United States, whose administration pressured the maker of a key drug to make it accessible on reasonable terms around the time when it was arguing against compulsory licensing at Doha. For the pharmaceutical industry, the Doha Declaration of 2001 ended less favorably than anticipated. It postponed compliance dates for low-income countries and allowed for some compulsory licensing and national production and use of generics in emergency circumstances.

Despite their presentation of TRIPS as an alternative to high-pressure bilateral confrontations, the European Union and the United States subsequently proceeded to pursue bilateral "TRIPS+" agreements prescribing higher levels of protection of intellectual property than mandated in TRIPS, and in some cases higher than in force in either the United States or the

European Union. They began with small nations with little capacity (or in the puzzling case of the Australian Howard government, little inclination of the political leadership) to resist the pressure to accept stronger rules than mandated in TRIPS. So now many non-OECD nations are constrained not only by TRIPS, but also by subsequent bilateral agreements, and multilateral agreements including importantly CAFTA.

How have things turned out? In the near-absence of solid empirical evidence, much of the legal and economic research has had to focus on the agreements as written, deducing their implications for accessibility and cost of medications and medical devices and services in nations that are not members of the OECD. The health effects for them appear, based on the documents themselves, to be overwhelmingly negative. Sanctions on trade in generic drugs limit the markets for a previously thriving generic pharmaceutical industry centered on India and foreclose access of other countries to imports of modern low-cost pharmaceuticals. There may be some improvements in availability of new patented drugs in these countries, but at prices that can be afforded only by the relatively wealthy. International price discrimination is limited by the possibility of informal reexporting, and by existing "most favored nation" agreements with countries often already more favored in other ways.

Of course, formal agreements are one thing, but implementation is quite another. Those opposed to TRIPS have had some reason to hope that inevitable discretion on implementation will mitigate its harsher distributional implications. Indeed, news reports suggest that Indian implementation is less effective than drug manufacturers would like. Its economic power in general and its expertise in pharmaceutical manufacture give it more latitude in translating the words of the agreement into reality for Indian society. Brazil has also had some success in pursuing its own interest despite the TRIPS agreement.

There has been much less discussion of implementation in smaller nations. Godoy's book shows that, in Central American nations, no news has not been good news with respect to access to pharmaceuticals on reasonable terms. Her focus is mainly on three nations: Costa Rica, with universal health care, El Salvador with a history of popular engagement on health issues, and Guatemala, with the largest economy of the three. All are included in the regional CAFTA agreement, which she characterizes, perhaps optimistically, as the "high water mark" of intellectual property protection after TRIPS.

Godoy's serious empirical research for this book is backed by her earlier analyses of the implementation of CAFTA in all six CAFTA countries, and her prior role in the creation of Red CEPIAM, the Central American Network on Intellectual Property and Access to Medicines, which addressed the effects of Intellectual Property Rights (IP) on the right to health, a common commitment in the three countries that are the focus of this book. For this book, she covered a lot of ground in each of the three. Her interviews include trade negotiators, pharma executives, public health advocates, physicians, patients, legislators, health sector unions, human rights lawyers, judges, state health officials, and IP officers. The results should be of interest to readers identifying with any of these groups, and to others working in economic development.

After a nice introduction outlining the multifaceted issues she addresses and describing the evolution of her research strategies, in chapter 2 she offers a readable primer on relevant intellectual property issues. Chapter 3 is a gem, a font of valuable and eye-opening information on the implementation of IP and, in particular, CAFTA in the three countries. She reminds us that CAFTA adds tougher obligations than TRIPS and indeed goes beyond U.S. legislation in mandating five years of data protection to compensate for unreasonable delays. It burdens public health officials with obligations to enforce private IP rights and prosecute infringement with criminal as well as civil penalties.

However some of Godoy's toughest critiques are aimed at national health administrations, legislators, and the courts. Guatemala, for example, granted fifteen years of retroactive data protection to me-too drug Nexium, and to the essential heat-stable AIDS drug Kaletra, unpatented in Guatemala. In 2009, ninety-eight drugs had test-data exclusivity in Guatemala against nineteen in El Salvador and only five in Nicaragua.

Chapter 4 enlightens the reader with the local politics of IP, pharmaceuticals, and health in nations noted for a long-standing political commitment to health rights. The survey yields fascinating information on the ambivalent positions of generics producers. For example a Salvadorian generics producer and continental generics industry leader, student of anti-IP academic Carlos Correa, privately accepts CAFTA. Indeed, several industry leaders were right-wing ex-presidents during the civil war; distrust of or animosity to leftist health rights advocates appears to trump immediate commercial interests. A more general key issue is lack of trust in public regulation of the purity and safety of drugs. This makes branding perhaps more important than IP. "Generics" are generally branded in Central America. In El Salvador, generic drugs were reportedly priced on average at thirty times their international reference prices. No wonder that there were no stable and effective coalitions between health advocates and generic producers during negotiations of CAFTA.

Godoy moves in chapter 5 to a very distinct issue, patients' rights litigation, focusing on indepth discussion of Guatemala based on her compilation of decisions on 271 cases. Guatemala has constitutional guarantees of health rights, and the human rights ombudsman's office litigates patients' rights cases. The vast majority of ninety-two cases on access to medicine were resolved in favor of the plaintiff. However, many cases granted the patient the right to use or keep using a branded version of a drug when a generic was available. Not surprisingly, pharmaceutical companies facilitated appeals of this kind. Apparently the presence of the individual plaintiff (often middle class) was more persuasive to the judiciary than the abstract interest of the nation in low cost health for all.

Indeed, the book is strikingly effective in reframing well-worn policy controversies: "[F]ar from focusing excessively on state accountability, the global access movement targets major drug companies and the Northern Hemisphere governments who do their bidding, while granting the states of Central America a free pass" (Godoy 2013 p. 16.). And what is needed of the states in question? "Particularly as regards social and economic rights, sometimes the challenge requires empowering a reticent state rather than retraining a repressive one" (Godoy 2013 p. 5).

Godoy turns in chapter 6 to a discussion of the evolution of global rights, including rights to health. The result is less satisfying. She offers an amalgam of informed empirical observations and what, to me, are unhelpful and unconvincing associations with political theorists of the left. When she locates the source of modern human rights organizations in the struggles with Latin American dictatorships, she misses the crucial role of European reactions to the trampling of rights in the Soviet Union.

This highly informative book is pessimistic regarding the prospects for distributing the benefits of modern pharmaceuticals to needy populations in Central America. It is therefore a shock to find a table showing that nations in the region have all experienced remarkable gains in life expectancy and drops in child mortality between1990 and 2006. Is full access to modern drugs really so important to the continuation of the remarkable progress achieved in recent decades?

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P Economic Systems

Mixed Fortunes: An Economic History of China, Russia, and the West. By Vladimir Popov. Oxford and New York: Oxford University Press, 2014. Pp. ix, 191. $40.00. ISBN 978–0–19–870363–1. *JEL* 2014–1434

In development economics, there are no three more important questions than (1) Why did the West get ahead of everybody else? (2) Why did Russia fail to catch up? and (3) Why is China succeeding now? Vladimir Popov endeavors, in his slender volume of fewer than 200 pages, to answer all three. There are very few economists as well placed to do so as Popov, currently at the United Nations, and previously a professor at the New School of Economics in Moscow and Carleton University, Ottawa, and lifelong student of China. Let me first give Popov's answers to the three questions before I discuss them.

For Popov, development is escape from the Malthusian trap: higher overall income is not "dissipated" into population growth without increasing mean per capita income over the long term. He dismisses popular explanations that see the forces behind the West's breaking of the Malthusian trap in institutional change (Landes 1998, Mokyr 2002) or "serendipity" of geographical and climatological accidents (Diamond 1997, Pomeranz 2000). He prefers an older explanation, going back to Karl Marx and Karl Polanyi. In Popov's view, the Malthusian trap is broken through "elimination of collectivist institutions [which gives] rise to increased inequality which in turn boost[s] savings and investments and the capital/labor ratio" (p. 20). The Western "big push" thus destroyed the "collectivist" institutions and replaced them with individualistic profit-maximizing agents (English enclosures come to mind). It was a costly approach because it increased poverty and mortality, but it eventually worked.

But why could not the same scenario be applied to the rest of the world? According to Popov, attempts at modernization, which were often done through colonization, failed because they broke indigenous institutions. This just increased poverty and inequality, but rather than leading to development, degenerated into a comprador capitalism (p. 52). In terms of prescriptions for long-term development, Popov, somewhat fatalistically, argues that "the Rest" should keep (or should have kept) its own "collectivistic" institutions and waited until global (meaning Western) technological progress has advanced sufficiently so that it can break through the Malthusian trap by increasing productivity while avoiding greater inequality and poverty.

On the second big topic (why did Russia fail?), Popov essentially repeats the argument that premature Westernization failed in Russia as it did elsewhere, because it destroyed autochthonous institutions (we are talking here of the Russia between Peter the Great and Pyotr Stolypin's early twentieth century reforms) while the return to collectivist institutions under socialism failed because central planning could be successful only during the first investment "generation." Afterwards, political incentives were such that planners preferred to invest in new plants rather than retool the old ones: that led to an excess of capital per worker, artificial shortages of labor, and low elasticity of substitution between capital and labor, resulting in low growth.

And why did China succeed? Again, this follows directly from the answers to the first and second questions: Because it waited (obviously not consciously) to start modernization until the early twentieth century, and because it kept planning for thirty years only. Thus, China, largely accidentally, on both counts did exactly what was best: it did not start modernization too early and it jettisoned central planning just as it was becoming inefficient.

Let me now review these main conclusions, which are here, of course, presented in their barebones form. There is a lot of empirical evidence, discussion of the actual modernization episodes, and general nuance that cannot be covered in a short review. What I called a "fatalistic" approach to development, where a developing country instead of trying to catch up with the West would sit and wait until conditions became better is, in my opinion, neither feasible nor useful for policymakers. When Egypt began its modernization drive under Mohammed Ali or Madagascar under Radama I (both in the early nineteenth century), it could not have known what Popov today argues to be true.

But perhaps the best rebuttal of the "do nothing until the time is right" hypothesis comes from the success of Japan; not the one after World War II, which Popov acknowledges, but the one between 1850 and 1940. Japan modernized well, preserved most of its own institutions, generated investible surpluses, and basically played according to the Western textbook (as Morishima (1984) nicely documented in an old, but still relevant, book). I also believe that the same argument could be made for Russia: had it avoided the war and the Bolshevik Revolution, it is likely that its high rate of growth could have been maintained and country fully modernized. So, early modernization *à la Occidentale* was not necessarily doomed.

It is, however, also true that Russia, under collectivistic socialism, experienced high growth and industrialization (which, as Popov writes, made it possible for the USSR to resist and ultimately defeat the second strongest industrial power at the time, Germany), but that effort eventually petered out. Popov's rather technical explanation for the Soviet slowdown is not on the same level of abstraction as the rest of his thesis. I do not find it particularly convincing either. There is no rule, I think, that would make planning efficient for thirty years only and never again. Rather I think that planning failed, not because of its intrinsically different efficiency in the 1930 versus the 1970s, but because the nature of technological progress changed. To be ahead of the curve in the 1930s, you had to build dams, which central planning did pretty well; to be ahead of the curve in the 1970s, you had to build cars to please consumers, which planning could not do.

This brings us to China, which Popov has extensively studied, and with which he is as acquainted as with the Soviet Union/Russia where he lived and worked. I find compelling Popov's argument that Mao's policies set the necessary basis for China's success: political independence from the West, higher education level (and not based on sterile rote Confucianism, but on more applied sciences), and longer life span of the population were all elements without which Deng's reforms would not have succeeded. Deng simply built upon the (checkered) legacy left by his predecessor. Many people are prone to forget the success of China, as compared to India for example, even during the Maoist period—that is, success, despite the misnamed "Great Leap Forward" and Cultural Revolution. By 1976, Popov reminds us (p. 63), Chinese life expectancy was sixty-five years, thirteen years more than in India.

I would be remiss if I were not to mention an important and interesting innovation that Popov brings to the study of institutions. Instead of relying on subjective assessments of institutional strength (perception of corruption, rule of law), Popov proposes that we should look at two simple indicators: the murder rate and the size of the shadow economy. In well-ordered countries both are low; in weak institutional settings both are high. Their use makes sense too: high murder rate directly challenges government monopoly on violence, high share of informal economy likewise challenges government monopoly on taxes. Both, therefore, show that institutions and enforcement are weak. Popov's analysis is particularly convincing when he uses these two indicators to illustrate the dramatic institutional collapse that happened with the transition in Russia (an already high murder rate tripled), as well in England during the Industrial Revolution (the murder rate increased and life expectancy went down by five years over a century). China, on the other hand, scores much better on both, on par with the rich world, and on the murder rate better than the United States. As Popov mentions, in many respects China displays the features of a developed country, despite the fact that its income level is around one-third that of the developed West.

Popov's book is short, but I think that it will have, per page, strong influence on how we look at economic development over the long term. Those who are interested in broadening their perspective and possibly challenging some of today's tropes, especially regarding the importance of property rights and the role of Mao in China's emergence, will find Popov's book useful both in what they shall learn from it and even more in extending further some of the arguments, only sketched but not fully explored, by the author.

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Q Agricultural and Natural Resource Economics • Environmental and Ecological Economics

U.S. Energy Policy and the Pursuit of Failure. By Peter Z. Grossman. Cambridge and New York: Cambridge University Press, 2013. Pp. xvii, 397. $36.99, paper. ISBN 978–1–107–00517–4, cloth; 978–0–521–18218–8, pbk.

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Most of this book is a blow-by-blow and quotation-by-quotation account of the efforts in consecutive sessions of the U.S. Congress and congressmen and by successive presidents to deal with the public perception of continuing U.S. energy crises after 1973. Of course, there never were energy crises, apart from occasional and temporary oil price spikes and brief gasoline shortages. In particular, the embargo on oil exports to the United States by Arab OPEC members in 1973 had no effect in either 1973 or early 1974, when the embargo was lifted. According to the statistics of the Energy Information Agency, oil imports rose monotonically until 1978. Why then were there long lines at the gas stations? Simply because, wanting to keep their gas tanks close to full, drivers stayed in line in order to fill their tanks much more often than previously. This was no secret in 1973, though not generally recognized, even within the U.S. government.

The long lines at gasoline stations and occasional price spikes burned a deep scar in the public memory. The apparent crisis symptoms, occasional spikes in gasoline prices, and the long lines were the source of persistent political efforts to, "do something." The political debates were extensive and heated, and any number of proposals were made and some were enacted. This reviewer has only a vague, newspaper-informed memory of the episodes and cannot give an opinion as to whether the detailed accounts of the arguments are accurate. But Thomas Schelling, who is a careful professional, provides an approving back-of-the-book blurb, which gives me confidence in the book's story.

U.S. energy policy failed, as the book's title proclaims, because the policy-making apparatus failed. The first task in making policy is, certainly, to identify the issues, which were market failures. That identification was never done clearly. The book characterizes the kinds of problems that lead to market failure: monopoly power, public goods, information asymmetries, search frictions, and unanticipated risk. All of these features, and, perhaps, more beside, exist in energy markets. By comparison, the policies offered to deal with the market failures were often simply to limit or direct demands through quotas and regulation of supply channels and, in some cases, uses, such as CAFE standards. There is a case for the latter because of the public good features of automobile and truck transport, but CAFE was intended as a demand limitation. There was never an attempt to reduce the riskiness in supply, which is inherent in oil markets. A comparison with banking regulation illustrates the point. Banks are required to hold prescribed levels of reserves just because of potential volatility in their portfolios. Reduction in volatility of oil prices in some circumstances would have also benefited from larger petroleum stocks, but an efficient and equitable requirement, which would be difficult to design, was never considered.

The solutions offered to the nonexistent crises often only aggravated problems. It is striking that the politicians did not try to educate the public out of its perception of crises. Nor, by default, did the popular press succeed, perhaps for a lack of trying, in correcting the popular view. By comparison, there were certainly academic specialists who understood the energy situations clearly. They were either not consulted or not persuasive.

After the turn of the century, it has become clear that the world is floating on oil. Domestic oil production in the United States has reduced imports drastically and the United States would become an oil exporter if a blocking law were done away with. Oil from Kazakstan is held up only by some engineering problems with its pipeline. More oil will come from Iraq, if only its internal strife were alleviated. Reforms in Mexico are underway to increase its oil production. Venezuela continues to be a question mark, but the world is doing well without its previous large exports. Demand continues to increase, but the Great Recession, which continues in Europe, has reduced it. China's demand is expanding, but the rate of expansion is slowing with the rest of the economy and it is trying hard to expand its hydraulic fracturing potential.

In the 1990s, a real energy crisis did emerge, but the need to, "do something" has been much less unanimous than in the case of the phony energy crises. Still, the reality of global warming and the need to reduce greenhouse gas emissions is starting to be more widely recognized. In order to reduce the emissions and resulting atmospheric concentrations of greenhouse gases, it is necessary to reduce the burning of fossil fuels, including oil. Yet, as the author makes clear, policies to deal with this crisis have had less general support than was true of earlier energy policy and, in turn, are often inefficient and ineffective. Again, potential increases in the price of gasoline have been a scarecrow.

The economics in the book never tries to be above the level of Econ.100. The value of the book is in its detailed review of popular and legislative history of attempts to formulate energy policy. So the book's audience will not, primarily, be economists, but the students of political policy making. For them, it will be useful.

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Nature in the Balance: The Economics of Biodiversity. Edited by Dieter Helm and Cameron Hepburn. Oxford and New York: Oxford University Press, 2014. Pp. xx, 416. ISBN 978–0–19–967688–0.

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Overall, this edited volume represents the very best compendium of current thinking on the economics of biodiversity. But it is a difficult book to summarize and evaluate. It consists of fifteen chapters of uneven quality (with fair overlap), written by more than forty authors and coauthors, totaling over 400 pages. The central theme is the economics of biodiversity, but the coverage spills over into neighboring subjects like ecosystem services, resilience, natural capital, national accounting, sustainability, preservation strategies, instruments and incentives, international and developmental consequences, and so forth. While I personally consider the economics of biodiversity to be important, perhaps even very important (especially in the light of climate change), it is a frustratingly elusive subject. This volume itself reflects the immense diversity of the economics of biodiversity. The book is divided into five sections: Concepts and Measurement; Valuing Biodiversity; Natural Capital and Accounting; International and Development Aspects; and Policy Instruments and Incentives.

What is biodiversity? This is a critical opening question. In its broadest terms, biodiversity (=biological diversity) refers to the variety or variability of life on earth. But economics typically wants measurement. The leadoff chapter, written by scientist Georgina Mace, is entitled: "Biodiversity: Its Meanings, Roles, and Status." This leadoff chapter is particularly clearly expressed. It is a great jumping-off first piece for anyone trying to learn about the subject. Mace skillfully elaborates that which we sort-of knew (and sort-of feared): the units of biodiversity are themselves many and varied, being highly dependent on the observer and the context. She writes:

> Different disciplines favour different measures of biodiversity. Ecologists tend to think about biodiversity in terms of the forms and functions of organisms in a place, especially in a community or an ecosystem, because it is the structuring of varieties in space and time that leads to functions and dynamics that they seek to understand. Evolutionary biologists similarly think about the dynamics, but with an increasing focus on the historical or inherited variation, and therefore the genetic and phylogenetic attributes. Conservation biologists are sometimes concerned with function and process, as they should be, but often also with preservation of species or genetic diversity, seeking efficient and achievable solutions of the allocation of limited resources. For nature conservationists and wildlife managers, biodiversity often simply means the maintenance of wild habitats and species (p. 39).

Economists love to conceptualize problems in terms of optimizing an objective function subject to scarcity constraints. But as the above quotation makes clear, we are unable to clearly define an objective measure of biodiversity, or its value, upon which there is even rough agreement. This is a core problem for the economics of biodiversity. We must proceed as best we can with partial measures of the biodiversity we are seeking to preserve. Nothing in the book is able to satisfactorily get around this core dilemma.

There is a further problem. "Biodiversity" and "ecosystem services" are often confounded as if they are the same thing. But they are not. One school of thought focuses on ecosystem services and emphasizes the extrinsic value to humans of conserving ecosystems. A second school of thought is that conservation should be based on ethical arguments about the intrinsic value of nature, so that biodiversity should be conserved for its own sake. In an excellent chapter entitled "Are Investments to Promote Biodiversity Conservation and Ecosystem Services Aligned?" authors Stephen Polasky et al. pose some operational questions about the two concepts and give some operational answers. Using data collected by the state of Minnesota, they measure ecosystem services by carbon sequestration and water quality. They measure biodiversity by richness of vertebrate species. They find a high (but not perfect) degree of alignment between strategies that target the value of ecosystem services and those that target habitat for biodiversity conservation. I recommend this chapter highly, for its conceptual insights and its constructive empiricism.

Another chapter that demonstrates some constructive paths of analysis even when the objective of biodiversity or ecosystem services is incomplete or fuzzy is by Ian Bateman et al. entitled "The UK National Ecosystem Assessment: Valuing Changes in Ecosystem Service." The primary purpose of the UK NEA, which involved more than 600 scientists from a wide range of disciplines, was to assess the state of the United Kingdom's ecosystems and the services provided by them as they have changed and will change over time. Six development scenarios are studied, which have varying degrees of environmental impacts. The chapter attempts to assess the impacts of the six development scenarios on such ecosystem services as food production, carbon storage, biodiversity (using birds as an indicator species), recreation, and urban greenspace. The quantification is rough, but, according to the authors, the analysis "sparked considerable interest in the media and had a substantial impact on UK environmental policy" (p. 79).

Two chapters cover "Natural Capital" and "Biodiversity and National Accounting." These chapters are instructive summaries of what is known and what has been done on an aggregate or national level. They offer useful insights, but the difficulties of actually adjusting real-world national income accounting to reflect biodiversity and sustainability represent formidable obstacles. This same problem of assessment haunts two other chapters devoted to the subject of valuing biodiversity more generally.

The chapter "Identifying and Mapping Biodiversity: Where Can We Damage?" by Kathy Willis et als. addresses the question of prioritizing the various unprotected landscapes in the world by ranking those that most need protection. I felt that the most useful part of this chapter consisted of a fairly comprehensive inventory of the data sets and tools currently available to assess worldwide spatial patterns of biodiversity and ecosystem services. The authors conclude that, while more data is desirable, a lot of usable data already exists, but it is not being used to its full potential.

The two chapters on international and development aspects are interesting but a little vague. The four chapters on policy instruments and incentives are also interesting and perhaps a little less vague.

Anyone interested in the economics of biodiversity should turn first to this book. Its contribution is to enumerate comprehensively all major aspects of current thinking on the economics of biodiversity. Its limitations are the inherent difficulty of the subject itself.

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Power to the People: Energy in Europe over the Last Five Centuries. By Astrid Kander, Paolo Malanima, and Paul Warde. Princeton Economic History of the Western World series. Princeton and Oxford: Princeton University Press, 2013. Pp. x, 457. $39.50. ISBN 978–0–691–14362–0. *JEL 2014–0708*

Power to the People provides a rich account of Europe's energy history. The depth and breadth of analysis enables the authors to cast new light on some central questions of economic history related to the Industrial Revolution. It also offers a wealth of new data about energy consumption in Western Europe over the last 200 years, which is available on the publisher's website and (although lacking source information) will be a wonderful resource for research projects on the long-run relationship between energy and economic growth.

For instance, the data shows that energy intensities in European countries differed greatly. England and Germany experienced very high levels of energy use per unit of GDP as they industrialized heavily in the nineteenth century. Later industrialization in other European countries did not lead to such dramatic increases and some, such as Italy and Sweden, experienced only declines in energy intensity from the 1800s on. For early industrializers, energy requirements were especially great. Iron production and, more generally, manufacturing, required vast amounts of heat and power for several reasons. They were not only manufacturing products for themselves, but also exporting. Also, they were using inefficient technologies. Furthermore, they had access to large and cheap coal reserves, which, the authors argue, is one of the key factors in enabling the Industrial Revolution and allowing early industrialization. By the time the French, Italians, or Spaniards were industrializing on a large scale, they were able to adopt more efficient technologies, their manufactured goods were not in great demand abroad, and they were more limited by domestic coal resources. Faced with such evidence, and mindful of the threat of climate change, one might draw partially optimistic conclusions about energy intensity trajectories in currently industrializing economies.

No doubt responding to the threat, in the last six years, many notable books and journal articles have been published on the history and long-run developments in energy markets (e.g., Fouquet 2008, Allen 2009, Ayres and Warr 2009, Wrigley 2010, Smil 2010, Mitchell 2011, Grübler and Wilson 2014, and Jones 2014). Yet, to those in this field, the completion of *Power to the People* was greatly welcomed because the authors, Astrid Kander, Paul Warde, and Paolo Malanima, are leading scholars in the field of energy history and because of the ambition of their project—to bring together historical data on energy use in a host of European countries.

When investigating the history of energy, a number of different perspectives can be taken: energy technologies, consumption, production and supply industries, political economy, the macroeconomic effects, the geopolitical implications, or the environmental impacts. *Power to the People* fits into the second of these strands, while offering insights for the grander history of three Industrial Revolutions. It brings together and synthesizes the evidence produced by a Tony Wrigley-inspired network of European economic historians focusing on energy. It covers, in three parts, the impressive growth in energy consumption in Western Europe (a seven-fold increase in per capita consumption since 1800) and the spectacular energy transitions (from biomass fuels to coal and to oil, natural gas, and electricity) that occurred since the First Industrial Revolution. Each part is written by one of the authors: Malanima studies the preindustrial period, Warde analyzes the First Industrial Revolution (1760–1830), Kander assesses the Second and Third Industrial Revolutions (post-1870).

Malanima emphasizes the constraints of the mostly agrarian, "organic," and renewable energy system, where land and climate as well as transport costs, bound population and economic growth. He highlights how the Dutch and then the English managed to intensify their agriculture, exploit power sources for milling and sea-faring (and, thus, trade), and exploit new, more dense energy sources, particularly peat. Both Malanima and Warde propose that the development of coal resources and associated technologies was stimulated by rising energy prices and efforts to save land and labor. Warde supports Allen's (2009) argument that the concentration of coal reserves heavily influenced the geography of European industrialization, which has recently been corroborated by the econometric analysis of Fernihough and O'Rourke (2014), who disentangle the direction of causality between energy and economic growth. However, Kander, Malanima, and Warde emphasize that the role of energy in economic growth probably changes at different levels of economic development, and may be waning in twenty-first century highly-developed economies.

Emerging as a recurring theme, Warde and Kander conceptualize energy systems as part of "development blocks," in which the success of an energy source and its growth in use were closely linked to other technologies and industries. These energy systems (such as the coal block, consisting of coal, steam engines, and the iron industry, later, the internal combustion engine-oil block, the electricity block, and, most recently, the ICT block) created mutual markets for each other's products, achieving economies of scale and declining costs. They argue that the implications of energy transitions depended on the market suction and market widening created by the links between, for example, oil demand and the car, and oil demand and oil tankers, respectively. As with their discussion on energy services, the authors do not use the key sources on the subject—here, particularly the coevolutionary ideas of Freeman and Louça (2001), and Geels and Schot (2007). This is unfortunate because it is an important reminder of the dynamic interdependence of resources, technologies, and industries, which economists too often ignore because of the difficulties of providing robust analysis. If a coherent economic theory could be developed, we might begin to build a bridge between past energy transitions, as discussed in this book, and, for example, encouraging possible future low-carbon transitions.

While the final, integrating chapter summarizes key points in the book nicely, it does not lead to a new understanding of the long-run evolution of energy markets. Analyzing eight different countries over hundreds of years at three different phases of economic development was an opportunity to identify patterns and draw conclusions, potentially of value for designing new theories of the relationship between energy use and economic growth, and for formulating policies related to future energy transitions and phases of economic development. Instead, it represents a generally well-crafted economic history, offering a great resource for economists to exploit and to draw new conclusions.

With many insightful graphs, plus useful explanatory boxes for the less initiated, it is highly accessible, and recommended to undergraduate students curious about the history of energy, to postgraduates specializing in a specific field, and to academics.

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