Journal of Economic Literature 2014, 52(2), 535–567
http://dx.doi.org/10.1257/jel.52.2.535

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.

B History of Economic Thought, Methodology, and Heterodox Approaches

Jacob Viner: Lectures in Economics 301. Edited by Douglas A. Irwin and Steven G. Medema. New Brunswick, N.J. and London: Transaction, 2013. Pp. vii, 159. $45.95, paper. ISBN 978–1–4128–5166–4. *JEL 2013–0680*

The history of Chicago economics has not yet ceased to attract the interest of economists and scholars of economic thought. Extensive inquiries have been laid down on the origins of a tradition of thought, research, and teaching that has been able to propagate its ideas and reasoning style well beyond the United States, to set the stage for heated debates in the realms of both economic theory and policy, and to fill the list of Nobel Prize Laureates with many of its distinguished pupils.

Douglas A. Irwin and Steven G. Medema's edition of Jacob Viner's lectures is a valuable contribution to the understanding of how some of the main features of Chicago Economics took shape. They offer a remarkable and high-quality testimony on how microeconomics was taught in the early 1930s by one of the most distinguished (and eclectic) neoclassical economists of the interwar period. When Paul Samuelson was an undergraduate student at Chicago, the relatively young university, settled in front of Lake

Michigan, was widely reputed to host "the best Department of Economics of the country" and Economics 301 was its "Holy of Holies" (1972, 5); Milton Friedman recalled his own attendance of Jacob Viner's Econ 301 as "the greatest intellectual experience of my life" (Breit and Hirsch 2009, 70).

According to Irwin and Medema, Economics 301, taught alternatively by Frank Knight and Viner, played a relevant role in nurturing what was later renown as the "Chicago price theory tradition"; Econ 301 can be regarded as "the hallmark of this inculcation, the core graduate course in price theory that long differentiated the Chicago approach." The "main thrust" of this approach "was an emphasis on Marshallian, partial equilibrium economic reasoning, and the use of this theory to understand real-world phenomena" (pp. 1–2). As an acute scholar and a follower of Marshallian economics (in whose development he gave notable contributions especially in the field of price theory, methodology, and international trade), Viner certainly played a central role in establishing the main features of Econ 301 along the above mentioned lines.

The availability of the excellent lecture notes taken by Marshall D. Ketchum during the 1930 Summer term allows us to have a "nearly verbatim transcription" (p. 2) of the lectures Viner had delivered in Econ 301. According to the editors, Ketchum's notes probably correspond to those cited by Samuelson as circulating when he was a student (1972, 7).

After a sketch of Jacob Viner's career as an economist in Chicago (1925–46) and Princeton (1946–60) (pp. 3–4), Irwin and Medema offer a lively collage of notable students' testimonies on Viner's peculiar and impressive teaching style (pp. 4–6); then they present the main results of their inquiry into the origin of the notes, their diffusion in different libraries around the country, trying to assess their reliability. A short correspondence between Viner and Ketchum preserved at Princeton's library, documents the origin of the notes, as well as Viner's mixed attitude toward their use by students (pp. 6–9). A last section of the introduction deals very briefly with the contents of the lectures, which are mainly divided in two sections: the first one concerning demand, supply, and industrial equilibrium; the second one devoted to a deep discussion of the theory of income distribution. Correctly, the editors point out that the former contains "more of Viner's original work," following "the treatment given in his famous 1931 article" on *Cost curves and supply curves* and stressing "the distinction between external and internal economies." Passing attention is devoted by Viner to the theories of monopoly and oligopoly. Medema and Irwin underline how, in Viner's opinion, "even monopolies face competitive pressures and are always subject to some price pressure," which is also a landmark of the following Chicago price theory (p. 9).

The second part of the course "basically present[s] and evaluate[s] the theories of other economists by calling attention to differences in approaches and assumptions," ranging from the classical school to Francis Amasa Walker, John Bates Clark, and other major contemporary authors. Viner is very polite in expressing his own opinions, such as his preference for English over Austrian value theory; his praise to the Lausanne School avoiding "circular reasoning" in the determination of factor rewards; his regards for Fisher's theory of interest as being "definitive." A thorough discussion with some links with contemporary economic problems concerns the relationship between wages and unemployment, which, according to Viner, should not be thought of as univocal, at least in the short run. It seems worth noticing that, in a brief discussion of profit theory, "Viner introduces the class to the work of Knight and Schumpeter."

As noted above, Irwin and Medema mainly focus on the influence of Viner's course on the following developments of Chicago price theory: yet something could be added concerning how this course came to be structured and refined and how its role grew in accordance with the shaping of the Chicago Department along the lines of a neoclassical, but still pluralistic, approach to the teaching of economics. In this respect, valuable information may come from the analysis of archival and official sources concerning the teaching offered at Chicago during the 1920s and 1930s.

In the period up to 1918, the department of economics hosted, both as faculty and graduate students, a whole array of individuals closely associated with the foundation of institutionalism: Thorstein Veblen, Robert F. Hoxie, Wesley C. Mitchell, Walton H. Hamilton, Harold Moulton, and John M. Clark. While a number of the institutionalists had already left Chicago before 1920, other figures—such as Leon C. Marshall, James A. Field, and Chester Wright, who had a weaker but still significant connection to the movement in its early days—marked the Chicago economics department in the first decade after the war. These men, with their teaching, research, and the close network they had established, left a clear institutionalist mark in the department of economics. Clark played a decisive role in impressing a vivid institutional twist to the teaching of general economic theory. In 1923, for example, he taught courses on Value Theory, History of Economic Thought, and Unsettled Questions in Economic Theory—largely a review of contemporary institutionalist literature—along with courses that clearly reflected his own research agenda, such as those on the Theory of Overhead Costs, and Social Control of Business.

After 1925 a significant change occurred. Viner, to whom the whole section of the courses in Public Finance had been entrusted since the early 1920s, began teaching a course in 1924 on neoclassical economics—the course that will eventually evolve into Econ 301. Interestingly, the course was presented as "a study of the general body of economic thought which centers about the theory of value and distribution and is regarded as 'orthodox theory,'" and it was intended as "preparatory to a more critical examination of this body of doctrine." The "critical examination" was left to Clark's course on Modern Tendencies in Economics that offered a "critical study of controversial questions in the general body of orthodox theory, and of some modern departures from orthodox theory." In 1926, Clark left Chicago for Columbia; James A. Field retired in 1927; while Leon C. Marshall left in 1928 to become a professor of law at Johns Hopkins. The same years other leading figures of the so-called "first Chicago School" joined the faculty: Henry Schultz in 1926, Frank H. Knight and Henry C. Simons in 1927, Lloyd Mints in 1928 (Emmett 2004: VII). In 1927–28, Viner took over Neo-Classical Economics, History of Economic Thought, and Modern Tendencies in Economics. In 1929 Schultz and Yntema were assigned to offer all the statistics courses, while Schultz also began to teach the newly established Mathematical Economics. This is the year in which the course on neoclassical economics changed its name into "301. Price and Distribution Theory": one among the streams contending the stage of interwar American economics was thus openly acknowledged as the dominating one. The Neo-Classical turn in the Chicago teaching of economics was firmly established.

Despite the growing dominance of neoclassical economics, Rutherford (2011: 142) points out that the intellectual influence of institutionalism continued to persist in Chicago through most of the 1920s and early 1930s. This pluralistic character clearly emerges from the courses' presentation and syllabi, and from the PhD qualifying. Knight, who taught Economics 301 alternating with Viner[1], was also offering a course in "Social Critique and Evaluation", in which:

> The original problem of modern economics, namely that of "social control" versus "laissez faire" is taken up for examination. The assumptions underlying the theory of free competition are made explicit and compared with the conditions of actual economic life, with a view in appraising the results of competitive individualism as a system of organization. As far as practicable the question of possible alternatives in the way of legislation and administration voluntary co-operation, opinion, education of public, etc., will be kept in view and the study carried out on a comparative basis. [Viner's 301 was a prerequisite]. (Course Catalogue 1929–30, Regenstein Library, University of Chicago)

Moreover, a quick glance at the PhD qualifying exams on economic theory reveals that students were requested to show their expertise in pure theory, as well as their familiarity with institutional economics and the history of economic thought.[2] For instance, in 1932, students

[1] Knight taught Economics 301 in 1933–34, 1934–35, 1938–29, and 1939–40.

[2] A complete series of PhD qualifying exams for "Economic Theory" was found among the Albert G. Hart papers at the Rare Books and Manuscript Library of Columbia University. Hart had been a graduate student at Chicago from 1931 to 1936, and he had completed his dissertation under Knight.

should write "the equations of general economic equilibrium, clearly specifying your 'givens,' your unknowns, and your fundamental assumptions" and to derive a demand curve through ordinary least square regressions of quantities of corn and beef on their prices. At the same time, the qualifying test also included questions on the unifying characteristics of the classical school and a quite engaging query on:

> Briefly state your conception of the nature of institutional economics, its relation to the price-type theory type of economics, and its value to the student. Comment on the work of Adam Smith, J. S. Mill and Alfred Marshall with regard to the adequacy of the consideration given by them to institutional factors, and discuss the desirability of replacing the standpoint of Marshall by that of any institutionalist or group of institutionalists. (PhD Qualifying in Economics, University of Chicago, Albert G. Hart Papers, Rare Book and Manuscript Library, Columbia University, Box 3).

This brief excursion over the history of Chicago interwar economics shows that the establishing of the Chicago school—as conceived today—was by no means a linear process and that more work on the history of the Chicago tradition needs to be done. More unpublished sources need to be made available to historians as Irving and Medema have excellently done in this small volume. After all, as Kinght once warned, "we know little of how traditions get established, while it seems clear that once established, a tradition does not get changed through calling attention to its absurdity, or that of factual assumptions on which it rests" (Knight 1955: 272).

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Luca Fiorito
Università Degli Studi di Palermo

Sebastiano Nerozzi
Università Degli Studi di Palermo

Michał Kalecki: An Intellectual Biography: Volume I, Rendezvous in Cambridge 1899–1939. By Jan Toporowski. Palgrave Studies in the History of Economic Thought Series. New York: St. Martin's Press, Palgrave Macmillan, 2013. Pp. xi, 184. ISBN 978–0–230–21186–5.
JEL 2013–0977

Michał Kalecki is an enigmatic economist in many respects. Besides anticipating Keynes's *General Theory*, he is credited for paving the way for connecting imperfect competition to business-cycle analysis, designing the first macrodynamic model unifying mathematics, statistics, and economic theory, as well as developing a theory of political business cycle.

Following the publication of the seven volumes of *Collected Works of Michal Kalecki* under the editorship of Jerzy Osiatynski, Mario Sebastiani (1994) and, more recently, Julio Lopez and Michaël Assous (2010) have investigated the logical consistency and originality of Kalecki's ideas. In this brief volume, Jan Toporowski attempts to reverse the trend by giving priorities to biographical materials, correspondences, and contextual elements. By embracing this point of view, this book succeeds well. There is no doubt that it sheds a new light on Kalecki's early vision of the capitalist economy.

The book focuses on the period 1899–1939. Chapters 1 and 2 are about Kalecki's early years, with a clear focus on the political context of Poland. Chapter 3 shows how Kalecki came to a systematic understanding of macroeconomics through economic journalism and the publication of papers in both political and economic journals. Chapters 4 and 5 make clear how Kalecki came about working in a particular community grounded in the practice of statistics, formal modeling, and economic theory. Chapters 6 and 7 take up Kalecki's ideas about socialism and the intrinsic instability of market economies. A chapter follows this on Kalecki and Swedish economists. Two chapters then investigate Kalecki's journey in London and Cambridge. A chapter on Kalecki's 1939 *Essays* then leads into two concluding chapters on Kalecki's works at Cambridge and Oxford.

What this book boils down to is that Kalecki mostly interacted with three kinds of groups, each with its own vision and political background. The first was a socialist community; the second was a group of high-level academia and monetary economists connected to the main Polish academic journal *Ekonomista*; the third was a group of econometricians. In reality, these groups were not impermeable, but instead, they overlap. The main contribution of this book is to reveal that Kalecki was the main link between these groups, making him eventually an economist at the crossroads of Marx, Wicksell, and Frisch, even though Toporowski does not express it in these terms.

There is a significant connection between Kalecki and Marx, as well with socialist economists like Hilferding and Luxemburg. Besides resorting to Marx's class categories, Kalecki explicitly related the principle of effective demand to Marx's problem of realization. However, Kalecki departed from Marx and his followers by emphasizing the possibility of the "reproduction" of the system. In Kalecki's opinion, the capitalist economy was prone to stagnation, but not systematic crisis. Generally speaking, the economic system is locally unstable and globally stable. Furthermore, Kalecki claims that instability issues had nothing to do with the dynamics of market power and class struggle. Whatever the intensity of income distributive conflicts, the economy will continue fluctuating. Toporowski shows how Kalecki's economic ideas were embedded in his political writings. With this respect, his book provides undoubtedly a better understanding of Kalecki's vision.

With respect to the connection of Kalecki with monetarist economists, Toporowski's argument is less compelling. The book documents how Kalecki eventually, after economic journalism activities, came up in 1929 working at the Warsaw Institute for the Study of Business Cycles and Prices, and how he finally intensively interacted from 1933 with the new recruit monetary economist Marek Breit. In 1934, one year after the publication of his *Essay in Business Cycle Theory*, Kalecki addressed, for the first time, an article for the purpose of Polish academics. This paper is critical insofar as it reveals how Kalecki tried to connect his approach to Wicksell and, more generally, to general equilibrium theories in vogue in Polish universities. In Toporowski's opinion, the true significance of 'Three Systems' is a critique of Wicksellian general equilibrium, supposedly established by price and wage flexibility" (p. 79). This paper, rather, reveals Kalecki's attempt to synthesize his and the Wisckeslian approach. By providing a new general equilibrium analysis of the determination of output, Kalecki's central purpose was to explore the interaction between the goods, labor, and money markets in a constrained demand economy, rather than refute mainstream economics. In this context, Kalecki advanced the "Keynes effect" as an argument why lower prices and wages may suffice to restore output to its full employment level. Along these lines, a falling price level raises real money balances, decreases the nominal rate of interest, increases investment, and provides a stabilizing positive feedback effect.

It is only after the publication of the *General Theory* that Kalecki really criticizes the possibility of an automatic tendency to return to full employment based on this effect. Meanwhile, Pigou and Haberler had advanced the real balance effect as an argument for why lower prices and wages suffice to restore full employment even if an increase in the real money supply could not cause any further reduction in nominal interest rates. Kalecki (1944, p. 132) pointed out that the real balance effect applies only to the monetary base and not to inside money (bank deposits backed by loans rather than reserves). Furthermore, Kalecki stressed the potentially destabilizing effect of lower price level: deflation increases the real value of existing debt, increasing the incidence of bankruptcy, thereby reducing the level of aggregate demand. If the effects of bankruptcy are strong enough, a lower price level is likely to move the economy away from full employment. In sum, price changes may potentially destabilize the system, regardless of the degree of price flexibility and changes in expectations.

By collaborating with the Institute's statisticians and his director Edward Lipinski, Kalecki attempted to assimilate statistics, mathematics and economic theory into a single unified framework, bringing him into Frisch's econometric research program in 1933. In chapters 13 and 14, Toporowski tackles an important issue in the early development of this program. He examines Kalecki's works at the Cambridge Research Scheme, described as having "some potential to realize Keynes's critique of Tinbergen's" (Toporowski 2013, p. 138). Regrettably, Toporowski's analysis of the specificity of Kalecki's approach leaves the reader craving for more. Did Keynes see in Kalecki's approach a good compromise between his own vision, which he sometimes described as "medical," and Tinbergen's mechanical approach? It is to be hoped that the author will elaborate more along this line in his next volume.

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Michaël Assous
Université Paris 1 Panthéon-Sorbonne P.H.A.R.E.

E Macroeconomics and Monetary Economics

The Great Depression of the 1930s: Lessons for Today. Edited by Nicholas Crafts and Peter Fearon. Oxford and New York: Oxford University Press, 2013. Pp. xiv, 459. ISBN 978–0–19–966318–7. *JEL 2013–1014*

The Great Depression left an indelible mark on the world economy. Beyond the sudden decline and slow recovery of output and prices, nations were forced to reconsider their financial, fiscal, and trade policies going forward. Indeed, many economists blame the lack of experience and slow response of regulators for the depth of the Depression. Crafts and Fearon have taken on the challenge of not only explaining the details of the 1930s, but also forming recommendations for the Great Recession and future crises. To assist them, they have assembled one of the best collections of Great Depression scholars available. The book is not a collection of previously published papers. Rather, authors summarize and extend the historical literature on a single topic (e.g., Federal Reserve monetary policy, Germany's default on reparations, British fiscal policy) before connecting it to the recent crisis in a meaningful way. The self-contained chapters allow authors to link and update many of their own seminal studies, making for an informed narrative and some interesting self-reflection.

The authors argue that the observance of the Gold Standard after World War I was responsible for the length and depth of the Depression. The few countries that accumulated gold after the War (i.e., the United States and France) immunized the flows instead of allowing inflation, while countries losing gold had no option but to deflate. This uneven adherence put pressure on the world economy over the 1920s, especially as Europe was rebuilding and Germany was struggling to make reparations. During the economic downturn, countries became even more faithful to the rule and monetary authorities (with the Fed being the most obvious example) often raised interest rates to protect gold reserves, rather than lowering them to prop up the stalling economy. It was usually not until a country got off the Gold Standard *and* undertook expansionary policy that it began to recover. This is not a new explanation for the Great Depression, but the intricate detail and consistent narrative improves on even the foundational work of Friedman and Schwartz (1963).

The world has changed dramatically since the 1930s, but there are still many relevant policy prescriptions that can be carried forward. For instance, countries are no longer constrained by the Gold Standard, yet the European Monetary Union installed a similar fixed exchange rate amongst its members. In fact, the Union might even be worse, as it does not allow for the suspension of the fixed rate during crises. Therefore, similar to the 1930s, the authors suggest that struggling countries need to be allowed to undertake their own monetary policy, or else need to be supported by stable countries. For the United States, the authors celebrate the quick and creative use of expansionary monetary policy during the Great Recession, which ran contrary to the tentative and protective approaches taken during the 1930s. They, however, warn that large-scale and reactive regulation such as the Dodd–Frank Act could cause long-term and unanticipated problems similar to surviving New Deal institutions such as deposit insurance and social security. Refreshingly, the authors highlight the turbulent political landscape and difficulty in passing certain types of legislation, rather than making recommendations in a vacuum.

Despite the sheer volume of topics, the book focuses more heavily on the United States than other countries. With the notable exception of chapters relating specifically to Britain and Germany, the few other chapters devoted to non-U.S. topics cover Europe as a whole. No chapters were written on how Canada avoided large-scale bank failures, or the many other unique country experiences. The "lessons" gleaned from the Depression also tend to focus on dealing with the U.S. financial crisis (which has been somewhat mitigated over time) rather than the European debt crisis, whose solution continues to elude regulators. As a result, some of the connections to the Great Recession are reflective in nature, commenting on how modern regulators have improved upon past mistakes rather than offering new prescriptions.

The book does not purport to be a study of the Great Recession, but more detail would be especially helpful for the events and policies examined by each author. The introductory chapter provides readers with a quick outline of the Great Recession, but it is rather sparse when compared with the narrative of the Great Depression. This would not have been a problem, except that subsequent chapters also primarily focus on the 1930s. Authors tend to come to the modern crisis only at the end of their chapters, and based on the nature of each topic, they pick up at different points in time and from different perspectives. In this way, some background knowledge of the current crisis is helpful when trying to fully comprehend the modern policy implications.

Crafts and Fearon have put together a tremendous examination of the causes and failed policies of the Great Depression. Their detailed historical account operates as a significant reference volume and narrative for readers new to the topic, as well as those who have some prior knowledge. The book also provides informative discussions for policymakers on how to handle modern financial crises.

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Matthew Jaremski
Colgate University

H Public Economics

The Other Welfare: Supplemental Security Income and U.S. Social Policy. By Edward D. Berkowitz and Larry DeWitt. Ithaca and London: Cornell University Press, 2013. Pp. x, 279. $45.00. ISBN 978–0–8014–5173–7.
JEL 2013–0764

The Other Welfare tells the story of the formulation and implementation of the U.S. Supplemental Security Income (SSI) program, a nationwide federal assistance program for aged, blind, and disabled individuals with low income. The authors, Edward Berkowitz, Professor of History and Public Policy at George Washington University and Larry DeWitt, former Public Historian for the U.S. Social Security Administration, provide an inside-the-Beltway perspective on the policy vision, political dealings, and social expectations that gave birth to SSI. Indeed, the book is a who's who of Congressional representatives and Washington policymakers. For those familiar with the SSI program, the details in the book will shed some needed light on the legislative wrangling that produced the program's cumbersome and often confusing structure. For those unfamiliar with SSI, the book is a well-documented reminder of the difficulties of efficiently and effectively managing federal income support programs across changing political and social environments.

SSI was enacted in 1972 and began paying benefits in 1974, replacing a collage of state-run programs. As the authors describe, the establishment of SSI was the culmination of a four-year debate over a more fundamental proposal for welfare reform—the Family Assistance Plan (FAP). FAP was a universal negative income tax program proposed by President Nixon as an alternative to the array of state and federal categorical welfare programs that provided a mix of cash, in-kind benefits, and services to specially targeted groups. Congress eventually rejected the universal nature of FAP, concerned in part about the potentially negative impact on employment. In its place it passed SSI, a federal categorical welfare program paying cash benefits to the subset of low-income individuals not expected to work—the aged, blind, and disabled.

One of the key contributions of the book is to highlight the uniqueness of SSI in U.S. social policy. As the authors point out, SSI is a welfare program run by the Social Security Administration. This unique arrangement was a purposeful attempt to destigmatize cash support by putting it under the umbrella of a popular and well-managed entitlement program, namely Social Security. As the authors say, "the creators of SSI . . . envisioned a program that would bring the dignity of the Social Security approach to elderly and disabled people who lived in poverty" (p. 231). Much of the book documents how this unique design benefited and cost the SSI program, in terms of management and public perception.

Giving the Social Security Administration the job of managing SSI also reflected the originators' views about the likely composition of future beneficiaries. Program creators believed that SSI would be dominated by elderly Americans who needed extra income to supplement their small Social Security checks. They also thought that this group would diminish over time, as more and more Americans became fully covered by the Old Age, Survivors, and Disability Insurance program. As the book makes clear, the creators were right about the elderly population, but greatly underestimated the potential for the adult and child SSI disability caseloads to rise.

The overarching theme of the book is that the original vision for SSI—to provide uniformly determined federal cash assistance to deserving low-income individuals immune from the stigma of welfare—was disrupted by partisan politics, unsynchronized Congressional compromises, and a turbulent launch of the program by the Social Security Administration. In particular, the authors argue that the near-continuous flow of one-off Congressional modifications to SSI produced a complicated patchwork of state-specific rules and state varying benefit levels that stood in direct opposition to the goal of uniformity put forth by SSI proponents.

This lack of uniformity made the program difficult to administer, especially at the national level. According to the authors, the Social Security Administration, which was accustomed to managing the much more homogeneous and stable retirement program, did not have the record-keeping technology or the staff to deal with the federalization of SSI or keep up with the changing rules and benefit levels. As a result, the transition was difficult; existing beneficiaries of state programs were dropped or over- or underpaid and new applicants faced long lines, closed offices, and harried SSA staff. The authors submit that the rocky launch of the program undermined states' confidence in federalizing benefits and led to some of the state variation in program rules still in place today.

Beyond making it a challenge to administer, the complicated structure of the program also made SSI vulnerable to the same misuses or marginal uses of more general welfare programs. The authors document this and discuss how the media frequently portrayed SSI in the same light as it did the increasingly unpopular Aid to Families with Dependent Children program. In their view, these news stories—including reports about cases of mothers coaching their children to qualify for SSI and immigrant families moving elderly parents to the U.S. and onto the SSI aged program—helped shape the public view that SSI was another type of welfare, rather than another type of Social Security.

This leads to SSI in the present day. The book notes that SSI has diverged considerably from the vision of its creators. It has evolved into a program primarily serving disabled adults and children, rather than the elderly. Furthermore, it is generally considered a welfare program, with the associated stigma and public suspicion. The authors provide compelling evidence that some of the blame for this should go to the legislative process that allowed SSI to evolve haphazardly without a unified vision. But one is left to wonder whether, absent these disruptions, SSI could have been the program its creators envisioned. The authors imply that they would say yes, but not all readers will agree. An alternative perspective is that SSI was destined to struggle from the beginning. By offering income support to a subset of low-income individuals deemed "worthy" of aid, SSI became forever vulnerable to changes in the definition of that group. In 1972, when aged and disabled individuals were not expected to work, there was little resistance to providing them cash support. Over time, as members of these "not expected to work" groups have become better integrated into the labor market, SSI has become more controversial.

Although *The Other Welfare* is about SSI, the story is one that might be recounted about any U.S. social program. The book nicely highlights the challenges of putting well-intentioned ideas into practice at the federal level. The authors show that, in the end, the implementation of a program, rather than its vision, is what we live with. In the case of SSI, this has meant leaving behind the vision of a program for a shrinking set of needy elderly and embracing a program that is now the last large-scale federal welfare program in the United States.

Mary C. Daly
Federal Reserve Bank of San Francisco

Government Failure: Society, Markets and Rules. By Wilfred Dolfsma. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2013. Pp. vii, 158. $99.95. ISBN 978–1–78254–606–1.
JEL 2014–0119

Government Failure is an idiosyncratic set of eleven chapters/essays of a "political philosophy nature" (p. 2). Chapters 1–5 cover general issues of government, markets and society. These chapters develop Dolfsma's own ideas and concepts against the backdrop of an extensive political and economic philosophy literature review. Chapters 6–8 apply Dolfsma's concept of government failure to changes in health care policies and in intellectual property rights (IPR). Chapter 9 (somewhat of an orphan) takes up the issue of institutional vulnerability. Chapter 10 addresses a dynamic welfare perspective for a knowledge society, revisiting the IPR issue. Chapter 11 briefly concludes.

The book is largely nontechnical and, therefore, accessible to economists as well as noneconomists. It is, however, no easy reading, particularly for readers unfamiliar with authors like Sen, Searle, or Luhmann. It is particularly interesting for economists, because it provides a perspective decidedly different from the mainstream.

The book's title only fits to some of the chapters, and it does not refer to the type of government failure that a mainstream economist would expect, although the topics of health care (chapters 6 and 8) and intellectual property rights (IPR, chapters 7 and 10) discussed as examples could have been chosen by any economist. What differentiates Dolfsma's view of government failure is that he defines government almost exclusively as the legislature, which sets rules for economic (and other) interactions, rather than as the executive power that also participates directly in the economy. Thus, government failure in Dolfsma's sense does not necessarily mean too much government. On the contrary, he sees rules that extend the scope of markets in health care as representing government failure. More precisely, (in chapters 1 and 5) he defines government failure as "rules and institutional structures for economic processes that are (1) too specific, (2) too broad, (3) that are arbitrary, or (4) that conflict with other rules it [i.e., the government] has set out to address other related issues (possibly primarily noneconomic)" (pp. 4 and 48).

Until reading Dolfsma's book, I had fairly narrowly held to a comparative institutions point of view of both market failure and government failure, as represented, for example, by Farrell (1987). According to this view, both markets and governments (and firm-internal transactions and private Coasean bargaining) are necessarily imperfect, but deal differently with similar hard issues, such as imperfect information in overcoming externalities. In contrast, Dolfsma predominantly takes a metaview of government as the rule setter for all these alternative modes of transactions. At the same time (without alluding to Gary Becker), he allows for the possibility that the society is embedded in the market in the sense that market thinking and market values take over and dominate noneconomic issues (chapter 2).

"Chapter 3, presenting research by Killian J. MacCarthy and Tao Zhu, indicates how influential government can actually be" (p. 3). Dolfsma claims that the research in chapter 3 shows that "what government does has real effects on the economy that is often less visible than the workings of the market" (p. 3). Since the case in point concerns quantity rationing, such government shaping of the market outcome is not at all surprising for economists.

Market failure in the traditional sense is well defined as a violation of at least one necessary condition for perfect competition in a complete set of markets. However, to Dolfsma (p. 131), this definition appears useless, because in reality the complement to market failure is empirically empty: Most markets are not perfectly competitive and for the small number of perfectly competitive markets the second-best theorem generates market failure. To me, Dolfsma's government failure is not as well defined as market failure, but the complementary set of nonfailing government is probably just as empty as the set of nonfailing markets. Ex post, government rules will always be either too broad or to narrow or arbitrary or conflicting with other rules. In principle, rules being "too specific" (Dolfsma's property 1 of government failure) or "too broad" (property 2) can be assessed ex ante by minimizing the expected consequences of type I and type II errors, but such an error analysis would be marred by subjective probability assessments and by the lack of a well-defined welfare function, against which the error consequences can be measured. This lack of welfare measure also mars against finding arbitrariness (property 3) and resolving conflicts with other rules (property 4).

Chapter 9 on the vulnerability of institutions could be right on the mark of government failure, because it deals with the change and boundary of institutions and the interaction between institutions. This is, however, kept purely at the political philosophy level (characterized mostly by Searle and Luhmann), rather than moving on to the government failure issue. To what extent are government rules influencing institutions, and is government itself such an institution? When reading this chapter, I was wondering if Dolfsma was talking about government as the prime vulnerable institution, but he never says so.

The lack of welfare measure is the second major conceptual issue (besides that of government failure) that Dolfsma has to deal with. In chapter 10, he provides a very readable analysis of the modern knowledge society and its dependence on the creation and the exchange of information as the major conflict for IPR. The exchange of information is influenced by transmission, storage, and decoding, which Dolfsma combines in a mathematical function generating social welfare "q." However, "q" is nowhere defined. Dolfsma rejects Paretian static welfare economics and calls for a dynamic Schumpeterian approach, but does not follow through in defining how such an approach can be made operational for a government to pursue. This is a hard problem, as identified, for example, by von Weizsäcker's (2013) new approach of incorporating adaptive preferences in dynamic social orderings. However, without an explicit standard for measuring success, it seems to me that an assessment of whether government rule-making failure is less or more than market failure is empty.

Such a short book (158 pp. with references and index) cannot cover all aspects of government failure. One fairly striking omission is the lack of meaningful discussion of behavioral economics. A number of the issues raised in chapter 6, on health care, have been brought up by behavioral economists. This would have been a chance for discussing these new approaches and their consequences for mainstream economics (such as game theory) in more depth. Another omission is the lack of discussion of metarules to resolve rule conflicts, for example, between different levels of government. An example from my area of expertise would be federal preemption of state regulation by federal regulatory agencies (always subject to judicial review). A third major omission is the lack of discussion of collective decision making. How is government failure influenced by the way the polity is organized? What political organization does it take to avoid or ameliorate government failures?

This book raises some deep questions, hoping the readers will find the answers. The most striking example of this hope is Dolfsma's favorable discussion of Sen's work (various sources), to which he devotes the whole chapter 4 on "Government policy: private incentives, public virtues?" At the end, Dolfsma challenges Sen to "resolve the issues regarding appropriate public policy and appropriate private incentives, both necessary to achieve the public good through collective action" (p. 43). Challenges such as these make the book worth reading.

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Ingo Vogelsang
Boston University

Open Budgets: The Political Economy of Transparency, Participation, and Accountability. Edited by Sanjeev Khagram, Archon Fung, and Paolo de Renzio. Washington, D.C.: Brookings Institution Press, 2013. Pp. vi, 264. $29.95, paper. ISBN 978–0–8157–2337–0. *JEL 2013–1087*

Activism often precedes evidence of its efficacy. That is to say: if people just about anywhere come up against a problem that threatens human rights, impedes social justice, or presents an urgent stress on the human condition, they are likely to act long before they (or others) design and conduct the research, weigh the evidence, explore the variables that might affect the outcomes, and so on. Taking action is appropriate in many cases; it saves lives, preserves the planet, and cleans up corrupt government.

Take transparency in government, for instance. Very few would argue against promoting transparency in our governing institutions, even in these times when ultimate transparency (à la Wikileaks and Edward Snowden) is highly contested. Rather, it is generally accepted that a transparent system of governance serves as a benefit to society. Transparency should, at a minimum, provide citizens with access to information about how they are governed and whether those in power are accountable for their actions. Twenty years of fighting corruption around the globe by my own organization, Transparency International, have shown that transparency correlates to better governance and development outcomes.[1]

Yet there is also a good moment to press pause and look deeper. If the past fifteen years have seen a virtual explosion of the transparency agenda, the past five (post–2008 crisis) have led to what I call the *open agenda*. Quite often this takes the form of the promotion of open data, and more specifically open government data.

The open agenda, while a new and important boost to the anticorruption field, builds on many of the staples of the "good governance" work of the last decade: access to information, transparency, and public financial management. It extends, however, to more recent areas of focus for openness, such as contracts and social spending. It also promotes greater openness with regard to the financing of politics. And critically, it pushes this same agenda in the private sector, where a wave of new reporting on environmental, social, and governance criteria have emerged. A novel element of the open agenda is its linkage of policy and process innovation to technology: using information and communication technology broadly and the Internet, in particular, to make information accessible, in simple (even machine-readable) formats and to enable public consultation and participation in governance. As government investment in ICT as a governance solution has boomed, so too has the expectation that technology can drive accountability.[2]

The theory of change for open data goes as follows: if you enable transparency of government via open data, you encourage its active use by the public, which in turn fosters a culture of accountability in government behavior (much of which was missing in the management of public finance prior to the 2008 crisis). Ultimately, transparency enables better management of resources, such as better schools, roads, and hospitals and better outcomes for society as a whole.

This theory is built on concepts (transparency, accountability, and participation) and a set of interactions that are complex and difficult to measure. In their edited volume, *Open Budgets: The Political Economy of Transparency, Participation and Accountability*, Sanjeev Khargram, Archon Fung, and Paolo de Renzio take on the challenge of looking at the evidence surrounding the transparency of public budgeting. They do so via the lens of the Open Budget

[1] See the comprehensive review by Lambsdorff (2005) and the evidence produced by Transparency International (2013), focusing on corruption and the achievement of the Millenium Development Goals.

[2] Dieter Zinnbauer, "False dawn, window dressing or taking integrity to the next level? Governments using ICTs for integrity and accountability", https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2166277.

Index (OBI), a civil society-produced index (to which de Renzio is a contributor) that assesses budget transparency across countries, and was first published by the Open Budget Partnership in 2006.[3] The OBI itself reflects the availability and quality of eight important budget documents, and thereby provides cross-country and time-series data that lends itself for analysis of the impact of the *open agenda* on people's lives around the world.

First and foremost, however, the book lays out a logical set of questions, to unpack and examine the open agenda's theory of change, here with the fiscal transparency created by open budgets as a proxy for openness.

1) Under what conditions does fiscal transparency emerge, and participation (either citizen-led or other's engagement with open budget data) along with it?

2) When do more fiscal transparency and participation enhance accountability by government?

3) Does more transparency per se foster more participation by citizens?

Based on the dataset generated by the OBI and supplemented by eight country case studies (presented in the volume in decreasing order of OBI performance: South Africa, Brazil, South Korea, Mexico, Guatemala, Tanzania, Vietnam, and Senegal), Khagram, Fung, and de Renzio provide a comprehensive, thoughtful and, taking the findings into account, cautious assessment of what we know about open data—here open fiscal data—its trajectory, and its impact on people and on policy outcomes.

The introductory chapter reviews the evidence to date, presents the conclusions, and provides an overview of the case studies. The case studies offer important historical context to the implementation of fiscal transparency measures and greater analysis of the country-level data that relates to it. For a policymaker, open data activist, or researcher interested in global trends data, the first fifty pages provide a range of useful material. The cases are deeper reads better suited to country experts.

Using the data available across countries, the editors identify four key factors that contribute to fiscal transparency and participation: (1) political transitions, (2) fiscal and economic crises, (3) prominent cases of corruption, and (4) external influence of global norms.

The other patterns that they identify related to fiscal transparency match those we have come to expect in development economics and democratization studies. In many countries, the general level of development is a strong indicator of fiscal transparency. Fiscal transparency is greater if there are free and open elections and more parties competing in those elections. Yet fiscal transparency remains threatened in autocracies, which consolidate power in conditions of obscurity, and this is a particular risk in countries where there is great natural resource wealth. Countries in conflict would likely show the same results as autocracies vis á vis fiscal transparency, a finding worth exploring.

Interestingly, the evidence presented shows that fiscal transparency leads to higher sovereign credit ratings, creating incentives for government to implement it. At the same time, evidence remains thin on the link between fiscal transparency and better human development. The data is simply not conclusive. Establishing such positive impact of the open agenda on development will be crucial to its longer term success as a donor policy priority.

Importantly, the authors use their findings to formulate a highly useful, if deceptively simple, graphic (p. 40) that reflects the challenges of change, and of gathering evidence, around the *open agenda*. Appearing as a kind of open data food chain, it shows that it is easiest to introduce transparency into government, more difficult to prove that transparency leads to participation, and yet more challenging to show transparency's impact on accountability. The evidence is there as you progress down the food chain, but there is less of it.

Time will tell if the link between the three is strong, but the good news is that the evidence base to measure these complex concepts and their interrelationships is growing. Driven by both technology and citizen activism, a wide range of organizations is firmly engaged in monitoring and evaluating the

[3] http://internationalbudget.org/what-we-do/open-budget-survey/

quality and extent of open government. The Open Data Index, a product of the Open Knowledge Foundation, uses local surveys to assess the access to government data in seventy countries.[4] The Open Data Barometer[5] of the Open Data Institute and World Wide Web Foundation explores the readiness and implementation by government toward open data, as well as its emerging impacts through peer reviewed surveys and secondary data. Both were introduced in 2013.

Governments, too, are taking notice of the potential in this agenda. In 2011, the United States and Brazil cosponsored a new initiative called the Open Government Partnership (OGP).[6] Originally just eight countries were involved, but at the time of writing, sixty-four had joined. OGP is a unique international effort, in that it creates a platform for government— together with the input of civil society—to commit to an open government action plan. While the commitments within OGP vary across countries, each country must include at least two of OGP's five grand challenges, such as increasing public integrity or improving public services. OGP eligibility requirements involve reaching qualifying scores in four openness measures (including the Open Budget Survey, the survey behind the OBI) and monitoring on country progress on Action Plans has started taking place via an Independent Reporting Mechanism.

In 2013, G8 governments signed up to an Open Data Charter,[7] creating a default position of open data for the G8 governments. In the words of the UK government, which held the G8 Presidency during the launch, opening data in key areas of government would, "help unlock the economic potential of open data, support innovation and provide greater accountability."[8] Efforts are underway to extend the Charter to the G20, to make its geographical and economic reach even wider. At the same time, the UN system is considering adding a new priority to its topline development goals, the Millenium Development Goals, in an upcoming revision of them post-2015: the addition of a goal on governance that would likely include an "open" component.

While the long-term impact of government commitments to open data have yet to be assessed, the transparency trend is a clear one. Claims that the open agenda threatens privacy, proprietary information, or security are increasingly seen as spurious, or at the very least, are being questioned more widely than ever.

The volume has limitations in its scope of analysis, many of which are acknowledged by its authors. The first is simply using fiscal transparency, or in this case budget transparency, as a proxy for open government data on budgets. Critical information about government income and expenditure, such as the revenues of state-owned enterprises, is often only available off budget. At the same time, the *open agenda* is now wide, and other areas of "open" activism go well beyond budgets. For example, other open spheres include open procurement, open contracts, and asset declarations. Evidence about these efforts can also contribute to understanding the impact of transparency.

At the same time, not all open data that is useful for social accountability is limited to government data, even if government lawmaking and regulation is often required to gain access to it. Good examples are the valuable data on the money donated to finance politics: information about such contributions is not readily available in many parts of the world. Another type of data under increasing pressure to become openly available is that of the beneficial ownership of companies. The UK government recently committed to create a public register of beneficial ownership, as a means to tackle illicit and corrupt financial flows, and legislation in the European Union and United States is moving in this direction, if slowly.

[4] See https://index.okfn.org/. According to the Open Knowledge Foundation's press release for the Index: "The Open Data Index is a community-based effort initiated and coordinated by the Open Knowledge Foundation. The Index is compiled using contributions from civil society members and open data practitioners around the world, which are then peer-reviewed and checked by expert open data editors. The Index provides an independent assessment of openness in the following areas: transport timetables; government budget; government spending; election results; company registers; national map; national statistics; legislation; postcodes/ZIP codes; emissions of pollutants." https://index.okfn.org/press.

[5] See http://www.opendataresearch.org/project/2013/odb.

[6] See http://www.opengovpartnership.org/.

[7] See https://www.gov.uk/government/publications/open-data-charter.

[8] See https://www.gov.uk/government/publications/open-data-charter.

There are already many early lessons to learn from work in this field. Transparency alone can't be the end game in (theories on) open data. As the editors of *Open Budgets* recognize, participation by citizens in government oversight, using open data, is complicated but necessary if the open agenda is to serve as an accountability tool. The case studies of Brazil and South Korea show that strong civil society participation can enhance fiscal accountability. But the participation-to-accountability link is not automatic, and more evidence is needed to draw conclusions about when and how the link can more often be made.

It is worth ending with a word of encouragement for research on open budgets and the *open agenda* more broadly. While both theory and data might be thin, the number of initiatives on open data and its monitoring by civil society mean there is in fact quality data in the pipeline—and lots of it. The *open agenda* has the potential, as this volume shows, to lead to positive effects on governance, at least in part by strengthening the hand of civil society to question government about its effective and just allocation of resources.

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Robin Hodess
Transparency International Secretariat

Cartels, Competition and Public Procurement: Law and Economics Approaches to Bid Rigging. By Stefan E. Weishaar. New Horizons in Competition Law and Economics. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2013. Pp. xii, 330. $145.00. ISBN 978–0–85793–674–5. *JEL 2013–1084*

In this book, Professor Stephen Weishaar makes a commendable effort to fashion a theoretical yardstick to systematically analyze how well legal frameworks regarding public procurement prevent the formation of bid-rigging cartels, with special focus on legal frameworks in the European Union, China, and Japan. Professor Weishaar does not focus on the "*ex post* identification of cartels, but is rather seeking to examine means to prevent their creation in the first place" (p. 2). With the aid of auction theory, he also makes recommendations regarding how these legal frameworks could be altered to better prevent cartel formation.

This book will be of special interest to policymakers eager to familiarize themselves with the legal and economic aspects of public procurement. However, readers should note that the evaluation methodology presented in the book is not supported by rigorous modeling or econometric analysis. Professor Weishaar's conclusions, therefore, must at best be regarded as informed guidelines. In spite of this, his surveys of the legal frameworks concerning public procurement in the European Union, China, and Japan make for a useful repository of facts and references. These surveys are perhaps the most significant contribution of *Cartels, Competition and Public Procurement*.

The book is divided into two parts. Part 1, comprising chapters 2 to 4, presents a brief overview of the economic theory underlying Professor Weishaar's analysis. In a foundational discussion in chapter 2, he makes the case for the involvement of a public authority in the detection of antitrust violations. Professor Weishaar next takes the reader through a brief tour of oligopoly theory in chapter 3. In chapter 4, Professor Weishaar discusses the auction theory literature that forms the basis for his method of evaluation. Concluding that "[auction] theory does not only offer advice to prevent the formation of bid rigging conspiracies but also suggests techniques to undermine the stability of such conspiracies should they be formed" (p. 60), Professor Weishaar sets the stage for his study in part 2 of the three specific legal frameworks.

Part 2, comprising chapters 5 to 11, presents Professor Weishaar's economic and legal analysis. The three regions consisting of the European Union, China, and Japan receive two chapters each, with the final chapter devoted to the "limits of economic theories." Chapter 5 begins with a detailed discussion of Article 101 of the Treaty on the Functioning of the European Union (Article

101 TFEU).[1] Professor Weishaar proceeds to analyze whether EU competition law effectively prevents bid-rigging conspiracies, concluding that, "it appears that the EU law addressing bid rigging conspiracies is designed reasonably well if public enforcement is indeed doing a good job" (p. 88). In chapter 6, Professor Weishaar "analyses how far the [Public Sector Directive 2004/18/EC] follows auction theoretic insights in order to determine if there are ways in which auction theory could help to prevent cartel formation and stability" (p. 89).[2] For example, he observes that "[reserve] prices or bidding ceilings may limit the ability of cartels to inflate the costs of procurement tenders" (p. 97). Noting that "Directive 2004/18/EC is . . . silent on the permissibility of such ceiling prices other than the general publication of the contract value," Professor Weishaar concludes that bidding ceilings "could . . . lead to a reduction in the procurement costs and hence to a destabilization of bidding cartels" (p. 97).

In chapter 7, Professor Weishaar reviews China's Anti-Unfair Competition Law (1993) and Anti-Monopoly Law (2008), Article 223 of China's Penal Code, and the Chinese public procurement laws (the "Bidding Law" and the "Government Procurement Law"). To the reader curious but unfamiliar with competition law in China, this survey of legislation will prove an interesting and useful reference. Professor Weishaar finds that "both the economic and administrative law provisions as such do not offer a sufficient deterrence effect to contain bid rigging conspiracies in situations that are best characterized by high profits and little risk of detection" (p. 127). From his auction theoretic analysis for China in chapter 8, Professor Weishaar finds much scope for improvement in the legal public procurement framework in China.

In chapter 9, Professor Weishaar reviews Japanese tort law, the Anti-Monopoly Law (reformed in 2005 and in 2009), and two laws that address civil servant involvements in bid rigging—the Local Autonomy Act and the Act concerning the Elimination and Prevention of Involvement in Bid Riggings. The chapter also contains a detailed and interesting discussion of the Japanese Federal Trade Commission (JFTC), where Professor Weishaar explains how the detection of bid-rigging cartels has been hindered by the JFTC's limited investigative powers. Chapter 10 presents an in-depth study of the Japanese construction industry and its predisposition toward collusion. According to Professor Weishaar, his analysis "indicates that there are signs of both more competitive conduct as well as discouraging factors, which suggest that the propensity of companies to collude may be rising again" (p. 209). He concludes: "Bid riggings will only be eradicated if politicians are prepared to actively counter the involvement of civil servants in cartels, alter the bidding and business evaluation systems so that they do not support collusion, and open up the construction sector to market forces" (p. 211).

In conclusion, policymakers will find in this book useful surveys of public-procurement legislation in the European Union, China, and Japan, as well as a readable introduction to the economic theory that Professor Weishaar advocates they apply to the design and analysis of these frameworks in efforts to deter the formation of cartels.

Brijesh P. Pinto
Competition Economics LLC

Michael A. Williams
Competition Economics LLC

I Health, Education, and Welfare

Paying Out-of-Pocket for Drugs, Diagnostics and Medical Services: A Study of Households in Three Indian States. By Moneer Alam. India Studies in Business and Economics. New York and Heidelberg: Springer, 2013. Pp. l, 152. $129.00. ISBN 978–81–322–1280–5.
JEL 2013–1095

In this monograph, Moneer Alam summarizes results of an interesting survey on the distribution of out-of-pocket costs for medical care in India. The survey covered 2,100 households in a

[1] Article 101 TFEU is available at http://eur-lex.europa.eu/LexUriServ/LexUriServ.do?uri=CELEX:12008E101:EN:NOT.

[2] Directives 2004/18/EC and 2004/17/EC are "[the] core laws on public procurement in the European Union" (p. 221). More on Directive 2004/18/EC may be found at http://eur-lex.europa.eu/LexUriServ/LexUriServ.do?uri=CELEX:32004L0018:en:NOT.

pair of "typical" districts in two rural states and in several neighborhoods in Dehli.

Nineteen percent of rural respondents and 12 percent of urban respondents spent more than one-fourth of their nonmedical monthly earnings on health care. This is a strikingly high figure. Not surprisingly, 10 percent of respondents had debt they attributed to injuries or illnesses. Overall, medical care consumes an average of over 10 percent of consumption.

Given India's extensive public health care network, why are so many households paying such high expenditures? First, two-thirds of expenditures on nonhospital care goes to private providers. This high rate is understandable, as respondents say public care is often far away, crowded, poorly run, and out of supplies.

What surprised me is that three-fourths of health care expenditures were for drugs, not for doctors or medical tests. Even more surprising to me, this high share of costs for drugs held even for hospital care and even for those with very expensive care.

The survey had familiar limitations, such as depending on recall data for health care expenditures and consumption, challenges measuring food produced by the family, and so forth. The author also ignores sampling error. For example, there are no tests of statistical significance of differences between states or subgroups.

The author's claims of causality are also sometimes unclear. For example, medical care can be a high share of consumption because care is costly (holding consumption constant), because an illness lowers income (which lowers consumption), or because a family reduces nonmedical consumption to pay for medical care. All three stories imply that high ratios of medical care to consumption are a problem, but the author emphasizes only the first story.[3]

The author's policy conclusions follow from the main results: The Indian public health care system has to be run more efficiently.

I would add one other major policy conclusion that the survey results suggest. Private sector care is not going to disappear in India or in other nations. Thus, it is important that the private sector deliver valuable care. I have no idea the fraction of care that is useful, useless (e.g., fake drugs that are ineffective), harmful, or socially harmful (e.g., short doses of antibiotics that promote drug resistance). The high usage of privately provided drugs emphasized in the study implies that India must increase the quality of private care and medications. This priority will require improving (1) the supply chain of drugs; (2) the training and certification of private sector care providers and informal drug sellers; and (3) consumers' ability to identify high-quality providers and drugs.

David I. Levine
University of California, Berkeley

[3] If health problems both increase medical expenditures and reduce the utility of consumption ("state dependent utility"), then a high ratio of medical expenditures to consumption need not indicate a problem—though there is not much evidence in the literature for this hypothesis.

The Biological Consequences of Socioeconomic Inequalities. Edited by Barbara Wolfe, William Evans, and Teresa E. Seeman. New York: Russell Sage Foundation, 2012. Pp. xx, 269. $42.50, paper. ISBN 978–0–87154–892–4.

JEL 2014–0144

Despite the vast literature analyzing the association between socioeconomic status (SES) and health, fundamental questions such as how and when this relationship emerges remain unanswered. The identification of the causal mechanism producing the SES health gradient is essential, not only for social scientists, but also policymakers. The design and implementation of effective interventions aimed at improving the life of the most vulnerable people hinges on the precise comprehension of these causal channels.

The Biological Consequences of Socioeconomic Inequalities presents new and absorbing evidence on the role of biology in explaining health disparities in the population. Can we use characteristics that are objectively measured as indicators of normal biological or pathogenic processes, (i.e., biomarkers), to assess and predict health outcomes? Are disparities in biomarkers also the result of early socioeconomic differences? Is it possible to use new technologies, such as magnetic resonance imaging brain scans, to document how deprivation during childhood manifests itself in brain development? Can we identify causal mechanisms explaining the association between disease pathologies and SES? Can changes in income explain changes in metabolic syndromes? Throughout its nine chapters, this book sheds light on these questions, proposing a potential research path for understanding the observable relationship between SES and health.

The text starts by documenting the association between (parental) income and psychical and mental health-related variables such as obesity, smoking, and psychological distress, among other important outcomes, but quickly progresses to more ambitious questions. In particular, by presenting new findings and describing the recent breakthroughs in the social and neurosciences, especially the innovative research examining longitudinal data containing biomarkers, the book seeks to demonstrate whether and how material deprivation affects basic physiological processes in humans. In other words, it represents an effort to get "under the skin" of the observed SES and health gradient and its ultimate objective is to identify the underlying causal mechanisms.

Is this book successful at doing this? Overall, it is. The descriptive analyses of the chapters examining the empirical relationships between SES backgrounds, health variables, and different types of biomarkers (including new evidence on differential patterns of growth of specific areas of the brain by SES) are properly accompanied by a depiction of the new research digging into the biological consequences of socioeconomic disparities and the identification of causal mechanisms. The analysis of the association between SES and childhood asthma (chapter 4), and the potential effects of interventions on brain aging (chapter 8) are good illustrations of this position. The first case investigates why asthma morbidity is significantly more frequent among children with lower SES backgrounds. Importantly, the text documents how inflammatory processes at the cell levels related to this chronic disease can be influenced by conditions commonly found in low SES environments, such as family stress, as well as social and neighborhood physical exposures. On the other hand, by documenting the recent evidence of the impact of physical and volunteer activity in later life on clinically meaningful improvements in executive function and attentional control, especially among low SES individuals, the evidence described in chapter 8 shows how the brain's potential for adaptive plasticity remains responsive to the environment even in late-age development. This insight has important implications for public policies and it provides a balance to the well-established evidence supporting the importance of early interventions.

In addition to a comprehensive analysis of the aforementioned evidence, the book provides the reader with a thoughtful description of the methodological limitations afflicting the literature. These include a long list of difficulties surrounding the analysis and interpretation of the available biological measures, the small sample sizes of many of the data sets utilized by researchers, and the econometric challenges that plague the empirical studies, such as omitted variable biases, measurement error in SES variables, and reverse causality. Many of them have been addressed in the recent literature documenting the disparities in adult outcomes—including health and health-related variables—across groups as a function of early endowments (Heckman, Stixrud, and Urzua 2006; Heckman et al. 2014). Researchers shall continue to incorporate the most recent econometric techniques into the empirical analysis of longitudinal data sets containing biomarkers.

It is crucial to mention that many of the existing methodological limitations in the analysis of the SES health gradient could be solved with more and better data. As correctly pointed out in the book, this is a necessary condition to continue dissecting the biological pathways through which SES translates, for example, into disease pathology. In this context, there is little questioning that specific efforts should be directed at the collection of new biological measures, even in small and experimental samples. This would inform researchers as to what biomarkers collect in large and representative studies (and when), and on whether they should be analyzed individually or as composite constructs (the evidence of chapter 2 regarding the role of the *allostatic load* in assessing and predicting health is particularly suggestive of the importance of this). Naturally, this would also contribute to identifying the elements driving the correlations between SES and neurobiological outcomes. At this point, early stimulation, quality of environmental input, and stress emerge as potential mediators.

The multidisciplinary nature of the book deserves additional recognition. In light of the evidence and given the level of sophistication and complexity of the new information (e.g., neuroimaging data), the coordinated efforts from researchers in different fields arise as a critical condition for continuing advancement toward a better understanding of the origins and consequences of socioeconomic disparities. This situation should alert the academic community. We need innovation in our graduate programs. Rigid and single-minded PhD programs in social sciences will limit our future capacity to progress in this area.

Now, readers of this book expecting to find a road map for evidence-based public policies will be disappointed. As the text explains, a salient missing piece is "the lack of expertise in translating research findings to policy" (p. 258). To be fair, this is not surprising. We are probably decades away from this ambitious process. This, however, should not discourage the interested reader, who will be excited to learn about the recent developments in this area and to envision the potentials.

All in all, the results and analyses from this book have major implications. Any researcher and policymaker interested in the association between SES and health-related variables should be aware of the new research described in this volume. The literature connecting biological and economic sciences is in its infancy. We have more questions than answers, but as the book documents, the steps towards a better understanding of both the actual associations and the causal mechanisms explaining the SES health gradient have been simply remarkable.

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Sergio Urzua
University of Maryland

L Industrial Organization

Out of Print: Newspapers, Journalism and the Business of News in the Digital Age. By George Brock. London and Philadelphia: Kogan Page; distributed by Ingram Publisher Services, La Vergne, Tenn., 2013. Pp. ix, 242. $24.95, paper. ISBN 978–0–7494–6651–0.

JEL 2013–1153

The ambition of *Out of Print* by George Brock is to place journalism and the business of news reporting into a sweeping arc of political and technological history. Why? Perhaps from this vantage, Brock can best meet his stated aim of dispensing "clear-eyed" analysis of the challenges brought about by digitization without pocketing the rosy glasses that portray today's turmoil and transformation as a transient phase of an enduring profession. *Out of Print* is written by a journalist for journalists with a journalistic style: while some of the historical and ethnographic bits can engage economists or others interested in the news industry, ultimately the analytics are too loose and the institutional detail too thin to contribute to a forward-looking understanding of the business.

The book unfolds in three parts. The first is devoted to historical development of journalism and the news industry, highlighting technological and political events that together shaped journalism as a profession and news reporting as a profitable enterprise. The second part dissects the disruptive effects of technology's one-two punch at the market for news—television and the Internet. The third and most insightful portion breaks the linear path and examines newsroom culture in light of the UK's phone-hacking scandal. While Brock at times strives for a global view, peppering the text with anecdotes from India and Australia, virtually all of the substantive discussion centers on England. The *Times* (London) takes no modifier.

In the early chapters, the notion that history and technology together shaped modern news reporting rings true. Brock cites high demand and weak political authority for a surge in news weeklies covering Parliament during the run-up to the English Civil War, rising elite prosperity for competition and emergence of editorial style in the early eighteenth century, and the monumental role of the free press in the American revolution as a model for plurality in the postrevolutionary period. While the nineteenth-century history of newspaper entry and competition in New York City has been better told elsewhere, Brock's focus on the editorial strategies of entering firms such as the (New York) *Tribune,* with its emphasis on original reporting, and the *New York Times,* with a stated mission of balance, offers a useful reminder that what are sometimes considered universal journalistic ideals emerged from shrewd business strategies in a competitive marketplace. But the dense thicket of quotations that Brock substitutes for narrative makes for tedious and fragmented reading, and the reliance on quoted opinion rather than factual evidence weakens what might otherwise be sound historical research.

The second part of the book begins in the second half of the twentieth century, which marks the peak of the advertising business model and its subsequent unraveling in the face of technological innovation. Brock is weaker when competition and demand, rather than politics, drive industry changes and even his valid points about the effects of digitization on entry, competition, and quality suffer from the lack of a theoretical framework that grounds discussion on these topics in economics. A number of the broad claims in this section are misleading ("there has never been a mass audience for serious news") or incomplete ("American newspapers were disappearing rapidly between 1950 and 1975 as television sucked the advertising out of local city markets"). This section also shows very little familiarity with research on digitization in media markets. But Brock hits many of the familiar impacts of digitization (massive duplication of coverage, implications of consumer choice, explosion of online advertising space) in conceptual but accurate terms.

For serious students of the industry, a few late chapters carry the book. Brock details how relentless pursuit of novel coverage over more than a decade by fiercely competing popular newspapers escalated into routine privacy invasions, with computer and telephone infiltration, bribery, and cover-up culminating in two official investigations and criminal convictions. Brock places these newsroom "failures" in the context of stresses imposed by technology. The idea that heightened competition could manifest itself not (as in the United States) in greater geographic or ideological differentiation, but as costly competition for exclusive coverage, falls nicely in line with economic thinking. Brock rightly criticizes as wildly unrealistic the idea that regulation of media content in a digital age might refocus news coverage on what some might call more substantive topics. But he also does not have the theoretical tools to frame how the state-mandated balance of the BBC might alter private sector competition in a way that contributes to the perceived problems. The nuggets of insight in this section contrast with the more shopworn ideas in the "business model" chapters.

Taken as a whole, the ultimate disappointment of the book is the absence of visionary or even new ideas about the future path of the journalism profession or the business of news. The transformations and challenges detailed in the book are, for the most part, well underway or even over and done, quite often in ways that readers of Shapiro and Varian's fifteen-year-old *Information Rules* would have predicted. The two biggest misses for forward-looking journalists are perhaps the role of experts and the role of machines, which are already showing signs of squeezing journalists from both ends in the newsroom. An exploding supply of content and very little extra time to read translates into quality competition, and PhDs occupy a growing number of column inches each year to satisfy that demand for expertise. On the other side of the coin, computer programs that translate standard inputs into narrative copy press on the low end of journalism. Robot copy from *Narrative Science* made a splash with its debut at *Forbes,* and widespread implementation of writing algorithms might only temporarily be slowed by today's vast pool of low-cost freelance writers. Brock's brief passages on "data experts" really do seem to miss the growing potential for machine algorithms to predict what consumers read and to decide what gets covered. Perhaps most of all, journalists of the future will need to learn how to listen to machines, because the machines will be right.

Lisa M. George
Hunter College and
Editor-in-Chief, Information
Economics and Policy

The Economics of Electricity Markets: Theory and Policy. Edited by Pippo Ranci and Guido Cervigni. Loyola de Palacio Series on European Energy Policy. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2013. Pp. viii, 226. $120.00. ISBN 978–0–85793–395–9.

JEL 2013–1164

This book provides a concise description of the institutions that govern competitive electricity markets. The editors, Pippo Ranci and Guido Cervigni, together with their four contributors, have witnessed the development of electricity markets in Europe from multiple perspectives: market participant, regulator, and academic researcher. They draw on their experiences to describe both the technical details of how markets operate and the advantages and disadvantages of alternative market designs.

As described in chapter 2, electricity supply requires central coordination by an entity known as the system operator. It is responsible for planning the operation of generation units and making real-time adjustments in order to match second-by-second fluctuations in demand. The need for this central coordination is a challenge for electricity market design. On the one hand, creating a standardized product that can be traded among all market participants will enhance market liquidity. On the other hand, the outcome of trading in a standardized product may not satisfy the physical constraints on generation and transmission that the system operator must account for.

A central theme of the book is the contrast between the two market design philosophies that have developed to resolve this trade-off. In the "U.S. model," wholesale markets are tightly integrated with the physical limitations on electricity supply. Market prices and quantities are the outcome of a single optimization procedure that minimizes the cost of supplying electricity, subject to generation and transmission constraints. By comparison, in the "European model," initial transactions occur in an idealized market that ignores these physical constraints. Subsequent adjustments to these ideal outcomes are made by transactions in separate markets to ensure that all constraints are satisfied. This lack of integration in the European model will lead to inefficient production decisions and higher costs.

Many wholesale electricity markets incorporate a mechanism to pay generators for their available capacity, even if it is not used to produce electricity. Chapter 3 describes the rationale for and operation of these capacity support programs. During periods of scarcity, the (administratively-set) market price may not be high enough to recover the fixed costs of the highest-cost generators that only run for a few hours each year. I wonder whether trying to correct the underlying market flaw—price-insensitive demand—would be better than concealing it beneath an additional payment mechanism. The other rationale is even less convincing: The capacity mechanism allows coordination of generation investment decisions, reducing uncertainty for investors and "a more certain environment is expected to reduce the rate of return required by investors, to the ultimate benefit of consumers" (p. 69). Yet other industries that require large sunk investments appear to function well without centralized coordination. Furthermore, setting an administrative target for the capacity requirement does not eliminate the potential costs from overinvestment. Instead, capacity programs transfer these costs from firms (which no longer face the risk of making unprofitable investments) to consumers.

Chapter 4 describes the institutional features of wholesale electricity markets that ensure that the capacity limits on transmission networks are not exceeded. A unique feature of electricity is that it flows along every parallel route between generators and consumers. The surprising consequence is that congestion on a small portion of the transmission network can affect the feasible combinations of generation and consumption on distant parts of the network. The chapter begins with an insightful demonstration of this result using a stylized example of a three-node triangular network. Depending on the location of congestion on this network, the marginal cost of additional consumption at a particular node is shown to be high, low, or even negative. This example is then developed to describe the alternative market mechanisms that are used to manage congestion. These further illustrate the differences between the U.S. and European market design philosophies.

The final third of the book contains three shorter chapters about wholesale market power, retail competition, and the integration of renewable generation. The last of these highlights the irony in current energy policies, both in Europe and elsewhere. One major objective of electricity deregulation was to place responsibility for investment decisions on profit-maximizing firms, instead of regulators. However, as a result of climate policies, decisions on the type and quantity of generation investment—even the output price that firms receive—are once again being made by regulators instead of firms. As Cervigni says, "The level of installed generation capacity and its composition are ceasing to be the result of decisions taken by market investors, which bear the corresponding risk" (p. 203). This unfortunate trend puts at risk many of the benefits achieved from electricity industry restructuring.

At the same time, technological innovation promises to greatly enhance electricity markets. Widespread adoption of real-time metering creates the possibility of incorporating price-responsive demand into wholesale markets. This would ameliorate many of the problems described in the book, from wholesale market power to insufficient capacity investment. Improved storage technologies, increased distributed generation, and greater use of electricity for transportation will also bring large changes to electricity markets. Despite the potentially disruptive effects of these technologies, there is only a brief mention of them at the end of the book.

One other missed opportunity for this book was to link the descriptive discussion of alternative market designs to the academic literature. I would have liked to see additional references to empirical analyses of the performance of different market structures. For example, Wolak (2011) shows that energy usage of natural gas generation units, holding their output constant, fell by 2.5 percent after California switched to a nodal pricing market in 2009.

Overall, this book will appeal to professionals and academics who wish to understand the organization and operation of electricity markets. It is useful both as general background and as a reference guide to particular institutions. Although the emphasis is on European electricity markets, it will be valuable for people working in other regions too. The debate over alternative market structures, described in this book, has influenced the design of every wholesale electricity market in the world.

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Shaun McRae
University of Michigan

Casinonomics: The Socioeconomic Impacts of the Casino Industry. By Douglas M. Walker. Management for Professionals series. Heidelberg and New York: Springer Science + Business Media, 2013. Pp. xv, 297. $79.99. ISBN 978–1–4614–7122–6.

JEL 2013–1161

A book titled *Casinonomics* suggests that it sheds light on the special economics of casinos. What might this special economics be? The American Psychiatric Association recognizes an impulse control disorder in its *Diagnostic and Statistical Manual of Mental Disorders*, where repeated unsuccessful efforts to control, cut back, or stop gambling is one of the symptoms. A reasonable inference is that gambling induced by mental disorder is not ordinary demand. How does analysis need to be altered when evaluating casinos? For example, what share of revenue comes from gambling disorders? How do benefit measures, such as consumer surplus, change in light of gambling's special features? Do casinos create or just "discover" already-created "disordered" gamblers? Are "disordered" gamblers responsible for external social harm and costs to others?

In addition to theory, there should be the collection and dissemination of current industry facts and figures. If the author has original empirical research, we would expect to see that, too.

Casinonomics is divided into three parts: The first deals with the economic benefits of casinos. The second addresses gambling disorder. The third discusses negative impacts. Introductory and concluding chapters round out the book.

For the most part, the reader will be disappointed regarding the discussion of theory. The book's early discussion of benefits, for example, is devoted to routine descriptions of expanding production frontiers, mutually beneficial transactions, trade, taxes, employment, wages, and the like—all readily available in textbooks. How these concepts might need to change for casinos or how they might relate to the presence of problem gamblers is not addressed. This is a serious oversight for an industry that may receive 38 percent or more of revenues from problem gamblers (Australian Productivity Commission 1999, p. P12).[1]

For "normal" gamblers, the book ignores the problem that, unlike rational consumers who use insurance, and unlike investors who avoid risk when possible and manage what they cannot avoid, these gamblers intentionally seek risk to add to their income stream.

Disappointment about *Casinonomics* carries into later sections. Page 190 (part of the chapter "Issues in Social Cost Analysis") displays a diagram of consumer and producer surplus with axes labeled "Quantity of Casino Gambling" and "Price." What are these? Are one's losses to the casino the quantity of gambling, or are one's losses to the casino the price of the gambling visit, or are they neither? These could have been addressed, and have been elsewhere.

The author does better when he reports the impact of casinos on state tax revenue (chapter 7) or presents data from his own econometric studies, such as his study linking gambling with binge drinking, drug use, and hiring prostitutes (chapter 10). In spite of the profession's caution regarding Granger causality,[2] we see it applied to regional economic performance (chapters 5, 6).

However, the book is periodically marred by a hypercritical tone regarding others' work. Researchers "have been confused" (p. 170). Studies are "low quality and rather confused" (p. 177). The author continues the pattern of his earlier book that was identified by an earlier reviewer as unjustified. Ernest Goss writes about Walker: "past research alleging ([Walker's] term) social costs are by his measure low quality and confused. He provides little evidence of factors that undermine their findings" (Goss, E., *Journal of Economic Literature*, 46, 3, September 2008, 748–749).

Walker also misrepresents others, including incorrectly suggesting that they hold an extreme position. Chapter 14 suffers from this problem. One example: To evaluate the social costs of gambling, the sample of problem gamblers must represent the larger population that one is hoping to describe.[3] In addition, there is the familiar issue of multicausality ("comorbidity" in *Casinonomics*). This also has been remedied in various ways. The author falsely claims others fail to correct for these features when corrections were made and the matter was known to the author. (See information in "Connecting Casinos and Crime," *Econ Journal Watch*, 5, 2, May 2008, 156–162.) Original research such as Thompson and Schwer (2005) is misrepresented, as are others. Therefore, I recommend that readers read the cited references rather than take the author's account of them.

The tendency to attribute extreme positions mars the book and makes it appear naïve at times. For example, *Casinonomics* spends many pages (pp. 157–59, 167–68) on the issue of abused dollars. Gambler thefts sometimes become reported crimes[4] and sometimes not because the victims are family and friends. The dollar value of nonreported crime is often referred to as "abused dollars." Walker writes, "stolen property is simply a transfer between thief and victim that does not change aggregate social wealth." Psychic costs and "behavior geared toward preventing involuntary wealth transfers" are the only two cost consequences he recognizes. He does not report that theft-caused destruction of distributive efficiency generates social costs and that its size can theoretically reach levels equal to the value of the property stolen and beyond.[5] Further, if the thief

[1] This figure is for casino table games and gaming machines. Other studies' estimates from Canada, Australia, and the United States imply a number as high as 50 percent.

[2] Granger notes, "my definition was pragmatic and any applied research with two or more time series could apply it, so I got plenty of citations. Of course, many ridiculous papers appeared. . . . I am not sure if the empirical studies on causation have proved to be so useful." Nobel Lecture, December 8, 2003.

[3] This well-known problem has been addressed in the literature. For example, see Ryan and Speyrer (1999).

[4] Jaret and Hogan (2014) documents the gambling addiction and criminal case of San Diego Mayor O'Connor involving over $2 million theft.

[5] E.g., see Grinols (2007, pp. 515–40), for an example of social loss from theft equal in value to the value of the stolen property.

suffers from gambling disorder—not unusual for abused dollars—use of a stolen $1,000 for gambling cannot automatically be assumed to transfer $1,000 of value to the compulsive-control-disorder-suffering thief.

Casinonomics covers a topic that first appeared on the contemporary American scene in the early 1990s. It is likely to continue for many years more. The unevenness of treatment between theoretical and empirical content of the present book, however, suggests that readers may have to wait for the next book to learn the economics of casinos.

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Earl L. Grinols

Distinguished Professor of Economics, Baylor University

N Economic History

Korean Political and Economic Development: Crisis, Security, and Institutional Rebalancing. By Jongryn Mo and Barry R. Weingast. Harvard East Asian Monographs, vol. 362. Cambridge: Harvard University Asia Center; distributed by Harvard University Press, 2013. Pp. xi, 218. $39.95. ISBN 978–0–674–72674–1.

JEL 2013–1220

In their new book, *Korean Political Development and Economic Development: Crisis Security and Institutional Rebalancing*, Jongryn Mo and Barry Weingast explain why South Korea (hereafter the Republic of Korea) is among the few sustained examples of success in the developing world. Readers familiar with their academic standing, earned from articulation of new concepts in rational choice political economy, have looked forward to this latest book. And indeed, it promises significant new insights. Employing a conceptual framework that Weingast designed with Douglass C. North and John Joseph Wallis, and which they refer to as NWW, the authors offer an explanation of how the Republic of Korea transformed itself into a rich industrialized and democratic nation, and suggest that the general argument fits other cases as well (2009).

NWW classifies the properties of human societies "into two categories of social order" that differ according to their degree and extent of access. "Limited access orders" attain stability by making organizations, privileges, and rights the reserve of the few. But stability comes at a price: exchange is relationship-based and "gales of Schumpeterian creative destruction" are inhibited. In traditional societies, NWW notes with particular insight, rents are distributed to individuals and groups as incentives to cooperate in both the reduction of violence and the repression of technological or organizational innovations that might create opportunities to redistribute social assets.

"Open access orders," by contrast, do not require that citizens obtain political permission to organize, or to truck and barter, or to express their views. Rule-of-law institutions facilitate exchanges that are impersonal, allowing gains from specialization and trade. Importantly, for sustained growth, open access orders make reorganization or technological innovation feasible without violence. Hence, open access orders can sustain long periods of stability, whereas closed access orders are prone to violent decline.

The main thesis of the book is the claim that a law of "double balance" brings economics and political freedom into equilibrium. There were three turning points that thrust the Republic of Korea on its path toward sustained growth in recent Korean history—Park Chung Hee's modernization of the fatherland from 1961 to 1979; the prefinancial democratic transition that took place from 1987 to 1997; and the postcrisis political reforms that occurred under Roh Moo Hyun from 2003 to 2008. Each serves as an example of how "[o]penness in politics and economics must go hand in hand. When the openness of one area falls behind the other area, the contradictions it creates in the economy give rise to correcting or equilibrating forces. The three major turning points in the Korean political economy all involved lack of balance. They are therefore testimony to the force of double balance" (p. 201).

NWW represents a brilliant effort to employ microeconomics and rational choice models to formulate a theory of the origins of social order. However, the claim that an iron law of double balance governs the global behavior of the system of international relations obliges the reader to indulge in a series of unlikely assumptions about systems, cognition, behavior, and dynamics.

Mo and Weingast define history as a linear procession of equilibrium with multiple fixed points. In it, an "unbalanced country" can either go "backsliding toward a natural state or progress towards open access" (p. 21). The possibility of veering (branching) off in multiple or divergent directions toward a new destination is not explored. Yet studies in the fields of evolutionary biology, statistical physics, and history show us that frequent occurrences of disruptive events rarely cause a return to an earlier pattern. This applies both to the evolution of an ecosystem as well as to a nation's history.

Mo and Weingast posit that the Republic of Korea was "unbalanced" in 1987, when their governing elite bowed to popular pressure to make the head of state stand for election. Its economic openness was too far ahead of its political openness, which was then brought into equilibrium. Yet this neat explanation overlooks the critical changes within the larger coalition of liberal international nations upon which the Republic of Korea's markets, security, and technology depended. Once the People Power Revolution in the Philippines toppled the Marcos regime in 1986, an authoritarian regime in Seoul could no longer expect the same forbearance from Washington that it had enjoyed since 1950. Its dependence on export-led growth subjected the legitimacy of the ruling coalition to standards shared by its principal, and more sizable, trading partners, causing the Republic of Korea's domestic governance to increasingly exhibit the typical features of the West. China, by contrast, as an immense economy, can dictate its participation in global markets. One must ask, how will the law of double balance fare as trade patterns within East Asia shift? Will the Republic of Korea, Japan, and Taiwan converge to China's values and begin to exhibit the typical features of the system they have joined?

Employing the concept of "an equilibrium development trap," while celebrating Schumpetrian notions of "creative destruction," will be viewed as flawed. Joseph Schumpeter rejected the kind of equilibrium model that Mo and Weingast propose. Schumpeter's view of the relationship between innovation and entrepreneurship emphasized overall system transformation as a nonlinear or discontinuous process with emergent properties. In Schumpeterian innovation driven development, change arises from within. An implicit contradiction exists between the power of double balance and the cognitive omnipotence attributed to the ruling coalition's precise calculations and complete knowledge of the costs and benefits of various strategies to attain a political settlement to perpetuate their social authority.

If the law of double balance does anything, it is this: it denies the possibility of rich, complex interactions among actors. Where does this control mechanism over such rich and complex actions reside? Moreover, the actors in NWW enjoy only limited autonomy; as collective representatives of a particular order, their freedom of action is determined exogenously. A law as mechanistic as double balance is more suited to simple physical systems, not to a dynamic system in which purposeful behavior arise as agents constantly react to the actions of others, and where nothing in the environment is fixed. The very idea of a corrective, equilibrative law is not "natural."

The corrective power of the posited law of double balance seems to be suffering from some sort of enervation thwarted by powerful tendencies in the global economy. Today, many authoritarian regimes attract more investment and grow faster than their democratic counterparts. Looking back, in fact, we see that the Republic of Korea's growth was most robust from 1963–93, a period when every president in that period was a former military leader. The authors even concede this. "The impact of democratization," they write, "can be seen most clearly in the steady fall in the rate of economic growth since 1987" (p. 11). The conundrum for global political economy, and for the leaders of emerging nations, is that nascent democracies are not living up to economic expectations.

NWW fits poorly into the relevant historical context of Asia, where cognition, legitimacy, and ethics have roots in philosophies, religions, and identities that are thousands of years in the making. An initial condition, its Confucian heritage, renders the Republic of Korea's system of public management by means of bureaucracy effective at credible commitment, without an independent legal system.

According to NWW, the vulnerability to, and tendency toward crisis are defining characteristics of closed access orders. Yet the Republic of Korea, China, and Japan enjoyed centuries of stability as closed access orders. Confucianism gave the Republic of Korea stability for approximately five centuries during the Joseon Dynasty (July 1392 to October 1897), long before its democratic transition.

NWW also asserts that civil control over the military contributes to the transition to open access; yet this too works poorly in an Asian context. In both Confucian East Asia and Hindu South Asia, political behavior was defined by moral standards that constrained violence and defined regime legitimacy with a hierarchy of values that differed from those of Western Europe. Scholastically determined merit rather than military prowess was the dominant ethos of China's ruling coalition, and was the basis for government service since the eleventh century. Warrior classes only rose to dominate the Chinese political economy and social order during moments of dynastic decline. Civil leaders exercised practical and ideological control over the military more than a thousand years before they did in the West without precipitating a transition to open access.

Mo and Weingast maintain that the "transition occurs when members of the dominant coalition have incentives to open access incrementally" (p. 192). Yet open access orders did not emerge incrementally in Asia. Across East Asia, open access followed a century of violence that either destroyed or replaced traditional elites, causing a radical break that weakened or destroyed the landed elites or forced their transition into commercial activities. By contrast, in India, incrementalism has not produced an open access order, despite sixty years of independence.

Despite the persuasive tone and clear writing, this new book does not provide a satisfactory framework to explain the Republic of Korea's trajectory. It adds no original quantitative or qualitative data, and therefore little to what is already widely disseminated in the published literature. Hence, the merits of the book must be assessed on the capacity of the conceptual framework to add insights to the study of the Republic of Korea's transition and its relevance to Asia in general.

The argument that political openness *must*, over the long run, increase economic openness does not explain India's trajectory, for one, and the argument that economic openness will produce political liberalism does not explain China. The belief that openness in political and economic areas must go hand-in-hand describes key features of the trajectory followed by the first few Western nations that were to industrialize, but cannot even be extended to describe the nineteenth-century advances of Germany, the Austria–Hapsburg Empire, or Russia. Had Asia industrialized first and not the West it would have set the terms for others to follow. Why didn't it? That is a question of system dynamics, not of mechanics.

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Hilton L. Root
King's College London and George Mason University

Q Agricultural and Natural Resource Economics • Environmental and Ecological Economics

Climate Economics: The State of the Art. By Frank Ackerman and Elizabeth A. Stanton. Routledge Studies in Ecological Economics. London and New York: Taylor and Francis,

Routledge, 2013. Pp. viii, 187. ISBN 978–0–415–63718–3. *JEL 2014–0349*

Climate Economics: The State of the Art by Frank Ackerman and Elizabeth Stanton is an extension of a consulting report written for the World Wildlife Fund. Unfortunately, the book reads like an advocacy document for action to substantially reduce greenhouse gas emissions as quickly as possible, irrespective of costs, rather than a book assessing the state of the art of the contribution of economics to the climate debate. The book takes aim at most of modern economics as applied to the climate change policy problem, while basing much of the critique on the weaknesses in a particular class of economics models known as integrated assessment models (IAMs). What is missing is an open and balanced assessment of the many unresolved issues in the economics of climate change and the burgeoning economics literature on the role of prices, rather than targets, for emissions as a basis for climate policy design (for example, see McKibbin and Wilcoxen 2002, Nordhaus 2006, Pizer 1999, and Weitzman 2013). Instead, the book attempts to discredit a substantial body of economic research and in its place promotes the approach of "targets and timetables" for emission reduction, no matter what the cost. This strategy, which underlies the approach of the Kyoto Protocol, has failed, to date, as a strategy for effective climate policies globally. In recent years, economics has offered a number of possible alternatives to target-based approaches by focusing on pricing carbon, but these approaches are not mentioned.

The authors are particularly critical of IAMs that combine climate and economics into a single model. Many economists agree that there are weaknesses in this approach because, to enable many complex interactions from both climate model and economic models, the models necessarily have to simplify both the climate impacts as well as the economics. There are many teams of economic modelers that do not attempt to integrate the climate models and the economic models, yet still use their economic models with more elaborate economic structures to address climate policy (e.g., see some of the models in Dixon and Jorgenson 2013, in particular McKibbin and Wilcoxen 2013). Many of the economic models that do not fit into the class of IAMs attempt to deal with the criticisms raised by the authors, but are ignored in this book. Ignoring the large number of economic models that are not IAMs, but which are used for evaluating climate policy, is surprising in a book about the state of the art in climate economics. For example the statement on page 117 that no models deal with oil prices and the effects on the costs of climate policies is clearly incorrect.

As outlined in many economic studies not mentioned in the book (e.g., the large collection of papers in Aldy and Stavins 2007 or the survey by McKibbin and Wilcoxen 2002), the key to climate policy in a highly uncertain world is designing policies that can effectively and sustainably reduce greenhouse gas emissions at lowest cost. This includes designing economic and political incentives into national institutions and using long-term markets in creative ways.

Despite acknowledging that climate policy is all about managing uncertainty, the authors very quickly state their belief that the climate science is almost definitive. The book begins with the words "Climate science paints a bleak picture: The continued growth of greenhouse gas emissions in increasingly likely to cause irreversible and catastrophic effects." That statement sets the benchmark by which the subsequent contribution of economics is measured. Emissions must stop quickly, no matter what. There is little role for economics or any analysis of trade-offs or of assessing costs and benefits because these don't matter when the science is so clear and the future of mankind is at stake. It is clear where the book is heading, but it takes a long time before the authors clearly state their views on the role of economics. One area where there is a genuine economic debate on how to reduce greenhouse gas emissions and where economics is actively contributing to policy design is in the role of incentives, particularly the different instruments of policy—for example taxes versus emissions trading. Yet the authors dismiss this completely on page 101 and point out that actually, the book is not about the contribution of economics: "There is an extensive literature on the choice of policy instruments. . . . We have not attempted to review that literature; the issues we are addressing here, such as the need for rapid emission reduction, could be expressed through any of several policy instruments." In fact there are important differences between policy instruments in a world of uncertainty (see Weitzman 1974; Pizer 1999; Nordhaus 2006; and McKibbin and Wilcoxen 2002). A carbon tax is not the same as a cap-and-trade system under uncertainty. Economists are beginning to progress the policy debate (see Morris, McKibbin, and Wilcoxen 2013; and Weitzman 2013), so long dominated by environmentalists that coordinating carbon prices may be a much better approach than focusing on the approach of setting "targets and timetables" independently of costs, as advocated by the authors. Yet this key aspect of the "state of the art" contribution of economics is missing.

There is some useful discussion of recent advances in the nature of uncertainty and the importance of "fat tails" in designing climate policy—particularly the work of Weitzman (2009, 2011). However, the authors ignore the important paper by Nordhaus (2011), which appears in the same symposium volume as Weitzman (2011). In the end, this book is an advocate's guide that questions and largely dismisses many of the contributions that economists have made to the climate policy debate. The authors' goal seems to be to clear a pathway through a substantial economics literature to establish the need for deep cuts in emissions without the inconvenient truth of needing to worry about the costs of taking action. The authors seem to believe these costs can't be measured using existing tools and don't really matter given the fundamental premise of urgency. This policy approach is dangerous because it has clearly failed as a basis for global climate policy design in the past, where countries have demonstrated that they do care about the costs of climate policies. Costs and the uncertainty about costs of abatement is a major reason why countries do not commit to carbon targets and why emissions continue to rise unabated. Indeed, the presence of extreme uncertainty is behind the price-based approach of Weitzman (2013) and others discussed above that the book ignores.

In recommending a new way forward that rejects using traditional economic approaches, the authors argue that, "The assumption of an extremely steep damage function at some point not far from beyond 2° C threshold *seems* well founded in science." There is enormous uncertainty around the 2° C warming threshold or the presence of an extremely steep damage function near this temperature. Based on this statement, the authors jettison the valuable role that economics does play in climate policy design in favor of a "standards base approach." It is an enormous jump to reject valuable knowledge gained from economic models in favor of an alternative because something "seems" well founded.

The environmental lobby will like this book. However, as a contribution to an important debate on how economics is used to design effective climate policy or as a summary of the contribution to the climate debate of the large body of economics outside IAMs, the book unfortunately falls short.

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Warwick McKibbin
Australian National University and the Brookings Institution

Disasters and the Networked Economy. By J. M. Albala-Bertrand. Routledge Studies in Development Economics. London and New York: Taylor and Francis, Routledge, 2013. Pp. xvi, 196. ISBN 978–0–415–66629–9, cloth; 978–0–203–40667–0, e-book.
JEL 2014–0350

There are two sides to *Disasters and the Networked Economy* by J. M. Albala-Bertrand. The first side is a very convincing description of how natural disasters trigger endogenous adaptation and changes in the economic systems, with all actors trying to mitigate the losses they suffer. The book uses several recent events, such as the 2004 Indian Ocean tsunami and earthquake and the 2010 earthquake in Chile, to show that disaster consequences cannot be understood without investigating local and regional responses from innumerable economic agents, such as households, firms, and public authorities.

From the description of their endogenous responses to a disaster, Albala-Bertrand derives a series of useful policy recommendations that are pragmatic and well grounded. He stresses the need for postdisaster response to be integrated in existing channels, through which affected actors cope endogenously with the shock. For instance, he suggests using existing institutions such as trade unions and employers' organizations to channel support after a disaster, making it more efficient, better targeted, and easier to monitor. He discusses the relative merits and risks of in-kind and cash support with the objective of supporting the adjustment of economic agents. He highlights the need to assess whether a given form of postdisaster support will complement or substitute endogenous responses, to avoid crowding out more efficient private actions. His recommendations are well informed and should be useful to anyone having to manage an emergency situation, or to prepare for one.

One weakness of the book is that it does not cover the literature trying to model the economic network and its response to shocks; for instance the work on bankruptcy propagation along credit chains (Battiston et al. 2007; Gatti et al. 2005; Weisbuch and Battiston 2007). Network models have even been applied to natural disasters and the economic response to them, providing insights into economic resilience. For instance, certain characteristics of firm networks such as concentration (how many clients and suppliers the average firm has) and clustering (a high clustering indicates that a firm's suppliers are likely to also be its clients) have been found to influence firm-to-firm shock propagation and the potential for local economic collapse in simple economic models (Coluzzi et al. 2011; Henriet, Hallegatte, and Tabourier 2012). However, these first attempts to model the network effects are still in their infancy, and they cannot be used for operational loss assessments. Albala-Bertrand is convincing when he stresses the importance of more research in this area.

The second side of Albala-Bertrand's book tries to convince the reader that disasters have no long-term consequences, and that their economic impact is generally overestimated. This part is not as convincing as the first one. The first chapter reviews the limitations of published economic analyses of natural disasters, and essentially sees no value in their findings. Albala-Bertrand rejects statistical analyses of the impact of disasters on growth (e.g., Loayza et al. 2012), because they use simplifying assumptions. Computable general equilibrium (CGE) models are discarded for their assumptions of perfect rationality, and input–output (IO) models for being static and demand-led. Overall, the author does not give any credit to models that are based on CGE or IO structures, even when they include additional features to represent disaster aftermaths (e.g., inventories and product heterogeneity in Barker and Santos 2010 and Hallegatte 2014; or constraints on short-term substitutions in Rose, Oladosu, and Liao 2007).

Most of the authors of these analyses would agree on these limitations, but Albala-Bertrand goes one step further when he concludes that nothing can be learned from them. Thus, the book discards too easily the assessments that find a long-term impact of disasters (at least for some of them), such as Hornbeck (2009) or Jaramillo (2009). Furthermore, in doing so, he confuses the absence of proof with a proof of absence—even if all these studies were wrong, it could not be concluded that disasters have no long-term consequences. If the reader agrees with Albala-Bertrand's conclusion on the uselessness of this literature, then the logical conclusion is that we do not know whether disasters have long-term effects, not that they have none.

The book then offers an analysis of the macroeconomic costs of disasters. This is where the book lacks consistency, since most of the criticisms Albala-Bertrand makes to other studies also apply to his own work. In particular, his analysis relies on many assumptions that are weakly supported by evidence or theory.

He first assumes that the capital destroyed by disasters has a productivity that is one-fourth of average capital productivity. It means that in a simple growth framework with decreasing returns, the affected capital has a productivity that is even lower than the marginal capital productivity, i.e., than the discount rate. In other terms, this capital should not have been built, and if destroyed should not be replaced. He then assumes that reconstruction investments have a multiplier equal to two, even though later in the book, p. 102, he affirms that disaster-related income losses do not imply a multiplier. With disasters affecting only low-productivity capital, no multiplier for income loss, and a large multiplier for reconstruction spending, he finds quite unsurprisingly that disasters increase output and income.

However, these assumptions are only weakly supported by evidence. Some of the infrastructure affected by disasters may have very high productivity, possibly higher than average productivity (DuPont and Noy 2012; Hallegatte, Hourcade, and Dumas 2007). The mere fact that, in most cases, a disaster is followed by very active reconstruction (that displaces other investments) suggests that the damaged capital has a productivity that is larger than the discount rate, i.e., larger than what Albala-Bertrand assumes.

Moreover, throughout the book Albala-Bertrand assumes the existence of large idle resources that can be mobilized to cope with the disaster, and the absence of supply-side constraints. He thus argues that destroyed production capacity can always be compensated by increasing output somewhere else (or later with only a delay in production). With no consideration of supply-side constraints, it is not surprising that disasters—which are largely supply-side shocks—are not found highly detrimental to economic activity.

There are good reasons why an "inefficient" economy with large unused resources is less vulnerable to natural disasters and other supply-side shocks (Hallegatte and Ghil 2008). West and Lenze (1994) illustrate this point on the landfall of hurricane Andrew in Florida in 1992: because half of the workers in the construction sector were unemployed at that time, reconstruction could be done rapidly and with little crowding out of other activities. And nobody would contest that many developing countries have large economic distortions and, thus, do not use their resources efficiently. Nevertheless, it is not so certain that they have large surplus of physical capital. Moreover, it is not obvious that a loss in production capacity can always be compensated by increased production elsewhere. Albala-Bertrand uses the example of a corner shop, whose clientele moves to another shop. However, what is true with a small-scale retail shop may not be valid when entire neighborhoods are destroyed or when large-scale, structural infrastructure is damaged (like the Oakland Bay bridge one-month closure after the 1989 Loma Prieta earthquake).

In fact, the existence of strong supply-side constraints is plainly illustrated by the length of reconstruction periods: reconstruction needs after the 2002 floods in Germany amounted to ten days of German investments; still, complete reconstruction took about three years, due to various technical and financial constraints. Only the careful consideration of these constraints—largely disregarded in Albala-Bertrand's book—can help us understand the economic impacts of disasters, and their effect on welfare.

His conclusion that disasters have no serious economic consequences should thus be taken with care. The reader may note that, with the assumptions used by Albala-Bertrand, countries would benefit from voluntarily destroying their own physical capital, which should give us pause. Overall, the book appears extremely optimistic regarding the ability of the affected population to react to a disaster, and the availability of alternative production capacity that can be easily mobilized. In fact, if the economic system is so good at reacting to a major shock, the reader wonders why it is as inefficient between disasters, as stressed repeatedly by Albala-Bertrand. Moreover, the book does not seem to take fully into account findings regarding the long-term impact of disasters on human capital (e.g., Dercon 2004), or the effect of frequent and repeated events on the ability of households to accumulate capital and assets (World Bank 2013).

There are (at least) three big issues that need to be clarified and defined before any statement on the economic impact of disasters can be made. First, there are time and spatial scale issues. For instance, the definition of *long term* is unclear in Albala-Bertrand's work. Furthermore, the book focuses on aggregate impacts at the country level, in which a shift of economic activity from the affected area to neighboring regions is an adjustment that reduces losses. However, a welfare analysis of the affected population would consider it a net loss. So any statement on the economic impact of disasters is scale dependent, and what is true at the aggregate scale may not be true locally, as illustrated by some excellent work at the local scale (Rodriguez-Oreggia et al. 2013; Strobl 2011)

Second, it should be clarified how the cobenefits from postdisaster support are treated, if they help solve preexisting problems and existing distortions. The most obvious case is the "stimulus" effect from reconstruction spending in a situation of insufficient demand during an economic crisis. Albala-Bertrand assumes that this effect is strong when he uses a multiplicator of two for reconstruction expenditures. Even though reconstruction can indeed increase GDP and income through a stimulus effect, the same benefits could have been obtained from a classical stimulus package, without having to go through a disaster. It sounds, therefore, questionable to attribute the benefits from the stimulus to the disaster as the book does, since they could have been captured without it.

Third and finally, there are many questions regarding the definition of a "catastrophe," which is defined in the book as a complete collapse of the economic system that cannot recover, even in the presence of foreign aid (*"external aid [. . .] cannot re-ignite the system, but only support its now helpless victims,"* p. 78). Even though many would agree that *"natural disasters are unlikely to affect the capability of a societal system to be viable"* (p. 79), it does not mean that natural disasters do not affect welfare and development over the long-term. The definition of a catastrophe used by Albala-Bertrand is quite extreme and this choice explains why natural risks cannot trigger catastrophes in his analysis. Using a more classical definition (e.g., *"an event causing great and often sudden damage or suffering"* according to Oxford Dictionaries), we have plenty of examples where a natural event triggered a catastrophe.

In sum, some statements are weaker and less supported by evidence than others in this book, and I see the strong claim on the absence of long-term impact of natural disasters on the economy as a working hypothesis, not as a solid conclusion. However, the description of postdisaster endogenous responses and network effects is interesting, and this issue is important and underresearched (and often simply overlooked). On these aspects, the book provides accessible and well-informed qualitative descriptions and examples, with relevant policy implications.

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Stephane Hallegatte
World Bank

R Urban, Rural, Regional, Real Estate, and Transportation Economics

Arctic Economics in the 21st Century: The Benefits and Costs of Cold. By Heather A. Conley. Contributing Authors: David L. Pumphrey, Terence M. Toland, and Mihaela David. Washington, D.C.: Center for Strategic and International Studies; Lanham, Md. and Boulder: Rowman and Littlefield, 2013. Pp. iv, 66. Paper. ISBN 978–1–4422–2487–2, cloth; 978–1–4422–2488–9, e-book.

JEL 2014–0364

The Arctic is cold, harsh, remote, and dangerous. It holds massive natural resources and many economic opportunities, but also unusual large costs and huge hidden risks. What makes economic development in the Arctic special? Mostly, it is the extreme cold, the extreme weather, the extreme remoteness, the extreme difficulty in transportation, and the generally untouched nature of the "nature" up there. In addition, climate change (even if it is just variance) is rapidly affecting humans' ability to access these large, heretofore untapped resources.

Arctic Economics in the 21st Century, a sixty-four-page report by the Center for Strategic and International Studies, describes the current state of known resources and economic opportunities primarily in the U.S. Arctic region, and discusses some of the barriers to harvesting these opportunities, including the lack of infrastructure and environmental and regulatory risks. This report is free on the CSIS website (https://csis.org/publication/arctic-economics-21st-century), so as an economist, I suppose my first comment should be that you cannot beat the price.

In a (too) short introduction, the authors describe some of the problems the United States might face in creating "a national economic strategy for the American Arctic . . . in an increasingly resource-constrained and politically polarized environment" (p. 2). There are valuable resources, especially hydrocarbons, that might exceed $1 trillion, as well as "rare earth or so-called strategic minerals, iron ore, nickel, and palladium" (p. 3), but due to cold, remoteness, and a shortage of infrastructure, some of which are unique to the Arctic, substantial challenges remain. (The conclusion chapter is excellent and should probably be read first.) While the authors dedicate a few pages to other Arctic areas (Canada, Greenland, Iceland, and some mention of Russia), the report is mostly about Alaska and the United States.

The report's six main chapters cover hydrocarbon (oil and gas) and mineral extraction, shipping, fishing, ecotourism, and infrastructure. While the report spends too much time on the value of the resources and not enough time on what makes the Arctic a particularly difficult place for economic development, toward the end the report focuses on many of these factors in a way that is useful and illuminating. The chapter on Arctic shipping, which covered the Northern Sea Route, the opening of the Northwest Passage, and the strategically important Bering Strait, demonstrates both the opportunities and risks involved with opening the Arctic. Similarly, the chapter on Arctic ecotourism (with its focus on Arctic cruise ships and the potential for disaster), and the penultimate chapter on Arctic infrastructure investment (or the lack thereof), highlights key issues and risks related to Arctic economic development. The portions of these chapters that point out that the United States is severely lacking icebreakers (and relies on the Russians to break ice to supply Nome, Alaska) should be read by all military and economic policymakers and anyone considering economic activity in the Arctic. The lack of infrastructure such as roads, deep-water ports, and airports will affect the costs of economic development for some time.

However, as economics deals in trade-offs and risks, it would have been nice to have more discussion of the risks involved in the Arctic. While the report mentions risks related to the lack of safety infrastructure, other risks due to uncertainty, extreme weather, and particularly climate variation as they affect business were not developed, even though these risks are a key issue in Arctic economics. Many, such as the risks due to climate variation, are a major focus of discussion elsewhere. The Nobel Peace Prize–winning UN Intergovernmental Panel on Climate Change (IPCC) Working Group II report recently noted that **"There is increased evidence that climate change will have large effects on Arctic communities . . .** Some commercial activities will become more profitable while others will face decline. Increased economic opportunities are expected with increased navigability in the Arctic Ocean and the expansion of some land- and freshwater-based transportation networks" (Larsen et al. 2014, p. 3; bold in the original). The IPCC report also noted climate change effects on resource exploration, agriculture and forestry, open and freshwater fisheries, marine transportation, and infrastructure. While the IPCC report came out after this report, the papers cited in the IPCC report predate this report and, therefore, these risks could have been discussed in more detail.

In the end, this report is exactly that: a report. It tends to be long on individual facts regarding current resources and short on economic analyses of what might change, and why, and what the effects might be. There were not any regressions, data analyses, or even a calculation of benefits and costs for any particular project in dollars and cents. For example, the first few chapters on oil and gas and mineral extraction start with a recitation of facts (how many cubic feet, dollar value, where located, etc.) from publicly available sources. While about the Arctic, one could have written a similar style report about hydrocarbons in North Dakota. These sections could have benefited from more of a focus on what makes Arctic development of these resources more difficult than those in North Dakota, the Gulf of Mexico, Saudi Arabia, or Africa. One area the report does cover is the potential for environmental impact, which, although true in all of these areas, may have special impact in fragile Arctic regions. Overall, however, the report feels at times like it is a summary of Internet searches or a bunch of Wikipedia pages, and so reads more like a particularly good student summary report on the Arctic than original research.

However, perhaps this is the report's true virtue. When I made this comment to my long-time close friend, coauthor, and social scientist Dr. Henry Huntington (Senior Officer–International Arctic for the Pew Charitable Trusts), he wisely commented that even if all the information is available, sometimes it is just nice to have it all in one place. I liked the extensive use of footnotes; just about every other sentence is footnoted with citations from the Internet, which will greatly aid future researchers on this issue. The case study in chapter 2 on Shell's "drilling efforts in the Beaufort and Chukchi Seas" (pp. 14–18) did a great job of combining all of the issues and difficulties of exploring and exploiting natural resources in the Arctic, including illuminating examples of a rig slipping off its mooring and almost running aground, a different rig running aground on Kodiak Island, and the effects of a short and varying season due to "encroaching ice floes" (p. 17).

I have a few minor quibbles. First, while the report has pictures (all of which are in the public domain), there are no tables, graphs, or maps in the report; a table on the location, amount, and value of hydrocarbons, for example, would have been useful. Also, the footnotes indicate that most of the information has come from Alaskan news sources or the U.S. Government, even though the *New York Times*, *Washington Post,* and *Wall Street Journal* have all run large and interesting stories on economic development in the Arctic, as have, I am sure, *Time*, *The Economist*, and other periodicals. It would have been nice to see more citations from more broad-based and established media, particularly as these sources may also have more integrative analyses of the issues involved.

In the end, this report is a good summary of current resources and (lack of) infrastructure in the Arctic, particularly Alaska and the U.S. Arctic. It has a wonderful list of footnotes and sources, which will be helpful to economists and future researchers focusing on energy, mineral extraction, transportation, fisheries, tourism, and climate change, particularly as they relate to the Arctic. It also has interesting issues related to the global political economy and economics/strategy/military security. Given its price (free), the interesting topic (economics in some of the harshest places in the world), and its approachable writing style, it would be appropriate for students in economics, political economics, and resource engineers. It is well written and easy to read, and so should be read by policymakers and all those interested in the Arctic.

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Michael A. Goldstein
Professor of Finance, Babson College