

Book Reviews

Editor's Note: Guidelines for Selecting Books to Review

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

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

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

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

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

A General Economics and Teaching

Uncommon Sense: Economic Insights, from Marriage to Terrorism. By Gary S. Becker and Richard A. Posner. Chicago and London: University of Chicago Press, 2009. Pp. 373. \$29.00. ISBN 978-0-226-04101-8.

JEL 2009-1160

The business model behind this book challenges the way that economists think about markets. First, a famous economist writes down his thoughts on a topic of his choice and sells them on the Internet at a price of zero dollars for unlimited access. Second, another famous economist responds to the first, and his thoughts as well

are sold to anyone with access to a computer at a price of zero dollars. Third, after the two economists' thoughts have aged and ceased to be novel, a publisher collects them, prints them on paper, and sells them for \$29.00 plus tax, even as they remain available on line for free.

Why would a rational, welfare-maximizing consumer pay \$29.00 for articles that are accessible for free? Why are the articles worth more after seasoning than when they were fresh and new? At the least, this marketing strategy should raise questions about the explanatory power of Chicago-style economics, which assumes, among much else, that consumers are rational and that arbitrage will lead markets to clear at a single

price. Unfortunately, *Uncommon Sense*, written by two leading adherents of the Chicago school, too often fails to analyze such discrepancies between theory and reality. In these pages, Gary S. Becker, a University of Chicago professor whose work on human capital and the economics of the family justly earned him the Nobel Memorial Prize in economics, and Richard A. Posner, a federal appeals court judge and prolific author on law and economics, engage in a dialogue on economic policy. When it is good, their dialogue sheds great light on important issues. Too often, though, the interchange leads to predictable assertions that markets work best and to distressingly little introspection about why they may not.

Uncommon Sense is taken verbatim from the blog that Becker and Posner began writing in December 2004, currently accessible at <http://uchicagolaw.typepad.com/beckerposner/>. (The authors shifted to this location in December 2009; at this writing, the older posts remain available at the previous location, <http://www.becker-posner-blog.com/>.) The entries have been grouped into topics, from sex to the university to natural disasters. There are several different entries within each topic, each consisting of an article by one of the authors and a response by the other. At the end of each section, either Becker or Posner contributes a couple of pages of “afterthoughts” noting subsequent developments related to the postings and reaffirming the authors’ views.

Perhaps the most encouraging part of this enterprise is the respectful attention to which each of the authors gives the other’s views. The authors are longtime academic colleagues (in addition to judging, Posner is a lecturer and former professor at the University of Chicago), and the blog has the comfortable feel of a debate that has been in progress for many years over lunch at the faculty club. For those of us who have come to associate blogging with incivility, *Uncommon Sense* demonstrates that polite online discussion is not an oxymoron.

The problem with this book is not the format but rather the content. All too often, the authors slide over the complexities of the issues they address. While their purpose is to expose how economists might think about matters of current debate, in many cases their conclusions suggest

that economists would just as soon avoid questions for which they have no ready answers.

Consider their discussion of one of the world’s more urgent problems—the need to conserve water. Becker points out that the bulk of fresh-water use in the United States is for agriculture and power generation, not for household consumption, and asserts that the absence of appropriate pricing encourages wasteful use of water. His proposed policy response is the use of pricing to reduce demand. Posner sees more important factors contributing to water scarcity, namely public ownership of water systems and property-rights arrangements that encourage excessive consumption. What these two authors’ posts on this subject have in common is an underlying belief that markets alone, properly structured, can resolve the problem of freshwater scarcity.

Before evaluating these assertions, it is worth noting that water is a unique commodity. Consumption of water is essential for the survival of humans and livestock and for the cultivation of most food crops. To this writer, himself a fan of water pricing, the commonplace fact that water consumption is a physical necessity would seem to have important implications for water policy. While anticipated scarcity of oil or iron ore might be expected to encourage substitution, for certain purposes water has no substitutes and is unlikely ever to have substitutes. Pricing and property rights might lead to greater use of desalination, as Posner suggests, but they will not bring forth innovations that replace water. Moreover, while many individuals can and do survive without personal access to oil or iron ore, the same is not true of water, to which every living individual requires access. Distributional considerations are thus of huge importance. Neither Becker nor Posner discusses the characteristics that make water fundamentally different from other resources, perhaps because that would complicate their simple free-market stories.

There is no question that “free” access to water leads to waste, and that pricing would serve to encourage conservation. But Becker, in focusing exclusively on pricing as a way to limit demand, fails to address the likelihood that the variable cost of water is so trivial for many users that even very large price increases may not bring about much change in consumption patterns. Water

pricing is surely a necessary condition for water conservation, but whether it is a sufficient condition is an empirical question that depends on the price elasticity of consumption, not a subject Becker discusses. If price elasticity is low, an effective approach to conservation may well require a mix of policies, incorporating not only pricing but also measures such as requiring low-flow shower heads and water recycling by golf courses, the types of government mandates for which Becker has no use.

Similarly, Posner's preference for private over public ownership of water systems is based on ideology, not economics. It is not at all obvious why a privately owned system necessarily results in greater conservation than a government-owned system. The property-rights issues he mentions are real, as some property-rights arrangements impede pricing and even encourage the waste of water, but Posner passes over the fact that addressing them would engage the government in a massive redistribution of wealth as it reallocates water rights among individuals—hardly the sort of arbitrary government intervention he would approve of. Even then, it is not apparent that clarification of ownership rights, by itself, would lead to adequate conservation in the face of scarcity. Water conservation is a complicated issue, and Becker and Posner perform no service by offering simplistic answers.

A similar unwillingness to confront messy real-world issues is evident in Becker and Posner's discussion of drug patents. Both agree that patents are essential to encourage socially valuable research and development by pharmaceutical manufacturers. Nonetheless, both would shorten patent lives. Becker would have the government approve drugs more quickly and then devote more effort to studying risks and side effects once a drug is in use, allowing a reduction of the patent term without reducing the period during which the drug can be sold under patent protection. Posner believes shorter patent terms will reduce companies' incentives to undertake excessive, socially wasteful research in hopes of discovering blockbuster drugs and lead instead to more valuable research in other areas. All of these points are provocative. But, as Becker admits, U.S. patent protection of pharmaceuticals has led to a perverse situation in which Americans pay

higher prices for drugs than people in the many countries where government price controls are in effect. Put differently, the U.S. patent system leaves American drug purchasers to subsidize pharmaceutical research for the benefit of drug purchasers in other countries. What should be done about this free-rider problem? Should it affect the way the United States handles drug patents? Would linking domestic patent protection for drugs with price controls establish a more reasonable relationship between consumer benefits and research incentives? As with water conservation, the subject is simply more complex than the authors are prepared to concede.

At times, these exercises in playing with economic theory become annoyingly facile. In October 2006, when the arrest of the leader of a polygamist Mormon sect was in the news, a Becker post suggested that polygamy might be treated as a matter of contract law rather than criminal law. If the objection to polygamy is that it exploits women and continues the subjection of women to men, he wrote, then why not allow women to write marriage contracts stipulating that the husband cannot take additional wives and providing for damages in the event of divorce due to violation. “[I]sn't it offensively patronizing to women to believe they cannot make their own decisions about whether to enter into marriages that contain other wives?” he asked (p. 27).

Becker clearly developed this idea to shed libertarian light on the implications of government regulation of behavior, and it is indeed an interesting thought experiment. But he fails to pursue it to a conclusion that contradicts his own views. A moment's thought will make it apparent that writing a marital contract addressing all relevant contingencies related to polygamy—a “complete” contract—would not only be impossible, but could create negative externalities. Imagine a prenuptial contract that allows a man to have a second wife only so long as his annual income exceeds \$100,000. How might such a provision be enforced if the husband's income later falls below the threshold and is inadequate to support both families? Should society permit a contract that gives absolute financial priority to the first wife and her children, even if later wives and their children starve? Or suppose a first wife agrees that her spouse may have up to two children by

a second wife, and the second wife then bears triplets. Should the law condone monetary damages in favor of the first wife, thereby harming the innocent triplets? Becker's modest proposal is less an illustration of the possibilities of resolving social issues privately than of the difficulties inherent in doing so. In fact, it makes a good case for government regulation of the marriage market.

Where this book is at its best is where the authors disagree on fundamental points. New York City's ban on trans fats in food sold in restaurants occasioned such an exchange in December 2006 and January 2007. Posner, surprisingly, supports the ban even though, as he concedes, "A strict Chicago School economic analysis of the ban would deem it inefficient" (p. 139). His reasoning is that most consumers lack the information to measure their total consumption of trans fats and to assess the risk from additional consumption. In this case, the city's ban could provide large economic benefits (such as lower rates of premature death) at little cost, justifying a transgression against libertarian principles. Becker reaches the opposite conclusion, suggesting that the market will respond to consumer concerns about trans fats, leading restaurants to eliminate them without regulation. "[W]hile city and other governments should continue to help provide the best information available about the effects of trans fats and other foods on health, market forces of supply and demand should determine the fats consumed," he writes (p. 145). This debate neatly illustrates important aspects of decision making under imperfect information and sets forth alternative ways of dealing with that imperfection. Becker is happy to allow individuals to make their own choices in the absence of effects on others, whereas Posner insists that "It is not paternalist to delegate a certain amount of decision making to the government" (p. 151), especially when there are some goods, such as a ban on trans fats from restaurant food, that the government can produce more cheaply than the free market.

The overriding conclusion this reviewer takes away from *Uncommon Sense* is one the authors likely did not intend: that economists' views on public issues may have as much to do with values and beliefs as with close analysis of data. Despite

all the scientism of the modern economic enterprise, with its massive equation systems and its impenetrable statistical methods, priors matter. The questions economists ask and the policy views they put forth are heavily influenced by preconceived ideas about the way markets work, the way individuals decide, the proper role of the state, and a host of other factors. There is nothing wrong with this—except that in its efforts to appear scientific, the profession often pretends otherwise. To their credit, Becker and Posner at least have laid their prejudices open to public inspection, and one can judge their insights accordingly.

MARC LEVINSON
Council on Foreign Relations

B History of Economic Thought, Methodology, and Heterodox Approaches

The Life and Times of Raúl Prebisch, 1901–1986.

By Edgar J. Dosman. Montreal and Ithaca: McGill–Queen's University Press, 2008. Pp. xi, 599. \$49.95. ISBN 978–0–7735–3412–4.

JEL 2009–0363

This is the first book length biography of Raúl Prebisch written. The author has provided us an account of Prebisch's life, from his birth in 1901 to his death in 1986. Virtually all economists and political scientists around the world, and not only within Latin America, are familiar with such themes as the center–periphery, declining terms of trade for primary products, import substitution, regional economic integration, generalized trade preferences, and the New International Economic Order. All this terminology, and yet others, formed part of Prebisch's considerable intellectual contribution during his years of executive leadership, first at CEPAL and later, UNCTAD, from the late 1940s to the late 1960s.

But that experience only represents less than half of the length of the book. Much of the rest details Prebisch's complicated relationship with his native Argentina, both before as well as after his international service. There, as a young man, he rapidly rose to manage the Central Bank, which he had virtually created, in the midst of the Great Depression in 1935. Even earlier, he

had been an active participant in economic and trade policy. The Roca–Runciman Treaty of 1933 which granted special privileges to Great Britain, was seen as his product, and Prebisch was continuously vilified for it—much to the distaste of Dosman, who, understandably, virtually always defends Prebisch against his critics.

Between 1935 and 1943, Prebisch was the principal architect of Argentine economic policy. After Peron's rise, he was summarily dismissed. The following years of forced retirement were difficult ones, until his 1949 *Manifesto* led to his extraordinary rise to Latin American and world prominence. But even then, the lure of Argentina persisted. In 1955, Prebisch returned after Peron had fallen, preparing a plan for the successor military regime. Once again there was adverse public reaction, multiplying with his continued stay after the Aramburu coup: as the book says, he became “the most hated man in Argentina.” There would be still another venture, this time at life's end, when he briefly served at the invitation of a newly elected civilian, Raúl Alfonsín. Once more, after having been involved with negotiations for an IMF agreement, he was denounced, coinciding with his eighty-third birthday, for “selling out Argentina to western imperialism.”

Prebisch's earlier experience is essential to understanding his later achievements. Dosman is right in presenting his subsequent rise to international fame more as continuity rather than as a definitive break. Prebisch, himself, in his 1983 essay, “Five Stages in My Thinking on Development,” enumerates a detailed process of evolution of his thinking, with only a bare mention of his “orthodox” period; it is not even included as one of the stages. The book might have been stronger—and more interesting to economists—had this evolutionary hypothesis been set forth and examined against his actual career more explicitly.

For many subsequent Third World thinkers, the secular deterioration in the terms of trade and the diversion of the fruits of technological progress, much empirically elaborated by Prebisch and CEPAL, is also the starting point. However, these go on and wind up with very much stronger and more radical conclusions about continuing First World exploitation of the rest. This is the realm of unequal exchange and dependency

theory, with strong support for policy efforts to achieve North–South delinking. That line of thought took ever greater hold with the ebbing of growth within Latin America, and the rise of military intervention in the 1960s.

Prebisch never was comfortable going quite that far, to the unhappiness of many of the intellectuals attracted to CEPAL. In its day, his advocacy of structuralism, justifying tariff and quota protection against import of industrial products, and evoking in the 1950s active intervention in favor of manufactures in many Latin American countries already was viewed as radical. Early on, Prebisch recognized some of the limitations inherent in that ISI strategy—there is little critical analysis of this central subject within the book, despite an abundant literature—and proposed regional integration as an essential complement. That, too, put in place as the Latin American Free Trade Agreement in 1961, did not work—and still does not—nor did the Alliance of Progress. This last policy, put in place with President Kennedy's election, incorporated many of the CEPAL themes, inclusive of national planning, but early on Prebisch desisted, as it became increasingly apparent that neither the United States nor much of Latin America was prepared to follow through.

Thereafter, Prebisch's next giant step was the creation of UNCTAD. That was an impressive effort, extending his efforts internationally in a fashion comparable to his earlier exit from Argentina. In this more global environment, earlier tensions with the World Bank, IMF, and GATT (and the United States) mounted. These sorts of difficulties, rather than a careful assessment of the many substantive issues in contention, occupy the bulk of the discussion.

By this time as well, ILPES, the institute designed for serious intellectual research in Santiago after his time at CEPAL—which he still chaired—had unraveled. Later, the Pinochet dictatorship supplanted Allende. Prebisch eventually moved to Washington for a period, and ultimately returned to Buenos Aires. His last years were ones of broad international recognition for his lifetime of efforts dedicated to a world of more rapid and equalizing growth.

Dosman fully fulfills the task he had initially set out—to provide a well written and comprehensive

account of Prebisch's life and times. Those more interested in the validity of Prebisch's ideas will have to continue to rely on the accumulated, and still growing, more technical literature relating to the North–South divide.

ALBERT FISHLOW
Columbia University

D Microeconomics

Unequal Democracy: The Political Economy of the New Gilded Age. By Larry M. Bartels. New York: Russell Sage Foundation; Princeton and Oxford: Princeton University Press, 2008. Pp. xiii, 325. \$29.95. ISBN 978–0–691–13663–9.

JEL 2008–0797

Since the mid-1970s, the distributions of income and wealth in the United States have become increasingly unequal, creating what the Princeton political scientist Larry Bartels terms “the New Gilded Age,” in the subtitle of his important new book, *Unequal Democracy*. Bartels begins the book by citing some of the most striking findings from the research of economists Thomas Piketty and Emmanuel Saez. For example, the share of total personal income going to the richest one percent of income-earners in the United States more than doubled between the late 1950s to 2005 from 10.2 to 21.8 percent. Even more dramatic, the share received by the richest 0.1 percent more than tripled over this same period, from 3.2 to 10.9 percent.

Of course, capitalist economies are not designed to create equality. They are rather designed to reward winners in marketplace competition and correspondingly punish losers. This indeed is the motor force that drives capitalist economies to ever-rising levels of efficiency, even as it also promotes inequality. But recognizing this central fact about capitalist economies still does not help answer the question on which Bartels focuses in *Unequal Democracy*, which is why U.S. capitalism circa 2005 was generating a much greater level of inequality than during the 1950s and 1960s.

Economists have been debating this question for a generation, starting with the seminal 1988 book by Bennett Harrison and Barry Bluestone, *The Great U-Turn*. Over time, the dominant explanation among economists has been about

the effects of “skill-biased technical change” on the opportunities available to working people. In this view, the ever-greater integration of computers into economic life has meant that markets increasingly reward those who work effectively with computers and related technical equipment, while correspondingly placing at a growing disadvantage those who lack solid computer-related skills. From this view, rising inequality is primarily due to the impact of technological forces as opposed to policy changes.

Proponents of this position have marshaled a wide range of supportive evidence (e.g., David H. Autor, Lawrence F. Katz, and Alan B. Krueger 1999). But this view has also been challenged by an at least equally strong set of counterevidence. For example, John E. DiNardo and Jorn-Steffen Pischke (1997) showed with German data that the returns for white-collar employees of working with pencils were comparable to those for working with computers (more generally, see also David R. Howell 1999 and David Card and DiNardo 2002). Such counterevidence also connects with alternative explanations for rising inequality in the spirit of arguments initially advanced in *The Great U-Turn*. From this alternative perspective, the rise of inequality has been mainly due to increasing global competition, and the weakening of institutions and policies—such as effective labor unions and minimum wage standards—that can protect working people against the pressures arising from globalization.

Bartels moves aggressively into this debate with an approach that builds from the second, policy-centered viewpoints but pushes these arguments further to reach a simple and striking conclusion: that the Democratic Party overall supports greater equality and the Republican party supports greater inequality. Thus, the more Democrats are elected to office, especially the Presidency, U.S. society becomes more equal, and the more Republicans are elected, U.S. society becomes more unequal.

Bartels presents an impressive array of statistical evidence to support his position, including the results of formal modeling exercises. For example, over roughly a half-century starting in the 1950s, Bartels shows that, on average, the real incomes of middle-class families have grown twice as fast under Democrats as they have under

Republicans, while the real incomes of working poor families have grown six times as fast under Democrats as they have under Republicans. He also finds that the average unemployment rate was much lower under Democratic administrations (4.8 percent) than Republican administrations (6.3 percent).

If, as Bartels finds, Democratic Party policies are indeed dramatically more favorable to middle- and lower-income people, and if such people constitute a decisive majority of the U.S. electorate, then this raises another obvious question: why don't Democrats dominate U.S. politics; and indeed, why, at least through George W. Bush's reelection in 2004, was the Republican party ascendant? Bartels pursues this question vigorously, still relying primarily on formal statistical modeling exercises to uncover explanations that have not been apparent through the more casual empiricism that dominate in the media.

Bartels devotes a full chapter to debunking the claim that Republicans success had been due to white working class voters making their political choices increasingly on the basis of hot-button cultural issues, such as abortion, religion, and gay marriage, as opposed to economic concerns. Bartels rather finds economic well-being consistently remains the first concern of white working class voters, and that these voters are broadly committed to greater economic equality. But he also finds that, to a significant extent, white working class voters frequently respond to economic concerns in ways that, anomalously, end up helping Republicans. His most surprising finding is that working class voters respond more positively to rising income growth for upper-income households than to income gains that primarily benefit themselves. Bartels argues that this is at least in part caused by the effectiveness of political advertising in setting the terms of political debates around election times. Republicans did have substantially larger advertising war chests over the time period on which Bartels focuses, and they exploited that advantage effectively.

The second half of *Unequal Democracy* presents a series of case studies in the political economy of inequality. These include a discussion of the 2001 tax cuts under George W. Bush, which were sold effectively as providing tax relief

across-the-board, even while the vast majority of benefits flowed to the rich. Bartels also examines the debate around repealing the estate or "death" tax. His findings here are quite surprising—that working class voters strongly oppose the estate tax as a matter of principle, even though they themselves would not benefit financially from its repeal.

Overall, *Unequal Democracy* provides fresh perspectives and wide-ranging statistical evidence to the task of explaining why the United States has become dramatically more unequal over the past fifty years. However, just as Bartels is able to show how previous analyses have missed major aspects of the overall story, his own arguments are similarly weakened by inattention to important parts of the historical record. This becomes evident even by considering data featured prominently in *Unequal Democracy* itself.

Most telling here is Bartels's figure 2.2 (p. 35), which plots the rise of inequality under Democratic and Republican administrations from 1947 to 2005. My own eyeballing of this figure conveys to me a pattern quite unlike that described in Bartels's verbal discussions. Thus, the figure shows that overall inequality had not risen by the end of the Republican Eisenhower administration in 1960 relative to when it began in 1952. Moreover, inequality rose under the Democratic presidency of Jimmy Carter at roughly the same sharp rate as it had in the immediately preceding Republican administrations of Nixon and Ford. Finally, considering Bill Clinton's Presidential term overall, inequality did not fall at all relative to the historically high levels that were attained under Ronald Reagan and the first President Bush. In fact, Bartels's figure 2.2 shows that the *only period* between 1947 and 2005 when the United States became significantly more equal was during the Kennedy/Johnson years.

Because Bartels overlooks this major historical pattern, he also neglects the dramatic transformation in policy-setting circles undergirding it. This was the shift in policy-setting influence from an ascendant Keynesian social democratic framework, whose U.S. intellectual leaders included Paul Samuelson and James Tobin, to what is often termed a "neoliberal" framework, led, of course, by Milton Friedman. This shift occurred during

the high inflation years of the 1970s, between the presidencies of Richard Nixon and Jimmy Carter. It was the Republican Nixon, after all, who declared that “I am now a Keynesian” when he imposed wage and price controls in 1971. By 1979, the year before Carter’s reelection bid, major leaders on Wall Street forced him to appoint Paul Volcker as Chairman of the Federal Reserve. Volcker quickly engineered a punishing recession as a means of eliminating double-digit inflation.

More broadly, the transformation of the U.S. policy environment meant that macro policy no longer aimed to balance concerns over inflation and unemployment, but rather concentrated almost entirely on inflation control. It was also during the Carter presidency that the movement toward deregulating business—including, in particular, global trade and financial markets—began gathering strong momentum. Indeed, the most significant formal moves to deregulate U.S. financial markets occurred during the Clinton presidency, when most of the remaining features of the 1930s-era Glass–Steagall financial regulatory system were repealed.

Bartels is clearly aware of this shift in the broad U.S. policy landscape, writing, for example, that “When Bill Clinton entered the White House in 1993, he apparently felt a good deal more constrained by the Federal Reserve Board and the bond markets than previous Democratic presidents had been” (p. 55). Bartels’s observation coincides closely with Clinton’s own views, expressed only weeks after winning the 1992 election, that “We’re Eisenhower Republicans here We stand for lower deficits, free trade, and the bond market. Isn’t that great?” (quoted in Bob Woodward 1994, p. 165). Important differences between Democrats and Republicans on economic policy did continue after the 1970s. But the terms of the debate had shifted decisively away from New Deal/Great Society-type commitments to egalitarianism.

As a result of having neglected this dramatic transformation of the U.S. political landscape, Bartels is also less well positioned to explain the significant extent of myopia and susceptibility to advertising that, according to his own evidence, shapes the political choices of white working class voters. It may be that such factors become more powerful forces in a political environment

in which the differences between Democrats and Republicans had diminished on central matters relating to economic equality.

Bartels could not have anticipated the more recent dramatic shift in U.S. politics: the recapturing of both houses of Congress by Democrats in 2006 and Barack Obama’s decisive election to the Presidency in 2008. One of the most notable features of the Obama’s Presidential campaign was his massive fundraising advantage over his Republican opponent John McCain. This neutralized the tool that, according to Bartels, had been a major factor in producing electoral victories to Republicans for the past generation.

Roughly one year into Obama’s Presidency as I write, it remains an open question whether Obama and the Democratic Congress will implement a strongly egalitarian program in the New Deal/Great Society tradition, or a tepid “Eisenhower Republican” type agenda, as with Bill Clinton. In either case, *Unequal Democracy*—because of both its many substantial achievements as well as its significant shortcomings—will serve as an important reference for anyone seeking a serious understanding of the ongoing politics and economics of inequality in the United States.

REFERENCES

- Autor, David H., Lawrence F. Katz, and Alan B. Krueger. 1998. “Computing Inequality: Have Computers Changed the Labor Market?” *Quarterly Journal of Economics*, 113(4): 1169–1213.
- Card, David, and John E. DiNardo. 2002. “Skill-Biased Technological Change and Rising Wage Inequality: Some Problems and Puzzles.” *Journal of Labor Economics*, 20(4): 733–83.
- DiNardo, John E., and Jorn-Steffen Pischke. 1997. “The Returns to Computer Use Revisited: Have Pencils Changed the Wage Structure Too?” *Quarterly Journal of Economics*, 112(1): 291–303.
- Harrison, Bennett, and Barry Bluestone. 1988. *The Great U-Turn: Corporate Restructuring and the Polarizing of America*. New York: Basic Books.
- Howell, David R. 1999. “Theory-Driven Facts and the Growth in Earnings Inequality.” *Review of Radical Political Economics*, 31(1): 54–86.
- Woodward, Bob. 1994. *The Agenda: Inside the Clinton White House*. New York: Simon & Schuster.

ROBERT POLLIN

University of Massachusetts, Amherst

Rational Decisions. By Ken Binmore. Gorman Lectures in Economics. Princeton and Oxford:

Princeton University Press, 2009. Pp. x, 200.
\$40.00. ISBN 978-0-691-13074-3.

JEL 2009-0402

Ken Binmore's new book reviews the classical theory of expected utility maximization with objective or subjective probabilities. The emphasis is on assumptions and interpretations, but mathematics is not completely avoided and the book offers sketches of the key proofs. Interlaced with this review is the development of some elements of a new theory of decision making, which applies when decisionmakers do not have well-formed subjective probabilities. All this is presented in the informal and erudite, sometimes polemical style that Binmore's readers are familiar with from his other books, and that is sometimes a pleasure and sometimes a pain. The content of the book was presented by Binmore as the "Gorman Lectures" (in honor of the Irish economist Terence Gorman) at University College London in 2006. This book is the second Gorman lecture that has been published in book format.

Ken Binmore has long emphasized that the classical, "Bayesian," theory of decision making is appropriate only in "small worlds," and that a different theory is needed for "large worlds." These expressions come from a classic text of Bayesianism, Leonard J. Savage's *Foundations of Statistics* (Savage 1972). "Small worlds" are worlds in which the decisionmaker can think through all possible future sequences of events and actions. "Large worlds" are worlds in which this is not possible. Binmore argues that he is reiterating a view expressed by Savage himself if he restricts the applicability of Bayesian decision theory to small worlds. Binmore is aware and acknowledges that his own views, although in line with some informal comments of Savage's on this subject, seem at odds with Savage's more mathematical discussion of small worlds (Savage 1972, chapter 5.5). In Binmore's view, most of our decisions are made in large worlds. When reviewing Bayesian decision theory, Binmore emphasizes the implicit small world assumption. The new theory that Binmore then proposes is intended to apply to large worlds.

There is, of course, a long tradition in decision theory of exploring alternatives to Savage's theory that do not require decisionmakers to hold

unambiguously defined subjective probabilities. An early literature in the 1950s introduced criteria such as "maximin" or "minimax regret." The next wave of articles, initiated by Truman Bewley, Itzhak Gilboa, and David Schmeidler in the 1980s, introduced amongst others the idea into the economics literature that decisionmakers' beliefs are represented by sets of probability distributions rather than just one probability distribution. Most recent work has sought to allow larger classes of functional forms for decisionmakers' choice criteria.

The theory that Binmore proposes in this book has much in common with the theories that were developed since the 1980s. He considers a framework in which a decisionmaker chooses from acts that give in each possible state of the world a lottery over outcomes where this lottery can be described by objective probabilities. This is the well-known decision theoretic framework of Anscombe and Aumann. The uncertainty concerns the likelihood of the different states of the world. As in the recent decision-theoretic literature, decisionmakers have sets of probability distributions, rather than just one probability distribution, for the states of the world. Binmore then suggests this choice criterion: decisionmakers translate each possible lottery into an equivalent lottery that gives only the best and the worst outcome positive probability. Decisionmakers then calculate, using their sets of probability distributions over states, what is the smallest and what is the largest possible probability of the best outcome. Decisionmakers have a Cobb-Douglas utility function over the smallest and the largest probability assigned to the event that the best outcome obtains, and choose the act that maximizes the value of this utility function.

The appearance of a well-defined set of possible states of the world is at first sight puzzling, given the earlier emphasis on large worlds in which, we are told, decisionmakers can't think through all possible future states of the world. Presumably, the states of the world that are included in the model are to be interpreted as those that the decisionmaker can contemplate, while the decisionmaker is aware that there is a larger, more complex uncertainty in the background. But the details of this interpretation, and the logical link

with the specific choice theory proposed here, do not become clear in Binmore's work.¹

Binmore gives an axiomatic justification for the Cobb–Douglas functional form that decisionmakers use in his theory to evaluate the lowest and highest probability of the best prize. Binmore does not seek axiomatic justifications for the existence of a lowest and highest probability, but he does discuss how such sets of probabilities arise, in particular in the context of objective randomization devices. His starting point is Richard von Mises's theory of objective probabilities based on the concept of "collectives," that is, sequences of sample outcomes that exhibit in some sense randomness, yet convergent long-run frequencies. Binmore discusses this theory in detail, and then modifies it so that it can accommodate the case in which only a set of objective probabilities for some event can be identified. This set is constructed as the set of accumulation points of the long-run frequencies. The set is not a singleton if the long-run frequencies of different outcomes fluctuate for ever. The discussion of von Mises's theory is one of the most interesting parts of the book. Binmore motivates this discussion with the idea that it will later help to construct also subjective probabilities, but this idea is not taken up later.

An important theme of the book is the updating of beliefs, Bayesian or not. Binmore explains how this updating of beliefs has to be very different in a small and in a large world. However, Binmore's own theory does not include any non-trivial updating. Binmore bypasses the problem by considering the belief adjustment only in the case that events are observed to which a single probability can be assigned.

The research contribution of this book thus consists of some very persuasive arguments about the importance of large worlds, and some attractive formal ideas about decision making that can, perhaps, be applied to large worlds, but that need to be worked out further. The book doesn't offer, and doesn't claim to offer, a complete theory of decision making in large worlds.

¹ Sujoy Mukerji (1997) and Paolo Ghirardato (2001) are examples of papers that investigate the connection between the large worlds problem and the expected utility model with nonadditive probabilities, which is closely related to the expected utility model with multiple priors.

The exposition of classical decision theory, which takes up the larger proportion of the book, includes revealed preferences, von Neumann and Morgenstern's expected utility theory with objective probabilities, and a version of Savage's theory of expected utility maximization with subjective preferences. There are digressions on probability theory, utilitarianism, and game theory. The intended contribution is a careful discussion of concepts and interpretation rather than of the mathematics. But to me it seems that the book succeeds at the opposite: the exposition of the mathematics is frequently masterful in its simplicity, whereas the discussion of concepts and interpretations is sometimes hampered by sloppiness, and it is too often sidetracked by polemics that overly simplify the views of those with whom Binmore disagrees.

Binmore's emphasis is on the exposition of classical decision theory and his own alternative, not on the exposition of other alternatives to classical decision theory. His discussion of the very early literature, concerned with minimax, minimax regret, etc., goes into detail. But the later literature is only occasionally referenced, although Binmore's own work is clearly related to this later literature. A useful complement to Binmore's book that the reader might consult for an overview of other alternatives is Gilboa's recent Econometric Society monograph, *Theory of Decision under Uncertainty* (Gilboa 2009).

If one focuses on Ken Binmore's novel research ideas, one might say that this is a book-length presentation of research that deserves to be worked out in more detail, and then to be reported in field journal articles. At another level, this is a textbook of decision theory that is more interesting and thought provoking than most alternatives, but that is at times too polemical and too easily sidetracked. Students can learn classical decision theory from this book, but need to be mature enough to know that even a very opinionated author is sometimes wrong, and that nothing can replace a student's own critical thinking. An excellent public lecture makes a book of slightly ambiguous nature. It is a book, though, that this reviewer found very rewarding.²

²I am grateful to Peter Klibanoff for discussions about issues related to this book.

REFERENCES

- Ghirardato, Paolo. 2001. "Coping with Ignorance: Unforeseen Contingencies and Non-additive Uncertainty." *Economic Theory*, 17(2): 247–76.
- Gilboa, Itzhak. 2009. *Theory of Decision under Uncertainty*. Cambridge and New York: Cambridge University Press.
- Mukerji, Sujoy. 1997. "Understanding the Nonadditive Probability Decision Model." *Economic Theory*, 9(1): 23–46.
- Savage, Leonard J. 1972. *The Foundations of Statistics*. Revised and enlarged edition. New York: Dover Publications.

TILMAN BÖRGERS
University of Michigan

Theory of Decision under Uncertainty. By Itzhak Gilboa. Econometric Society Monographs. Cambridge and New York: Cambridge University Press, 2009. Pp. xiv, 214. \$85.00, cloth; \$29.99, paper. ISBN 978-0-521-51732-4, cloth; 978-0-521-74123-1, pbk.

JEL 2009-0793

After reading David M. Kreps's (1988) *Notes on the Theory of Choice* many years ago, I thought it inconceivable that I would ever again see such disarming informality and good humor in the presentation of the key ideas and theorems of decision theory. I was wrong. Itzhak Gilboa's new book, while bringing Gilboa's unique perspective clearly to the fore, reminds me a lot of the joy of reading Kreps.

While the comparison to Kreps is very natural given the stylistic similarity and the fact that both books are about decision theory, the books are very different. I think Kreps's book will remain my recommendation for students wanting to learn the fundamentals of decision theory in a very user-friendly way. However, I would now also recommend Gilboa for learning the *why*, in addition to the *how*—the philosophy in addition to the mathematics. To be sure, Kreps's book has its philosophical side, but this is vastly more emphasized in Gilboa's book than in Kreps's.

This is particularly evident in the book's first five chapters. Unlike Kreps, Gilboa does not go directly into the mathematical modeling of decisions. Instead, in chapter 2, he first asks whether the possibility of formulating such a model is consistent with the modeled agent possessing free will. That is, if an outside observer

knows the choice the agent will make, does this mean that the agent is not truly free to do otherwise? Similarly, Gilboa is not content to take as given that we know what "probability" means. Indeed, one of the main themes of the book is that we have no idea what this loaded concept is all about. The stage is set for this theme in chapters 3 through 5, where Gilboa analyzes various views of what probability is and how we would define it—namely, the principal of insufficient reason, relative frequencies, and subjective probabilities. Much of the discussion is deeply philosophical, analyzing, for example, Hume's critique of the logical basis for induction as part of his consideration of probability as relative frequency. Readers eager to get to the decision theory may get impatient with these chapters, while readers who want to think about the underpinnings of our usual theories will find the discussion quite fascinating.

Only after this discussion does Gilboa begin his analysis of classical decision theory. He covers the existence of utility functions on uncountable sets, eschewing the usual topological approach of a continuity assumption in favor of the path less taken of separability. I found it refreshing to see this approach—in all honesty, I'd forgotten how elegant it is. I will teach this material his way next time. He also discusses intransitive indifference and semiorders, a topic I find very interesting but which most texts do not even mention. He gives the classic example of a sequence of cups of coffee, each with one grain of sugar more than the preceding cup, to illustrate the idea. (Personally, I prefer a sequence of bottles of beer, each with one drop of water replacing one drop of beer relative to the preceding bottle, but each to his own.)

Next, he turns to the Holy Trinity: namely, von Neumann–Morgenstern, Savage, and Anscombe–Aumann, the classic theorems on expected utility with known probabilities (von Neumann–Morgenstern), unknown probabilities (Savage), and a mix of the two (Anscombe–Aumann). In all three cases, his focus is much more on philosophy (i.e., what does this mean?) rather than mathematics (i.e., how do we prove this?). For example, while Gilboa does discuss three different proofs of the von Neumann–Morgenstern theorem, none of the three is given in as much detail as in Kreps or most micro texts like Andreu

Mas-Colell, Michael D. Whinston, and Jerry R. Green (1995). He says nothing at all about the proof of Anscombe–Aumann’s representation theorem (though, as he notes, the proof is pretty straightforward).

Finally, while both Gilboa and Kreps go through all of the Savage axioms and what they mean, Kreps spends a large portion of his time discussing qualitative probabilities and the notion of “tight” and “fine” to show mathematically how subjective probabilities are derived from preferences. Gilboa, on the other hand, discusses this in a page or two before turning to two chapters on how we should think about these results. One chapter discusses the idea of states of the world and related conceptual issues, including my own least favorite example, Newcomb’s Paradox. The other chapter turns to a more traditional critique of the axioms, particularly state independence and the sure-thing principle, with nice discussions of Aumann’s example of a man who faces uncertainty about whether his wife will survive surgery and of the Ellsberg Paradox.

After this, Gilboa is on to material not in Kreps or any of the standard textbooks. After his cogent critique of subjective expected utility, he turns to three of the primary models proposed to address these failings. The reader unfamiliar with decision theory may think these three models are chosen because they are areas in which Gilboa and his frequent coauthor David Schmeidler have worked. However, it would be more accurate to say that Gilboa and Schmeidler have worked on these three models because they are clearly the primary alternatives out there.

First, Gilboa discusses Schmeidler’s (1989) notion of Choquet expected utility. In a masterful piece of exposition, Gilboa achieves what I never even thought to try in a classroom: he explains why the Choquet integral is the “right” way to compute expectations with respect to a nonadditive probability measure. I plan to be bolder next time I teach this. He also discusses how the rank-dependent probabilities of prospect theory relate to Choquet expected utility.

Next, Gilboa turns to maxmin expected utility, due to Gilboa and Schmeidler (1989) where the decisionmaker is modeled as having a set of probability beliefs and evaluating any given action

according to its minimum expected utility over the set of beliefs. He also compares it to Truman F. Bewley’s (2002) model of Knightian uncertainty. In Bewley’s model, again, the decisionmaker has a set of probability beliefs, but here one action is preferred to another if and only if it gives higher expected utility for every probability belief in the set.

Finally, he discusses Gilboa and Schmeidler’s (2001) notion of case-based decision theory. In a sense, this discussion brings the book full circle. Gilboa began with a careful discussion of how we should think about probability, focusing primarily on empirical frequencies and subjective probabilities. Here he obtains something which looks a lot like subjective probability but where these probabilities generalize the notion of empirical frequencies.

As emphasized earlier, this book is focused much more on the why than the how. While it contains some nice proofs and clear explanations, a student who wants to know the main theorems and how to prove them will need to supplement this book with Kreps or one of Fishburn’s excellent books. However, for any student or researcher who wants to focus on the why question, this is one of the best books out there to read.

REFERENCES

- Bewley, Truman F. 2002. “Knightian Decision Theory: Part 1.” *Decisions in Economics and Finance*, 25(2): 79–110.
- Gilboa, Itzhak, and David Schmeidler. 1989. “Maxmin Expected Utility with Non-unique Prior.” *Journal of Mathematical Economics*, 18(2): 141–53.
- Gilboa, Itzhak, and David Schmeidler. 2001. *A Theory of Case-Based Decisions*. Cambridge; New York and Melbourne: Cambridge University Press.
- Kreps, David M. 1988. *Notes on the Theory of Choice*. Boulder and Oxford: Westview Press.
- Mas-Colell, Andreu, Michael D. Whinston, and Jerry R. Green. 1995. *Microeconomic Theory*. Oxford and New York: Oxford University Press.
- Schmeidler, David. 1989. “Subjective Probability and Expected Utility without Additivity.” *Econometrica*, 57(3): 571–87.

BARTON L. LIPMAN
Boston University

Institutions and Economic Performance. Edited by Elhanan Helpman. Cambridge and London: Harvard University Press, 2008. Pp. vii, 611.

\$55.00. ISBN 978-0-674-03077-0.

JEL 2008-1204

Elhanan Helpman has collected thirteen fascinating chapters which ask, from a variety of viewpoints and with a wealth of alternative approaches, what the economic consequences of a society's institutional arrangements are. What makes these chapters fascinating is the frequency with which they challenge accepted wisdom, or question evidence based on simple cross-sections. The wealth of approaches—from theory to historical analysis, to case studies and panel data econometrics—goes along with the variety of questions asked, which range from the time-honored issue of the effects of inequality on growth, to more unusual topics such as why can small guerrilla movements be so stable and long-lived. Rather than providing a thorough review of the book—and not doing justice to many of the contributors—I shall entice the reader with a few examples of what this book offers.

Catholic countries, where the family lies at the center of society, share a number of characteristics. Female participation in the labor market is relatively low, since the survival of family values requires women spending enough time at home. Labor mobility is also low because you can only benefit from your family if you live close to your relatives. Family firms are relatively frequent. The provision of welfare by the State or other public entities is limited since children, the elderly, and those in poor health are looked after by various family members. Along with nonmonetary transfers (e.g., daughters taking care of elderly parents and parents-in-law), monetary transfers within the extended family are also frequent and explain why a spell of unemployment in a Catholic country affects individual consumption less than elsewhere (Samuel Bentolila and Andrea Ichino 2008). The organization of Catholic societies helps understand why groups that are ethnically homogeneous and rich in social ties find it less necessary to rely on formal institution, such as a government-run welfare program. But why are Catholic countries also laggards in growth? What are the drawbacks of a society built around the family?

Siwan Anderson and Patrick Francois in their contribution to the book argue that the problem with groups organized around kinship is that

such ties make it difficult to punish transgressors. Formal rules, instead, help enforcing punishments by taking some of the authority in decision making out of the hands of group members. This conjecture helps to explain a peculiar finding from an experiment run in a large slum in Nairobi, inhabited by a variety of social groups. Contrary to the hypothesis that ethnic homogeneity makes formalization less necessary—Anderson and Francois find that groups formed along ethnic lines are more likely to adopt formal procedures. The difficulty an ethnic group faces in punishing transgressors would seem to offer an argument for the view that family firms underperform competitors run by professional managers and owned by outside investors—a view which runs against David Landes's thesis in his book *Dynasties*. So, why do Catholic societies remain family-based and keep underperforming? Why, differently from the groups in the Kenyan slum, are they reluctant to adopt more formal procedures? This is the fascinating question this chapter raises.

The empirical and theoretical findings on the effects of inequality on growth are mostly inconclusive. Daron Acemoglu and his coauthors, in their chapter in the volume, explain that this is because most studies fail to distinguish between economic and political inequality. The effects of economic inequality on growth depend on the degree of political inequality and thus on the quality of a country's institutions. When institutions are weak—such as in Colombia in the nineteenth century, the case they study—economic inequality helps because it provides a counterbalance for rapacious politicians. The interest of this “inverted U” hypothesis goes beyond the growth literature. It challenges the time-honored view of Latin American underdevelopment that attributes the diverging paths of North and South America to their different levels of economic inequality. It is also consistent with earlier work on Africa (Robert H. Bates 1981): greater land inequality (in Kenya) was conducive to better outcomes because large farm owners had the power to check politicians. In Ghana, a large number of small farmers were unable to solve their collective action problem and failed to restrain politicians from engaging in highly distortionary policies.

Why is the discovery of a natural resource (oil in Norway or silver in Spain-controlled Peru in

the sixteenth century) a curse for some countries but not for others? Starting from this observation, Auricio Drelicham and Hans-Joachim Voth analyze the reasons why Spain, after having become the richest country in the world, declined so rapidly. The simple answer is that resources are a curse where institutions are weak. But the case of Spain is more challenging because by the time of the *Conquista*, in the late fifteenth century, Spain had developed reasonably good institutions with a balance of power between parliament (the Cortes) and the king. So why didn't these institutions prevent silver turning into a curse? What happened is that the flow of silver enriched the Spanish kings and weakened the Cortes because parliament lost its main instrument to control the sovereign: denying the fiscal resource for his military adventures. While in Britain the warring instincts of Henry VIII's were constantly reined in by the state of his finances, the belligerence of Charles V and Philip II's was encouraged by the sheer size of the financial resources at their disposal. Through its effect on institutions, the windfall from American silver eventually sentenced Spain to a centuries' long decline.

Returning to growth in the Americas, another fallacy the book exposes is the view, associated with the work of Stanley L. Engerman and Kenneth L. Sokoloff (2005), according to which the different paths followed by various countries in the Continent can be traced back to different factor endowments that resulted in differences in the use of production based on slave labor. Countries that had a comparative advantage in crops produced by slaves in large plantations (such as sugarcane) had extreme economic inequality. This hampered the evolution of those institutions that are necessary for sustained growth—for instance voting rights and the provision of public schooling. Nathan Nunn, in his contribution to the volume, confirms the finding of a negative association between past slave use and current economic performance. But he finds no evidence that large plantations, and thus large-scale slavery, was more detrimental for growth than other forms of slavery. In fact, small-scale plantation slavery seems to have been more harmful for growth. This finding debunks the view that differences in factor endowments explain the growth experience across the Americas.

How can guerrilla movements remain so small and still last so long and impose such deadly outcomes?

What makes guerrilla warfare—and the associated problem of organizing a counterinsurgency—so different from conventional military confrontations? James Fearon suggests that the answer lies in the information externality associated with insurgencies. Counterinsurgency works by gathering intelligence about who and where the active rebels are. This means that the larger the insurgency movement the higher the risks of infiltration, betrayal, and detection. These diminishing returns to guerrilla warfare explain why this kind of conflict tends to remain small, stable, and deadly. Fearon, however, is less convincing in his attempt to explain why guerrilla warfare is more likely in poor countries. He starts debunking the conventional view that poverty makes for civil war because in poor countries there are more poor, underemployed people who find rebellion attractive as a “job.” If this was the case, guerrilla warfare should be even more frequent in rich countries because higher per capita income means that there is more wealth to tax or appropriate. But after this smart observation, Fearon is left with the only option of assuming that risk aversion rises with the level of income, a hypothesis for which the evidence is not compelling. A more plausible explanation might be that, as income rises, the effectiveness of counterinsurgency improves faster than the ability of guerrillas.

Avner Greif, in an intriguing chapter, discusses the constraints an administration imposes upon elected politicians. This is an important and mostly overlooked issue (an exception is Alberto Alesina and Guido Tabellini 2008). Politicians though entrusted with a mandate from the electorate, can only govern through an administration. This gives administrators the power to control politicians. One of the major effects of constitutions—and possibly the main reason constitutions seem to affect prosperity—is, according to Greif, the constraint they impose upon administrators. Readers who have spent some time in government will find this obvious. But Greif is among the first to point out this important role of constitutions.

Discussing the institutional origins of the industrial revolution, Joel Mokyr has contributed what

in my view is the most forward looking chapter in the volume. Mokyr argues that the traditional emphasis on the role of British formal institutions in making the industrial revolution possible has been overemphasized: the role of formal institution has been less crucial than North's interpretation has suggested. What has been overlooked is the role of a "culture" that created an environment in which inventors and entrepreneurs could operate and cooperate freely. Mokyr identifies these cultural traits in the British gentlemanly, which meant that a "gentleman" could be trusted: gentlemanly, more than formal institutions, provided the shared code that made possible the rise of capitalism.

The relationship between culture and institutions, to which Mokyr hints at in his contribution, is at the center of some of the most interesting research developments of the past few years. Some authors have worked on trying to identify the role of culture in affecting both institutional arrangements and economic outcomes—a task that faces a formidable identification problem. If, as suggested by Luigi Guiso, Paola Sapienza, and Luigi Zingales (2006) that culture is "those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation" (p. 23), then culture is a time-invariant characteristic of a society. Thus it cannot be separated from other time-invariant characteristics, such as geography or the origin of the colonizing power in case of developing countries (see, e.g., Tabellini 2008 and Francesco Giavazzi, Fabio Schiantarelli, and Michel Serafinelli 2009 for attempts at solving the identification problem). Others (Guiso, Sapienza, and Zingales 2008), following Putnam, trace a country's current culture back to its institutions many centuries ago. The feedback between culture and institutions is a fascinating and yet unresolved research question.

In his introduction to the book, Helpman observes that "the rapid transformation of this area of scientific enquiry has been achieved thanks to the dismantling of disciplinary barriers." It is a sign of this dismantling that the most forward-looking chapter in the volume has been written by Joel Mokyr, an historian.¹

¹ I thank Alberto Giovannini for insightful comments.

REFERENCES

- Alesina, Alberto, and Guido Tabellini. 2008. "Bureaucrats or Politicians? Part II: Multiple Policy Tasks." *Journal of Public Economics*, 92(3–4): 426–47.
- Bates, Robert H. 1981. *Markets and States in Tropical Africa: The Political Basis of Agricultural Policies*. Berkeley and Los Angeles: University of California Press.
- Bentolila, Samuel, and Andrea Ichino. 2008. "Unemployment and Consumption Near and Far Away from the Mediterranean." *Journal of Population Economics*, 21(2): 255–80.
- Engerman, Stanley L., and Kenneth L. Sokoloff. 2005. "Colonialism, Inequality, and Long-Run Paths of Development." National Bureau of Economic Research Working Paper 11057.
- Giavazzi, Francesco, Fabio Schiantarelli, and Michel Serafinelli. 2009. "Culture, Policies and Labor Market Outcomes." National Bureau of Economic Research Working Paper 15417.
- Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2006. "Does Culture Affect Economic Outcomes?" *Journal of Economic Perspectives*, 20(2): 23–48.
- Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2008. "Long Term Persistence." National Bureau of Economic Research Working Paper 14278.
- Tabellini, Guido. 2008. "Institutions and Culture." *Journal of the European Economic Association*, 6(2–3): 255–94.

FRANCESCO GIAVAZZI
Bocconi University

F International Economics

The Development and Testing of Heckscher–Ohlin Trade Models: A Review. By Robert E. Baldwin. Ohlin Lectures series. Cambridge and London: MIT Press, 2008. Pp. xi, 227. \$35.00. ISBN 978–0–262–02656–7.

JEL 2009–0819

The Heckscher–Ohlin (HO) or factor abundance theory of comparative advantage remains pivotal to the field of international trade. Understanding the empirical validity of this theory is therefore crucial, not only as an explanation of the direction of international exchange but also as a basis for inquiry regarding the effects of trade and trade restrictions on factor prices and the distribution of income. In this timely monograph, Robert Baldwin surveys the key theoretical developments over the past seventy-five years that have enriched the HO theory and more importantly the key empirical work that has sought to test the HO theory as an explanation of

the pattern of international trade. Since I was a central contributor to efforts during the 1980s to test the HO theory, I was particularly interested to read how one of the most respected international trade economists of our time would chronicle perhaps one of the most important periods in the field of international trade. I was not disappointed. Baldwin provides a highly readable and informative look at the evolution of a field from one focused on the machinations of abstract theoretical models to one increasingly immersed in data and rigorous statistical analysis. I particularly welcomed the retrospective sections on what Ohlin regarded as important for understating trade patterns relative to what later mathematical framings of the core HO propositions by theorists such as the late Paul Samuelson, whose enormous contributions during the 1940s and 1950s were perhaps instrumental to cementing the belief that the HO cum Samuelson theory was “the theory” of international trade. The elegance of the theoretical general equilibrium framework constructed was so seductive that Wassily Leontief’s (1953) now infamous empirical finding that the trade pattern of the United States did not fit the prediction of the HO theory was an “inconvenient truth” that sent shock waves through the field of international trade. The first reaction was to consider what theoretical deviations could account of Leontief’s result. These efforts were then quickly followed with increasingly refined and rigorous empirical tests employing large datasets spanning numerous countries. Baldwin covers all these developments with insight and depth.

Chapter 1 introduces the subject of the monograph and also provides a brief historical perspective on the key insights of Heckscher and Ohlin. Chapter 2 reviews the key developments in trade theory over the past seventy-five years. This starts with the basic structure of the Heckscher–Ohlin–Samuelson model and its prediction for the pattern of trade. This is then followed by a discussion of key implications of the basic neoclassical general equilibrium framework: factor price equalization, the relationship between goods prices and factor prices (the Stolper–Samuelson theorem), and the relationship between factor supplies and goods production (the Rybczynski theorem). The chapter then moves to the development of the Heckscher–Ohlin–Vanek (HOV) or factor content

model; this model is the basic framework of all modern tests of the HO proposition that a country will export its relatively abundant factors and import its relatively scarce factors. Finally, the chapter covers developments associated with new trade theory including differentiated products and the form of the HOV model when factor prices are not equalized between countries.

Chapter 3 discusses Leontief’s (1953) famous test, its paradoxical finding for the United States, and the ultimate resolution by Edward E. Leamer (1980) who showed that Leontief’s method of computing the ratio of the factor content of exports to the factor content of imports may not correctly identify a nation’s abundant factor within the framework of the HOV model. This chapter contains a detailed critique of Leamer’s methods and, on this account, Baldwin presents a highly useful diagrammatic analysis of the influence of a nation’s trade balance on the computation of a nation’s factor contents and how this would impact inferences regarding a nation’s abundant factors. One thing that could have been made clearer in this discussion is that the different “paradoxes” Baldwin discusses, such as those reflected in the seemingly contradictory findings of Leamer and Richard A. Brecher and Ehsan U. Choudhri (1982), is that these differences stem from a difference in the definition of factor abundance that each study sought to reveal. As pointed out in my textbook, *Applied International Trade Analysis* with Abraham Hollander and Jean-Marie Viaene (Harry P. Bowen, Hollander, and Viaene 1998, chapter 8, footnote 13), the computations of Brecher and Choudhri (1982) reveal absolute factor abundance (i.e., whether a factor is exported or imported) whereas those of Leamer reveal relative factor abundance (i.e., whether the net exports of one factor exceeds those of another factor when the content of a factor in net exports is divided by its total supply within a nation). Hence, both Brecher and Choudhri and Leamer were right vis-à-vis their respective choice of the definition of factor abundance.

Chapter 4 reviews the major multicountry multifactor tests of the HO theory based on the HOV model including that of Bowen, Leamer, and Leo Sveikauskas (1987), Daniel Treffer (1995), and Donald R. Davis and David E. Weinstein (2001). But Baldwin also includes a number of less often

cited papers such as Robert W. Staiger (1988), Staiger, Alan V. Deardorff and Robert M. Stern (1987), and Peter Debaere (2003). I personally think this chapter is very well balanced and provides a real sense of the discovery process over this period with respect to trying to understand why the HOV model did not perform well in the data. As the efforts of this body of work now indicate, for the HOV model to “fit the data” one needs to allow neutral differences in productivity and the absence of factor price equalization. However, as Baldwin notes, while these modifications enable the HOV model to better fit the data, we still lack an explanation of the neutral differences in total factor productivity between countries.

Chapter 5 turns from testing the HO trade theory using the HOV model to empirical investigations of three core propositions derived from the neoclassical general equilibrium framework that embeds the HO theory: the Stolper–Samuelson theorem, the Rybczynski theorem, and the Factor Price Equalization theorem. This chapter is not as exhaustive as earlier chapters, but Baldwin does a nice job of picking the more important empirical studies that have examined for these relationships and in particular the debate as to whether the globalization induced changes in the volume and composition of trade can account for change in the relative returns to factors of production. In this regard, the subject matter of this chapter probably contains the most fertile ground for future research.

In conclusion, this is a highly readable and standalone guide that both documents and critiques the enormous efforts over the past fifty years to assess the empirical validity of the HO theory as well as key propositions derived from the neoclassical general equilibrium framework. The monograph can certainly be used in place of the chapter or two in advanced trade theory texts such as Robert C. Feenstra (2005) (and, yes, even Bowen, Hollander, and Viaene 1998) that cover these topics. I highly recommend this monograph as a “must read” for any serious scholar of international trade.

REFERENCES

Bowen, Harry P., Abraham Hollander, and Jean-Marie Viaene. 1998. *Applied International Trade Analysis*. London: Palgrave Macmillan.

- Bowen, Harry P., Edward E. Leamer, and Leo Sveikauskas. 1987. “Multicountry, Multifactor Tests of the Factor Abundance Theory.” *American Economic Review*, 77(5): 791–809.
- Brecher, Richard A., and Ehsan U. Choudhri. 1982. “The Leontief Paradox, Continued.” *Journal of Political Economy*, 90(4): 820–23.
- Davis, Donald R., and David E. Weinstein. 2001. “An Account of Global Factor Trade.” *American Economic Review*, 91(5): 1423–53.
- Debaere, Peter. 2003. “Relative Factor Abundance and Trade.” *Journal of Political Economy*, 111(3): 589–610.
- Feenstra, Robert C. 2004. *Advanced International Trade: Theory and Evidence*. Princeton and Oxford: Princeton University Press.
- Leamer, Edward E. 1980. “The Leontief Paradox, Reconsidered.” *Journal of Political Economy*, 88(3): 495–503.
- Leontief, Wassily. 1953. “Domestic Production and Foreign Trade: The American Capital Position Re-examined.” *Proceedings of the American Philosophical Society*, 97(4): 332–49.
- Staiger, Robert W. 1988. “A Specification Test of the Heckscher–Ohlin Theory.” *Journal of International Economics*, 25(1–2): 129–41.
- Staiger, Robert W., Alan V. Deardorff, and Robert M. Stern. 1987. “An Evaluation of Factor Endowments and Protection as Determinants of Japanese and American Foreign Trade.” *Canadian Journal of Economics*, 20(3): 449–63.
- Trefler, Daniel. 1995. “The Case of the Missing Trade and Other Mysteries.” *American Economic Review*, 85(5): 1029–46.

HARRY P. BOWEN
Queens University of Charlotte

I Health, Education, and Welfare

The Money Myth: School Resources, Outcomes, and Equity. By W. Norton Grubb. New York: Russell Sage Foundation, 2009. Pp. xv, 400. \$35.00. ISBN 978-0-87154-366-0.

JEL 2009-0900

In both practice and research, school finance has historically been treated as a subject quite distinct from educational policy. Attention was focused primarily on revenues and expenditures and on how resources were distributed across school districts, with little or no attention to what went on within schools or to student outcomes. In recent years, the achievement focus of the standards movement has strengthened the linkages between school finance, general issues of educational policy, and student outcomes. That

shift is visible in school finance court cases that now focus less on distributional equity and more on whether the funding is adequate for desired student outcomes, and also in debates about how to make money more productive (Helen F. Ladd and Edward B. Fiske 2008; National Research Council 1999).

In *The Money Myth: School Resources, Outcomes, and Equity*, W. Norton Grubb continues this expansion of scope by exploring the relationship between various types of resources at the classroom level and student outcomes and by going beyond the traditional equity concerns of school finance to reconceptualize the meaning of an egalitarian education system. He sets out four goals for the book. The first is to analyze the money “myth,” which he defines as the view that money is the answer to most educational problems. The second is to link effective resources at the classroom level in high schools to a wide range of student outcomes. The third is to elaborate the concept of educational equity and the fourth is to provide a new and “grand” narrative of what an egalitarian school system would look like. The goals are ambitious and his success in meeting them is uneven. One limitation is that he combines precise empirical analysis in some sections with assertion and opinion in others.

The first part of the book, which comprises five chapters, provides the basis for what Grubb somewhat awkwardly refers to as the “new, improved school finance.” Key to this approach is his argument that policymakers and the courts should focus less on the distribution of money across schools and more on the distribution of the resources that are effective in promoting student outcomes. He defines four types of resources starting with “simple” resources such as teacher–pupil ratios and teacher salaries that, as he documents, are closely linked to per pupil spending on schools. He then introduces three other types of resources that are much less closely linked to spending: “compound” resources such as staff development time, “complex” resources such as innovative teaching or teacher use of time, and “abstract” resources such as school climate and principal control. To determine which of these resources are effective and to what extent, he argues that it is important to focus on what goes on in schools and classrooms. That is, one must

get inside the proverbial black box of education production functions.

To estimate the effectiveness of various types of resources, he makes extensive use of data from National Educational Longitudinal Survey of the Class of 1988 (NELS88). Although he acknowledges that the surveys are dated—which they clearly are—Grubb chose this data set because it includes data on multiple measures of student outcomes, information on school and classroom characteristics and practices, and rich measures of family background. His analysis is based on large empirical models that are for the most part relegated to appendices so that he can focus in the text on the results, divided into categories of explanatory variables. This approach works quite well except that the careful reader would have appreciated more complete notes for the appendix tables clarifying what sets of variables are included in which specification.

Although the analysis is ambitious and addresses many policy relevant issues, the interpretation is sometimes misleading and even odd. One concern is how Grubb categorizes variables into his various concepts of resources. For example, he treats the placement of a student in a particular high school track as a “compound” resource and the percentage of students receiving subsidized lunches in the school as an “abstract” resource. From the perspective of policymakers, more familiar terms such as instructional practices, school climate, or peer groups, might have been more helpful. Another concern is that he interprets his models as causal when in fact they measure associations. To cite just one example, Grubb places a causal interpretation on his finding that placement of a student in the general (in contrast to the academic) education track in high school is associated with lower achievement, and then uses that finding in several places throughout the text to argue that money is not used effectively. An alternative explanation is simply that the weaker students are placed in that track. More generally, without attention to what determines the distribution of various types of resources across schools and classrooms, Grubb is on shaky ground making any causal statements about the effectiveness of many of the variables he calls resources.

Most useful in this first part of the book is Grubb’s careful attention to the students

themselves as resources (chapter 4). For this analysis, he goes beyond simple measures of family background such as the income and educational levels of students' parents to examine the determinants of students' "connectedness to schooling" (p. 41). In contrast to other parts of his analysis, he is more upfront about the potential limitations of his modeling effort in this chapter and usefully supplements it with a review of qualitative studies. That allows him to draw attention to two sets of hard-to-measure factors that may affect student outcomes. One set includes measures of family or community dysfunction and of school instability that arises from the chaotic housing market for low income households. The other set refers to the counterproductive ways that some students are treated in the classroom, such as through zero tolerance policies, low expectations, or harsh correctives. His analysis leads directly to the conclusions that to improve educational outcomes for disadvantaged students, educators will need to change practices within schools and policymakers will need to supplement school policies with a variety of noneducational policies designed to address the needs of children outside the classroom.

In this way, Grubb provides convincing evidence that money is not the solution to all education problems. That conclusion will come as no surprise to most readers of this book. Indeed the basic starting premise of the book, namely that many people believe that more school funding alone will improve student outcomes, is a straw man. One need only refer to the major education reform effort of the past twenty years—standards-based reform—to make that point. Designed to assure that education policies support ambitious student outcomes in a coherent manner, standards based reform goes far beyond school funding as a policy panacea. Grubb's contribution to current policy discussions thus comes less from his analysis of effective school resources and more from his emphasis on students as a resource and the concomitant need for noneducational policies to address the challenges that many of them face.

In the second part of the book (chapters 6–8), Grubb elaborates on the meaning of school finance equity. Chapter 6 lays out a "landscape of equity" in which he highlights the multiple, and

often conflicting, interpretations of equity that have emerged in the legislative, legal, and academic debates over time. That chapter also introduces several dynamic concepts of equity, which he systematically and usefully quantifies in chapter 7 using the longitudinal data from NELS88. One conclusion from that analysis is that the main predictors of the divergence across schools of students' test scores as they progress from grades eight to twelve are family demographic and family background measures. He also provides evidence of bursts of inequity that occur at the transition points, such as the middle school to high school transition. Chapter 8 moves away from the national data and focuses on case studies of twelve schools in the San Francisco Bay area to shed light on the practices of schools serving disadvantaged students. This chapter is intended to demonstrate the inadequacies of current school approaches to the challenges of dynamic inequity documented in the previous chapter. Given the book's focus on high schools, it is a bit odd that only three of those schools are high schools.

In the final third of the book, Grubb develops the implications of his approach for school level practice (chapter 9); district, state and federal roles (chapter 10); and school finance litigation (chapter 11). In the final chapter, he elaborates his grand scheme for promoting a more egalitarian education system and makes policy recommendations related both to the operation of schools and to family background factors outside the school.

In chapter 9, he argues that schools need to become learning communities, and that to do so they need to promote school leadership models in which decision making is distributed among many educators, including teachers. This argument draws heavily on the excellent work on distributed leadership of other researchers such as James P. Spillane (2006) and is at best only weakly linked to Grubb's own empirical findings. The link, he argues, is that "compound" resources require cooperation within schools and that "abstract" resources require a community of resources. Yet, it is notable that his empirical models include no measures of different types of school leadership. Grubb then goes on to argue in chapter 10 that, if schools are going to operate as effective learning communities, they must have the capacity to do so. Thus, the challenge

for the Federal government, states, and school districts, he argues, is to develop more coherent approaches to capacity building at the school level. Much of discussion in that chapter revolves around the failures of current policies to focus on capacity building. As one example, Grubb highlights the federal and state accountability policies that impose pressures for quick fixes focused on narrow outcome measures rather than providing incentives for capacity building.

With respect to school finance litigation, Grubb recognizes that the court system has been in a better position to address and rectify inequities in spending than in complex resources for which the remedies become difficult to specify and monitor. As a result, he is critical of much of the past litigation in this area. Among the new legal approaches he identifies as promising is the one used in *Williams v. State of California* (2004). In contrast to previous cases that focused on whether states provided sufficient funding to districts to achieve specific state outcome goals for students, the Williams case shifted the focus to individual schools and to specific inputs. In particular, it requires all schools to have fully credentialed teachers, up-to-date text books, and appropriate facilities and, importantly, gives families the right to file complaints against schools that are out of compliance. Grubb appears to applaud this decision for its attention to school resources that have been shown to be effective in raising student achievement—although, curiously, none of them are specifically measured in this book—and the self-enforcing features associated with the complaint mechanism. At the same time, this case seems far removed from the more ambitious vision of educational equity that Grubb supports.

That broader vision of an egalitarian system is the subject of the final chapter, which starts with a wide ranging discussion of three approaches to school reform. Grubb identifies these as the administrative/progressive approach with its focus on efficiency and data-driven decisions; the neo-liberal approach with its focus on principals as entrepreneurs and teachers as expendable; and the complex/constructivist approach that he presents as his new vision. Grubb defines this approach as one that emphasizes instruction, the effective use of resources, and attention to the needs of students. Moreover, it requires

distributed leadership, internal accountability, and professional learning communities within schools; attention to capacity building within the education system as a whole; and noneducational policies to address the nonschool needs of students.

The book is useful in that it draws attention to several timely policy issues. First, Grubb's empirical analysis highlights the multiple educational goals of high schools, not all of which can be measured by test scores. With his use of twelve outcome measures, including both test and nontest measures, he shows that some resources, such as guidance counselors, are more predictive of higher graduation rates than of test scores. The implication is that accountability systems that focus on test scores alone may well generate pressure for resource patterns not consistent with a broader set of educational goals. Second, he convincingly shows that money may well be necessary in some cases but is often not sufficient. That is certainly not the new idea that Grubb suggests, but is still worth highlighting and is directly relevant to school finance litigation given that courts typically find it easier to monitor funding patterns than patterns of hard-to measure resources.

Third, he highlights that what goes on inside schools and classrooms matters, and that we may need new ways of structuring schools and the nature of leadership within schools to make them work more effectively. Related is the central role that states and districts must play in building capacity within schools. There appears to be a big leap, however, from Grubb's empirical analysis of student outcomes to his conclusions about the nature of the appropriate types of school restructuring. Although many readers are likely to agree with his argument that collective leadership in schools would be desirable and that districts need to pay more attention to developing capacity at the school level, such conclusions are at best tenuously related to his extensive empirical work.

A fourth issue is that what goes on outside schools can also have large effects on student outcomes over time and that, therefore, any effort to improve educational outcomes must include policies to address the disadvantages of families that impinge on their children's success in school. Included would be increased attention to

areas such as health and nutrition services, family support and child welfare services, and housing policies to minimize mobility. This observation provides a much needed counterweight to the common tendency for policymakers to expect schools alone to offset a wide range of social disparities.

It is notable, however, that Grubb's "new improved school finance" pays no attention to the sources of tax revenue for schools or to school aid formulas. So readers interested in those old fashioned (but to some of us still important) issues will not find them in this book. Moreover, despite Grubb's attention to multiple education outcomes, there is virtually no reference to the standards movement that put ambitious curriculum and student outcomes at the heart of education policy reform.

Both researchers and policymakers would do well to pay attention to Grubb's focus on the need for capacity building within schools and districts and on the noneducational policies related to family background that are necessary for an egalitarian education system. Readers should not expect to find any silver bullets; indeed Grubb is careful to emphasize there are none. He concludes with what many readers may interpret as a discouraging note, namely that a whole new politics—one that replaces self interest with a politics based on principle—may be required to achieve his vision of an egalitarian education system.

REFERENCES

- Ladd, Helen F., and Edward B. Fiske, eds. 2008. *Handbook of Research in Education Finance and Policy*. New York and London: Taylor and Francis, Routledge.
- National Research Council. 1999. *Making Money Matter*. Committee on Education Finance, edited by Helen F. Ladd and Janet Hansen. Washington, D.C.: National Academies Press.
- Spillane, James P. 2006. *Distributed Leadership*. San Francisco: Wiley, Jossey-Bass.

HELEN F. LADD
Duke University

J Labor and Demographic Economics

Against the Tide: Household Structure, Opportunities, and Outcomes among White and Minority Youth. By Carolyn J. Hill, Harry J. Holzer, and Henry Chen. Kalamazoo, Mich.:

W. E. Upjohn Institute for Employment Research, 2009. Pp. ix, 181. \$40.00, cloth; \$16.00, paper. ISBN 978-0-88099-342-5, cloth; 978-0-88099-341-8, pbk. *JEL* 2009-0921

Why do Hispanic and especially black youths continue to experience disadvantages in education, labor markets, nonmarital parenting, and incarceration? Does growing up in a household headed by a single parent raise the risk of socioeconomic disadvantage in adulthood? If so, why do children from "nonintact" households fare worse? And what can and should be done to improve their outcomes?

These are the central questions addressed by Carolyn J. Hill, Harry J. Holzer, and Henry Chen in their slim book, *Against the Tide*. The first chapter introduces issues and provides a detailed summary of the data, methods, and findings. The second describes outcomes in young adulthood (the early twenties), and how the status of young adults changed between 1986 and 2004, based mainly on the 1979 and 1997 National Longitudinal Surveys of Youth (NLSY). The data show persistent, yawning gaps, especially between blacks and whites, in educational attainment, grades, test scores, unmarried parenthood and, among young men, arrests and incarceration. For example, in 2004, about 12 percent of whites were high school dropouts, compared to 21 percent of Hispanics, 19 percent of black women, and nearly 28 percent of black men. Drop-out rates for blacks did not budge between 1986 and 2004, while they fell by 18 percentage points for Hispanic men, eight points for Hispanic women, six points for white men and three points for white women (who have the lowest drop-out rates).

The third chapter describes changes between the late 1970s and the late 1990s in household structure for twelve-year olds, and estimates relationships between household structure at age twelve and outcomes in young adulthood. Analyses examine whether household structure accounts for differences in young adult outcomes among blacks, white and Hispanics. At age twelve, 57 percent of both white and Hispanic youths lived with both biological parents, compared to 20 percent of black youth. Though the small difference in household structure

between whites and Hispanics cannot account for Hispanic–white differences in outcomes, the enormous household structure differences between blacks and whites provide a potentially more promising explanation for black disadvantages in young adulthood. Youth who lived with both biological parents at age twelve fare better on most outcomes in their early twenties, yet the regression results suggest that household structure explains large portions of black–white gaps only in the areas of educational attainment and male incarceration. Interestingly, their regressions show that household structure accounts for only about two points of a huge (22 point) black–white gap in ASVAB (Armed Services Vocational Aptitude Battery) test scores. Thus, academic aptitude or learning does not appear to be an important mechanism through which household structure differences contribute to black–white gaps in educational attainment. This result is echoed in a recent analysis of the NLSY79 and NLS97 that shows marked improvement in the relative test scores of blacks but far less improvement in relative schooling (Joseph G. Altonji, Prashant Bharadwaj, and Fabian Lange 2008). Also consistent with earlier studies (e.g., Sara McLanahan and Gary Sandefur 1994), estimates in *Against the Tide* indicate that there are only small differences in outcomes among youths who were raised by a divorced, a remarried, or a never married mother (or father).

The fourth chapter explores mechanisms that might link household structure to young adult outcomes in the 1997 cohort. Family income around age twelve plays only a small mediating role, much smaller, in fact, than found in studies of the NLSY79 cohort (McLanahan and Sandefur 1994). Human capital enrichment, parenting and neighborhood safety together explain 10 to 40 percent of the disadvantages associated with household structure.

The fifth chapter summarizes and offers suggestions for research and policy. Hill, Holzer, and Chen call for future research to focus on causal inference, both for estimates of the overall effects and for mechanisms. The bulk of chapter 5 is devoted to an evidenced-based, critical discussion of five “policy efforts”: discouraging single parenthood, raising income of unmarried working parents, improving schooling and neighborhood

environments of youth, improving parenting and supervision of youth, and limiting racial disparities in employment and incarceration/crime.

Scholars or policy analysts who seek an overview of recent changes in families and in young adult outcomes will appreciate the careful descriptive information presented in *Against the Tide* and the informative policy discussion. Readers familiar with prior research will value *Against the Tide* as a replication and refinement (with more recent data) of the earlier literature. Teachers of graduate courses in social policy, family sociology or labor economics will want to consider adding *Against the Tide* to their lists of assigned readings.

The primary weakness of *Against the Tide* is its treatment of causality. The technique used to support causal inference is regression with individual fixed effects (differences over time) or family fixed effects (differences between siblings). Readers who find those approaches valuable despite their limitations will be disappointed that the authors present no coefficient estimates or confidence intervals from these regressions. Instead they report *F*-tests of the joint significance of household structure variables. Though household structure variables are sometimes jointly significant, without coefficient estimates, the signs and sizes of the effects of most interest are not discernable.

The many strengths of *Against the Tide* include careful documentation of trends in household structure and youth outcomes, and a logically organized set of descriptive analyses that are explained clearly and interpreted carefully. Hill, Holzer, and Chen sustain focus on differences in outcomes and processes among Hispanic, white, and black youth, and by gender, a practice that is far too rare in this literature. Though Hill, Holzer, and Chen do not successfully answer every question they pose, in *Against the Tide* they make a strong case for the importance of research to improve causal inference and to inform policymakers about improving the lives of disadvantaged youths.

REFERENCES

- Altonji, Joseph G., Prashant Bharadwaj, and Fabian Lange. 2008. “Changes in the Characteristics of American Youth: Implications for Adult Outcomes.” National Bureau of Economic Research Working Paper 13883.

McLanahan, Sara, and Gary Sandefur. 1994. *Growing Up with a Single Parent: What Hurts, What Helps*. Cambridge and London: Harvard University Press.

SANDERS KORENMAN
City University of New York

K Law and Economics

When Brute Force Fails: How to Have Less Crime and Less Punishment. By Mark A. R. Kleiman. Princeton and Oxford: Princeton University Press, 2009. Pp. xxi, 231. \$29.95. ISBN 978-0-691-14208-1.

JEL 2009-1365

Two of the most dramatic social phenomena of the last half century in the United States are the substantial rise in crime that occurred during the 1960s and the equally dramatic drop in crime that began roughly contemporaneously with the advent of the Clinton Administration. The good news is that we have improved things from the violent and crime-filled days of the late 1980s and early 1990s; the bad news is that we have increased our prison population immensely in the effort. We may now be enjoying the return to the crime levels of the early 1960s, but we also have a prison and jail population that is almost seven times larger.¹

In his lucid and insightful new book, Mark Kleiman, a Professor of Public Policy at UCLA, offers a set of proposals designed to do better: he wants to cut crime in half over the next ten years, but this time while reducing rather than increasing the prison population. In addition to his comprehensive understanding of the criminal justice system and the areas of social spending that also might favorably influence crime, Kleiman is broadly knowledgeable about the relevant theoretical and empirical research, candid, politically pragmatic, and astute. I have long admired his ability to pull together disparate strands of research and select reasonable point estimates from the din of conflicting empirical studies in order to provide a cogent assessment of the costs

and benefits of competing policy choices, and this skill is on display throughout the book. Unlike so many who write in the area of criminal justice, Kleiman is relentlessly nonideological and non-doctrinaire, so he doubtless steps on the toes of many ideologues from both left and right.

Since Gary S. Becker (1968), economists have embraced the first broad theme of Kleiman's book—that we should be thinking in a more consequentialist manner about crime and how to reduce it at reasonable cost. (John J. Donohue 2005.) Once that proposition is established, almost everything turns into an empirical question about what works and how much it will cost. Kleiman artfully distills literatures and resolves scholarly debates while acknowledging (in discussing the relationship between drugs and crime) that the issues are “conceptually complex and empirically obscure” (p. 155). This description applies to issues throughout the book, from the impact of guns, police, and incarceration on crime to questions about the potential crime-reducing effect of programs from preschool enrichment to lead paint abatement. Many of his proposals are based on Kleiman's best read of the very conflicting evidence, which means that it is unlikely that he will get every call correct—he comments with some appropriate restraint that a “well-evaluated experiment” showed that nurse home visitation for expectant mothers reduced the arrests of their high-risk children by 69 percent compared to the matched control group! (p. 127)—but I suspect he will have a much higher batting average than most.

The main theme of Kleiman's book is that the United States has poured too much money down the mass incarceration sinkhole, and that by changing direction through a combination of better criminal enforcement practices and prudent expenditures on numerous social programs, we can achieve the desired 50 percent reduction in crime while inflicting less pain on our citizens. Economists will be very comfortable with Kleiman's call for a clear focus on the goal of reducing the total costs of crime, including direct victimization costs, precautionary and avoidance costs, as well as enforcement costs. But although Kleiman doesn't mention it, those who followed some of the suggestions of George J. Stigler in his seminal 1970 article on optimal enforcement

¹ There were 332,945 prison and jail inmates in 1960 (Justice Policy Institute 2000). According to the Bureau of Justice Statistics, there were 2,304,115 prison and jail inmates in 2008.

of the law will be at odds with Kleiman's other broad theme that the most important way to change behavior through the criminal justice system is through swift and certain punishment.

In 1764, Beccaria published a famed treatise "On Crimes and Punishments" in which he stressed that greater criminal deterrence results from certainty and swiftness of punishment rather than from increased severity. Unfortunately, some have interpreted Stigler's work as reversing the Beccarian prescription by arguing that risk-averse criminals could be deterred at least expense by catching fewer criminals (thereby reducing detection and trial costs), while punishing the few that are caught more harshly. According to Stigler (1970), "increasing the punishment would seem always to increase the deterrence" (p. 527), which is true if other things remain equal. Unfortunately, they do not. As Kleiman stresses, "severity is the enemy of certainty" and swiftness. (p. 173–74). The Stiglerian recipe means costly and delayed punishments with far less certainty than is optimal. Sadly, apparent Nobel-level "advances" in economics actually undermined previous knowledge, but it is good to see Kleiman restoring the enlightened thinking on criminal justice to where it was—almost 250 years ago!

One of Kleiman's favorite examples of how important it is to get these concepts right stems from the development of a Hawaiian probation program, called H.O.P.E. An unusually enterprising judge perceived the abject failure of the Stiglerian recipe of catching only a few probation violators and hitting them hard, and switched to a Beccarian approach of providing much less severe but immediate sanctions to almost all violators. The result, Kleiman tells us, was a dramatic move from a high-crime, high-punishment equilibrium to one of low crime and low levels of punishment. H.O.P.E. reduced probation violations by 90 percent, and has grown from its original 35 probationers to 1,000. Kleiman envisions this program could be scaled up nationally to include 400,000 probationers, with major benefits for crime reduction, although he judiciously concedes that until we try no one knows whether crack users on probation in Los Angeles would respond in the way that meth users on probation in Hawaii did.

Interestingly, Becker once announced at a University of Chicago Law School seminar that he and Stigler had disagreed on another point that is central to Kleiman's analysis, again with Stigler championing what Kleiman strongly argues is the incorrect position. The issue is whether one should count the pain inflicted on prison inmates as a social cost in conducting the cost-benefit analysis of incarceration. Becker stated that, while he thought you should, Stigler thought you shouldn't, since the point of incarceration was to inflict pain on the criminals. The problem with Stigler's view, however, is that it can lead a society to inflict tremendous pain even though the same crime reduction could be achieved from equivalent expenditures that did not impose such suffering. We lock up one percent of our adult population, while concentrating that burden on certain minority groups. Tens of thousands of prisoners in the United States are in long-term solitary confinement. Kleiman argues it is a moral imperative to consider whether different approaches can achieve similar crime reduction benefits at lower cost. Presumably, if one billion spent on, say, reducing the amount of lead in the air could reduce crime as much as one billion spent on more solitary confinement, we should be opting for the improved air quality. Ignoring the costs imposed on prisoners and their families will lead to too much "brute force."

These are important lessons, and hopefully, legislators, judges, and prosecutors—who are often blind to the strategic ways in which criminal law and enforcement can be intelligently structured to reduce crime—can absorb them. Of course, some of the problems are more structural: for example, heavy expenditures on preschool enrichment programs that will reduce crime in ten or fifteen years are not attractive to politicians more concerned about the next election than the next generation. (Donohue and Peter Siegelman 1998.) With some acidity, Kleiman notes that "The sheer impatience of citizens and politicians demanding that Something Be Done About Crime right this minute has in it an ironic echo of the inability of many criminals to take the future fully into account in deciding whether to commit a crime today" (p. 126). Academics and citizens who are concerned about crime will also profit from reading Kleiman's book, which is

clearly written in crisp prose with no equations and relatively few graphs. One could easily stimulate student discussions on how to attack crime by reviewing Kleiman's final chapter, which offers sixteen pages of suggestions arrayed in ten categories, ranging from policing, prosecution/courts/sentencing rules, and corrections to drug policy, guns, and social services.

Fighting crime is all about choices and sacrifices. It is conceivable we could engineer the desired 50 percent drop in crime while cutting the prison population by 500,000 by simply legalizing marijuana, cocaine, heroin, and methamphetamines. We already know that ending Prohibition in 1933 paved the way for enormous crime drops by eliminating a massive illegal market. But even if widespread legalization of currently illicit substances dramatically reduced crime and criminal justice enforcement (and corrections) costs, it would come at the expense of dramatic increases in the number of addicts and substance abusers, of which we now have roughly 18 million, most of whom abuse alcohol (Donohue, Ewing, and Peloquin forthcoming).

Kleiman, who has previously written extensively on drug issues, doesn't want to follow this complete-legalization approach, but instead sets forth an elaborate set of recommendations that he thinks can achieve a major crime reduction with fewer disruptive social consequences than we would observe in a world in which substances as harmful as cocaine were legal. (Note cocaine and other drugs prohibited in the United States have been pretty close to legal in Portugal since 2001 with little sign of dire consequences, but then again the United States tends to be more out of control than most European countries these days. What works in Portugal may not be advisable given the large and vulnerable U.S. underclass.)

Contrary to Kleiman, some have argued that the U.S. increase in incarceration has been clearly beneficial, and indeed should be expanded further. (John DiIulio championed this position in a 1996 article "Prisons Are a Bargain, by Any Measure.") To give a sense of the manner in which Kleiman analyzes various policies, consider whether even *greater* levels of incarceration could achieve his desired 50 percent crime reduction. If one believed the relatively high elasticity estimates suggesting that an increase in incarceration of 50 percent would lead to a crime

drop of 10 percent, we would need six doses of 50 percent inmate-population increases to get close to Kleiman's desired fifty percent cut in crime (the result would be a 47 percent drop in crime). Starting from roughly 2.5 million in prison today, this would leave us with 28.5 million in prison at an added cost of roughly \$780 billion (at \$30,000 per inmate per year). While Kleiman doesn't go through this precise calculation, one can distill his likely response from his general discussions on mass incarceration.

First, Kleiman would note that, while the costs of crime today are enormous, they probably are less than \$1 trillion now that crime has already fallen so dramatically. Therefore, spending \$780 billion to get a 50 percent crime-reduction benefit valued at less than \$500 billion is not a sound investment. Second, Kleiman would note that the elasticity estimate of 0.2 (which on p. 113 he attributes to Donohue 2009a) is likely too high, so that the real crime drop from the move from 2.5 million inmates to 28.5 million would yield less than the desired 50 percent crime drop. For example, with an elasticity of 0.1, the added \$780 billion expense in housing 26.5 million more prisoners would buy a reduction in crime of only 22.5 percent, which would further degrade the appeal of prison expansion. Third, the costs of prison are greater than simply the budgetary cost of running the prison and feeding the inmates. Lost productivity of inmates who could have legitimate jobs is a nontrivial cost and the suffering of those behind bars as well as of their families, however measured, would only add to the social costs of the prison buildup strategy (Donohue 2009a). The bottom line is that we are unlikely to get another 50 percent drop in crime via greater punitive harshness, and even if we could, it would not be cost effective to do so, since the pain would be worse than the gain. Thus, we must look elsewhere and Kleiman canvasses the entire array of strategies, big and small, that might help cut crime at reasonable cost.

The death penalty is actually a perfect illustration of Kleiman's two broad themes that severity is the enemy of swift and certain punishment and that our policies should focus on crime reduction, rather than the pointless infliction of suffering. The best empirical work reveals not a hint of deterrence from capital punishment, yet death penalty regimes prolong the agony of the victim's family

(not to mention the convict's family) while fostering contempt for the criminal justice system, cost many times what life imprisonment would cost, deepen apprehensions of racial discrimination, and inflict gratuitous pain on too many wrongfully convicted inmates who are ultimately exonerated while (worse still) occasionally executing the innocent.² Talk about lose-lose. But somewhat oddly, Kleiman never mentions capital punishment anywhere in his otherwise comprehensive assessment of all aspects of the criminal justice system.

The world is too complicated to have best policies line up uniformly with the views of left or right, and Kleiman picks and chooses judiciously among the best from each in a way that is rare. Ordinarily, if you hear how a person feels about, say, gun control, you know how they will come out on almost everything, but Kleiman is too truth-oriented and knowledgeable to be cabined in this way. Kleiman is unusual in his call for extending right-to-carry laws to the entire nation while expanding the Brady Bill to require background checks on all gun transfers. No wonder the Brady Bill had little impact on crime given the gaping loophole that private transfers are exempted. This enables criminals to simply show up at gun shows and other venues to buy all the guns they need, beyond what they steal in the roughly four million burglaries that occur each year in a country in which 34.5 percent of American households have guns.³

Before endorsing the Kleiman's right to carry (RTC) suggestion, policymakers should consider many additional issues beyond those raised in his book. Would a national RTC law deter crime as some have argued (indeed, Becarria himself felt this way, although that was at a time when there were no police forces or even prisons) or trigger more gun toting by criminals in the type of arms race that government intervention should try to stop? Would more lawful gun carrying

mean more lost and stolen guns that end up in the hands of criminals, or are we already so gun-saturated in this country that increased criminal access to guns will not be noticeably enhanced by a policy that leads to more guns left in automobiles in a world with over one million annual auto thefts?⁴ (See Donohue 2003).

Moreover, if the best research suggests that RTC laws have no net effect on crime, as Kleiman asserts, does this mean that the benefits and harms are sizeable but largely offsetting, or that there is just no effect? If the former, does it matter if the gun owners or the non-gun owners fare worse? Ordinarily, economists don't like expenditures that shift burdens to others, but perhaps the gun owners are paying out of their own pockets only to shift the burdens to themselves. In fact, some of the most recent, admittedly "obscure," evidence may be a bit worse than Kleiman believes: aggravated assaults do seem to rise when RTC laws are passed (Ian Ayres and Donohue 2009; Donohue, Abhay Aneja, and Alexandria Zhang 2010). If we are all in fact a bit less safe, while some gun owners incorrectly feel *much safer* and some gun opponents erroneously feel *much less safe*, are RTC laws a good thing?

Perhaps being a bit less safe on the RTC margin is worth the gain of the political trade-off that Kleiman advocates—the left gives up on RTC laws but gains on the Brady Bill extension to private transfers. But this involves speculation on two fronts. From a political standpoint, one must conjecture that, by conceding on the RTC issue, the left could get a gun control measure through over NRA opposition. But the NRA has mastered the art of gutting gun control measures, as the Brady Bill itself and the assault weapons ban have shown. One must also speculate empirically that the Brady extension would help more than a national RTC law would hurt. The most spectacular crime drop in the country has come in New York City at a time of relentless opposition to gun carrying. While Kleiman would be willing to allow Plaxico Burress to carry his initially Florida-registered gun in a New York City

² See the recent symposia in *Criminology and Public Policy*, "The Impact of the Death Penalty on Murder," and in the *American Law and Economics Review*, "Introduction to the Death Penalty Symposium."

³ According to the FBI's Uniform Crime Reports, there were 2.2 million burglaries in 2008 that were known to the police, but roughly half of the burglaries went unreported. The General Social Survey found that 34.5 percent of households owned at least one gun in 2006.

⁴ According to the FBI's Uniform Crime Reports, there were 950,000 motor vehicle thefts in 2008. The true number is over one million when one accounts for nonreporting.

nightclub, city prosecutors put the former New York Giants' receiver in prison for two years.

Should New York City be allowed to experiment with a tough anti-gun approach or is a national rule on guns appropriate despite the widely different conditions throughout the country? These are indeed complex questions and, while Kleiman is very thoughtful in analyzing these issues, the reader must keep in mind that he is frequently divining best estimates from complicated and frequently contradictory empirical literatures. Still, Kleiman has his eye on the ball and knows and cares about the research, which almost certainly suggests his views should be more accurate and well-grounded than those of virtually any politician, who (with the exception of New York City Mayor Michael Bloomberg) often seem to sacrifice crime reduction either for political gain or because of ignorance.

Kleiman stresses the important lesson that the goal of the criminal justice system is to reduce crime, rather than to inflict pain, and while the latter can be a means to achieve the former, at times they are in conflict. Recognizing these conflicts and understanding that reducing crime should triumph over any atavistic desire for punishment would be an important step to more sensible and effective crime-reduction policies. The budgetary crises that states such as California are now confronting will make many policymakers interested in finding ways to restrain crime at lower cost. Hopefully, Kleiman's excellent book will help them achieve this goal.

REFERENCES

- Ayres, Ian, and John J. Donohue. 2009a. "More Guns, Less Crime Fails Again: The Latest Evidence from 1977–2006." *Econ Journal Watch*, 6(2): 218–38.
- Ayres, Ian, and John J. Donohue. 2009b. "Yet Another Refutation of the More Guns, Less Crime Hypothesis—With Some Help from Moody and Marvell." *Econ Journal Watch*, 6(1): 35–59.
- Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy*, 76(2): 169–217.
- DiIulio, John. 1996. "Prisons Are a Bargain, by Any Measure." *New York Times*, Jan. 16, A19.
- Donohue, John J. 2003. "The Final Bullet in the Body of the More Guns, Less Crime Hypothesis." *Criminology and Public Policy*, 2(3): 397–410.
- Donohue, John J. 2005. "Fighting Crime: An Economist's View." *Milken Institute Review*, 7(46): 47–58.
- Donohue, John J. 2009a. "Assessing the Relative Benefits of Incarceration: Overall Changes and the Benefits on the Margin." In *Do Prisons Make Us Safer? The Benefits and Costs of the Prison Boom*, ed. Steven Raphael and Michael A. Stoll, 269–341. New York: Russell Sage Foundation.
- Donohue, John J. 2009b. "The Impact of the Death Penalty on Murder." *Criminology and Public Policy*, 8(4): 795–801.
- Donohue, John J., Abhay Aneja, and Alexandria Zhang. 2010. "The Impact of Right to Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy." Unpublished.
- Donohue, John J. Forthcoming. "Introduction to the Death Penalty Symposium." *American Law and Economics Review*.
- Donohue, John J., Ben Ewing, and David Peloquin. Forthcoming. "Rethinking America's Illegal Drug Policy." NBER.
- Donohue, John J., and Peter Siegelman. 1998. "Allocating Resources among Prisons and Social Programs in the Battle against Crime." *Journal of Legal Studies*, 27(1): 1–43.
- Justice Policy Institute. 2000. "The Punishing Decade: Prison and Jail Estimates at the Millennium." Washington, D.C.
- Stigler, George J. 1970. "The Optimum Enforcement of Laws." *Journal of Political Economy*, 78(3): 526–36.
- JOHN J. DONOHUE III
Yale University
- Regulating Vice: Misguided Prohibitions and Realistic Controls.* By Jim Leitzel. Cambridge and New York: Cambridge University Press, 2008. Pp. xv, 301. \$85.00, cloth; \$29.99, paper. ISBN 978-0-521-88046-6, cloth; 978-0-521-70660-5, pbk. JEL 2008-1367

There is a long history of societies grappling with whether and how to regulate individual behavior. Particularly when it comes to behavior regarded as "vice," this philosophical and practical question is at once interesting, difficult and important. *Regulating Vice: Misguided Prohibitions and Realistic Controls*, by Jim Leitzel, presents both an historic context for government regulation of vice, and a proposed set of guiding principles for how societies should handle the problem.

In the book, Leitzel outlines a set of principles on which he argues vice regulation should be based. Leitzel uses as a jumping off point John Stuart Mill's "harm principle," which Mill describes in *On Liberty* (1859). In Mill's own words, ". . . the only purpose for which power can be rightfully exercised over any member of a civilized community, against his will, is to prevent

harm to others. His own good, either physical or moral, is not a sufficient warrant.” (in Mill, *On Liberty* (1859), as quoted from Leitzel, p. 19). Put differently, regulation should focus essentially on the reduction of negative externalities.

Leitzel first makes the case that an absolute adherence to Mill’s harm principle, a focus primarily on externalities caused by vicious behavior, leads to unwise public policy in some cases. “If we accept the harm principle,” he says, “then Mill’s reasoning suggests that we cannot adopt policies with the primary aim of reducing adult vice—even though many adult vice decisions may well be less than rational and involve serious negative consequences.” (Leitzel 2008, p. 73). In the other extreme, full prohibition of all vices is bad public policy, he argues, because of the restrictions it places on informed clear-thinking adults who choose to engage in these activities and who do not hurt others in the process. Public policy should, therefore, place value on these rational decisions by individuals, while recognizing that some choices to engage in vice are not fully rational ones. The case for prohibiting children from consuming some drugs, for example, is based on this idea. Consider a child who consumes a self-harming drug without imposing harm on others. A literal interpretation of the harm principle would preclude the prohibition of this behavior. Consideration of the child’s inability to make decisions that take full account of future implications leads to a different prescription.

Leitzel argues that the same logic that supports the prohibition of purely self-destructive behaviors among children might apply to policies regarding less-than-rational decisions by adults. Essentially, Leitzel recommends that Mill’s harm principle be updated to account for the cognitive errors that behavioral economists and psychologists underline. Whereas Mills says regulation should focus on the externalities generated by vicious behavior, behavioral economics adds that government may have a role in helping people to avoid making self-destructive choices.

Specifically, Leitzel argues that vice regulation should consider the following: “(1) children, (2) addicts, (3) external harms, (3 1/3) endangered health and other negative impacts on nonaddicted adult consumers . . .” Children have not yet developed the ability to fully weigh the

present and future costs and benefits of their actions. Addicts similarly make consumption decisions whose rationality should be viewed with strict scrutiny (Leitzel makes this argument after devoting all of chapter 2 to the idea of rational addiction). External harm encompasses Mill’s suggested focus on externalities. The latter consideration is argued with care (as evidence, consider that it is labeled #3 1/3 rather than #4), but it is the most interesting departure from Mills, and most of the thought-provoking policy prescriptions in the second half of the book follow from this modest proposal. It is for #3 1/3 that much of the behavioral economics and psychology research is relevant.

In taking on this role of helping individuals to make decisions, he goes on to argue, government should be careful to impose restrictions that are only minimally costly to those who are able to make clear rational decisions (something Leitzel terms “Robustness”). In this, Leitzel’s philosophy is similar to the guiding idea behind Richard H. Thaler and Cass R. Sunstein’s *Nudge* (2008), “libertarian paternalism” more generally (Sunstein and Thaler 2003), and Colin Camerer’s “asymmetric paternalism” (2003). Public policy should, Leitzel proposes, be appropriate regardless of whether adults make rational or irrational decisions. He explains, “we require this robustness precisely because we cannot ascertain how much vice is rational, nor distinguish the rational component from that which flows from a degradation of the selecting faculties” (Leitzel 2008, p. 74).

One example of a “robust” policy is a requirement that purchases of heroin be made with at least three days notice. This preorder could be revoked at any time prior to purchase. Such a policy is closely tailored to a form of irrationality associated with heroin use, the loss of self-control while under the drug’s influence. If one conceptualizes two selves, a sober and a high self, such a policy respects the rational decisions and desires of the sober self, while protecting the sober self from the actions of the high self. The policy assumes, of course, that three days is enough time for the self-control problems to be resolved. But, it is robust in the sense that it treats rational and irrational decisionmakers differently; it restricts specific types of less-than-rational decisions

while respecting the choices of adults who have the chance to thoughtfully weigh the benefits and costs of using the drug.

The second half of the book is filled with examples like this one, set alongside entertaining and informative stories from the history of vice and its regulation. One chapter examines drug and alcohol regulation, deftly jumping from stories about the experience of alcohol prohibition in the United States from 1920 to 1933 to an even-handed debate over the merits of drug legalization versus prohibition. The chapter includes an interesting discussion of the costs of the recent efforts to prohibit drug trade in the United States, the “war on drugs.” Anyone who has watched “The Wire,” a television show on HBO that chronicles the drug trade in Baltimore, will not help but think of the “Hamsterdam” experiment. Other chapters explore commercial sex, gambling, and vice on the Internet.

Leitzel struggles throughout the book to balance the protection of individual rights with a belief that the state has an obligation not only to protect individuals from others, but also sometimes from themselves. It is not easy to balance these two concerns, and Leitzel does not oversimplify things. As a result *Regulating Vice* is a fun and informative read. Anyone interested in how economic thinking can inform public policy, and anyone with an interest in the past, present or future of vice regulation is likely to enjoy the book.

REFERENCES

- Camerer, Colin, Samuel Issacharoff, George Loewenstein, Ted O'Donoghue, and Matthew Rabin. 2003. “Regulation for Conservatives: Behavioral Economics and the Case for ‘Asymmetric Paternalism.’” *University of Pennsylvania Law Review*, 151(3): 1211–54.
- Leitzel, Jim. 2008. *Regulating Vice: Misguided Prohibitions and Realistic Controls*. Cambridge and New York: Cambridge University Press.
- Mill, John Stuart. 1978. *On Liberty*. Indianapolis and Cambridge: Hackett Publishing, 1859.
- Sunstein, Cass R., and Richard H. Thaler. 2003. “Libertarian Paternalism Is Not an Oxymoron.” *University of Chicago Law Review*, 70(4): 1159–1202.
- Thaler, Richard H., and Cass R. Sunstein. 2008. *Nudge: Improving Decisions about Health, Wealth, and Happiness*. New Haven and London: Yale University Press.

JONATHAN GURYAN
University of Chicago

L Industrial Organization

Playbooks and Checkbooks: An Introduction to the Economics of Modern Sports. By Stefan Szymanski. Princeton and Oxford: Princeton University Press, 2009. Pp. xx, 225. \$29.95. ISBN 978–0–691–12750–7.

JEL 2009–0963

With the market for books using the economist's thought process expanding past academia and into the mainstream, it is becoming increasingly difficult for authors to find the right level of rigor that doesn't isolate too many potential readers. A second complication is breadth. Should the author choose a broad list of topics that scratches the surface of each, or a small list of topics with a great amount of detail?

Stefan Szymanski's *Playbooks and Checkbooks* skillfully manages both of these issues and is also an entertaining read. This book is accessible to the noneconomist or those with limited training. Similar to *Freakonomics*, several nontechnical definitions of terms common to the trained economist are given. There is even a section titled “A Short Note About Economists” in the introduction that provides a brief synopsis of five contributions from economists that are vital to the sports literature. Fortunately these discussions are brief and are not likely to interrupt a reader who is already familiar with, say, Nash equilibria or the Coase Theorem.

Although Szymanski writes for a broad audience, this book is probably best suited for a college course on economics and sport as some of the explanations probably go beyond what a casual reader would prefer. To this end, Szymanski offers “A Beginner's Guide to the Sports Economics Literature” at the end of the book, which points readers to the academic literature discussed in the book. It is easy to imagine an introductory economics and sports course that uses this book to navigate this increasingly diverse field.

This book also gives more than economic explanations for the state of modern sport. Szymanski carefully includes many historical perspectives that enhance his economic arguments. In fact, some parts of the book read more like history than economics. One recurring theme that will interest readers from either continent is why professional sports leagues in America

and Europe are so different. For example, in the first chapter, Szymanski ties the growth of sport in Europe during the eighteenth century to the freedom of assembly, a right that was not previously given outside of the noble class because rulers feared rebellion. This also explains why sport in France was slower to develop, since their citizens were not given the freedom of assembly until much later. As sports clubs began to form, so did demand to watch the contests, which almost immediately created a conflict between those that wished to keep their games at the amateur level and entrepreneurs who saw the potential for economic rents. Szymanski illustrates this amateur–professional conflict by discussing two organizations formed in the late nineteenth century—baseball’s National League in the United States and soccer’s Football Association (the precursor to modern day FIFA) in England—and the vastly different outcomes that resulted.

Another positive aspect of this book is its treatment of sports and antitrust. Sport franchises need some degree of cooperation since any contest requires at least two teams or competitors. Cooperation can manifest itself in a variety of ways, including profit sharing, local monopolies, and suppressing players’ wages. Nearly all acts of cooperation between sports franchises can be interpreted as an antitrust violation, and nearly any business outside of a sports franchise would be guilty under American or European antitrust law. But this reveals the unique nature of the sports industry—a monopoly in sports is a bad outcome for the firm since there is no one to play!

This aspect of the sports industry has resulted in an incredibly complex history of judgments. Szymanski capably summarizes this history in both the United States and Europe, and also discusses some of the less obvious consequences of these rulings.

For example, most sports economists know that Major League Baseball enjoys an antitrust exemption from an unusual 1922 Supreme Court ruling that determined baseball games were not interstate commerce and therefore exempt from federal antitrust law. This was despite the fact that visiting teams regularly cross state lines in order to play a game both then and now. Szymanski discusses how this ruling, which even

later Supreme Courts found confusing, impacted congressional hearings in 1951 and more recently in 2005 for steroid use. Szymanski also discusses the famous Curt Flood case. Most are aware that Flood’s appeal was the beginning of free agency in Major League Baseball. However, free agency was not the result of a court judgment but rather collective bargaining between owners and the newly formed players’ union. Szymanski carefully explains the formation of the union, why owners would agree to something that would clearly raise player salaries, and the impending cycle of collusion and cheating between owners. Other chapters include a discussion of sports broadcasting and public funding for sporting events again using an impressive amount of historical background.

Economics students and casual readers will benefit from Szymanski’s use of basic economic analysis to explain the sports industry. Economists will see many of these explanations coming, but are almost certain to learn something from Szymanski’s impressive amount of historical background. The cumulative result is an interesting read for someone with any amount of economics training.

ROBERT BAUMANN
College of the Holy Cross

N Economic History

The Economy of Renaissance Florence. By Richard A. Goldthwaite. Baltimore: Johns Hopkins University Press, 2009. Pp. xviii, 649. \$55.00. ISBN 978–0–8018–8982–0.

JEL 2009–0996

Reading a book like this one in 2010 would surely give economic historians or economists a strange feeling. In the opening pages, Richard Goldthwaite, the doyen of historians of the Florentine economy, argues that Florence has been and should be studied more than other major Italian cities of that age not because it is more important but because its archives are richer. And throughout the whole book he sticks to this principle. He deals with issues that are well documented, either by his own research or by the work of economic historians of the (vanishing) traditional breed. Goldthwaite does not use explicit economic modeling and uses very

little data, at least by the standards of economic history nowadays. Throughout the book, there is not a single statistical test, only twelve tables and two graphs. What Goldthwaite looks for are facts and stories—what people did, how they interacted with each other, and how their behavior was affected by institutions and policies, both in Florence and abroad. Therefore, the average reader of the *Journal* may be strongly tempted to close the book and zip to the latest issue of a learned journal. Readers who decide to resist to their instinct are going to be rewarded as they will learn a lot about how an advanced economy worked in the past.

The book covers the three centuries from around 1300 to around 1600, the economic heyday of the city. At the beginning of that period, the city was already huge and prosperous, one of the biggest industrial cities in Europe and a major financial and banking center. Part 1 of the book deals with international banking and trade, which were closely intertwined. The Florentine merchant houses were not specialized at all. Rather they pursued any business opportunity, from trade to international finance. They organized and funded the supply of raw materials for Florentine industry and the sale of its silken and woolen cloths, but they also lent to very rich borrowers, including, most famously, the English crown, which in exchange pledged their tax proceeds. The merchant houses used their own capital but also deposits, usually from the great and the good of all countries where they operated. Goldthwaite outlines the trade patterns and their changes in time; highlights the key role of the florin, the reference gold coin in Western Europe for three centuries; and describes the organization of these merchants-cum-banking companies. For nowadays standards, they were small and extremely flexible, and they became smaller and more flexible in the fifteenth and sixteenth century. Their real strength was the extensive network of agents and correspondents all over Europe: the Bardi, one of the three largest, from 1315 to 1345, had twenty-five branches but it employed 346 people, plus fifteen partners (ten of whom from the family). But each merchant could be partner or make business with many such companies. The author describes in detail the education and career of merchants and, to the

extent that sources are available, also their business practices.

The second part of the book deals with the rest of urban economy. Chapters 4 and 5 deal with manufacturing, by far the main source of employment in the city. The Florentine textile industry had developed thanks to the Arno River, which provided water and power, and had become a market leader in Europe for high-quality products. Production was based, as everywhere in Europe, on a putting-out system—but strictly confined to the city. The author describes the organization and its changes over time, stressing, as for international banking, the flexibility of firms and their high turnover. Workers were organized in guilds, but the author stresses their nature as political associations rather than their economic role. Florentine guilds did not restrict the access to profession nor stifle innovation. Chapter 6 describes the banks catering for urban market—including local branches of international banks as well as smaller local firms, plus pawnbrokers, both Catholic and Jews. Local banks appeared thoroughly modern in their business and the resort to banking services was quite widespread. Artisans and workers were routinely paid with checks and had bank accounts. And the whole system worked well with almost no state intervention, at least until the late sixteenth century.

The last chapter marshals the macroeconomic evidence that the author deems acceptable for his standards of scholarship. It outlines the government policies for promoting economic activities (implemented mostly under the Medici in the late sixteenth century) and its fiscal policy (a rat race to fund the growing military commitments) and the political role of merchant elites. Then the author sketches out the situation of Tuscany outside Florence—its political situation, its natural resources, its agriculture, and so on. He also deals with the integration of the territories that Florence acquired since the thirteenth century into a regional economy. Last but not least, the author argues that Florentine society was very upwardly mobile, at least for the standard of the time and that the distribution of wealth by household according to the 1427 Catasto was fairly equal (although inequality increased in the next century).

As a whole, at the end of the book one has the impression of a really vibrant, flexible, and free-market city. The standard of living was undoubtedly high and not only for the wealthy, as witnessed by the art treasures of the city, but also for the working class. Literacy and numeracy was very common, and the majority of children attended a primary school.

After 1600, Florence did decay, surely in comparison with the cities of Northern Europe, but also probably in absolute terms. By 1800, Florence was a small provincial city, the capital of a tiny agricultural state. The author does not explain this decline, nor the economic rise of the city before 1300. His micro-perspective makes it difficult to understand the causes of macro-trends. But it offers a wealth of fascinating stories and facts—a real joy to read for anyone who likes to relive the past.

GIOVANNI FEDERICO
European University Institute

The Invisible Hook: The Hidden Economics of Pirates. By Peter T. Leeson. Princeton and Oxford: Princeton University Press, 2009. Pp. xvi, 271. \$24.95. ISBN 978-0-691-13747-6.

JEL 2009-1003

In this accessible and easily read book, Peter Leeson reinterprets the history of piracy, overwhelmingly that of the Caribbean pirates 1700–1726, in the light of economic theory. Pirates, he argues, were not the grog-crazed fiends portrayed in *Treasure Island*. Rather they rationally pursued opportunity in a particular institutional setting. Faced with the lack of an overarching legal authority, they created a unique set of voluntary institutions that allowed them to maximize the benefits of their craft. They were true “captains of industry,” who plied their unusual trade with diligence, discipline, and élan.

Pirates of this period adopted constitutions, “articles of agreement” that all subscribed to voluntarily. These constitutions were surprisingly egalitarian. All the crew had a vote in counsel, mostly with one man one vote. Further, the articles prescribed a surprisingly egalitarian division of the loot. The captain got no better food and bedding than the men. Any man could enter the captain’s cabin at any time to converse with him. When the spoils were divided, the captain got

only two shares compared to the standard one—a much smaller pay differential than observed on merchant ships at that time, where the differential would be 4–5 to one.

The pirates’ development of a reputation for picturesque and diabolical cruelty—torturing and killing their victims, and wantonly burning captured ships—was similarly rational business strategy. To ply their trade effectively, pirates needed to establish a reputation for crazed blood thirst, yet at the same time assure those who surrendered that they would get quarter. They thus established the pirate flag, the Jolly Roger, which promised death to all who resisted, but humane treatment to those surrendering without a fight. To prevent victims from concealing valuables, they had to torture those suspected of concealment. But to encourage compliance and surrender they abstained from inflicting gratuitous suffering on captives.

Leeson’s explanations of the structure of the pirate enterprise in the golden age of Caribbean piracy is of the species of “armchair economics” (not used here in a derogatory sense) found in a variety of recent popular economics books. As in the conventions of this literature, whatever Pirates did, including various forms of torture, is interpreted as an optimal response to incentives. The fun is to solve the puzzle—to come up with the explanation that makes odd behaviors somehow rational.

Unfortunately, many of the interpretations offered here are unconvincing. For example, why were pirate captains typically paid so little compared to merchant captains? Leeson explains this as coming from the need to avoid jealousies and conflicts in their dangerous line of work. “Suspicious of unfairness, favoritism, and simple envy created unhappy specters for pirate ships . . . pirates eliminated the greatest potential source of these emotions—large material inequalities” (p. 69). But he has no evidence, other than this supposition, to support this resolution of the conundrum. Presumably a good pirate captain, like any business leader, would have an enormous value to the enterprise through his sailing skills, ability to plan and prepare, and ability to inspire and lead. Why would the crew resent rewarding an obviously competent leader with four shares, or even ten or twenty shares? As long as it was specified in advance how many shares the captain

was entitled to, there would be no conflict in the division of the spoils. And rational pirates will not envy the leader who fattens their own reward through his skills. “Envy” is not a move in the particular board game that Leeson is playing here.

Similarly why did flying the “Jolly Roger” prove an effective tactic? Leeson argues that only serious pirate ships would use this signal, so that it would always constitute an effective threat of annihilation unless a ship surrendered. But why didn’t the signal get degraded by its use by lightly armed and not really dangerous proto-pirates? Anyone could make a Jolly Roger. And if flying it would induce fat merchant prizes to surrender to even lightly armed vessels, why didn’t the fighting quality of pirate crews degrade so that the Jolly Roger was no longer effective. In practice, the reason may be that the Jolly Roger only appeared around 1700, and the Caribbean pirates were mostly extirpated by 1726, not enough time for the signal to lose its power. But there is no reason to think that we were here observing some kind of long-run signaling equilibrium.

The book has a general tone that celebrates the pirates as an overall success of industry and enterprise. Leeson, indeed, goes beyond describing them in neutral scientific terms. Instead, he writes with respect and admiration, proving that the difference between monstrous cruelty and endearing roguery seems to be the elapse of about two-hundred years. However, the pirates created such a nuisance by their actions that by 1726 they were largely eliminated, with at least four hundred hanged by the authorities, and unspecified others dying in combat or interne-cine strife. Really successful pirates would have found a way to limit their predations so that it would be less costly to tolerate the losses than to invest resources in suppressing them. A business model that ended thus would not be called a success if it were a modern firm or industry. GM may have failed, but it lasted a lot longer, paid a lot of dividends, and its former employees still receive generous retirement benefits.

The Invisible Hook is, however, an entertaining read that illuminates well a curious historical episode. Its readability is enhanced by the design, which is a tribute to the publisher’s art.

GREGORY CLARK

University of California, Davis

Q Agricultural and Natural Resource Economics · Environmental and Ecological Economics

Economics of Forest Resources. By Gregory S. Amacher, Markku Ollikainen, and Erkki Koskela. Cambridge and London: MIT Press, 2009. Pp. xxiv, 397. \$60.00. ISBN 978-0-262-01248-5. *JEL* 2009-1085

Forests provide a vast array of important services, only one of which is their market production of timber and related goods. The fact that most of the other services such as carbon storage and support of biodiversity have not been so far appropriately valued by markets does not of course mean that they are any less important. Unlike the values associated with the extraction of timber and other forest products, the carbon storage and biodiversity values of forests are mainly existence values; that is, these services are obtained by preventing most commercial exploitation of the forest resources. It appears that the non-market existence values of a significant part of the remaining forests are greater than any values arising from either converting the forests into other uses or from the extraction of timber from the forests even if “optimal forest rotation” rules were applied (Randall A. Kramer, Thomas P. Holmes, and Michelle Haefele 2003). Textbook-derived “optimal” forest rotation periods may be optimal only because the existence value of the forests are not considered as a consequence of the lack of adequate markets and institutions that may internalize such existence values.

The currently advanced countries (the “North”) followed a pattern of economic growth that especially in the early stages of development required the conversion of most of their natural forests into other uses.¹ In general this pattern of growth was extremely demanding on what at the time was considered a “noneconomic” good, the natural environment, including natural forests

¹ More than 95 percent of the original forests in Western Europe have disappeared over the last two centuries (Dirk Bryant, Daniel Nielsen, and Laura Tangley 1997). In North America, forests still cover about 50 percent of the original forest areas but most of the existing forests have been heavily intervened and have lost a significant part of their biodiversity and carbon retention capacities (Peter Potapov et al. 2008).

which were extremely abundant. In fact, most analysts concur that the exploitation and subsequent conversion of the natural forests and other natural resources was a key factor that permitted the rapid economic growth of the North in its early stages of development (Ronald Findlay 1992; Edward B. Barbier 2005). Whether or not the rapid and persistent rates of economic growth and the current level of affluence achieved by the North would have been possible without the “free” environmental good in its crucial early stages of development is nonetheless an open question.

Abundance of forests in the early stages of development in the North meant that property rights on the forest resources were weak or non-existent. Forest resources and, more generally, the world environment were indeed so abundant that its marginal value was nearly zero so that no market for their services was in fact needed. This facilitated both the unhindered exploitation of forests for timber as well as the mostly irreversible transformation of forest lands into other uses which supported the early stages of economic growth. Thus, in the early stages of Northern development, the issue of determining optimal forest rotation periods was mostly of secondary importance.²

The North has dramatically transformed its economy into a service and high tech oriented one making its production sector less dependent on the environment than it was in the past; that is, production in the North has become somewhat dematerialized. This has implied that pressures for forest conversion into other uses have diminished and, hence, the issue of optimal management of the remaining forests as sources of timber has become relevant. While the structure

² It is then somehow surprising that most principles of optimal forest rotation management were developed very early in the development process of the North when such issue was important mainly for few forest resources. Faustmann produced his seminal contribution to the economics of the optimal forest rotation period by the middle of the Nineteenth Century. However, their studies remain dormant for the next hundred years or so. It was not until Paul A. Samuelson’s famous 1976 contribution, at a time when world forests begun to be perceived as scarce and when property rights on at least some of the remaining forests of the North have been developed, that the issue was revived.

of consumption in the North has also changed, it has not dematerialized nearly as much as production (Ramón López 2008). Consumption based byproducts, and emissions and consumer demands for energy and durable goods that are highly environmentally demanding have continued rising rapidly. This pronounced difference between the rates of dematerialization of production and consumption has been reconciled by international trade. The increasing consumer demands for dirty or resource-based goods in the North is mostly satisfied not by its own domestic production as it was in the early stages of development but by dirty and resource demanding production originated in the “rest of the world.”³

The currently developing countries, the rest of the world or the “South,” have come late to the party; unlike the North in its early stages of development, the South faces the challenge of economic growth in a context of increasing worldwide environmental scarcity. However, the South has nonetheless imitated the North’s early patterns of environment-demanding economic growth that has further exacerbated such scarcity. Also, the very existence of a developed North increasingly demanding dirty goods from the South has made the development process in the South much more environment-demanding and frontier-expansive than in the early stages of growth in the North. After a few decades in which for the first time in history both the North and the South have experienced significant economic growth, the world environmental resources—most notably the capacity of the world climate to resist the massive gas emissions

³ For example, between 1990 and the early 2000s the percentage of Europe’s processed meat consumption originated in Brazil rose from 40 percent to 74 percent of all imports. In 2003, Brazil’s cattle production—80 percent of which based in the Amazon—was mainly export driven (David Kaimowitz et al. 2004). The same is true for the extraordinary expansion of soybean production in the Amazon that was established mainly to satisfy demands from the North. Cattle and soybeans directly compete with forests and these industries are estimated to be responsible of more than 80 percent of the deforestation in the Amazon over the 1990s (Rhett A. Butler 2009). With respect to mining products, it is estimated that nearly one third of all active mines and exploration sites are located within areas of intact ecosystems of high conservation value, mostly located in the South (Marta Miranda et al. 2003).

originated by world economic growth without major disruptions and the capacity of forest ecosystems to continue sustaining life—have become prominently scarce (Bryant, Nielsen, and Tangle 1997; Potapov et al. 2008).

Scarcity of course brings economic value to the environment but this value needs effective markets or effective government regulation to force the economy to internalize them. However, development of these markets is an extraordinarily complex process that requires the emergence of important new institutions both at the national and world levels. The process of formation of these new institutions is full of obstacles, which, if at all successful, is likely to require a long period of time to fructify.⁴ In the meantime and for the likely prolonged period of time during which such comprehensive institutional framework is not available, a major task is how to at least in part mitigate the risks of catastrophic and irreversible environmental changes via piecemeal policies. But this requires an understanding of the factors that make economic growth more environmentally demanding than necessary.

Even as forests can still be today considered abundant in the South, the fact that the North has wiped out most of its own forests and that other environmental demands (especially related to climate conditions) are so large means that the remaining standing forests in the South have acquired significant economic value.

A corollary from the previous discussion is that the economics of forest resources should go well beyond the standard problem of elucidating the optimal time to harvest forest resources. It also needs to consider the preservation of a natural asset—the remaining forest ecosystems—that appears to be increasingly more scarce and socially valuable, a value that is generally not recognized by the existing institutional and market conditions. This, in turn, requires a good understanding of the economics of deforestation. The literature suggests that economywide forces, including exchange rates, international trade, income inequality, social policies, and public

spending policies, are more important than forest policies themselves to understand forest economics (Sven Wunder 2003; Barbier 2005). The implication of all this is that forest resources economics requires a general equilibrium framework rather than merely the conventional partial equilibrium one to shed light on the most important modern issues concerning forest resources.

The partial equilibrium framework is insufficient to understand forestry economics. The lack of certain markets and institutions affecting forest activities mean that the management of forests does not depend merely on forest product prices, input prices, and interest rates. There are effects originated in the general economy that are transmitted directly to the forest sector rather than through the prices on which the conventional forest economics has relied, effects that could easily dominate the price effects. Moreover, most remaining forests are located in the South where they face increasing competition from other sectors, particularly agriculture, cattle ranching, mineral, and oil and gas extraction. There is indeed a vast literature that has shed some light into the effects of economywide factors on forest resources emphasizing the impact of trade openness, real exchange rates, and public expenditure patterns on deforestation.⁵

The *Economics of Forest Resources* book authored by Amacher, Ollikainen and Koskela (MIT Press, 2009) provides a rigorous treatment to the problem of optimal forest rotation period presenting a lucid summary of the main generalizations of the famous Faustmann rotation model. After introducing the Faustmann model, the book presents a number of generalizations of this model by allowing for the existence of amenity services of the standing forest, by considering a dynamic extension of the Faustmann's model using a two-period model and an overlapping generation model, as well as by allowing for age class models and uncertainty. In addition, the book presents a clear and rigorous discussion of the forest tax implications of the various models with the aim of determining the optimal tax policies under each set of assumptions. Despite the

⁴ The mostly failed Copenhagen climate change meetings in December 2009 is an example of the difficulties that the development of new international institutions encounter.

⁵ See, for example, the comprehensive surveys by Barbier and Joanne C. Burgess (2001) and Brian R. Copeland and M. Scott Taylor (2004).

rigorous mathematical approach, readers of the book, who will be graduate students and graduate instructors, will nonetheless find the analysis quite accessible.

The book's main merit is the clarity of the presentation while at the same time preserving a rigorous mathematical analysis. Moreover, within the confines of its optimal forest rotation and optimal forest taxation focus, the analysis in the book is quite comprehensive and up to date by taking stock of much recent literature covering these issues. Readers will find the results of a vast and fairly dispersed literature condensed in one source and presented with a high degree of clarity. However, intuitive discussions of the main results are few and the empirical motivation of the various topics is quite scant. Little space is devoted to important empirical studies that have tested some of the hypotheses arising from the various models considered. This insufficient attention to intuition, motivation, and empirical findings makes the book quite dry, which, in turn, may imply that the audience is likely to be circumscribed mainly to graduate students.

Now we examine the structure of the book in the context of our previous discussion on the importance of the various values of forest resources. The book devotes just one chapter (chapter 6) out of a total of twelve chapters to examine issues of deforestation and another chapter (chapter 7) to conservation of biodiversity in boreal forests. The analysis of these issues appears quite insufficient and superficial. Moreover, the small space allocated to deforestation and the existence value of the forest vis-à-vis the great emphasis on the determination of the optimal rotation periods for timber extraction lead us to conclude that the book is not well balanced.

This lack of balance is illustrated by the following estimates that compare conventional forest values with just their carbon retention values: the losses of tropical forests are estimated to cause between one-fifth and one-quarter of all anthropogenic annual carbon emissions into the atmosphere (Georg Kindermann et al. 2008; IPCC 2007). Tropical forests are estimated to retain between 100 and 250 metric tons of carbon dioxide per hectare (Paul J. Crutzen and Meinrat O. Andreae 1990; Lisa Naughton-Treves 2004). Using the latest European Climate Exchange

trading quotes, the per hectare existence value of the forest resource ranges therefore between \$1,800 and \$4,500. These values are at least as large as the timber present values per hectare, which are estimated to range between \$200 and \$4,400 (SCBD 2001). According to the SCBD study, except for exceptional cases, the present value of forests lands in alternative uses (including agriculture and others) is also below the carbon retention value.

While apart from chapters 6 and 7, the book does devote two other short sections to the determination of optimal rotation age and optimal taxation when forest "amenity" values are present, the overwhelming focus of the book is on the timber value of forests. Behind the unbalanced approach of the book is the fact that the theoretical framework used throughout it is not adequate to analyze deforestation, understood as an often irreversible conversion of forest lands into other activities. The various theoretical models used throughout the book rely on conventional convex marginal analysis that does not allow for irreversible conversion of the forest system, and additional assumptions often used even rule out meaningful corner solutions.

Moreover, the exclusive focus on partial equilibrium analysis prevents the authors from providing more useful insights on deforestation. As discussed earlier, the majority of the tropical forests as well as large segments of the boreal and temperate forests face increasing competition from other activities such as agriculture, mining, and oil and gas extraction that often entail the complete conversion of the forest resources into a nonforest ecosystem. This implies that factors that increase the economic incentives for these competing industries are likely to affect "forest rotation" in a dramatic way: they may reduce the supposedly "optimal rotation age" to zero often implying an irreversible conversion of the forest areas into other uses. While this corner solution may be privately optimal but not socially optimal, it is still important to analyze forest resources when the relevant markets and institutions to internalize the true value of the forests are not in place. But this arguably most important outcome falls largely outside the realm of most of the analysis of the book, which allows for neither corner solutions nor irreversible outcomes.

The most important determinants of “forest rotation periods” are thus not specific forest policies such as harvest and property taxes so much emphasized throughout the book, but economywide and macroeconomic policies that affect other sectors of the economy that compete with the forest sector for its land base. Weak property rights on forest lands greatly increase the sensitivity of the forest resources to competing activities, thus rendering even more important the consideration of the nonforest sectors and, hence, of the mostly economywide policies that affect the economic incentives to such sectors. When macroeconomic conditions are most favorable to cattle production, agriculture, mining, and oil and gas industries, forest areas tend to suffer a complete and permanent conversion into other uses, not merely shorter forest rotation periods. In Brazil, for example, the rate of forest loss is extraordinarily responsive to the economic incentives of agriculture and mining, which are highly tradable goods (Butler 2009; Kaimowitz et al. 2004). Periods of high real exchange rates (undervalued), high prices for certain agricultural commodities, and rapid economic expansion based on tradable industries (such as the year 1995) have implied rates of deforestation in the Amazon of up to four times bigger than periods of appreciating real exchange rate and growth based on nontradable sectors such as the 2007–09 period (Butler 2009; Kaimowitz et al. 2004; Erik Reed and Miranda 2007). This same pattern of high sensitivity of deforestation to exchange rates and general economic conditions has been shown to apply in other countries in Africa and Asia as well (Wunder 2003; Jean-Louis Arcand, Patrick Guillaumont, and Sylviane Guillaumont Jeanneney 2008). While the *Economics of Forest Resources* does look at insecure property rights in one brief section of chapter 6, it does not consider a key implication of weak property rights—the increased sensitivity of the forest resource sector to developments outside of the forest sector itself.

In summary, the book constitutes an important source for scholars interested mostly in the management issues concerning forest rotation periods for timber extraction. It is also relevant for evaluating the impact of various sector-specific government policies for timber extraction and timber supply. While the book does extend part

of the analysis to consider nontimber or “amenity” values of forests, it still assumes full internalization of such values by individual producers. This makes this part of the analysis somehow less appealing and relevant for policy analyses. People interested in understanding deforestation in the context of economic development and in the roles of the standing forests as providers of vital nontimber values in a context of highly imperfect institutions would find the book of more limited interest.

REFERENCES

- Arcand, Jean-Louis, Patrick Guillaumont, and Sylviane Guillaumont Jeanneney. 2008. “Deforestation and the Real Exchange Rate.” *Journal of Development Economics*, 86(2): 242–62.
- Barbier, Edward B. 2005. *Natural Resources and Economic Development*. Cambridge and New York: Cambridge University Press.
- Barbier, Edward B., and Joanne C. Burgess. 2001. “The Economics of Tropical Deforestation.” *Journal of Economic Surveys*, 15(3): 413–33.
- Bryant, Dirk, Daniel Nielsen, and Laura Tangley. 1997. “The Last Frontier Forests: Ecosystems and Economics on the Edge.” Washington, D.C.: World Resources Institute.
- Butler, Rhett A. 2009. “Deforestation in the Amazon.” <http://www.mongabay.com>.
- Copeland, Brian R., and M. Scott Taylor. 2004. “Trade, Growth, and the Environment.” *Journal of Economic Literature*, 42(1): 7–71.
- Crutzen, Paul J., and Meinrat O. Andreae. 1990. “Biomass Burning in the Tropics: Impact on Atmospheric Chemistry and Biogeochemical Cycles.” *Science*, 250(4988): 1669–78.
- Findlay, Ronald. 1992. “The Roots of Divergence: Western Economic History in Comparative Perspective.” *American Economic Review*, 82(2): 158–61.
- Intergovernmental Panel on Climate Change. 2007. “Climate Change 2007: The Physical Science Basis.” Contribution of Working Group I to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change. Solomon, Susan, Dahe Qin, Martin Manning, Zhenlin Chen, Melinda Marquis, Kristen Averyt, Melinda M. B. Tignor, and Henry LeRoy Miller, eds. Cambridge and New York: Cambridge University Press.
- Kaimowitz, David, Benoit Mertens, Sven Wunder, and Pablo Pacheco. 2004. “Hamburger Connection Fuels Amazon Destruction.” Center for International Forestry Research Technical Report, Bogor, Indonesia. http://www.cifor.cgiar.org/publications/pdf_files/media/Amazon.pdf.
- Kindermann, Georg, Michael Obersteiner, Brent Sohngen, Jayant Sathaye, Kenneth Andrasko, Ewald Rametsteiner, Bernhard Schlamadinger, Sven Wunder, and Robert Beach. 2008. “Global Cost

- Estimates of Reducing Carbon Emissions through Avoided Deforestation." *Proceedings of the National Academy of Sciences*, 105(30): 10302–07.
- Kramer, Randall A., Thomas P. Holmes, and Michelle Haefele. 2003. "Contingent Valuation of Forest Ecosystem Protection." In *Forests in a Market Economy*, ed. Erin O. Sills and Karen Lee Abt, 303–20. Dordrecht and Norwell, Mass.: Kluwer Academic. www.srs.fs.usda.gov/pubs/ja/uncaptured/ja_kramer003.pdf.
- López, Ramón. 2008. "'Sustainable' Economic Growth: The Ominous Potency of Structural Change." Paper presented at the Conference on Sustainable Resource Use and Dynamics, Ascona, Switzerland.
- Miranda, Marta, Philip Burris, Jessie Froy Bingcang, Phil Shearman, Jose Oliver Briones, Antonio La Viña, and Stephen Menard. 2003. "Mining and Critical Ecosystems: Mapping the Risks." Washington, D.C.: World Resources Institute.
- Naughton-Treves, Lisa. 2004. "Deforestation and Carbon Emissions at Tropical Frontiers: A Case Study from the Peruvian Amazon." *World Development*, 32(1): 173–90.
- Potapov, Peter, Aleksey Yaroshenko, Svetlana Turubanova, Maxim Dubinin, Lars Laestadius, Christoph Thies, Dmitry Aksenov, Aleksey Egorov, Yelena Yesipova, Igor Glushkov, Mikhail Karpachevskiy, Anna Kostikova, Alexander Manisha, Ekaterina Tsybikova, and Ilona Zhuravleva. 2008. "Mapping the World's Intact Forest Landscapes by Remote Sensing." *Ecology and Society*, 13(2): 51.
- Reed, Erik, and Marta Miranda. 2007. *Assessment of the Mining Sector and Infrastructure Development in the Congo Basin Region*. Washington, D.C.: World Wildlife Fund, Macroeconomics for Sustainable Development Program Office.
- Samuelson, Paul A. 1976. "Economics of Forestry in an Evolving Society." *Economic Inquiry*, 14(4): 466–92.
- Secretariat of the Convention on Biological Diversity. 2001. "The Value of Forest Ecosystems." Convention on Biological Diversity Technical Series 4.
- Wunder, Sven. 2003. *Oil Wealth and the Fate of the Forest: A Comparative Study of Eight Tropical Countries*. London and New York: Taylor and Francis, Routledge.

RAMÓN LÓPEZ
University of Maryland at College Park

R Urban, Rural, and Regional Economics

Making Cities Work: Prospects and Policies for Urban America. Edited by Robert P. Inman. Princeton and Oxford: Princeton University Press, 2009. Pp. xvii, 382. \$75.00, cloth; \$29.95, paper. ISBN 978–0–691–13104–7, cloth; 978–0–691–13105–4, pbk. JEL 2009–1120

The book's title is a good summary of it. "Making" and "work" signal optimism, a faith in the *potential* of American cities, based on theory and past success stories; the subtitle suggests the contributing authors describe policies they think would achieve the potential. They tackle an impressive variety of subjects. After Robert Inman's introduction, we have Edward L. Glaeser on growth, Kenneth Small on transportation, Witold Rybczynski (the only noneconomist) on urban design, Joseph Gyourko on housing, David Card on immigration, Jacob Vigdor on race, Janet Currie on poverty of inner-city children, Richard Murnane on education, Philip J. Cook on crime, and Inman on finances. Some of these report their own recent research, and all discuss large bodies of research by others, with copious references. There is little on non-American cities.

The book grew out of a conference held at the University of Pennsylvania in 2007 in honor of the late Kathryn Engebretson, an economist who worked in the private sector, the city of Philadelphia, and the William Penn Foundation—and it is a fine tribute to her. I recommend it highly, to many different readers. The authors succeed in writing for the policymaker, usually eschewing advanced economics and econometrics. Some mention a host of individual cities as examples (Glaeser, Small, and Rybczynski are especially notable). Inman's introduction is a stimulating integrated guide to the essays, far more helpful than the string of mechanical summaries I get from so many editors of conference volumes.

Urban economists will find good classroom readings, and other economists can learn a lot about modern urban economics as a field. Teachers and scholars in urban geography, history, planning, and politics also will benefit. Even when one disagrees with an author, which might be often, reading the essay is provocative, and can stimulate classroom discussion: "Is that variable really exogenous?" "What about spatial autocorrelation?" "I come from New Lancaster, and our schools (or police, or transit authority, or . . .) just can't do that!" "Is the Hudson really 'a deep river that runs 315 miles into upstate New York'?" (p. 33).

A major theme is that some cities have succeeded over the last three decades or so, others

not. Inman says “the new urban enigma, and the central agenda of this book” (p. 6) is to explain that diversity in success. Glaeser reminds us it’s not simply Sun Belt and Rust Belt: there’s as much variation within the group of older cities as across that divide (p. 40). Every contributor accordingly takes pains to connect growth and prosperity with specific policies. Inman notes that the proposed strategies are “*people-* not *place-based* ones” (p. 12, italics in original).

The authors generally are upbeat about cities, precisely because there *are* success stories. As Inman reminds us, in the new economy both efficient production and efficient consumption require the personal interaction that is favored by density. Cities are “the low-cost supplier of proximity” (pp. 1–2), and also “retain their historical role as centers for economic and cultural innovations . . . proximity is again the key” (p. 2). They potentially can provide external benefits, because city government is “the primary provider of education and child care services” and typically is “the most efficient provider of any supplemental services required for poverty or first-generation immigrant children” (p. 19).

Not absent, but less developed in most essays, is the political economy: Just how do a mayor and city council get the right policies in place? The American political system has multiple layers of government but not a clean Musgrave layer-cake model of financing, it has metropolitan fragmentation, and the basic political technology favors place policies. Inman says that for any policy the issue is “*Are all citizens—city, state, and national taxpayers included—better off with the policy than without?*” (p. 13, italics in original). “All”? I suspect “yes” answers are hard to find.

More than one contributor makes a point that you don’t need to read Tiebout to know cities shouldn’t be financing redistributive policies as much as they do now. In his chapter on finances, Inman says we need to relieve city governments from the responsibility for city poverty: a city’s region or state should finance services and income support for poor people above what the city’s own taxpayers prefer (p. 349). But we don’t learn about the practical politics of how to get there. In a footnote he admits there may be potential gains if a city cooperates with its suburbs, but also admits the gains might not materialize, and he refers the

reader to another source for a “thorough discussion of the difficulty” (p. 354).

Every essay in the book is worth reading. I have little space to comment on them individually, so must be rudely selective. Glaeser summarizes ideas he has helped make familiar to urban economists: the skilled city, the consumer city, urban amenities, the key role of housing supply. He describes the histories of Boston, New York, Chicago, Los Angeles, and Detroit in interesting ways (for example, seventeenth-century Boston was the first American “consumer city” (p. 31)). His engaging essay is especially useful to readers not already familiar with urban economics. The discussion of econometric complications in empirical estimates could have been more extensive, and I think a sweeping generalization that “few U.S. cities have much significant manufacturing left” (p. 38) is *too* sweeping.

Rybczynski’s piece on design will benefit planning students, though they will miss the visual material they are accustomed to. He surveys many urban objects and complexes in a useful way, but there is nary a picture. Small’s review of transportation is similarly chock-full of specifics, and has many neat facts and ideas not often found in urban economics courses. Three recommendations are provocative: policymakers should cater to heterogeneous preferences by differentiating transportation products; highway designers should favor curvature and lane widths that increase the capacity to move passenger vehicles at moderate speeds, even if that sacrifices very high speeds and accommodates large trucks less; governments should reward success by basing transit subsidies on ridership.

Vigdor, Murnane, and Cook connect worker skills to their own subjects of race, education, and crime, respectively. Vigdor ventures the generalization that “segregated neighborhoods aggregate individuals *predisposed* to poor labor-market outcomes” (p. 220, my italics), and says it’s unclear whether neighborhoods produce any additional effect. “Unskilled workers were bound to fare poorly . . . regardless of where they lived. Any influence of place must be small relative to the influence of skill itself” (p. 220). Murnane notes that improving urban education matters more than ever now, because skills are so important, but it’s also harder: “schools lack the human

resources provided by middle-class students” (p. 277), and they find it harder to compete for the skilled workers needed in the production of education itself. He describes concrete policy initiatives cities can pursue on their own, without grants from above, but it’s easy to identify barriers to consensus and implementation even within one city’s polity. Cook sees safe streets as necessary to attract skilled workers: “Thus the notion that poverty is the mother of crime has been turned on its head” (p. 297). He is typically optimistic: “crime is . . . potentially malleable, with policies more feasible and immediate than those required to ‘reshape society’” (p. 306).

In such a wide-ranging book any reader can claim there are important omissions. Here are two things I think are missing. First, in reporting the many cross-section regressions using cities as observations, the authors do not mention how they handled heteroscedasticity and spatial autocorrelation. I am willing to assume such a sophisticated bunch did the right things, though they don’t say so, but an academic reader will want to know details, and both academics and policymakers would be interested in the spatial patterns that were detected.

Second, there is no explicit reference to social capital theory. Here and there are brief discussions of related concepts (peer effects, neighborhood effects, social interactions and social networks, social sanctions). Cook and Card pay the most attention to them. Cook also mentions (pp. 316, 321) the concept of “collective efficacy” from Robert J. Sampson, Stephen W.

Raudenbush, and Felton Earls (1997). Card feels peer-group externalities “may well be the most important cost of increased immigration in many natives’ minds” (p. 189), but says they are hard to value (his research presented here is on market effects on wages and housing costs and effects on city finances). Overall, such mentions are scattered in various essays and their weight is not great. Authors who cite James Coleman and Gary Becker don’t do so for their influential work on social capital, and no one cites Robert Putnam. No one even cites Glaeser, David Laibson, and Bruce Sacerdote (2002), though some cite Glaeser and coauthors on social interactions.

Many policymakers would expect and benefit from a more focused and substantial treatment of social capital, even if it questioned the empirical relevance. Surely now social capital is a factor in the continuing debate on place versus people policies. Citizens’ convictions that social capital matters increase their desire for place policies, and social capital helps explain why people as well as structures are durable hard-to-move assets in distressed communities.

REFERENCES

- Glaeser, Edward L., David Laibson, and Bruce Sacerdote. 2002. “An Economic Approach to Social Capital.” *Economic Journal*, 112(483): F437–58.
- Sampson, Robert J., Stephen W. Raudenbush, and Felton Earls. 1997. “Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy.” *Science*, 277(5328): 918–24.

ROGER BOLTON
Williams College

Copyright of Journal of Economic Literature is the property of American Economic Association and its content may not be copied or emailed to multiple sites or posted to a listserv without the copyright holder's express written permission. However, users may print, download, or email articles for individual use.