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Book Reviews

Editor's Note: Guidelines for Selecting Books to Review

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

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

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

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

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

A General Economics and Teaching

Crunch: Why Do I Feel So Squeezed? (And Other Unsolved Economic Mysteries). By Jared Bernstein. San Francisco: Berrett-Koehler, BK Currents, 2008. Pp. xi, 225. $26.95. ISBN 978–1–57675–477–1. *JEL 2008–0749*

Well before the onset of the current economic downturn, middle-income families in the United States complained of growing difficulty making ends meet. According to Jared Bernstein, they were feeling squeezed for a simple reason: they *were* squeezed. Although GDP grew by 15 percent in real terms between 2000 and 2006, the median worker's real earnings fell by 0.7 percent during the same period. Real median household earnings actually declined by 5 percent between 2000 and 2006, the steeper drop reflecting a reduction in hours worked.

In *Crunch*, Bernstein lays out his views on the causes of middle-class income stagnation and offers suggestions for what might be done to ameliorate the problem of rising income inequality, which he describes as the central economic problem of our era. After a long tenure as a senior economist and director of the Living Standards Program at the Economic Policy Institute in Washington, D.C., Bernstein was appointed as Chief Economist and Economic Policy Advisor to Vice President Joe Biden in January 2009. In announcing the appointment, the vice president described Bernstein as "an acclaimed economist, and a proven, passionate advocate for raising the incomes of middle-class families."[1] Because President Obama appointed Mr. Biden to head the White House Middle-Class Economic Task Force, those who are curious about the future course of the administration's economic policies would be well advised to give *Crunch* a close look.

Thanks to Bernstein's breezy, nontechnical style, they won't find it a chore to read. He begins with a gentle jab at his fellow dismal scientists, recounting the old tale of a doctor who has just told an unfortunate woman she has only six months to live. "Can't anything be done?" she asks. "Marry an economist," the doctor tells her. "It won't cure the illness, but it will make six months seem like five years" (p. 1).

In one inspired early section, Bernstein casts his wisdom in the form a pitch-perfect satire of high noir detective fiction:

> I was working late in my DC office. I'd been running some new simulations on my macro model, but nothing was converging, so I figured I'd close up my spreadsheet and find a corner in some dark speakeasy to lick my wounds.
>
> That's when she walked in. She had a neckline as low as the Nasdaq in '01, curves like sine waves, and a dress tighter than the global oil supply. She had my attention even before she pulled out two reports I'd seen that very morning . . . (p. 12).

But apart from such excursions, the book's narrative is structured mostly in Q&A format. Each section begins with a clearly worded question prompted by people's everyday interactions with the economy, which Bernstein then attempts to answer. "Is Social Security about to go bust?" "If I hire an immigrant, am I hurting the native born worker?" "How come child care workers earn so little?" And so on. These questions, Bernstein reports, come from "non-economists, mostly taken from e-mails questionnaires and the blogosphere . . . " (p. x).

Each section typically concludes with a "crunchpoint," Bernstein's term for his pithy summary of the section's central message. Following his answer to a question about automation in factories, for example, he concludes, "We should resist the temptation to avoid labor-saving technology, and we should use the benefits from implementing it to improve job quality for all" (p. 131).

Here and there, he also throws in an original haiku (p. 127):

> The economist . . .
> I listen as he tells me
> What I cannot have.

In response to the question about Social Security's future, Bernstein efficiently swats aside unfounded claims that the system won't be there for today's young workers when they retire. As he puts it in his crunchpoint, "With a few sensible, manageable changes, this venerable intergenerational contract can live on, a mutually supportive dance that's been linking the young and the old for generations" (p. 103).

Bernstein also correctly identifies sharply rising medical costs as the real threat to the long-term viability of the nation's entitlement programs. He argues persuasively that steps to bring these costs under control will be central to the success of efforts to provide universal health coverage.

Bernstein appreciates the distinction between deficits used to pay for additional consumption and those used to pay for additional investment. For example, a deficit used to finance an expansion of the Head Start program will yield future increases in productivity that will help pay down the resulting addition to the federal debt. In contrast, the tax cuts to high-income households in recent years, which mostly paid for one-shot increases in luxury consumption, will generate no comparable flow of future revenue.

Although Bernstein argues that additional investment in education may well be justified by its effect on future productivity, he is also properly skeptical of claims that such investment will substantially reduce income inequality. Earnings

[1] See http://change.gov/newsroom/entry/vice_president_elect_biden_announces_chief_economist/.

dispersion, he notes, has risen even among college graduates in recent years. Even if everyone got a college degree, we wouldn't see the huge reduction in inequality many would expect.

Although Bernstein is deeply sympathetic to the organized labor movement, he is also less inclined than many of labor's most ardent supporters to take a hard line on trade. Globalization, he acknowledges, is here to stay. The greatest failure of U.S. trade policy, he argues, is to have failed to bargain more aggressively for fair terms in agreements with our trading partners.

The U.S. market has long been by far the most valuable sales outlet for the world's major exporters. Access to our market is worth much more to each of them than our access to any of their markets is worth to us. We should be exploiting this asymmetry, Bernstein argues, to forge trade agreements that make it easier for American workers to share the gains from trade. Without insisting that all countries should have the same environmental protection legislation or workplace democracy standards, one can argue that firms in countries with especially lax legislation in these areas enjoy a competitive advantage. American negotiators could level the playing field a bit by insisting that our trading partners adopt stricter standards themselves as a condition for gaining access to our markets.

Many economists once thought that political support for free trade would become universal once people understood that specialization by comparative advantage makes the total economic pie larger for all trading partners. As has become abundantly clear, however, the fact that each country's pie gets bigger provides no assurance that everyone within each country will get a bigger slice. On the contrary, many important groups within each country typically suffer losses when borders become more open. And as Bernstein notes, those who lose from trade are becoming increasingly well organized. Unless steps are taken to ensure that the gains from trade are shared more widely, protectionist sentiment may regain the upper hand.

But Bernstein also seems to realize that demanding stricter labor and environmental standards from our trading partners will take us only so far. Countries like China and India, where highly skilled labor is often available at less than a tenth the wage of their American counterparts, would have a decisive edge in many markets even in a much stricter legal environment.

Bernstein is therefore wise, in my judgment, to focus his policy recommendations on steps to strengthen the social safety net. When trade throws people out of work, for example, we should provide more extensive training allowances and unemployment compensation. We should increase the earned income tax credit. We should do more to assure adequate pensions for retirees. And most important, we should move toward a single-payer system of universal health coverage.

What's not to like? The one issue on which Bernstein and I disagree most strongly is the cause of rising income inequality. He believes the growing income gap is rooted in the growing disparity in bargaining power between capital and labor. My own view is that increased competition has much more to do with it.

To be sure, the decline of the union movement cannot have helped workers' wages keep pace. But the pattern of rising inequality has been more or less the same in every labor market for which we have data, whether unions are heavily involved or not. Thus, as Bernstein himself noted, there has been sharply rising income inequality among college graduates, most of who were not represented by unions even at the height of the labor movement. We also see the same pattern of rising inequality in occupations, such as dentistry, where neither unions nor low-wage competition from abroad are important factors.

In contrast, the consistent pattern is for income dispersion to rise whenever a labor market becomes more competitive. When professional baseball players were bound to their teams by the reserve clause, for example, salaries were relatively uniform and were largely unaffected when the industry experienced large revenue gains from national television contracts. Only when the reserve clause was challenged successfully in the mid-1970s did we see player salaries begin to track ability differences more closely and reflect industry revenue gains over time.

The pattern has been the same with executive pay. There was never a legal reserve clause for executives, but there was a de facto one. To become the CEO of a large American corporation, it was once necessary to spend one's entire career with the company. And when one of the small number of plausible internal candidates was chosen, salary was a bilateral negotiation between that candidate and the board. CEOs were paid less than one-tenth of what they earn today. Most, of course, were perfectly content to accept the going rate. But if they weren't, they had no place else to turn. Today, with so many CEOs recruited from outside the company, we now have an effective spot market for executives. And, as in professional sports, the salary structure has changed accordingly.

Despite my disagreement with Bernstein about the cause of rising inequality, I have no quarrel with his policy recommendations for dealing with it. Making it easier for workers to form unions may not lessen earnings inequality to any significant degree, but it will surely help curb the flagrant abuses of existing safety regulations that have become so much more common in recent years. And many of the other most pressing problems associated with inequality will be mitigated by a stronger social safety net paid for by more progressive taxation.

Bottom line: Bernstein's central crunchpoint is on the money. People feel squeezed because they *are* squeezed. And voters seem to have decided that it's time to do something about it.

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The Economics of Intercollegiate Sports. By Randy R. Grant, John Leadley, and Zenon Zygmont. Singapore and Hackensack, N.J.: World Scientific, 2008. Pp. xxvi, 535. ISBN 978–981–256–879–3, cloth; 978–981–256–880–9, pbk. *JEL 2008–0757*

The topic of the book, intercollegiate sports, is one ripe for the writing. As the authors state in the preface, "the economic structure of college sports is uniquely different" (p. xix) from the economics of professional sports. There are two aspects of college sports that complicate the economics of college sports relative to professional sports: the athletes are more than athletes, they are also students; and the athletic departments of colleges are not for-profit entities, but rather are a portion of a larger nonprofit entity with a focus on education and research. While economists have studied the economics of intercollegiate sports and books for the layperson exist, this appears to be the first textbook on the subject.

Sports can be used as a foundation around which to teach almost all microeconomic topics, and is one that is bound to capture the interest of many undergraduates. As an example, this book covers an impressive array of economic topics, with the practical focus on college sports. The book covers topics in the theory of the firm (the athletic department and the university); industrial organization (cartels on both the buying and selling sides of the market; pricing strategies, including third-degree and second-degree price discrimination); labor economics (for athletes and coaches, discrimination); market failures (externalities and public goods); and social economics (race and gender issues). It is intended for an audience of undergraduates with a background in introductory economics.

What is the Goal of Schools, Athletic Departments, and the NCAA?

An important topic is missing, however. An economic analysis of intercollegiate sports requires a discussion of the fact that some of the main actors in the markets are nonprofits: athletic departments are in schools that are nonprofits and the NCAA is organized as a nonprofit. An entity that has an objective other than profit maximization will make different decisions than will a profit-maximizing entity.[1]

In the second chapter, "Cartels in College Sports," the authors examine the evidence regarding whether the NCAA is a cartel without discussing what the organization's objectives might be. Their interpretation of the evidence suggests that they view the NCAA and its member schools as maximizing profits, at least with respect to the athletic department; for example, they consider high profitability of the athletic department as evidence that there is a cartel.[2]

[1] For example, suppose that one firm maximizes profits and another maximizes revenue. The former will select output where marginal revenue is equal to marginal cost, whereas the latter will select output where marginal revenue is equal to zero.

[2] Because it is the school that is nonprofit, not the athletic department, the athletic department can earn "profits," which the school itself uses internally.

Is profit maximization a reasonable assumption for the NCAA and its member schools? A school might desire to maximize profits from sports so that it can use the proceeds toward its other objectives, perhaps providing financial aid to needy students or reducing class size or increasing research. It is less clear why an athletic department would wish to maximize profits, since it would simply give the profits to the school. For example, one might expect that the athletic director and other administrators would distribute some of the profits to themselves in the form of high salaries or other perks. Because the NCAA is made up of schools, it may maximize profits on behalf of its members, although again principal–agent issues are likely to arise such that the NCAA does not minimize costs (and hence isn't maximizing profits).

However, there are a variety of acts by the NCAA, schools, and athletic departments that do not seem consistent with an (implicit) assumption that these agents are motivated by profits. In particular, why do schools offer sports that make no profit? Why does the NCAA require that schools that wish to be part of Division I sponsor fourteen sports.[3] Why does the NCAA require that Division I schools provide a minimum of two hundred athletic scholarships,[4] over half of which must be awarded to student-athletes in nonrevenue sports?[5] None of these actions seem explicable if the NCAA and athletic departments are maximizing profits. Other examples of actions by schools, athletic departments, and the NCAA abound.[6]

Institutional Details

The book likewise does not provide the relevant institutional detail to inform the economic analysis. The first chapter, "The History of Intercollegiate Athletics and the NCAA," is a mix of historical facts and the introduction of some economic terms. However, many of the sports facts are irrelevant for an economic analysis of college sports. The authors include trivia that is bound to "turn off" many students (e.g., the state of higher education at the time of the American Revolution and the role of baseball during the Civil War); it is difficult to see how this information is relevant to the study of college sports today.[7]

In addition to a plethora of intercollegiate sports trivia, the first chapter does not give enough institutional description and facts to prepare the reader for the economic applications to come, nor is sufficient description added in later chapters.[8] For example, in the first chapter, the discussion on limits on payments to student-athletes has little detail on the current NCAA regulations. While the authors mention that athletic scholarships are limited to tuition and mandatory fees, room and board, and required books (what the NCAA terms a full grant-in-aid or full GIA), it is not clear that this is the current rule (it is mentioned as a rule enacted in the early 1950s), nor do the authors discuss the limits on other payments to student-athletes, be they in the form of earnings from work or non-athletically-based scholarships. These details are also not described in chapter 2, "Cartels in College Sports."

Economic Analysis Lacking

The authors' attempt to link intercollegiate sports to economic analysis is often weak. For example, the authors attempt to use a typical supply and demand (marginal cost and marginal benefit) diagram to evaluate the efficiency of Title IX.[9] The explanation is incomplete and erroneous. First, the authors fail to explain whether the marginal cost (MC) and marginal benefit (MB) curves are the costs and benefits of the athletic department, the college, or society as a whole. The authors state that there are positive externalities, so the marginal benefit curve will not be

[3] *2006–07 Division I Manual*, bylaws 20.9.4 and 20.9.7.1.

[4] *2006–07 Division I Manual*, bylaw 20.9.7.4(b).

[5] In general, men's basketball and football are considered revenue-generating sports, and all other sports are not. There are exceptions, such as the women's basketball team at the University of Connecticut.

[6] This is especially true in chapter 4, "Athletics and Academics."

[7] That is not to say that the entire history of college sports and the NCAA should be excluded—indeed, the history of the founding of the NCAA may be important to understand the objectives of the organization today.

[8] This is generally true. An example of a sufficiently but not overly detailed discussion of the institutional facts and background necessary for the economic analysis to come is on the topic of Title IX in chapter 8, "Race and Gender Issues in Intercollegiate Sports."

[9] See section 8.13, "An Economic Analysis of Title IX."

the same for the athletic department and society as a whole. Each of the graphs, one labeled "efficiency argument *against* Title IX" and the other "efficiency argument *for* Title IX," contain two quantities, Q_O, the optimal quantity (where MC and MB intersect) and Q_T, "the quantity . . . offered because of institutional response to Title IX." In each graph, Q_T is not equal to Q_O and, therefore, each graph has a deadweight loss when comparing the quantity offered by the school in the presence of Title IX versus the optimal quantity. Thus, both graphs suggest that Title IX is an inefficient policy.

However, no conclusions can be drawn regarding efficiency from these graphs because it is not clear what the equilibrium quantity is in the absence of Title IX. If the MC and MB represent the costs and benefits of the athletic department, then the optimal quantity—from the athletic department's point of view—obtains in the absence of Title IX. However, in that case MC and MB do not include the external benefits to society of sports, and one cannot evaluate the social efficiency of Title IX. On the other hand, if the MC and MB represent society's costs and benefits, then the graphs do not indicate the equilibrium offering of sports opportunities in the absence of Title IX, and efficiency cannot be judged.

Finally, the authors indicate that, in response to Title IX, schools reduce the number of sports opportunities for men. While factually this appears to be true for some schools, it is inconsistent with the graphs. If the graphs accurately represent the benefits and costs to the athletic department, then reducing the opportunities for men is inefficient, as reducing opportunities moves the athletic department to the point where marginal benefits are greater than marginal costs. Some athletic departments argued that they had a fixed athletic budget and that increasing opportunities for women necessitated reducing opportunities for men. If true, then Title IX changes the marginal cost curves for athletic departments, but no change in the MC is shown in the graphs.

Many economic terms are not even adequately defined. For example, chapter 9, "Reforming College Sports," includes "capture" and "tipping point" as "key terms" in the chapter. Neither term shows up in the index. Here is the sum total discussion of "capture" in the chapter : "The Drake Group[10] believes that organizations like the Knight Foundation,[11] while well-intentioned, are—to use an expression from regulatory economics—*captured* by the NCAA and thus unable to implement any meaningful reform" (p. 490). A student who's had a class in regulatory economics may well understand what it means that the Knight Commission may be "captured," but no student who hasn't already been taught the economic concept of capture is going to understand it from this single sentence. A similar example from the same chapter regards the term "tipping point": "Perhaps Gladwell's (2000) *tipping point* analogy is applicable. As more and more schools resort to desperate measures to extricate themselves from the arms race, perhaps a sea change in attitudes towards big-time sports programs will emerge" (p. 492). Few students are going to find these two sentences illuminating on the meaning of "tipping point" nor its economic implications. Yet the authors define both of these concepts as "key terms" of the chapter. Unfortunately, these are not isolated examples.

Finally, the book contains lengthy discussions of issues in intercollegiate sports that have no economic discussion/analysis at all. These discussions include sections from chapter 7, "The Media and Intercollegiate Sports," (e.g., sections 7.8.2: "Creation and Expansion of the BCS"; 7.8.4: "Rule Changes"; and 7.8.5: "Instant Replay") and most of the sections in chapter 9, "Reforming College Sports" (e.g., sections 9.4.2: "Allow Student-Athletes to Transfer without Losing Eligibility"; 9.4.3: "Create Minor League Affiliates of the NBA and NFL"; 9.5.3: "Restrict Sources of Outside Income for Coaches"; and all subsections in 9.6: "Academic Reforms"). The discussion of reforms typically merely describes the possible reform, but contains little if any economic analysis.

[10] The Drake Group's motto is "defending academic integrity in the face of commercialized college sport." http://www.thedrakegroup.org/index.html, accessed March 17, 2009.

[11] The authors no doubt mean the Knight Commission on Intercollegiate Athletics (which is a project of the Knight Foundation). Its mission is to "work to ensure that intercollegiate athletic programs operate within the educational mission of their colleges and universities." http://www.knightcommission.org/, accessed March 17, 2009.

Yet many of these reforms clearly have economic implications; consider, for example, the idea of reducing the number of football scholarships (section 9.5.2) or of eliminating all athletic scholarships (section 9.6.9). The authors do not discuss the fact that both of these are forms of cartelization, only slightly different than putting limits on the amount of a scholarship. The authors strongly objected to NCAA cartelization in terms of limiting the amount financial aid.[12] Eliminating all athletic scholarships is the ultimate limit; essentially it sets financial aid at $0. Such a rule has all sorts of economic consequences in terms of how schools would recruit student-athletes, yet there is no economic analysis of this proposed reform.

Poorly Organized

The book is organized in such a way that it will be difficult for students to learn the economics. For example, the discussion in chapter 2, "Cartels in College Sports," is confusing because it comes before the discussion of labor markets. The first sections of the chapter discuss cartels in an output market. This discussion is economically coherent, although it is difficult to see how it applies to the NCAA because the authors have yet to explain what product is being cartelized and how the market for televised college games works. Some explanation is given later in the chapter, though much of it does not occur until chapter 7, "The Media and Intercollegiate Sports." Then in the discussion of whether there is evidence that the NCAA is a cartel, the authors suddenly turn the discussion around to consider whether there are low input prices. Low input prices would be evidence of a cartel on the buying side, not the selling side, which is all that has been discussed at this point. There should be a separate discussion of each cartel—the output cartel and the input cartel—and the discussion of the input cartel should not be presented until after the labor market for athletes is discussed.

In addition, the discussion of the labor market for college coaches (chapter 5) should be discussed before the labor market for college athletes (chapter 3). To the extent that the standard models of labor markets apply, they apply more closely to the market for coaches than the market for student-athletes, since in the latter case student-athletes are playing two roles—student, or consumer of a college output, and athlete, or input to a college output.[13]

Conclusion

All in all, this book is insufficient to be used as a textbook. The economic analyses are insufficiently explained and are surrounded by too much extraneous discussion. Rather, the book is more of an op-ed against the way the NCAA and college sports are currently implemented.

As an example, consider the discussion of the "beer and circus" "theory" (pp. 308–18). There is no economic foundation for this theory, no economic analysis of the theory, and no economic evidence offered in support of or in contradiction to the theory. The thesis of the theory is that schools "keep students drunk and preoccupied so they will not notice the fact that they are getting little in the way of education" (p. 308).

The entire discussion is filled with silliness, such as "Rarely mentioned is that if you can show that you can pay list price tuition, especially if you are an out-of-state student, you will be admitted to virtually every public college or university in the country regardless of your GPA or your SATs" (p. 310). "[T]he fixation on sports is accompanied by a profound lack of interest in undergraduate education" (p. 314). "Unfortunately, there is a major drawback to being a professor. She must teach" (p. 315). "It is exceedingly simple: research matters and teaching does not" (pp. 315–16). "Are all schools like this? Absolutely not. But the schools that are most like this tend to be the same schools with a big-time sports program" (p. 316). Research universities "need undergraduate tuition to survive even though they have no interest in educating undergraduates. So, how do they keep the undergraduates contented and unaware that the degree they are earning will be of little value? By encouraging them to party until the day they graduate" (p. 316).

[12] See chapter 2, "Cartels in College Sports."

[13] In addition, the discussion of the labor market for coaches does not refer at all to the models of labor markets that were introduced in chapter 3, "The Labor Market for College Athletes."

The authors conclude the discussion with "Is the Beer and Circus story accurate? We will leave that to you to decide" (p. 317). As presented, it is so ridiculous as a general story that it doesn't bear mention.

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The Dismal Science: How Thinking Like an Economist Undermines Community. By Stephan A. Marglin. Cambridge and London: Harvard University Press, 2008. Pp. xvi, 359. $35.00. ISBN 978–0–674–02654–4.
JEL 2008–0002

Hostility to commerce dates to the very beginnings of market exchange. Aristotle, to pick an early example, condemned commerce as debased and immoral, fit only for inferiors, such as foreigners or slaves. Students of commerce, economists for short, have also been attacked. "Anti-economics" has a long history, too; indeed, criticism of economics and criticism of commerce have tended to go together. Stephen Marglin carries on both traditions—*The Dismal Science: How Thinking Like an Economist Undermines Community* discredits the market and economics alike. *The Dismal Science* belongs also to a third tradition, that of bending the history of economics to the purposes of heterodoxy.

Marglin has read widely and thought long; his indictment of economics traverses four centuries. The principal charge is this: (1) markets undermine community; (2) economics promotes markets; ergo, (3) economics justifies the undermining of community. The minor premise says that economics has an ideological agenda to promote the market. There is, says Marglin, an established "church of economics" (p. 5), one whose creed, shared by congregants Left and Right, is "markets are good for people" (p. 3). Thus, economics does not merely describe the world, nor merely evaluate it; it seeks to fashion the world in the image of the market.

The anticommunity charge is but part of a much longer indictment. In fact, *The Dismal Science* treats the market-destroys-community argument as a scaffolding on which to hang a learned, if sometimes motley, collection of criticisms aimed at economics. Economics, says *The Dismal Science,* is too formal in method, too narrow in orientation, too beholden to individualism, too scientistic, and too indifferent to pressing social questions, such as poverty and inequality. Worst of all, economics is too unrealistic about the true wellsprings of human action; its centerpiece "is the rational, calculating, self-interested individual with unlimited wants for whom society is the nation-state" (p. 36).

Marglin's book does not merely argue for the inadequacies of economics, it also offers an historical explanation for them. Economics' many failings, says *The Dismal Science*, are the product of its Enlightenment origins. Since the time of Adam Smith or thereabouts, economics has been in the thrall of what Marglin calls modernity. Economics' flawed foundations—individualism, self-interest, maximizing behavior, and unlimited wants—are not only false and destructive, they were installed at the founding of systematic political economy.

Marglin recognizes the good done by markets and liberal thought more generally. But *The Dismal Science* does not pretend to be a balanced appraisal. It avowedly makes a case for the dark side of the market and for the claim that economics, unique among the many disciplines that have studied exchange, drank too deeply at the well of Enlightenment ideas, making it blind to community.

The first three chapters make the argument that markets weaken community. It seems likely that the impersonal exchange of markets and the personal arrangements of mutual assistance are, at least in some measure, substitutes. If farmers buy fire insurance, there will be fewer community barn raisings. The Amish, who, in rejecting the market, rely upon their neighbors, have a stronger form of mutual dependence than do ordinary farmers who rely upon insurance companies and building contractors. If such forms of mutual dependence are good, then markets come at a price.

But this is only the beginning of an analysis. For community comes at a price, too. Even benign communities tend to oppose the virtues of tolerance, openness, and diversity. Marglin recognizes that some communities, such as those erected upon plantation slavery, can be brutal and oppressive.[1] The ties that bind are sometimes shackles.

Yet elsewhere, thinking again of the Amish, Marglin asserts that "other regarding behavior is a moral obligation" (97), which forgets that other regarding behavior need not be altruistic. History's many repressive communities demonstrate how often other-regarding behavior can be malevolent. To the extent that markets weaken such communities, this should count in markets' favor.

On the other hand, Marglin is right that some intellectual traditions are long-standing in political economy, and individualism is one of them. It is also true that differences between foundational assumptions—individualist versus communitarian—are fundamental, perhaps even insuperable.

Individualism opposes Marglin's conception of community as something greater than actual members of it. The individualist says: if communities are good, then people will form them, join them, or leave them as they see fit. Where's the problem? Marglin's communitarian reply is that community (unlike mere association, such as the bowling league) cannot be chosen; it is given. Individuals are born into communities as children are born into families.

The individualist says that there is nothing that is good for the community that is not good for at least some members of it. The communitarian disagrees fundamentally, which is how Marglin can argue that, even if markets make every single person in a community better off, this can be bad. "Bad for whom"?, the individualist wonders.

So the individualist economist is not blind to community. She likely lives her life inside several of them. Rather, the individualist economist endorses a different view of community's ethical standing, one that says that communities are good only insofar as they benefit people.

A further point: it is ahistorical to make Adam Smith the father of individualism. Smith was no individualist. He devoted an entire book, *The Theory of Moral Sentiments*, to explaining how society transforms individuals into moral beings. Smith did not understand them as preferences, but the moral sentiments are profoundly endogenous.

Smith also knew that markets came at a cost. The great engine of economic growth—the division of labor—makes us wealthier and possibly freer as well, but it has a price, the dulling, narrowing effects of specialization.

This pattern is typical of the book. It engages critically with foundational ideas, and some of its criticisms of contemporary economics are well placed. But the sheer scope and heterogeneity of *The Dismal Science*'s criticisms creates two difficulties: one, a tendency to contradict itself and, two, an ahistorical tendency to regard economics as a monolith, fundamentally unchanged since its installation during the Enlightenment.

An interesting epistemological argument, for example, says that Max U misses something as a theory of human action. In particular, it misses "experiential knowledge," or know-how. Constrained optimization depends solely upon propositional knowledge, or know-that. I know that my preferences are these, prices and income are these, and so on. There's no room for know-how; how to ride a bike or how to bank a billiards shot. In using Max U, then, economics fails to recognize experiential knowledge, which, not coincidentally, often resides in communities.

At the same time, however, Marglin embraces Friedrich A. Hayek's (1945) idea that markets are superior aggregators and mobilizers of know-how. Hayek's point was that know-how cannot be reduced to propositions and, thus, remains inaccessible to central planners. As a partisan of know-how, Marglin makes a case against economics but simultaneously for markets, undermining his central conceit that economics everywhere aids and abets the market.

Historians of economics will react to *The Dismal Science* by observing that the target of its many criticisms does not exist. There is no stable set of inquirers called economists that is four hundred years old, still less is there a stable set of foundational intellectual commitments called economics (p. 95). As Jürg Neihans (1990) reminds us, Richard Cantillon was a banker. David Hume was a philosopher who made his living as an essayist and librarian. Francois Quesnay was a

[1] To his credit, Marglin also recognizes that Thomas Carlyle, the man who coined the epithet "Dismal Science" as a racist double entendre, endorsed plantation slavery on grounds that it was a system morally superior to the "wage slavery" of factory work. (See Levy 2001.)

court physician. When Robert Malthus became famous, he was a parson. David Ricardo brokered stocks before joining Parliament. Karl Marx was a journalist. John Stuart Mill worked for the East India Company.

Only in the late nineteenth century did political economy professionalize. What is more, the founders of American economics as a professional, academic discipline—the first economists, if you like—rejected outright many of the ideas and values *The Dismal Science* ascribes to economics. The men who founded the American Economic Association were progressives who fundamentally opposed individualism, knew little of constrained optimization, and made no brief for rationality. They were known as the "ethical economists" because they rejected self-interest as the motive for human action. As evangelical Protestants, they advocated intervening in markets as part of their project of bringing the Kingdom of Heaven to Earth, that is, as part of founding a Christian community.

Economists will react to *The Dismal Science* by wondering, if perhaps some of the many arrows fired at economics hit their target, what we should conclude about markets? Is it not possible that markets, like all human creations, are imperfect and fallible, but, on average and all things considered, better for human welfare than all known alternatives for organizing economic life? This is, of course, a contingent question, but it's the one this book might better have asked.

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EconoPower: How a New Generation of Economists Is Transforming the World. By Mark Skousen. Hoboken: Wiley, 2008. Pp. xiv, 274. $24.95. ISBN 978–0–470–13807–6.

JEL 2008–0752

Mark Skousen's new book, *Econopower*, enters an increasingly crowded field of "popular economics" books. For the most part, the predecessor books are somewhat narrowly focused—either on the research of an individual (i.e., *Freakonomics, Predictably Irrational*) or on a particular set of policy recommendations (i.e., *Nudge*). In contrast, *Econopower* aims to be a somewhat more complete tour through where economic research has been going, and the increased involvement of economists in policy and "real world" applications. This broader focus is exciting in principle; in practice the resulting book feels somewhat unfocused—a hodgepodge of investing advice, discussion of *Freakonomics*-like research, attempts to understand important questions in economic growth, and a liberal sprinkling of libertarian proselytizing. The lack of focus is frustrating, largely because some parts of the book are much better than others, and one wishes the better parts were longer and more complete, ideally at the expense of the worse.

The book begins with an introduction that lays out Mr. Skousen's basic point: economists have become more important, have colonized many other social sciences, and are increasingly interested in real-world problems. There is certainly some truth to this, although I disagree with Mr. Skousen's feeling that " . . . only a minority of economists are attracted to applied economics" (p. 5), which, based on a look around top departments and students on the job market, does not appear to be the case. In this section, Mr. Skousen discusses the seven "Power Tools" of economics, which include "accountability," "economizing and cost–benefit analysis," and so on. It is here that the political angle of the book begins to evidence, with an emphasis on the importance of the market in addressing economics problems and a plea for lack of government intervention where it is not needed.

It is here, also, that it becomes clear that this book is not intended for an academic audience. For example, although savings and investment are certainly important objects in a lot of work in economics, most economists probably would not describe the importance of savings as a central "tool" in their work. Of course, Mr. Skousen is not writing for an academic audience; this is a popular book, intended for an educated lay person. This makes it a somewhat difficult book to review, and I have tried to put myself in the appropriate shoes to do so, although perhaps with limited success.

Part 1 of the book contains investing advice: how to use the insights from financial economists (Jeremy Siegel, David Swenson) to learn to "beat the market." The book contains some reasonable tips—save in your 401K, companies should have default 401K enrollment, and it's difficult to beat the market in investing. This is all sound advice; it is not likely to be news to academics, but may well be useful to someone not in the field. This section of the book ends with several chapters (chapters are very short) discussing social security. In this section, the politics of the author are in evidence, as it begins with extolling the virtues of the Chilean private pension system, envisioned by the "Chicago Boys" in the 1970s, and going on to argue for reforming social security to focus on private accounts. Although the social security discussion followed to some extent from the investing advice, this section of the book ends with a seeming non sequitur chapter on whether money is important for happiness (apparently, only sort of; money—according to Skousen—is beaten out by satisfaction with work, recreation, love and friendship, and worship).

The book then moves into more familiar popular-economics territory, with a series of chapters on using economic thinking to solve social issues, including traffic (congestion pricing), high medical spending (health savings accounts), and poor education (school choice). Again, these solutions are all very reasonable and, while they are unlikely to be a surprise for professional economists, they may well be exciting for people outside of the field.

In this section, Skousen also discusses economic work on crime, including the evidence from John Lott's *More Guns, Less Crime* work. Skousen concludes this chapter with the statement that "All this confirms a long-standing legal principle in America: People should have the constitutional right to own a gun for self-protection" (p. 111). This chapter on crime illustrates my primary frustration with the book, even as a book for a nonacademic audience. Understanding what is responsible for the drop in crime in the 1990s is complicated, and this question has produced a wide variety of work. Some of this has been written up (for example, Levitt wrote up his own work in *Freakonomics*) but, because Skousen has not personally engaged with this literature, there seemed a real opportunity for an outside perspective to lay out the literature and comment on the arguments from all sides. Instead, the book completely ignores most of the relevant papers and contents itself with a discussion of John Lott's work, which is interesting but controversial, and ends with an essentially political statement that really doesn't seem to follow from the preceding discussion. It seems a lost opportunity.

Skousen moves on to what he calls "Solving International Problems," discussing the role for free-market thinking in environmental regulations (cap-and-trade) and development (microcredit). It is at this stage of the book that Skousen really loses focus, even relative to the earlier parts. The chapters jump around enormously—from a comparison of income in India and Hong Kong, to why Egypt is poor, to the Laffer curve and the flat tax movement. This section could have done with a chapter explaining why these concepts are connected (if they are)—what is the overall message that we should take away? The end of this section discusses the economics of religion, and is quite interesting. This seems like a topic Mr. Skousen knows something about. But again, as with the discussion of crime, the book suffers from not enough detail on the really interesting bits.

The final part of the book deals with forecasting, and I found this to be the most interesting. Skousen discusses the success of macroeconomic forecasting models and exposes the virtues of internet betting sites as predictors of elections. There is an interesting chapter that discusses the benefit of moving back to the gold standard, although seems to stop short of claiming that is a good idea. It's too bad (and no fault of Mr. Skousen's, of course) that this section of the book (and the book overall) was written before the financial crisis; the chapter on whether we can have another Great Depression would likely be more interesting to read had it been informed by recent events. The book ends with the claim that Hayek is today's most influential economist and a quick chapter on the need to speak out about and teach economic freedom.

My instinct is that most academic economists would, as I did, find this book somewhat frustrating. I agree with the premise that economists are having more influence than ever, but I continually felt as I read the book that Mr. Skousen had missed many of the best and most exciting examples of this. Within my field—economic development—a natural thing to discuss is the move toward randomized evaluations and work with NGOs in developing countries to solve on-the-ground problems like high levels of intestinal worms and teacher absence. Economists have started working in-house at tech firms—Google, Yahoo, Microsoft—and, although Mr. Skousen discusses the Google IPO auction, he does not go into any detail about the role economists are playing more generally in these firms.

Having said this, I'll emphasize again that academics are not the intended audience and I think the book fares much better in its intended area—as an entertaining tour through the current state of economist's involvement in the real world. After having read this book, one could have a reasonable sense of at least *something* that is going on in any of a number of fields of economics—finance, development, health, behavioral economics, macro-forecasting, etc. I still feel the book could have used more focus and, in the end, I am not sure that it really found its footing. Is this a book about the value of libertarian principles in economic situations? Is it a book about how economists are tackling applied problems? Is it a book about how to invest your money? The truth is that it seems to be a book about all of these questions and more, and no one question gets a totally complete answer. In the end, though, perhaps this is what differentiates a successful popular book from the academic papers on which it is based. Most people do not want to read the *Journal of Political Economy* on the beach, but they just might want to read this.

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B History of Economic Thought, Methodology, and Heterodox Approaches

Hunting Causes and Using Them: Approaches in Philosophy and Economics. By Nancy Cartwright. Cambridge and New York: Cambridge University Press, 2007. Pp. x, 270. $85.00, cloth; $29.99, paper. ISBN 978–0–521–86081–9, cloth; 978–0–521–67798–1, pbk.

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For the past thirty years, Nancy Cartwright has been one of the most significant philosophers of science. Beginning with a focus on physics, she was at the forefront of the movement to use philosophy to help to understand the practices of physics as seen from the working physicists' point of view rather than simply to pronounce on those practices from an Olympian, but perhaps irrelevant, perspective. Starting with her *Nature's Capacities and Their Measurement* (1989), she has steadily taken in a wider scope of sciences, including social sciences.

In Cartwright's view, economics is not some poor stepchild to physics but a significant part of a complex world in which the sciences are not (as so often thought by philosophers, physical scientists, and economists alike) arranged in a clear hierarchy in which each of the "special" sciences is reducible to the more basic sciences—physics forming the bedrock. Cartwright has also been a major player in the philosophical analysis of causation, a role that suits her turn toward economics, which has been undergoing a causal revival in, for example, the work of Granger in time-series econometrics, Heckman in micro-economic policy analysis, and the program of natural experiments in applied microeconomics. Given this background, a new book by Nancy Cartwright—particularly one that singles out economics in its subtitle—is surely a welcome event.

Hunting Causes and Using Them unfortunately represents a missed opportunity. It is not a systematic treatise but a compilation of occasional papers written with various particular—and mainly philosophical—targets in view. The papers have been too lightly edited to form coherent chapters in a unified volume. They are frequently repetitive and notation shifts from chapter to chapter. It is often difficult to appreciate fully the point of the chapter without the full context of the debates to which they originally contributed. They are heavy sledding for an *economist* not already immersed in those debates.

Despite professing to seeing useful insights in various approaches, Cartwright's method is more critical than constructive. And she sometimes misunderstands the approaches that she criticizes. For example, I do not recognize my own position in her account of my analysis of causal order (chapter 14). She attributes causal judgments to me that straightforward application of the formal definitions of chapter 3 of my *Causality in Macroeconomics* (2001) contradict. This is unfortunate, as she is a deeply insightful philosopher with a rare connection to actual practice and, even here, her discussion is full of genuine insights about causation and the problems of modeling it. A constructive treatise that tempered her criticism with a lucid exposition of its objects would have been exceedingly helpful.

Three themes dominate *Hunting Causes*. The first is that *cause* is a plural concept. The methods and metaphysics of causation, she believes, are context dependent. Different causal accounts seem to be at odds with one another only because the same word means different things in different contexts. Every formal approach to causality uses a conceptual framework that is "thinner" than causal reality. She lists a bewildering variety of approaches to causation: probabilistic and Bayes-net accounts (of, for example, Patrick Suppes, Clive Granger, Wolfgang Spohn, Judea Pearl, Clark Glymour); modularity accounts (Pearl, James Woodward, Stephen LeRoy); invariance accounts (Woodward, David Hendry, Kevin Hoover); natural experiments (Herbert Simon, James Hamilton, Cartwright); causal process accounts (Wesley Salmon, Philip Dowe); efficacy accounts (Hoover); counterfactual accounts (David Lewis, Hendry, Paul Holland, Donald Rubin); manipulationist accounts (Peter Menzies, Huw Price); and others. The lists of advocates of various accounts overlap. Nevertheless, she sometimes treats these accounts as if they were so different that it is not clear why they should be the subject of a single book. And she fails to explain what they have in common. If, as she apparently believes, they do not have a common essence, do they have a Wittgensteinian family resemblance? She fails to explore in any systematic way the complementarities among the different approaches—for example, between invariance accounts, Bayes nets, and natural experiments—that frequently make their advocates allies rather than opponents.

The second theme is her distinction between schemes that deductively *clinch* causal inferences and those that inductively *vouch* for them. Her idea is that certain schemes of causal inference work by making such strong background assumptions that inductive arguments are turned into deductive arguments. She is surely right that many arguments take the form of clinchers, conditional on background assumptions. But she is wrong to imply that advocates of these forms of argument are insensitive to the tentativeness and the fallibility of those strong background assumptions. Such sensitivity means that arguments that take the form of clinchers are, in reality, always practically vouchers.

For example, with Bayes-net approaches a statistical model describes data from which probabilities are inferred; and causal order, in turn, is inferred deductively from those probabilities. The inferences are based on strong assumptions. For instance, analysts frequently assume *causal sufficiency* (i.e., there are no omitted variables of a type that would confuse causal inference), the *acylicality* of causal structure, and the *linearity* of functional relationships. Serious users of Bayes-net approaches are deeply aware of the fragility of the statistics—both the quality of the data and the modeling assumptions (e.g., stationarity and homogeneity). And they are aware that the assumptions about causal structure may fail in practical cases, which is why they have investigated the implications of alternative assumptions—e.g., latent variables (relaxing causal sufficiency), nonlinearity, and cyclical models.

And what is the alternative? Absent the strategy of embedding clinchers within maintained, but criticizable, assumptions, Cartwright provides no account of how evidence vouches for causal claims.

The final theme is the distinction between hunting and using causes highlighted in the title. The distinction gets its bite in Cartwright's belief that the strategies that successfully allow the identification of casual mechanisms frequently serve policy applications ill. Building on a longstanding theme of her work, real world processes are seen as the complex composition of a variety of deeper tendencies. The function of scientific experiments is to isolate those tendencies through stringent controls so that they can be exhibited in pure form. The application of scientific knowledge in practice is frequently complicated—if not thwarted altogether—because the real world is open and, unlike in the laboratory, the complicating tendencies are uncontrolled. In such cases, it is not necessarily reliable to infer that effects found under stringent controls will play out similarly in the world.

Her insight trades on the old distinction between *internal* and *external* validity. For example, we may discover in a randomized controlled trial that a drug is effective against the malaria parasite; and, yet, for a variety of social and biological reasons, the drug may prove to be practically ineffective in patients. One lesson, perhaps, is that randomized controlled trials need to be supplemented with epidemiological studies. The exact same issues can arise with respect to natural experiments in economics: can the mechanism that they isolate be carried over to other policy contexts?

The theme of hunting versus using causes is elaborated in the final chapter on the use of counterfactuals in economics. Cartwright argues that the relevant counterfactuals isolate a cause from its own causes and set it to some value come what may. Using the same *implementation-neutral* strategies counterfactually to evaluate policies typically results in "imposters"—the wrong counterfactual for the issue at hand. Genuine policy analysis typically, though not always, requires *implementation-specific* counterfactuals. (Not always because some policies need to be robust across different implementations if they are to be useful since, in some cases, targeting is practically restricted.)

Cartwright is clearly correct that good policy requires the right counterfactuals and that, naturally, economists sometimes get it wrong. Yet, as a generic criticism, her case is not persuasive. For example, a straightforward reading of the Lucas critique, which Cartwright cites in other parts of the book with other purposes, is precisely as a plea for understanding counterfactuals in a causally structured, implementation-specific manner. Implementation of policy requires the specification of conditional rules and not a come-what-may setting of particular variables.

Nancy Cartwright has once again written an intellectually challenging book, full of insights. It is too bad that the presentation is not well adapted to an audience of econometricians and applied economists, for whom the issues that she considers are important and not always clearly thought through.

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Milton Friedman: A Biography. By Lanny Ebenstein. Houndmills, U.K. and New York: Palgrave Macmillan, 2007. Pp. xi, 286. $27.95. ISBN 978–1–4039–7627–7. *JEL 2007–1226*

Milton Friedman granted Lanny Ebenstein permission to write his biography, but evidently did not carefully examine his qualifications for the task. Friedman did not have a colorful personal background—certainly nothing comparable to that of John Maynard Keynes, who is often cited as an economist whose influence paralleled Friedman's—so there is little need for a biographer to dwell on details of earlier undergraduate and graduate years, marriage, and family (the first fifty pages of the biography). These pages are based on the Friedmans' 1998 memoirs—*Two Lucky People*. The reader who sought these conventional facts might do better to read the original source rather than Ebenstein's transcription.

At a minimum, a biography of Friedman should provide a well-rounded portrait of the man and, above all, an accurate account of the original ideas that he expounded as a professional economist, generated by his prolific intellect.

The one aspect of Friedman's personality that the biography succeeds in conveying is the chapter that describes Friedman's interaction with his students in the course on price theory and the money and banking workshop at the University of Chicago.

However, there are many other aspects of Friedman's personality that a biographer should report. For example, Friedman's style as a debater reveals an aspect of his personality. He was always courteous to his opponents in a debate, never attacked ad hominem. He concentrated on the weaknesses of the opponent's arguments and invariably emerged as the victor in the debate. Another example, from the time he underwent open-heart surgery when he was sixty, his mortality must have taken root in his psyche. Yet, Friedman's joie de vivre overcame any inclination to spend his future lifespan as an invalid. He embraced physical activity; he played tennis singles and doubles with students and friends as long as he could summon the energy to play the game. He ignored speed limits when he drove his car and, at an advanced age, refused to yield the driver's seat to a chauffeur. He enjoyed simple pleasures, bridge, listening to radio talk shows and, above all, entertaining friends and guests at dinner in his home or in a restaurant.

The chief deficiency of Ebenstein's work is its failure to provide an authoritative account of Friedman's intellectual achievements. On this matter, it is instructive to compare Walter Isaacson's highly praised biography of Albert Einstein, of world renown, with Ebenstein's biography of Friedman.

Isaacson's success in portraying the full measure of facets of Einstein's character may be attributable to his eventful life and the availability of his personal correspondence but, in addition, Isaacson realized that he had no background in science to enable him to do justice to Einstein's scientific achievements. Here is where the distinction matters between the biography of Einstein and Ebenstein's biography of Friedman. Isaacson enlisted the help of Brian Greene, professor of physics at Columbia University, to save him from error in discussing Einstein's scientific achievements. Isaacson's summary of the content of Einstein's 1905 series of brilliant new concepts of general relativity and later the problems with quantum theory that Einstein struggled with but failed to resolve are adequate for the nonprofessional reader to grasp their significance.

If Ebenstein had recognized that he lacked the background in economics to give an authoritative report of the scope of Friedman's theories and the originality of his policy recommendations, he could have obtained the help of a Gary Becker or an Edmund Phelps to assure that his understanding of Friedman's contributions was not marred by his uncertain grasp of the issues. Thus, Ebenstein misinterprets objections that Harry Johnson and Don Patinkin raised about Friedman's essay, "The Quantity Theory of Money: A Restatement." Friedman replied to them respectfully, not as Ebenstein does, as if they were carping critics, rather than distinguished economists.

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Harry Johnson: A Life in Economics. By Donald E. Moggridge. Historical Perspectives on Modern Economics. Cambridge and New York: Cambridge University Press, 2008. Pp. ix, 486. $90.00. ISBN 978–0–521–87482–3.

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For twenty-eight years, from 1949 to 1977, Harry Johnson "bestrode our discipline like a Colossus," as James Tobin wrote in an obituary. Harry published about five hundred academic papers and fifteen books, tackling a great variety of theoretical and policy topics. This book by a fellow Canadian is the first full-length biography of this enormously influential economist.

In my view, the most valuable and interesting feature of the book is the detailed account and analysis both by Harry himself (mostly in letters) and by the author of the various universities and economics departments where Harry studied or worked as a faculty member in his busy and ever-changing life, namely Toronto, Cambridge, (England), Harvard, Manchester, Chicago, and the London School of Economics (LSE). Harry was interested in how institutions were run, in traditions and biases, and in "national styles in economics," and, not least, in personalities. Wherever he went he was critical. There are numerous letters and notes by the very perceptive Harry in the book. For a historian of institutions as well as history of thought, this book should be a goldmine. Of course, the book also covers his numerous other activities: his membership of committees of various kinds, his incredible conference going, his early life, and—most important—his academic articles.

Harry did his first undergraduate degree in Toronto and then joined the Canadian army. Immediately after the war he spent one year in Cambridge (England), obtaining a first class honors degree. In that period, he already made an impression, especially on Nicholas Kaldor. He was made a member of the Political Economy Club—at a meeting of which he opened the discussion on a paper presented by John Maynard Keynes!

After a brief interlude in Toronto as an instructor, he spent two years at Harvard. Here he was especially influenced by Schumpeter. He became exceptionally well read; a friend described Harry as "a walking bibliography."

Next came a very important period in his life, namely seven years back in Cambridge (England). He was 26 when he started there in 1949, and this was the beginning of his publications career, especially in trade theory—the field in which finally he made his major impact. He became a Fellow of King's College and also was elected to join the "Apostles," a highly select club or discussion group, that had famous philosophers, as well as Keynes, as members. He was indeed highly regarded and very well treated in Cambridge; this is worth noting since much later he wrote some very critical articles about the Cambridge "style." He became assistant editor of the relatively new *Review of Economic Studies*, thus beginning his long career as an editor. Milton Friedman visited Cambridge for the year 1953–54 and recognized Harry's brilliance, writing back to Chicago that they should keep him in mind for an appointment.

Then came two years at Manchester University and finally, in 1959, he joined Chicago. There is a great deal in the book about the Chicago Department of Economics, dominated at the time by Milton Friedman. It was exceptional in the vigorous intellectual debates and the commitment to research. It was also notable for the interaction among members of the department, a contrast with Harvard. This, to me, is the most interesting part of the book, and is dealt with at some length. At first, Harry was somewhat uncomfortable with the fierceness of the debates—though he acquired the "Chicago style" eventually. He wrote a paper criticizing the Chicago methodology. Eventually he became a convert, especially to a moderate form of Friedman's monetarism and, in England, a missionary.

Never to come to rest, he always seemed to consider moving and, in 1966, he took up a Chair at the LSE, as a joint appointment with Chicago. He was at the LSE for seven years and this was also an eventful stage in his career. He aimed to make the LSE into a wholly graduate school in economics (and related fields) and to develop a suitable program and appointments. He put tremendous energy into trying to bring about institutional changes but was only partly successful. This is a long and complicated story, fully reported in the book, and it left Harry dissatisfied with England and its ways. There is a long discussion of his concept of a graduate education. Essentially he wanted to introduce the U.S. system—of which Chicago was the best model—to Britain. Again, this part of the book is extremely interesting. It also describes the student "troubles" of the 1960s that affected the LSE very seriously.

Harry had his first stroke in 1973 and gave up his LSE position. He replaced it with a one-term a year appointment at the Graduate Institute of International Studies in Geneva. He had his second stroke in 1977 and died in May 1977, aged 54 years.

At the end, Moggridge lists four reasons given by James Tobin for Harry's exceptional influence: Harry's "willingness to accommodate fellow scholars and students, his powers of exposition and synthesis informed by an almost limitless knowledge of the literature, his internationalism, and his status as a character in a profession with few colourful personalities" (p. 413). Moggridge also discusses Harry's character—his efficiency, his busyness, his breadth of interest, and, of course, his phenomenal written output. He remarks, correctly, that Harry's frame of reference was broader than that of most academic economists. Harry was concerned with contemporary society and its evolution. He also discusses something that is indeed a subject of discussion among the declining number of the "Harry Johnson generation," namely why someone who was so much cited once is very little cited now. Here the answer tells us (in my view) as much about the way academic economics is done now as about Harry's writings. But it must also be granted that some of the issues or topics that Harry wrote about extensively—such as the theory of tariffs and the monetary theory of the balance of payments—are simply not of such great interest now, which is also true of his various once-famous surveys.

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Augustin Cournot: Modelling Economics. Edited by Jean-Philippe Touffut. Cournot Centre for Economic Studies Series. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2007. Pp. xv, 148. $90.00. ISBN 978–1–84720–586–5, cloth; 978–1–84720–654–1, pbk.

JEL 2008–0340

Nearly every economics student knows of Antoine Augustin Cournot, but I would wager that few economists appreciate the many contributions that Cournot has made, both within economics and mathematics and in other fields, such as philosophy. *Augustin Cournot: Modelling Economics* is a compilation of seven conference papers discussing Cournot's prescient ideas and their applicability today. According to the authors of the introduction, Thierry Martin and Jean-Philippe Touffut, "the aim is above all to pay homage to Cournot's originality and modernity in the area of social mathematics, and more precisely in the field of economics" (pp.1–2). The book succeeds in that aim.

Most of the chapters in this volume follow a general format of discussing Cournot's contributions and giving examples of how we use them today. This modern take on Cournot is the primary theme of the book. A minor theme is the question of why Cournot's work went largely unrecognized for many years. Cournot published his first economic book, *Recherches sur les principes mathématiques de la théorie des richesses* (*Researches into the Mathematical Principles of the Theory of Wealth*), in 1838. This book was essentially ignored for more than thirty years and was not published in English until 1897. Only two authors address the question of this delay directly, but at least the question is included in this homage.

Not surprisingly for a conference volume, this book is inconsistent. Some chapters are written with an engaging simplicity or sense of humor, while others are quite dry. The contributors' backgrounds include philosophy, history, and statistics, as befits the multitalented Cournot. Although they are not organized in this manner, the chapters can be roughly categorized as focusing on economics or on statistics and probability. I was surprised to find that the most compelling chapters were written by economists. Beyond the variety of writing styles and subject matter, though, one important consistency in this book is the thorough research that went into each chapter.

Jean Magnan de Bornier discusses several fundamental economic concepts published in *Recherches* and provides an answer to why this work took so long to be noticed. Upon reading this chapter, one marvels at the many ideas Cournot first described as a 37 year-old. For instance, he discusses elasticity and marginal cost, and even finds that price should equal marginal cost under perfect competition. Lest we become too jealous of this would-be John Bates Clark medal winner, Magnan de Bornier explains why Cournot's work was poorly received. For instance, Cournot failed to describe his theory of costs as something novel and did not even give marginal cost a name.

Magnan de Bornier points out the importance of clear writing for "selling" ideas. Robert Aumann's and Robert Solow's well-written chapters reinforce this concept. Aumann's chapter is a slightly edited version of his Nobel Prize lecture. This was justified for inclusion because it shows an application of strategic equilibrium, which Cournot first formulated in his theory of duopolies. One finds no mention of Cournot in the lecture, but this is a pleasant summary of strategy in repeated games in the context of war and peace. Solow's chapter considers Cournot as an early macroeconomist. Like Magnan de Bornier, Solow points out Cournot's insights while highlighting the flaws that likely prevented his work from immediately reaching a broad audience. Solow sums up this duality in a clever line about a "piece of analysis that shows how well [Cournot] does what he can do" (p. 113). This chapter also includes an amusing look at a one-sided debate between Cournot and Adam Smith.

Bernard Walliser and Alain Desrosières each write on Cournot's mathematical contributions. Walliser focuses on Cournot's use of mathematical modeling in economics. Reading Cournot's pioneering thoughts on modeling is enjoyable. These include a warning about unnecessary formalism that should still be heeded. Walliser analyzes Cournot's early ideas through a less enjoyable general discussion of the forms models take today. Turning to a broader subject, Desrosières discusses the development of the use of statistics in the social sciences. His chapter moves from a history of thought in the field in the early nineteenth century to ways in which Cournot's ideas, such as the separation of objective and subjective probabilities, have influenced modern applications. Desrosières elegantly combines a careful reading of Cournot with original analysis of current statistical reasoning.

Glenn Shafer's chapter on "Cournot's principle" puts Cournotian modeling techniques into action. Cournot's principle states that a selected event of small or zero probability will not occur. Shafer provides a history of the rise and fall in acceptance of this principle and argues for its applicability. The bulk of this chapter comes from Shafer's work with Vladimir Vovk on applying a game-theoretic interpretation of the principle to market prices to create a new market efficiency hypothesis. This chapter is an outlier due to its length (which makes it roughly one-fourth of the book) and the formality of the modeling in an otherwise descriptive text.

Delving more deeply into Cournot's writing alone, Thierry Martin takes on the topic of probabilistic epistemology. He describes how Cournot constantly reflected upon and questioned the probabilistic approach as he contributed to probability theory. Cournot wanted to ensure that the theory was more than just an interesting "mental exercise" (p. 23) that had little significance for the real world. Martin's chapter provides a careful analysis of Cournot's philosophical writing and serves as a reminder of the basic questions about the interpretation of probability that were debated in the last century. Unfortunately, this chapter may be too dense for those who have not studied philosophy to fully enjoy.

Augustin Cournot: Modelling Economics is not a biography, but rather a reflection on those ideas of Cournot that persist today and what we can still learn from this great thinker. One cannot help but wonder at the wide range of accomplishments detailed in this book, but the discussion of Cournot's missteps is an unexpected highlight. Economists may not care to learn about Cournot's epistemological concerns, which are mentioned in several chapters. On the whole, though, this book should appeal to those who would like to learn more about Cournot as well as the various settings in which his thoughts have been embraced or rejected.

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C Mathematical and Quantitative Methods

The Cult of Statistical Significance: How the Standard Error Costs Us Jobs, Justice, and Lives. By Stephen T. Ziliak and Deirdre N. McCloskey. Economics, Cognition, and Society series. Ann Arbor: University of Michigan Press, 2008. Pp. xxiii, 321. $24.95, paper. ISBN 978–0–4720–07007–7, cloth; 978–0–472–05007–9, pbk. *JEL 2008–0347*

In its March 1996 issue, this Journal carried a terrific article written by Deirdre N. McCloskey and Stephen T. Ziliak (1996). The main points of this article were: (1) *statistical significance* is not the same thing as *substantive significance*; (2) applied econometric research should be focused on substantive—in this case, *economic*—significance; and (3) much, if not most, applied econometric research " . . . takes statistical significance to be the same as economic significance" (McCloskey and Ziliak 1996, p. 98). It's worth a brief review of the 1996 article as prelude to the review of a new book by the same authors.

To illustrate point (1) above, suppose a regression is run to estimate the demand function for gasoline and the price elasticity of demand is estimated to be –0.80 with an estimated standard error of 0.50. What conclusion is to be drawn? It might be concluded that since the corresponding t-statistic is just –1.6 (= –0.8/0.5), price is insignificant; end of story. McCloskey and Ziliak would claim that such a conclusion is a mindless use of a statistical rule-of-thumb and doesn't begin to address issues related to economic importance.

What, in this case, is the economic significance referred to in point (2) above? These days a test of whether own-price affects the demand for gasoline is a mindless exercise. Indeed, if the estimated elasticity turned out to be zero or point-zero-something, a savvy economic researcher would suspect a big data error, a crazy leverage point, or something else that questions the result. An economist should understand that the substantively interesting hypothesis would be whether the demand for gasoline is elastic or inelastic. Is the (absolute) elasticity meaningfully bigger or smaller than unity? A test of the unit price elasticity hypothesis would employ a t-statistic given by

$$[-0.8 - (-1.0)]/0.5 = 0.2/0.5 = 0.4,$$

implying that the evidence is not able to reject the hypothesis of unitary price elasticity of demand for gasoline. So is this where we say, "end of story"? McCloskey and Ziliak would say, "No, not yet."

Is –0.8 substantively the same as –1.0, or is it importantly different from –1.0? If the data had produced an estimate of –.98, nobody would think that to be importantly different from unitary elasticity. But the data, presumably processed in a way that is expected to generate the best point estimate of the elasticity, came out with –0.8, not virtually –1.0. Is the estimate of –0.8 importantly different from a unitary elasticity? That depends on what use is to be made of the estimated elasticity. If the government is thinking of increasing the price of gasoline via a gasoline tax in order to reduce oil imports, the relevant question might be, "How much would oil imports be reduced through the use of a politically feasible increase in the tax on gasoline?" An elasticity of –0.8 might produce a saving in imported oil that is importantly less than if the elasticity were –1.0. An elasticity of –0.8 might well mean that the demand for gasoline is inelastic from the perspective of the substantive issue generating the research, despite the value of the t-statistic.

In 1996, McCloskey and Ziliak were making the point that applied economic research is rarely free of a substantive decision context and it's that which should be the basis for generating the questions asked of the data. To pursue this point, they evaluated all the full-length papers using regression analysis that were published in the *American Economic Review* (*AER*) during the 1980s. McCloskey and Ziliak came up with nineteen questions to be posed to each of the roughly 180 relevant articles. All nineteen questions were phrased so that a "yes" answer signifies "good practice." Here are just five interesting and representative questions, each beginning with "Does the paper . . . ":

#8: consider the power of the test? (4.4)

#13: discuss the scientific conversation within which a coefficient would be judged large or small? (28.0)

#16: consider more than statistical significance decisive in an empirical argument? (29.7)

#18: in the conclusions, distinguish between statistical and substantive significance? (30.1)

#12: discuss the size of the coefficients? (80.2)

The number in parentheses following each question is the percent of the articles for which the answer to that question was a "yes." Although question #12 produced an overwhelming proportion of positive responses, the proportion positive on questions #8, #13, #16, and #18 were distinctly underwhelming. Overall, the proportion of "yes" answers was below 1/3 for eight out of the nineteen questions and above 2/3 for only five out of the nineteen questions.

Among McCloskey and Ziliak's conclusions about their review of "best practice" papers in the 1980s is the following: "We would not assert that every economist misunderstands statistical significance, only that most do, and these [are] some of the best economic scientists" (McCloskey and Ziliak 1996, p. 111, bracket added). Personally, this reviewer believes that the word "misunderstands" is somewhat more pejorative than is appropriate, but "misuses" would certainly be justified by what McCloskey and Ziliak found.

Now we find a full-sized book by the same authors, with order reversed to Ziliak and McCloskey (2008). The basic topic is the same as in the 1996 article with a good bit added, including:

- Fuller treatment of the importance of power, or type II error, as opposed to its traditional neglect in empirical research,
- An updating of the earlier findings on applied econometric research to include an evaluation of all the relevant articles published in the *AER* during the 1990s,
- An extension of the analysis to include significant attention to empirical research in psychology and medicine, and
- A substantial review of the development of methodology in the field of statistics as a way to understand how empirical research came to be so dominated by statistical significance in so many applied areas.

Oddly, the first chapter in the book is unnumbered, though it has the title "A Significant Problem" and is required reading for anyone not thoroughly familiar with McCloskey and Ziliak (1996). It lays out the problem of *statistical significance versus substantive importance*, including some real-life examples. Ziliak and McCloskey distinguish clearly between what they call the philosophical disciplines that tend to focus on the issue of *whether an effect exists* and the scientific disciplines that focus on magnitude, *how big the effect is*. Science, Ziliak and McCloskey say, needs to learn both *whether* and *how much*. But the putatively common practice of ending the empirical analysis by checking whether a calculated test statistic does or doesn't reject the null hypothesis according to a 5 percent or some other α-sized critical region is basically to stop at the question of existence without addressing the scientific issue of magnitude (Ziliak and McCloskey 2008, especially pp. 4–13). As well as introducing the book's core issue, this chapter nicely previews what's coming in the rest of the book, including an introduction to some of the heroes and villains of the piece.[1]

Chapters 1 and 2 stress the loss of possibly important substantive information that can arise from rote use of statistical significance. Some interesting applications based on problems related to choice among diet pills, the safety of the painkiller Vioxx, the efficacy of an anxiety-reducing drug, and the like are employed to motivate the analysis. When research focuses on statistical significance to the virtual exclusion of the opportunity or other costs of making the substantively wrong decision, Ziliak and McCloskey term this "The Sizeless Stare of Statistical Significance," their title for chapter 2.

In chapter 3, Ziliak and McCloskey take up what they regard as the common excuses practitioners make for their focus on significance tests: mainly, variations of the notion that it's objective, hence scientific. Chapter 4 goes on to deal with the Power of the test, making the point that neglecting that half of the problem is hardly scientific. And what's more, Ziliak and McCloskey point out, Gosset,[2] who invented the famous and still ubiquitous t-test, understood that the purpose of testing was to make decisions that are properly informed by the pecuniary costs and benefits attaching to the possible conclusions in the empirical problem.

Chapters 5–7 provide a thorough treatment of empirical practice in applied econometrics. Here Ziliak and McCloskey explain their nineteen-question approach to judging the quality of papers in applied econometrics. They update their earlier findings by applying their analysis to all of the relevant papers published in the *AER* in the 1990s. They compare the results for the two decades and ask, in the title to chapter 7, "Is Economic Practice Improving?" Compare the "percent-yes" results on the five illustrative questions already listed above:

[1] Here, in scrambled order, are ten of the persons first mentioned in this chapter and featured often in the book. Perhaps the reader would care to guess ex ante who will turn out to be the heroes and who the villains: Edward Leamer, Leonard "Jimmie" Savage, R. A. Fisher, Karl Pearson, Egon Pearson, Harold Hotelling, Arnold Zellner, Jerzy Neyman, W. Edwards Deming, and William Sealy Gosset.

[2] Gosset, who had to publish under a pseudonym as a requirement of his employment at Guinness Brewery, is the famous *Student* and Ziliak and McCloskey's #1 hero.

	Question				
	#8	#13	#16	#18	#12
1980s	4.4	28.0	29.7	30.1	80.2
1990s	8.0	53.5	20.9	56.7	78.1

The articles of the 1990s were little different on questions #8 and #12, noticeably worse on #16, and considerably improved on #13 and #18. Despite a few such instances of improving practice, however, there's no sharp difference overall between the two decades. In each decade, the proportion of "yes" answers was below one-third for eight out of the nineteen questions and above two-thirds for only five out of the nineteen questions. And the percent of "yes" answers averaged over all nineteen questions was 45.0 in the 1980s and 46.2 in the 1990s. About the best that can be said is that there's no convincing evidence of a meaningful change in quality from the 1980s to the 1990s.

Chapters 8–10 complete the economics portion of book. Chapter 8 takes a brief look at size of parameters in economics, drawing especially on econometric testing of purchasing power parity. Chapter 9 is an attempt to show that Ziliak and McCloskey aren't picking on the mediocre practice one might expect of mediocre economists. On the contrary, they identify (based on the *AER* articles) plenty of the best economic scientists—Nobel and other prize recipients—publishing articles that show up very badly in terms of their scores on the nineteen questions of "good practice."

Chapter 10 is an essay about why applied econometrics seems to be mired in the practice of largely ignoring substantive economic importance. They trace this state of affairs to some of the giants of statistical methodology, especially R. A. Fisher,[3] and the simple fact that it's relatively easy to teach "economic statistics" from a textbook that concentrates on hypothesis testing based on an accepted standard for the size of the critical region. Thus, " . . . almost all the teachers of econometrics claim that statistical significance is the same thing as scientific significance" (Ziliak and McCloskey 2008, p. 106), and " . . . few econometrics textbooks make the distinction between statistical and economic significance to balance the scores, sometimes hundreds, of pages devoted to explaining Fisherian significance" (Ziliak and McCloskey 2008, p. 107).

Chapters 11–13 take up the case of applied psychometric research. Ziliak and McCloskey find psychology to be in as poor shape as economics in terms of its narrow focus on statistical significance. Thus, "Some psychologists knew about the work of Jerzy Neyman and Egon Pearson[4] . . . But textbook authors, editors, and teachers—inspirited by Fisher's promise of raising their fields to the level of hard science—helped Fisher win the day" (Ziliak and McCloskey 2008, p. 143).

Statistical analysis in the field of medicine is taken up in chapters 14–16. Ziliak and McCloskey find medicine to be as guilty of the "sizeless stare" as economics and psychology. Here they relate the attempts of Harvard professor and journal editor Kenneth J. Rothman to shift statistical analysis in medicine to " . . . measuring *clinical* significance, not statistical significance" (Ziliak and McCloskey 2008, p. 165). Alas, they conclude, "It appears that one editor working in isolation cannot turn a science equipped with personal computers and canned programs away from the Significance Mistake, even well-placed editors speaking from important scientific institutions" (Ziliak and McCloskey 2008, p. 174).

Chapters 17–23, just under one-quarter of the book by page count, comprise essays in the history of thought in the field of statistics, as shaped by the Ziliak and McCloskey perspective. These are quite uneven in quality and in tone, the latter sometimes unnecessarily harsh. Chapter 18 is a fine, informative essay on Karl Pearson, despite his critical role in the development of null-hypothesis-test-centered analysis—precisely

[3] R. A. Fisher is clearly the major villain in Ziliak and McCloskey's story.

[4] Neyman and Egon (son of Karl) Pearson pioneered the decision theoretic approach to statistical inference with the classic Neyman–Pearson Lemma (Neyman and Pearson 1933). Neyman subsequently used the term *inductive behavior* in characterizing their 1933 solution to the two-decision problem. See also Erich L. Lehmann (2008, pp. 24–25, 165–69). Neyman and Egon Pearson are two of Ziliak and McCloskey's heroes.

what Ziliak and McCloskey rail against throughout the book. The authors are much less balanced and charitable in their essay on R. A. Fisher in chapter 21. Apparently, Fisher's villainy is just too much for Ziliak and McCloskey.[5] Chapter 20 is a delightful bio-essay on William Sealy Gosset, informed by the very interesting fuller treatment of Gosset in a recent article by Ziliak himself (Ziliak 2008).

Chapter 24 closes the book with the hopeful title "What to Do." Unfortunately, Ziliak and McCloskey's solution is little more than an admonition to do the right thing. Maybe that's why conditions have remained as they are for so long. The authors recognize early on in the book that getting "back to size in science . . . is more difficult . . . and cannot be reduced to mechanical procedures . . . How big is big is a necessary question . . . [but] has no answer independent of the conversation of scientists" (Ziliak and McCloskey 2008, p. xiv, bracket added).

Despite my firm belief that most applied econometricians would benefit from adopting the methodological position presented by Ziliak and McCloskey and that economics as science would be improved significantly thereby, I can't close without something of a rebuke. As often happens when someone is pushing what the mainstream considers an extreme or fringe position, the arguments become narrowly and harshly focused. This comes through too often in Ziliak and McCloskey. In its particularly narrow perspective, their treatment of the professional accomplishments of a number of exceptionally gifted economists is simply unjustified. Included among such economists are Gary Becker, Trygve Haavelmo, Harold Hotelling, Lawrence Klein, and Paul Samuelson. It is especially unfortunate, for example, that Ziliak and McCloskey misrepresent the significance of Haavelmo's pathbreaking article of 1944 and never even mention his major contribution of 1947, a piece which Ziliak and McCloskey should find quite simpatico.

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[5] Lehmann's brief essay on Fisher is far more objective and informative (Lehmann 2008, pp. 230–35).

E Macroeconomics and Monetary Economics

The Evolution of Monetary Policy and Banking in the US. By Donald D. Hester. Berlin and Heidelberg: Springer, 2008. Pp. viii, 205. ISBN 978–3–540–77793–9. *JEL 2008–1228*

There have been several books reviewing the evolution of monetary policy in the past few years. This book also provides a description of monetary policy in the post–World War II era, but adds to it a review of the evolution of the banking sector. As Hester notes, these two areas exert a fair bit of influence on each other. For instance, the tightening of monetary policy from 1960 through 1980, especially in the presence of ceilings on nominal interest rates, spurred a number of financial market innovations to circumvent different regulations. Thus, a book highlighting these connections provides a useful new perspective on some issues related to monetary policy.

Although it discusses both the evolution of monetary policy and banking, the book largely separates the discussion of these two topics into their own sections. (There are, of course, a variety of references to developments in the banking sector in the monetary policy part of the book and vice versa, but the sections of the book are clearly separated with respect to their focus.) This decision involves some trade-offs. An advantage of separating the discussion is that the author is able to elaborate on issues that had more substantial impacts on one topic but more modest ones on the other, for instance the wage and price controls imposed by the Nixon Administration on the conduct of monetary policy, without the reader feeling like part of the story has been dropped. On the other hand, a greater emphasis on monetary policy/banking sector connections might have provided a considerably richer discussion of situations in which these connections were particularly important, such as during the early 1990s when the Basel Capital Accords were being implemented and the economy was in a recession.

The first section of the book discusses the evolution of monetary policy. Chapters are organized around the various Chairmen of the Federal Reserve. These sections contain useful reviews of the economic developments and discussions of how they either shaped or were shaped by easing or tightening monetary policy. Discussions of economic developments in the text are supplemented by tables containing consistent sets of major economic and financial indicators that are useful in allowing the reader to follow the discussions of economic developments. These sections also review the particular items that the Federal Reserve has targeted with monetary policy, such as nonborrowed reserves or the federal funds rate, and discusses why the Federal Reserve changed targets and what those changes meant for the implementation of monetary policy. The author also provides some useful discussion regarding the relationships between the Federal Reserve and the different Presidential administrations; at times, such as during "Operation Twist," political dynamics have been an important factor in monetary policy. The author also includes some discussion of the advances in economic thought, such as the development of the IS/LM model and the Lucas Critique, and relates these to the conduct of monetary policy.

While the review of monetary policy was generally quite useful, there was some room for improvement. Hester provides copious amounts of data to illustrate his points. In addition to the tables noted earlier, numerous statistics are presented in the text, sometimes with only minimal commentary. The use of so many numbers detracts from the discussion and makes it more difficult to follow. There were also some topics where greater consideration would have been helpful. For instance, from the 1950s to the early 1970s, the United States was operating under the Bretton Woods exchange rate system. This system and its impact on the economy were referred to at times, but more background information on Bretton Woods and a fuller discussion of its implications for the U.S. economy and monetary policy would have been useful (see, for instance, Barry Eichengreen 1996). There also could have been a bit more in-depth discussion of some of the deeper economic forces affecting monetary policy. While some factors that might directly affect monetary policy, such as inflation, GDP growth, and unemployment, were reviewed quite nicely, more discussion of other factors, such as productivity growth, would have been useful. Further, although Hester spends a fair bit of time reviewing data and the state of the economy, it would have been useful to discuss the difficulties associated with using real time data in formulating monetary policy. As noted by Athanasios Orphanides (2003), among others, real-time data is subject to considerable revision and may at times give a false impression of the state of the economy.

The second portion of the book concerned the evolution of the banking system. Most of the major developments of the financial system are reviewed in this section. For instance, Hester describes the competition for deposits between thrifts, banks, and money market funds and the factors that led to the development of negotiable orders of withdrawal and money market deposit accounts. The discussions highlight the major economic and policy factors contributing to the changes in the financial sector. Other topics covered include the collapse of the thrift industry and large numbers of bank failures in the late 1980s/early 1990s, changes in the residential mortgage market that prompted development of new residential mortgage related products, and removal of the barriers limiting the types of financial services a particular financial holding company could provide. Major pieces of legislation regarding the financial sector, such as the Depository Institution Deregulation and Monetary Control Act of 1980, are also described. Thus, Hester provides a useful review of the major developments in the banking sector over the past fifty years.

The section of the book concerning the evolution of the banking sector is notably shorter than the section reviewing the evolution monetary policy. While the book does discuss major developments in banking and the most important factors leading to those changes, more detailed background and contextual information would have been useful. For instance, in discussing the removal of the barriers separating commercial and investment banking, Hester mentions the increased use of Section 20 subsidiaries to get around the rules. However, he does not discuss the other methods financial institutions were using to chip away at the barriers nor the regulatory landscape—involving a variety of federal and state laws—that allowed for some of those other methods to be employed. Additionally, some other features of the regulatory environment that had considerable impact on the behavior of financial institutions, such as Regulation Q and the associated interest rate ceilings, are given only cursory treatment. Readers who have a solid background with respect to the financial industry will be able to follow the discussion, but I was less sure that readers without such a background would be able to do so as well. Overall, the book seemed most appropriate for students with some background in money and banking, such as those in a senior seminar.

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Monetary Policy Strategy. By Frederic S. Mishkin. Cambridge and London: MIT Press, 2007. Pp. x, 549. $48.00. ISBN 978–0–262–13482–8.
JEL 2008–0060

Frederic S. Mishkin, a long-standing contributor to the monetary policy literature and author of a bestselling textbook in the field, has alternated over the past fifteen years his professorship at Columbia Business School with the roles of Research Director of the New York Fed and, up to a few months ago, Governor of the Federal Reserve Board. Those circumstances grant Mishkin a privileged perspective denied to the common armchair economist, and make his writings on monetary policy subject to a tight relevance constraint that many readers are likely to appreciate.

That privileged perspective permeates much of the present volume, which collects nineteen pieces on monetary policy "in practice," seventeen of which had been previously published in academic journals or conference volumes between 1992 and 2005. The two remaining pieces include an introduction offering a historical account of the joint evolution of ideas and practice of monetary policy since the 1970s (with a comprehensive reference section), and a concluding what-have-we-learned piece where Mishkin summarizes the main lessons of the earlier chapters and offers his views on how monetary policy should be conducted.

At the risk of oversimplification, the message underlying the present collection of writings can be summarized in the following points:

(1) The pursuit of price stability should be the overriding, explicit goal of monetary policy.

(2) While there are different strategies that may allow a central bank to succeed in achieving that goal, theorical considerations, as well as the evidence offered by the alternative arrangements adopted by different countries in the recent past, suggest that the inflation targeting framework is preferable to others, including an exchange rate peg or monetary targeting.

(3) The key elements of such an inflation targeting strategy are: (i) an institutional commitment to price stability as the primary long run goal of monetary policy, (ii) the public announcement of a medium-term numerical inflation target, (iii) an information-inclusive strategy, with no special role for monetary aggregates, (iv) high transparency through enhanced communication with the public about policymakers' intentions and objectives, and the reasons for deviations

from previous objectives, and (v) increased central bank accountability for attaining its objectives. All of those elements are instrumental in attaining the kind of macroeconomic environment, with low and steady inflation and stable output and employment, that has characterized the growing number of economies that have adopted an inflation targeting framework.

(4) The United States should abandon its current "just do it" approach to monetary policy—which relies in excess on the qualities of the Fed chairman at the helm—and adopt an explicit inflation targeting framework. The same recommendation applies to emerging market economies, but only so long as they already have good fiscal, financial, and monetary institutions in place. In their absence, a hard currency board or outright dollarization may be the best course of action.

Most of the articles reprinted in Mishkin's volume present a well developed set of arguments to support the previous points. The majority of the arguments will be familiar to academic economists working in the field or nonacademics that have followed closely developments on the monetary policy front in recent years and have a working knowledge of monetary economics. While Mishkin makes occasional use of a simple model by Svensson (1997) to support some of his points, the truth is that most of the arguments rely on educated common sense and, with few exceptions, do not require any sophisticated academic training. Yet, and despite the familiarity of his arguments, Mishkin offers an original perspective on a number of issues. Thus, chapter 8, written with Ben Bernanke in the very early 1990s, portrays the monetary targeting strategies of post–Bretton Woods Germany and Switzerland as already containing some of the key elements of subsequent inflation targeting strategies. Some of those elements include (1) a clear and consistent communication with the public of the targets and their underlying assumptions (including medium-term inflation goal), as well as the reasons for any deviations from the targets, (2) short-run flexibility, allowing short-term deviations from the monetary targets in order to avoid large fluctuations in output or to accommodate changes in velocity. In Mishkin's view the existence of an explicit framework built around an overriding goal of long-run price stability and the associated central bank accountability, as found in the German and Swiss post–Bretton Woods experiences, holds the key to avoiding the inflationary consequences of the time-inconsistency trap described by Finn E. Kydland and Edward C. Prescott (1977) without the need to adhere to ironclad rules. Using Mishkin's language, what is needed is a framework that provides "constrained discretion" to policymakers. The inflation targeting framework is, in his view, the best among the possible frameworks that would satisfy that requirement.

A nonconventional aspect of Mishkin's approach to the study of the evidence on monetary policy is his reliance on case studies. Thus, many of the contributions to this volume contain fairly detailed descriptions of the monetary policy strategies, the associated key decisions, and the observed outcomes for a number of advanced, emerging, and transition economies. While that effort at synthesizing a large amount of information is highly welcome and may provide a useful reference for many purposes, Mishkin's reliance on those case studies as the basis for his assessment of the relative merits of different monetary policy strategies is bound to be controversial. The reason is that, with such a small sample of countries, identification of the role of policy in the determination of observed outcomes becomes a difficult task, with one's subjective filtering of the available information likely to distort any inference. Interestingly, there is a small but growing empirical literature that uses statistical methods to uncover systematic relationships between the economic performance of different countries and their adoption or not of an inflation targeting strategy using a large sample of inflation targeters or nontargeters. That literature, exemplified by papers of Laurence Ball and Niamh Sheridan (2005) and Mishkin and Klaus Schmidt-Hebbel (2007), has identified large improvements in performance after the adoption of inflation targeting but, perhaps surprisingly, has failed to uncover any significant differences in performance between targeters and nontargeters. The latter finding is only mentioned briefly in chapter 1, with the emphasis shifting quickly to other results in the literature pointing to greater stability of inflation expectations in inflation targeting countries (though the latter also relies on a small sample of countries). One also finds it somewhat surprising that Mishkin's paper with Schmidt-Hebbel, which contains arguably the most comprehensive statistical analysis of the performance of targeting versus nontargeting countries, did not make it into the volume.

All the articles contained in the present volume, including the final summary, were wrItten before the fateful summer of 2007, the date marking the beginning of an international financial crisis with no recent precedent in the industrialized world, and which was followed by a deep and widespread economic downturn that shows no signs of coming to an end at the time of writing this review. During the current crisis, the role of central banks as guarantors of financial stability (in addition to price stability) has taken center stage. Furthermore, the consensus view is that such a role is likely to become permanently enhanced once the crisis is over, possibly in ways that will involve close coordination among central banks. With hindsight, it is interesting to note the (relatively) small weight that issues related to financial stability are given in Mishkin's volume. This is in accordance with the mainstream view about monetary policy before the recent episode. In the limited space devoted to those issues, Mishkin reaffirms the importance of financial stability as a goal of monetary policy to be attained through the central bank's role as a lender of last resort in case of liquidity crisis, possibly supplemented through an enhanced role in prudential supervision (see chapter 2). That role, however, must be limited and traded off against the risk of causing a central bank to lose its focus on the price stability objective. In the same chapter, Mishkin presents a scorecard for the Fed on different criteria. Interestingly, he gives it an "excellent" grade on its commitment to financial stability. That assessment, though originally published in 2000, would most likely have been seconded by a majority of U.S. economists until right before the crisis, which illustrates the extent to which the deep sources of the crisis were neglected by mainstream economists. It is far from clear that a similar judgment would be made now.

Several chapters of the book also contain discussions on the roles that asset prices should play in the design of monetary policy. Here Mishkin takes an unambiguous stance against having central banks respond to (apparent) asset price bubbles, over and above the "normal" response to their impact on macro aggregates called for by the inflation targeting framework itself. He offers several arguments to support his position, including the risks of moral hazard resulting from systematic attempts by the central bank to prop up asset prices if they crash. In the latter case, a quick and decisive response by the central bank may be needed in order to preserve financial stability and to compensate the likely decline in aggregate demand. That view contrasts with the one often associated with BIS economists (see, e.g., Claudio Borio, William English, and Andrew Filardo 2003) and which stresses the need for central banks to continuously monitor the buildup of financial imbalances and to use that assessment as the basis for their policy decisions, thus supplementing the inflation-focused benchmarks that characterize the inflation targeting regimes.

Whatever is left of inflation-focused monetary policy arrangements (as we know them) at the end of the current crisis, Mishkin's *Monetary Policy Strategy* will remain for years to come a useful and clear testimony of the evolution of monetary policy thinking and practice over the past two decades.

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F International Economics

Regional Monetary Integration. By Peter B. Kenen and Ellen E. Meade. Council on Foreign Relations Book series. Cambridge and New York: Cambridge University Press, 2008. Pp. xvi, 230. $80.00, cloth; $24.99, paper. ISBN 978–0–521–86250–9, cloth; 978–0–521–71150–0, pbk. *JEL 2008–0423*

This little book takes you to a tour of all of the world's regional monetary arrangements—past, present, and future—in just 198 pages, and you may wonder what you would get out of it. Well, quite a lot, actually. Peter Kenen and Ellen Meade have managed to pack an enormous amount of information, along with expert evaluation of a large bibliography (which covers another twenty pages), in this no-nonsense book.

As professors always do, they start with a brief introduction that well motivates the topic, teasing the reader with the question of whether we will see more monetary unions in the next two decades. Too bad, then, that they immediately provide the realistic but demotivating answer: not much will happen. Chapter 2 predictably presents the required theoretical background, a user-friendly survey of the optimum currency area principles. This is augmented with a review of the costs and benefits of currency unions, which they call "consolidations to include the adoption of a foreign currency. Thus equipped, we can embark on our trip. Chapter 3 takes us to Europe. Chapter 4 moves us back in time to the nineteenth and twentieth century, with visits to Scandinavia, Africa, and a host of other places. Subsequent travels bring us to North and South America in chapter 5, to East Asia in chapter 6, and back home in the last chapter, which tells us what this all means for the United States and its reigning currency.

The general view put forward by the authors, backed by much quoted evidence, is that monetary unions are born of exceptional historical circumstances (postwar reconciliation in Europe, post-colonialism institution-building in Africa and the Caribbean) unlikely to happen elsewhere, at least over the next two decades. They rightly emphasize the role played by politics in Europe as well as the long-held commitment to, and experience with, exchange rate stability on the continent.

So, there will be no "americo" for North America. Their reasoning is, in fact, quite simple: the United States has simply no good reason, economic or political, to give up its dollar. In fact, this point is so strong that they could just left it at that. But as professors do, they report all the detailed arguments that have been made for and against a single currency in North America. No common currency for South America, they predict, because neither Argentina nor Brazil can provide the anchor that Germany offered in Europe and because there is too little regional trade. No currency union in East Asia either, because the two regional giants, China and Japan, will never agree to give up control of their currencies.

I mostly agree, although two decades is a long time to feel safe about predictions. Many central banks in South America have gained a reasonable degree of independence in the last decade and could establish strong credibility over the next one. Trade could very well expand within South America as standards of living rise. Similarly, the ASEAN countries and Korea might decide to get together and adopt a common currency alongside the yen and the renmimbi; as Kenen and Meade rightly emphasize, this would require the acceptance of mutual surveillance and, hence, criticism, which seems currently impossible but could be acceptable to the next generation.

The last chapter is, perhaps, the least convincing. Paradoxically, when thinking about the dollar, the authors shorten their universal long view. Their main concern seems to be that the euro could have negative effects on the United States. They claim that the euro "favors trade among its members at the expense of trade with outsiders." Recent empirical works by Richard Baldwin, Frauke Skudelny, and Daria Taglioni (2005) and Harry Flam and Hakan Nordstrom (2006) provide evidence that there has been no trade diversion, quite to the contrary. Kenen and Meade also seem concerned that the euro could challenge the dollar's dominant role but they do not explain why this could be a bad thing for the United States and for the world as a whole. Anyway, they espouse the frequently heard view that "much will depend on the speed with which East Asian countries build up their reserves and how they choose to manage them" (p. 189). I am not sure that their preoccupation with China's reserve management strategy is fully warranted. The current crisis has already illustrated China's vulnerability to adverse shocks to the world economy, which should reinforce this country's traditional reluctance to engage into international monetary power play.

More importantly, Kenen and Meade are concerned that the dollar would depreciate if China, for instance, would stop buying U.S. Treasuries, switching instead into euro-denominated bonds. This presumed link between reserve accumulation and the dollar exchange rate is tenuous at best. It is not empirically established. As for theory, it is an example of thinking about a market only in terms of demand and ignoring supply. I would expect issuers of international bonds, of which governments are a small proportion (under normal circumstances), to provide investors with the instruments that they wish. If the demand for U.S. dollar-denominated assets falls, borrowers will issue euro-denominated bonds. What this means is not that the dollar would depreciate but that U.S. borrowers would either discover the discomfort of currency mismatch or have to offer better terms to their lenders. This would certainly be a change of historical proportions, the end of the "exorbitant privilege" of the United States, but it would not necessarily mean dollar depreciation.

These quibbles should not detract from the book's many achievements. Few people have the knowledge to produce such a tour guide and those who do would probably want to write 1,000 pages, complete with hundreds of footnotes that no one would really ever read. Kenen and Meade have written a concise and highly competent book that makes a fairly large literature easily accessible. The book should be of interest to nonspecialists who want to understand regional monetary integration, and it should find its way to reading lists in upper undergraduate and graduate courses in international monetary economics. Specialists will also appreciate a ready source of information on regional integration issues and the associated literature.

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G Financial Economics

Pop Finance: Investment Clubs and the New Investor Populism. By Brooke Harrington. Princeton and Oxford: Princeton University Press, 2008. Pp. xii, 242. $29.95. ISBN 978–0–691–12832–0. *JEL 2008–0858*

Investment clubs became a pop culture phenomenon in the United States in the mid to late 1990s. The number of clubs registered with the National Association of Investors Corporation grew from about one thousand to six thousand from 1994 to 1998, and then declined steeply following the subsequent meltdown in the technology sector. In an investment club, about ten to fifteen people pool financial resources and choose stocks to invest in. Brooke Harrington surveyed over a thousand investment clubs about their performance, and also spent some time in 1997–98 analyzing seven investment clubs in depth to understand the decision-making processes at each club. This book reports the results of her research.

The performance numbers are simply staggering. From 1986 to 1997, the clubs that responded to the survey on average reported a return below that of the S&P 500 index by 20 percent *per year* in terms of arithmetic means (and over 10 percent per year in terms of geometric means). There is some evidence that mixed gender clubs outperform same-gender clubs, but even mixed gender clubs report spectacular underperformance. It's hard to imagine such a degree of ineptitude, and an immediate thought is that Malkiel's *A Random Walk Down Wall Street* should be required reading for all investors. With regard to investment clubs, questions that arise include: why do they continue to exist, and can they nevertheless offer a benefit to small investors?

Given their performance, investment clubs can hardly be an important part of their members' retirement plans. In addition, the amounts at stake seem to be low. For the seven clubs studied in depth, contribution amounts in the range of $35 per month to $150 per are reported. Early in the book, there is a mention of "Cate," who has a net wealth of a million dollars but only about $4,500 invested through an investment club. Later, an anecdote is presented on how one of the clubs took nine months before deciding to buy twenty shares of Cisco, with many other anecdotes also involving less than a hundred shares of a stock. Overall, in terms of a direct impact on either the wealth of their members or on asset prices in general, the clubs appear to be inconsequential.

Instead, the clubs appear to serve a social purpose: as Harrington points out, Americans may be bowling alone but they are not investing alone. The most interesting parts of the book describe the dynamics of club meetings. Across the clubs studied in depth, about half the time at meetings is spent on matters other than investment, such as personal lives and social events. At least at one club, members recognize the failure of the group as investors and yet enjoy their friendship and continued meetings. A phone survey of twenty-four clubs that disbanded suggests that break-ups occur more for logistical reasons than performance-related ones.

On the process via which the various clubs decide to buy or sell particular stocks, Harrington makes a compelling case that the members have identities as investors, and both the process and the stocks they choose reflect their own perceptions of those identities. The idea of a narrative or story that ties in to members' identities seems to be important in the discussions at different clubs. Readers interested in behavioral finance will take particular note of the sectoral specialization across gender that Harrington documents. Women are drawn to consumer products stocks, especially those they have a personal familiarity with. Even for stocks outside the consumer products sector, they often evaluate the stocks in terms of their own response to the products being sold. Men appear to be drawn to more industrial sectors such as semiconductors and chemicals. A natural corollary is that mixed gender clubs should have a more diversified portfolio, and one wonders whether diversification in part explains their superior performance (compared to same-gender clubs).

Can the clubs serves a broader purpose? It appears that the process of presenting proposals to buy or sell stocks involves some level of fundamental analysis and making forecasts of future earnings. Thus, the educational role played by the clubs can perhaps be useful in overcoming the fixed costs that people must incur before investing in the stock market. The chapter on follow-up interviews with club members conducted in 2004 bears this out, with members commenting that the experience was empowering and the education was valuable. Perhaps "wiser" must inevitably be accompanied by "sadder," but members also comment in the later interviews on their own delusions during the bull market of the 1990s. Stock market participation across the world remains relatively low and investment clubs can play an important role in increasing participation. Nevertheless, it is disappointing that it takes some investors so long to grasp the obvious benefits of diversification.

The overall message that emerges is that investment clubs have been an important social phenomenon but as yet have not had a broad impact on the stock market. Readers who are interested in group decision making will certainly enjoy the book. Those interested in behavioral finance will find some nuggets related to gender differences, but one does not get a sense of broader patterns of departures from rationality that may explain known phenomena such as, for example, the disposition effect. And some of us (including this reviewer) who believe that utility maximization, in fact, provides a reasonable description of the world will view investment clubs as a consumption good for their members, with their investment losses being compensated for by the utility derived from belonging to a group.

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Plight of the Fortune Tellers: Why We Need to Manage Financial Risk Differently. By Riccardo Rebonato. Princeton and Oxford: Princeton University Press, 2007. Pp. xxvi, 272. $35.00. ISBN 978–0–691–13361–4.

JEL 2008–0103

In view of the turbulences in financial markets that started in 2007, this book is a fascinating read. Riccardo Rebonato is a distinguished practitioner, head of market risk and quantitative research with the Royal Bank of Scotland. He has also published many technical papers and books on derivatives. As such, he has the combination of technical skills and experience that makes him uniquely qualified to evaluate the practice of financial risk management.

This book should appeal to a wide audience. Rebonato writes in an engaging prose that is free of mathematics, yet intellectually rigorous. He provides a top-level view of risk management, founded on real-world situations. Although Rebonato does criticize some of the current risk management practices, he also understands the need to propose constructive alternatives. It would be too easy to fall into a trap of nihilism. Indeed, institutions are in the business of managing risk. The last chapter proposes useful practical guidelines for risk managers.

Perhaps the most striking quote in the book is from another risk manager, who was slowly realizing that the estimates of economic capital he was computing for his bank were unreliable: "How do I tell them?" (p. 137).

This observation points to a major failure in the risk management profession, which is the impression of accuracy in risk measures, especially when estimated at high confidence levels. *Economic capital* is the amount of capital an institution would voluntarily set aside to support its business activities. This is typically estimated as a *Value-at-Risk* (VAR) measure derived from the distribution of profits and losses at a very high confidence level such as 99.97 percent over a year.

The measurement of economic capital is a recent development, which has its roots in the VAR framework developed for trading portfolios in the early 1990s. Initially, VAR was typically measured over short horizons—daily—and with fairly low confidence levels such as 95 percent or 99 percent. VAR quickly gained recognition because it provides a single summary measure for the distribution of profits and losses as a dollar number that is easy to understand. The main benefit of this approach, however, is that it requires institutions to build up their risk profile of their portfolio from current positions and recent historical data. VAR is simply a summary statistic for the entire distribution, which should be examined as well. This framework also makes it easy for the risk manager to evaluate the effect of various scenarios.

In his book, Rebonato generally condones the typical VAR market-risk framework, which best corresponds to "frequentist" attributes: (1) the horizon is short, (2) a lot of data are available, (3) the confidence level is not too high, and (4) the process is generally stable.

In contrast, Rebonato strongly disapproves of blind extensions of this framework to the economic capital framework, which has opposite attributes: (1) the horizon is long, typically one year and as a result (2) not many data points are available, especially (3) to estimate quantiles at very high confidence levels, and (4) when subject to economic cycles of five to ten years. The author advocates instead reliance on Bayesian methods, which take into account prior information culled from our wealth of knowledge.

I agree wholly with these conclusions. Deutsche Bank, for example, one of the leaders in risk management, reported that its economic capital as of year-end 2007 was 13,611 million euros, using a 99.98 percent confidence level and annual horizon. This number covers market risks, credit risks, operational risks, and their interactions. In my view, it makes little sense to report five significant digits. This practice gives the erroneous impression of a very high degree of precision, which is not the case. In my opinion, this economic capital number is probably measured within a range of several billion euros, at best.

Rebonato's book demonstrates this point. He explains that frequentist methods are ill-suited to these measures. He also argues that humans have developed reasonably good heuristics for dealing with medium-probability events but certainly not for very infrequent events. In other words, we have a good intuition of a potential daily loss at the 95 percent confidence level, but certainly not at the 99.98 percent confidence level over one year. As a result, we rely on black boxes developed by quantitative risk managers, leading to percentile measures that he describes as "science fiction."

In truth, these observations should be familiar to risk managers who have good understanding of the many pitfalls in their systems. It has been pointed out that, even in the simplest case of a single stationary risk factor distribution, quantiles are estimated with much greater error when the confidence level increases (Philippe Jorion 1996). For large-scale systems, the problems increase exponentially. Approximations are routinely used to map complex instruments on risk factors, which create additional errors in the process. In addition, the entire dependence structure must be specified for the set of multivariate risk factors. After the Long-Term Capital Management failure of 1998, risk managers should routinely stress their models and associated parameters. Yet this message does not seem to get across.

Regulators, however, have maintained a skeptical attitude toward these risk measures. For instance, the capital charge for market risk is based on a 99 percent, one-day VAR times some multiplier, or safety factor. This approach achieves the aim of a high capital requirement while maintaining a low confidence level. The model can also be backtested because it implies two or three worse losses per year. On the other hand, Rebonato criticizes the Basel II rules, which are based on a 99.9 percent quantile at a one-year horizon. In other words, this implies a worse loss in one of a thousand years. The book, however, conveys the impression that banks can use their internal credit portfolio models to measure their regulatory capital, which is not the case. The Basel Committee has expressed reservations about these models, which it called "half-baked," and has imposed a prespecified dependence structure that is presumably more robust.

Overall, Rebonato addresses a major issue in risk management. During the two years since the book was written, it has become clear that blind faith in economic capital numbers has contributed to the current financial crisis. Credit rating agencies, for instance, have used simplistic economic capital methodologies to assign credit ratings to tranches of structured products. The subprime crisis has demonstrated the flaws in these ratings. Rebonato states that "the rating agencies do not say that a AA credit rating means that the probability of default over a year of a AA firm is 0.03 percent" (p. 201), but this is in fact the basic methodology for rating structured products.

Likewise, the insurance giant AIG relied heavily on its "economic capital modeling initiative" to justify its foray into financial products. Up to 2007, it maintained that it had $15 billion in excess of its economic capital. The firm was massively short credit default options, however, and eventually required a $170 billion bailout by the U.S. government because its failure would have endangered the stability of the financial system.

This is why this book is important. It should be the next reading for financial engineers and risk managers.

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Why Are There So Many Banking Crises? The Politics and Policy of Bank Regulation. By Jean-Charles Rochet. Princeton and Oxford: Princeton University Press, 2008. Pp. x, 308. $50.00. ISBN 978–0–691–13146–7.

JEL 2008–0452

This collection of published articles by one of the foremost contributors to the field of financial intermediation and its regulation—a field that has come to maturity vis-à-vis its grounding in microeconomic theory over the last quarter century—covers a great deal of ground. These include an exploration of sources of banking crises and their resolution cum prevention via regulatory processes, such as provision of lender of last resort facilities by central banks, prudential supervision and management of systemic risk, and solvency (capital) regulations. He also considers some proposals relating to market discipline as a complement.

The fundamental issues in banking theory pertain to (1) the opacity of individual assets (loans) intermediated by them; (2) large leverage (low capital) ratios justified via diversification across many low-risk investments; and (3) the possibility of coordination failure among bank investors (depositors), as panics in a pure sense or in response to noisy signals pertaining to a bank's solvency. Such failures, as well as excessive ex ante risk-taking by banks, give rise to a real possibility of their unravelling, leading to distress sales of loans about which the originating bank may have asymmetric information, as well as possibilities of a contagion (systemic risk) across other banks. Minimum capital regulations, and closure rules cum prudential supervision, provide buffers against these risks. In empirical work, costs of systemic banking crises appear large—15 to 20 percent of an affected country's annual gross domestic product (GDP), conservatively. In a significant set of cases, such crises are triggered (preceded by) by real shocks to terms of trade or by the collapse of domestic asset price bubbles, hence defaults.

After discussion of basic themes, as well as examples of recent financial crises in chapter 1, Rochet with his coauthor Xavier Vives provide, in chapter 2, an analysis of a second generation model of potential runs when bank depositors condition their actions (withdrawal choices) on both noisy imperfectly correlated signals about solvency as well their anticipation of the choices of other informed agents. They show that a lender of last resort who is well informed about the fundamentals of a bank can ameliorate the costs arising from such coordination failure and complement (at lower cost) ex ante capital and liquidity constraints.

In chapter 3, coauthored with Xavier Freixas and Bruno Parigi, Rochet provides some new perspectives on the lender of last resort function, as well as its institutional requirements, in a modern context of (partial) liquidity provision developed interbank loan cum Repo markets. They do so in an environment in which markets and regulators suffer from having limited interim information on the nature of shocks—relating to solvency or depositors' liquidity demand—that a bank is subject to. They also take into account the possibility of (two types of) moral hazard in banks: vis-à-vis ex ante screening and ex post monitoring of the loans they make. They show that the appropriate roles of a private interbank market and a public LLR depend on the nature of the moral hazard involved.

In chapter 4, Rochet introduces more notions and trade-offs pertaining to macroeconomic dimensions of banking cum financial crises, which are then expanded on in chapters 5 and 6, both coauthored with Jean Tirole. Building on earlier work by Holmstrom and Tirole on the role of adequate bank capital for provision of monitoring incentives, he shows that a purely private reaction to macro shocks leads to too many closures in bad times, which can be improved on by (tax-financed) aggregate liquidity injections. However, in the absence of some precommitment by authorities to limit such injections, banks over-expand loans. Chapter 5 considers more explicitly a role of interbank monitoring in their loan markets. In chapter 6, lessons are drawn regarding differing payment systems.

The themes introduced by Rochet and Tirole in chapter 6 are expanded on in chapter 7, coauthored again with Freixas and Parigi. In it, these authors develop a very detailed taxonomy of what are, in essence, differing networks of interbank flows, of liquidity as well as shocks to solvency affecting other banks. They provide detailed conditions under which contagion or systemic risk would arise from the liquidation of a subset of insolvent banks to others inherently not.

Chapters 8 and 9 are devoted to the theme of bank capital regulation, as well as more integrative approaches such as that of Basel II, in which it is one of three main "pillars" of a regulatory framework. Chapter 8 is based on an older paper by Rochet, in which the main impact of capital regulation arises via its effect on banks' risk–return trade-offs in a mean–variance portfolio choice setting, rather than say through its loan monitoring incentives or by ameliorating the likelihood of bank runs based on coordination failure. In chapter 9, Rochet uses a dynamic intertemporal model in continuous time, and uncertainty, to flesh out the complementary roles of ex ante capital constraints, regulatory closure rules, and market discipline provided by junior debt claims in the regulatory process.

Overall, this book provides a commendable, indeed magisterial, synthesis of a vast set of issues and modeling tools germane to a micro-foundations based framework for analyzing financial crises and instability and its regulation in the overall societal interest of preserving as best as possible the beneficial unctions financial intermediation in the capital allocation processes of a modern economy. It ought to be a required reading for policy analysts and research students alike.

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H Public Economics

The Gender Impact of Social Security Reform. By Estelle James, Alejandra Cox Edwards, and Rebeca Wong. Chicago and London: University of Chicago Press, 2008. Pp. 284. $35.00. ISBN 978–0–226–39200–4. *JEL 2008–1303*

Pension systems are being modified in many parts of the world. The growing financial pressure of aging populations makes policies that strengthen the link between individual contributions and benefits particularly important. Indeed, many countries have replaced a defined benefit system with a defined contribution one, since aging populations may make defined benefit pay-as-you-go schemes economically unstable. Defined contribution systems, on the other hand, are financially stable because no more can be taken out of the system than has been put into it.

Pension systems and their reforms may have different impacts on men and women because of different employment histories and demographic characteristics. Women and men live different lives. Typically, women participate less in the labor market, have lower earnings, and often experience interrupted careers. Besides, they have longer life expectancy and are more likely to become widows than men are to become widowers. *The Gender Impact of Social Security Reform,* by Estelle James, Alejandra Cox Edwards, and Rebeca Wong, examines the design of pension systems from a gender perspective. The book describes different ways of organizing public pension systems and gives the pros and cons of different features with respect to gender. Is a particular design more likely to favor women than another design? And in what respects? The authors compare the outcome for men and women in Chile, Argentina, and Mexico and present empirical findings from the transition economies of eastern and central Europe as well as some OECD countries.

A considerable literature has examined women's pension benefits, but the focus has been on the distribution of benefits and poverty relief. Very few studies have looked at incentives and potential behavioral changes, lifetime benefits and replacement rates for men and women, which gender receives net redistributions (lifetime benefits minus lifetime contributions or taxes), and which gender pays for these redistributions. A pension system with positive work incentives may lead to higher female labor force participation and thereby to higher pensions for women. Annual benefits will produce different results compared to lifetime benefits because women live longer and are often permitted to retire earlier than men. Looking at redistributions gives us yet another perspective. Women may receive net transfers even though their annual benefits are lower than those of men. The ratio between lifetime benefits and lifetime contributions and taxes, a measure of redistribution, may be much higher for women than for men. So far these additional issues have not attracted much attention either in the scientific literature of pensions and their reforms or in the general pension debate. It is therefore particularly valuable that an extended and comprehensive analysis is now available. The James, Edwards, and Wong volume provides, in addition to careful country studies, a thorough and detailed analysis of how men and women fare under different pension. Pension systems are described by their design features and the effects on work incentives, pension benefits, and income distribution for men and women are analyzed.

The authors describe how certain rules either favor or disfavor women, while others may both favor and disfavor them. A minimum pension guarantee or a means-tested or flat benefit relieves poverty in old age but there will be incentives, mainly in the lower part of the income scale, not to supply labor to the market. The link between benefits and contributions may be more or less tight. If benefits are based on final salaries, men are favored as they typically have steeper age–earnings profiles. They get a larger pension relative to their lifetime wages and contributions. As life expectancy is determined not only by gender but by a complex mixture of education, employment, income, life style, etc., wealthy people are likely to live longer than poor people. Unisex life tables (which apply the average mortality of men plus women to both genders) might therefore disadvantage a woman with low life expectancy due to socioeconomic status, as well as the average man. Joint annuities offer no support for the rising number of divorcees and single parents.

The net effects of different design features are finally an empirical issue. In order to quantify the effects of a pension design on the outcome for men and women, James, Edwards, and Wong provide estimates of the outcome in three Latin American countries: Chile, Argentina, and Mexico. These countries have recently reformed their pension systems, with a clear trend toward tightening the link between contributions and benefits in order to secure the long-term sustainability of pensions. The traditional government-run, pay-as-you-go, defined benefit scheme has been replaced with a new multipillar system that includes a mandatory defined contribution plan—individual accounts that are fully funded and privately managed—and a public benefit that provides a safety net.

Pension benefits in the old systems were determined by the wages or salaries received during the final working years together with the number of years worked. Women could retire earlier than men with no actuarial penalty. Married women received a tax-financed widow's pension. In Chile and Argentina, they had to relinquish their own pensions while receiving a widow's benefit, but in the new systems women can keep both pensions, which encourages market work. In the new systems, married men pay for a joint annuity, which is an important feature of new pension systems in Latin America. Benefits were not automatically indexed for inflation in the old systems, while indexation for inflation is common in the new systems.

Because none of the new systems has been in effect long enough to mature, James, Edwards, and Wong construct representative men and women using household survey data on current behavior at different ages, education levels, and marital status. They simulate earnings histories and calculate benefit amounts, replacement rates, and the ratios between expected lifetime benefits and lifetime contributions or taxes in the new and the old systems. There are three groups of "typical" women—the average woman, the full-career woman, and the ten-year woman (who participates in the labor force for ten years early in life, before having children). Five education levels are represented. They then calculate how the typical woman fares compared to the full-career man under the rules of the old and new systems. Comparisons of the new and old pension systems are problematic as the old system was unable to disburse future benefits and, therefore, could not be sustained. We do not know what adjustments may have been made to make the old system solvent. To avoid this problem, James, Edwards, and Wong calculate relative changes in the position of men and women in different groups. The impact of the public system on factor income is left out of the simulations, which may be acceptable, especially since the secondary effects are difficult to determine.

As may be expected when it comes to earnings-related and defined contribution plans, the calculations show that women's pensions are about 30–40 percent lower than men's pensions due to women's lower participation in the labor market and, therefore, less pension contributions, but also to their lower retirement age. The calculations show that the annual pension for women in Chile and Argentina would be increased by nearly 50 percent if the age of normal retirement were raised from 60 to 65. The pension gap is reduced, however, partly because of the public pension benefit directed at low-wage earners and partly because of intrahousehold transfers from husband to wife in the form of joint annuities. Women gained from the pension reform in comparison to men despite the fact that many rules in the old system already benefited women—for example, in Argentina a high level of compensation for only ten years of work, a minimum pension, and an early retirement age. Female–male ratios of lifetime benefits in the new systems exceed those in the old systems in all three countries, with intrahousehold transfers from husband to wife in the form of joint annuities playing the largest role. Moreover, the ratio of lifetime benefits to lifetime costs or the rate of return on costs is much higher for women than for men; that is, redistribution goes from men to women. Women in the lower education groups receive the highest postreform/prereform ratios because of the equalizing role of the public benefit.

The new systems contain incentives that encourage women to work outside the home. In particular, Mexico's flat benefit encourages work in the formal sector. It is a flat benefit per day worked rather than per worker, regardless of days worked, and thereby directed to those who have low incomes due to low hourly wages, not a low level of participation in the work force.

The theoretical arguments and the results from the simulation model show that crucial features in a pension system with reference to gender aspects are (1) that rules of the system should not penalize or discourage women's work in the labor market, (2) a strong safety net that protects low earners and thereby women, and (3) joint annuities or unisex life tables, indexed pensions and pensions paid in the form of annuities, which are of particular significance for women because of their longer life expectancy.

Why should we subsidize women's pensions? Why provide a tax-financed widows' benefit? A normative discussion accompanies the positive analysis. Women's work at home raises men's market income. The authors argue that this should be a reason for income distribution within marriage. Joint annuities or splitting contributions between the two spouses either continuously or upon divorce can be thought of as an enforcement of the implicit contract between husband and wife. On the contrary, the tax-financed widow's pension is redistribution in favor of couples. Single men and women subsidize couples and two-career couples subsidize one-career couples who get the same benefit from only one contributing member.

The comparisons between the old pay-as-you-go defined benefit plans and the new multipillar systems provide a basis for making decisions about pension reforms, but the fact that the choice of a pay-as-you-go or funded scheme does not have any gender effect on incentives and redistribution by itself should have been made clear. Otherwise the reader might believe that funded pensions are of significant importance for the gender outcome. The gender effect depends on the link between an individual's contributions and benefits; that is, whether the pension schemes are organized as defined benefit plans or as defined contribution plans. Both defined benefit and defined contribution plans may be either pay-as-you-go or funded schemes. Most commonly, defined benefit schemes have been pay-as-you-go while defined contribution plans have been funded. However, recent pension reforms in Poland, Latvia, and Sweden have introduced a pay-as-you-go defined contribution plan, specifically the notional defined contribution plan. A recurring criticism of pension schemes financed on a pay-as-you-go basis is that they allow politicians to promise pension benefits that have little likelihood of being delivered because the costs become apparent after a considerable period of time. Advocates of privately funded pension schemes argue that this pension promise is more secure. Still, if the value of the fund falls because of a collapse in the value of the assets in which it is invested, contributors will end up with pensions lower than they expect. The historical evidence and the collapse in stock market values in recent years show that this is a distinct possibility.

In a familialist welfare state, the emphasis is on derived rights through the family. Joint annuities, which are an important feature of new pension systems in Latin America, may ensure certain redistribution within the family and reduce the differences in pensions between men and women. However, benefits are dependent on marriage and offer no support for the rising number of divorcees and single parents. Another objection not raised in James, Edwards, and Wong is that derived rights may also act as work disincentives by encouraging reliance on family income.

Indexation before and after retirement is crucial for both men and women. Women as a group gain more from indexation of annuities since it benefits long-living individuals in particular. If real wage growth is positive, it is especially important for women to have a system with wage indexation. Because they have a longer retirement period, price indexation makes them fall behind the standard of living of the working population to a greater extent than men. But wage indexation is much more expensive than price indexation. James, Edwards, and Wong suggest wage indexing the public benefit for successive cohorts but price indexing during the retirement period of each pensioner. It is a good thought that pensioners should enjoy a higher standard of living as a result of ongoing productivity increases, but this may turn out to be excessively costly in a situation with an aging (and even declining) population.

The James, Edwards, and Wong volume provides valuable insights into the gender dimensions of alternative designs for reform. Detailed design features matter. Understanding different ways of organizing public pension systems and their consequences is of great interest not only to policymakers but also to students of economics, sociology, social work, and other social sciences.

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I Health, Education, and Welfare

Spin Cycle: How Research Is Used in Policy Debates: The Case of Charter Schools. By Jeffrey R. Henig. New York: Russell Sage Foundation; New York: Century Foundation Press, 2008. Pp. ix, 297. $32.50. ISBN 978–0–87154–339–4. *JEL 2008–0904*

Research on school choice has been contentious. As evidence, consider the vigorous and even nasty exchanges between John Witte and Paul E. Peterson, Caroline M. Hoxby and Jesse Rothstein, and Paul E. Peterson and Alan B. Kreuger that have played out on the pages of preeminent journals (Bob Davis 1996; Hoxby 2007; Krueger and Pei Zhu 2004a, 2004b; Peterson and William G. Howell 2004; Rothstein 2007). Consider also the episode that serves as the launching point for Jeff Henig's recent volume, *Spin Cycle.* In August of 2004, a charter school advocacy group paid more than $115,000 for space in the *New York Times* to chastise its editors for coverage of a charter school study that did not meet professional research standards. The ad was signed by twenty-four prominent social scientists and, in its wake, several new studies were quickly disseminated by scholars anxious to make sure those listening were not fooled by the shoddy research of others (Martin Carnoy et al. 2005; Hoxby 2004; F. Howard Nelson and Tiffany Miller 2004; Joydeep Roy and Lawrence Mishel 2005).

Of course, rancor between scholars is nothing new and, when it becomes petty and personal, the affect for most onlookers is comic. For Henig, however, the highly polarized way in which researchers have engaged the public debate on school choice is bad for democracy. Research on the consequences of social interventions, Henig believes, has much to offer the public and policymakers as they struggle with important issues. And Henig devotes an entire chapter to making the case that the research on charter schools is no exception. When the researchers that the public hears, however, obstinately refuse to find common ground, overstate the strength of their evidence, and challenge each other's motivations and competency, they reinforce the notion that social science research is just another form of partisan rhetoric. In the process, researchers and scholars lose their privileged standing as experts, leaving ideological simplifications to shape opinions and guide decisions.

In *Spin Cycle,* Henig takes on the task of understanding why researchers have engaged the public debate on school choice in such a combative way. Through a deft combination of interviews, surveys, and content analysis, he examines the roles played by partisan strategists and advocacy groups, research funders, the media, and the researchers themselves. The result is a balanced and insightful investigation.

In Henig's account, the most active culprits are conservative political strategists. As part of an effort to move political dynamics in the United States toward antigovernment policies, conservative think tanks have successfully framed questions about school choice along the fundamental ideological divide of markets versus governments. In response, members of the Democratic coalition who have taken positions in these ideological battles have felt compelled to attack claims made by charter school advocates, and to enlist research findings where convenient. The role of research funders and the mainstream media has been more passive. Decisions to fund research made by government agencies and most large foundations do not appear to be ideologically driven and, although the ideological stances of editorial boards at major newspapers clearly influence the allocation of op-ed space, they do not appear to infiltrate news reporting on charter schools or charter school research. Nonetheless, by pushing researchers to formulate simplistic policy stances, by relying heavily on a small group a strong voices who tend to represent extremes, and by failing to support sustained, nuanced examination of educational policy issues, foundations and news agencies have failed to shape a more informative discourse.

As for researchers themselves, at least some, in efforts to make their work more relevant, have been too willing to give those involved in the difficult business of influencing policy what they want—fast, simple, and confident conclusions. Henig argues that researchers should keep in mind that their most important contributions to policy formation are long term and lie more in getting people to recognize uncertainty than in providing immediate answers.

Following his diagnosis of the problem, Henig offers recommendations for improving the contributions of research to policy making. Most significant among these is a call to maintain commitment to the institutional norms that help distinguish scholarly research from partisan advocacy. Most important, for Henig, is the peer review process, but also standards of tenure and promotion that weight long-term contributions over short-term popularity, conceptions of research as a collective and cumulative process, and authoritative signals of quality each help distinguish the scholarly endeavor. Henig's vision of a more insulated form of policy research presents a dilemma for the researcher who shares his view. Going slow, articulating nuance, and highlighting uncertainty will help preserve the value that policy research provides. As the very case Henig has chosen to examine demonstrates, however, researchers who take such an approach are likely to be drowned out by researchers inside and outside of academia who are willing to provide the more timely and decisive conclusions that foundations, reporters, and advocates demand.

For social science researchers anxious to have their work inform public policy, this book encourages much needed reflection on exactly what we have to offer and provides a lesson in how easily our role can be perverted. Outside a circle of friends doing school choice research, however, I am not sure to whom I would recommend this book. There seems to me too much on the small world of school choice research to ask colleagues in other fields to wade through. And although I think there may be important lessons for university administrators and foundation officials about the role of policy research in contemporary democracy, it is not clear which lessons are specific to school choice research and which ones are more general. So, precisely because I think Henig has written an insightful book, I wish it were not so focused on a specific fight among a small group of charter school researchers.

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Improving School-to-Work Transitions. Edited by David Neumark. New York: Russell Sage Foundation, 2007. Pp. viii, 294. $35.00. ISBN 978–0–87154–642–5. *JEL 2007–1358*

As described by James J. Heckman (1994), there was in 1992 a "new consensus" that the United States needed to prevent non-college bound youth floundering for years before finally settling into stable jobs. This consensus gave rise to the School-to-Work Opportunities Act, passed in 1994, which provided federal funding to support state school-to-work programs such as mentoring, internships and apprenticeships. In 1999, after its initial five years, the Act was not reauthorized and the school-to-work movement began to fade from view. In its place came the school accountability movement, which emphasized classroom- and test-based approaches to learning.

In the first chapter of this edited volume, the editor makes the case for why it matters whether these various school-to-work programs were effective. The extent to which readers agree will determine whether they find this book worth buying and reading. In my view, the case is compelling: whether as a first or last resort, policymakers are likely to use work-based programs to reach students that cannot be reached by conventional classroom-based approaches. As a summary of recent work-based developments and an important contribution to the related empirical evidence, this volume will be a valuable resource for social scientists working in the field. It will also be a useful guide to policymakers looking for information and evidence on recent initiatives.

That said, the thread running through the volume is not as strong as it could be. In part this is because the introductory chapter appears to define the problems that school-to-work programs are designed to solve somewhat narrowly, as school-to-work transitions characterized by joblessness and job instability. This may be a part of the problem, but this narrowness makes it hard to interpret some of the evidence presented later in the volume. For example, chapter 2 of the book, introduced as providing descriptive evidence on school-to-work transitions in recent years, presents no facts on job stability. I wonder why school-to-work initiatives cannot be defined more simply, as programs intended to improve students' long-run labor market performance. Whether they do is an empirical question; whether programs are especially effective for certain types of students is a second empirical question; whether any benefits can be attributed to impacts on early career job stability or, for example, motivation in high school is a third empirical question.

Even viewed through this broader lens, three chapters (2, 7, and 8) are interesting but do not seem to address the core questions that the volume sets out to answer. Chapter 2 uses Census and National Education Longitudinal Study data to describe differences in educational attainment, employment rates and teenage motherhood across races. Readers will know that a large fraction of U.S. youth look bad on these outcomes and a summary would probably suffice. To the extent that this volume is the right place to develop these facts, it seems strange to focus on race while treating other dimensions (geography, parental education, and so on) as variables to be controlled for. Time and space devoted to this analysis would have been more usefully spent analyzing the costs of these programs. This is especially important if they are to be viewed as an alternative to classroom- and test-based approaches to learning (i.e., school accountability), which cost relatively little (Caroline M. Hoxby 2002).

Chapter 7 is a descriptive analysis of links between two-year college faculty and the local labor market. The introductory chapter places this chapter in the context of the lower-profile but important college-to-work problem, but this connection does not seem like a strong one: the chapter concerns only one aspect of college-to-work programs and the reader is given neither a broad-brush view of the evidence relating to their effectiveness nor any evidence to suggest that this depends on the strength of these faculty links. Chapter 8 analyzes the skill requirements of low-skilled entry-level jobs using the survey responses of around four hundred San Francisco Bay Area employers that offer them. The jobs are found to require basic math, reading and writing skills, communication skills, and problem-solving skills; the author concludes that work-based programs must provide broad skills transferable to a wide range of settings. While I thought this analysis was interesting, the same conclusion could be drawn from the well-established facts about firm and occupational mobility (Robert H. Topel and Michael P. Ward 1992).

Chapters 3 through 6 are the volume's core chapters. These present facts on participation in school-to-work programs and analyses of their effectiveness. These chapters are well written, make careful use of data, and are appropriately cautious in drawing conclusions from what are, necessarily, observational analyses based on fairly small samples. They do not quite form a coherent whole however, with chapters occasionally overlapping and occasionally presenting contradictory findings.

Chapter 3 uses the 1997 National Longitudinal Survey of Young People to assess participation in school-to-work programs in the late 1990s. The chapter's conclusion, that race is the defining characteristic of these participants, is interesting and important, but it would be useful to know whether this is because certain types of schools offer these programs or because certain types of students enroll in them. More troubling is the tension between this conclusion and the findings presented in chapter 4. While that chapter seeks to analyze the effectiveness of these programs, it begins with a brief analysis of program participation, and finds that neither race nor any other variables are consistently linked to participation. These chapters use the same data and it is not clear why they come to different conclusions.

The remainder of chapter 4 analyzes the effectiveness of these various programs, measured in terms of educational and labor market outcomes. This analysis is fascinating, and this is perhaps the strongest chapter in the volume. The obvious worries are that affected cohorts are still young and that causal effects cannot be identified by comparing outcomes among students that did and did not participate in these programs, even after adjusting for a range of background characteristics. I was reasonably convinced that the authors' estimates are not driven by selection biases and even more convinced the authors have done all they can to address these. As such, I look forward to the authors' promised analysis of the medium-run effects of these programs. My only wish for this chapter is that it was longer. This analysis feels like the heart of the volume and it could have been spread over at least two chapters. To force it into one, readers are referred to an earlier paper for much of the core material and most of the remaining space is given up to an analysis of program effects according to the predicted probability of attending college. While this seems like a natural and interesting dimension on which to look for heterogeneous effects, it would be nice to see these in the light of a more comprehensive discussion of average effects.

Chapter 6 is another strong chapter. This provides a clear and careful evaluation of the effects of National Academy Foundation Career Academies, "schools within schools" that combine academic courses with technical classes and employer partnerships related to the Academy's theme (in the analysis sample, either Finance or Travel and Tourism). Although all outcomes are measured in high school, these outcomes are wide-ranging and include attendance, GPA, course-taking, and college preparation activities. Estimates are relatively imprecise, due in part to small samples, but the authors find that Career Academies improve student engagement, increase college-level course-taking, and increase the probability of being admitted to a four-year college.

It is not clear whether these results could be achieved if the Career Academies model was only partially implemented. This question provides the main rationale for chapter 5, which investigates whether Career Academy effects vary with the degree of implementation. To assess this hypothesis, the authors classify academies within seven schools (twenty-two school–cohort combinations) along three dimensions of implementation (course-taking, course purity, and course coverage) and assess whether these determine if, within schools, academy students outperform nonacademy students. The results reveal no consistent academy main effects and no consistent associations between academy effects and implementation measures.

Again, while this analysis is interesting, one worries that the heterogeneous effect cart is being put before the average effect horse. I suspect readers would prefer to see more convincing analyses of average treatment effects for particular programs or, at a minimum, a more extensive discussion of the analyses conducted by others. In this case, it would be especially helpful to know more about the MDRC Career Academy evaluation referred to in both chapters 5 and 6. This was based on a randomized trial and chapter 5 notes that the MDRC evaluation showed that treated students earned around 10 percent more than control students (four years after high school graduation). As another example of chapter overlap and contradiction, chapter 6 notes that the same study found no effects on standardized achievement scores or initial postsecondary outcomes. Even if these different interpretations can be reconciled, it would be useful to come away from such a volume with a clear understanding of the findings of what appears to be the only randomized trial of an important school-to-work program.

Overall, this book is an important and timely reminder that the current classroom- and test-based approach to learning may not be the most effective one for high school students, particularly those that will transition directly from school to work. For readers interested in these programs, the book provides a useful summary of recent initiatives and careful analyses of their effectiveness.

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Color and Money: How Rich White Kids Are Winning the War over College Affirmative Action. By Peter Schmidt. Houndmills, U.K. and New York: Palgrave Macmillan, 2007. Pp. viii, 263. $24.95. ISBN 978–1–4039–7601–7.
JEL 2007–1364

The title of Peter Schmidt's book, *Color and Money: How Rich White Kids are Winning the War against Affirmative Action*, conveys his thesis: selective college and university admissions favor affluent majority students and less affluent students, majority and minority, are "losing out" in the process. Schmidt, a senior reporter at the *Chronicle of Higher Education*, attributes this effect largely to the presence of race-conscious admissions and other policies at America's most selective colleges and universities. Schmidt argues that Caucasian lower-income students are being shut out of these institutions, raising questions of social justice and fairness.

Schmidt's book covers two main topics—an analysis of selective higher education's admissions processes (remember that such institutions produce only a relatively small fraction of all American graduates but are widely considered to provide not only superb educational experiences but also stimulate powerful social networks) and a narrative of the University of Michigan cases culminating in the 2003 Supreme Court decision. The Court decisions in *Grutter v. Bollinger* and *Gratz v. Bollinger* upheld the constitutionality of race-sensitive admissions but outlawed the "mechanistic" consideration of race and ethnicity. Thus, in the two cases, the Court majority found student body diversity to serve as a compelling interest but split on the specific question of whether the admissions processes at the law school and undergraduate liberal arts and sciences school passed the "narrow tailoring" requirement. As a participant in that litigation (I served as vice president and general counsel of the University of Michigan during that period), I am mentioned at several points in the book. I also have served as president since 2007 of a selective private liberal arts college, Oberlin College, which joined in the amicus curiae (friend of the court) effort in support of the University of Michigan's policies.

With this disclosure, I can report that I found Schmidt's book compelling, well written, and persuasive in parts. His coverage of the University of Michigan litigation was generally accurate, although his emphasis may be questioned. For example, in discussing the admissions processes at the undergraduate level and at the law school, Schmidt might have focused more on the extremely competitive nature of the processes, recognizing that many highly qualified students simply cannot be admitted unless the university was to expand its student body significantly. Admissions processes at virtually all selective American colleges and universities have in common an expanding—and increasingly competitive—applicant pool as well as a desire to craft a class of individuals (as opposed to a rank ordering based on numeric or other credentials). As Scott E. Page's book, *The Difference*, asserts, this concept of producing dynamic groups derives from social scientific observations about excellence and productivity.

Schmidt notes the important role the amici played in influencing the Court. In particular, he mentions influence of the amicus brief signed by twenty-nine former high-ranking military leaders and others (including General Norman Schwarzkopf). This brief pointed out that, to eradicate the racial tensions in the armed services exposed by the Vietnam War, the military academies deliberately created admissions policies considering racial and ethnic diversity strikingly similar to those practiced at other elite educational institutions in America. This brief commanded significant attention in the oral argument and made the position of the U.S. solicitor general much more difficult. As Schmidt's book aptly details, the Justice Department brief tried to walk a fine line by arguing that race-neutral alternatives obviated the need for any race-conscious admissions policies. Yet the presence of race conscious policies at the academies undercut the government argument.

Schmidt also dismisses much of the social scientific research supporting the benefits of student body diversity. Schmidt asserts that "Unable to come up with solid evidence to back its claim that affirmative action yielded educational benefits, the higher education establishment settled on an alternate plan: It would make such assertions anyway, and use spin, exaggeration, and a false sense of certainty in its assertions to pull the wool over the justices' eyes" (p. 162). Although this statement certainly grabs the reader's attention, the book fails to support such a claim. It is true that the University of Michigan and its supporters introduced evidence to provide empirical support for the proposition that Justice Powell's plurality opinion in the Court's 1978 Bakke decision assumed that diversity of geographic, socioeconomic, racial and ethnic, and intellectual identity benefited educational institutions and served classic American ideals. This evidence included significant expert witness reports as well as testimony from highly regarded educators and educational experts including Derek Bok and William G. Bowen. In describing the court cases, Schmidt admits that to the extent there were critiques of the social scientific research, these occurred outside the court proceedings and therefore were not questioned. This may have been a tactical decision by the plaintiffs' attorneys, as Schmidt suggests on page 171.

However, the vast body of social scientific research at the time of the litigation, as well as afterward, has underscored the educational importance of diverse student bodies. Mitchell J. Chang et al. (2006) find that cross-racial interaction increases self-confidence and cognitive development among college students. Anthony Lising Antonio (2001) provides evidence that casual racial interaction expands students' leadership skills and cultural knowledge, even when compared to more formal activities such as cultural awareness workshops. Thomas F. Nelson Laird (2005) finds that students with more diversity experiences, such as diversity-related coursework and positive interactions within diverse peer groups, tend to score higher on both tests of academic self-confidence, social agency, and critical thinking. Nisha C. Gottfredson et al. (2008) confirms Nelson Laird's results: Gottfredson et al. predicts student outcomes based on instances of informal interactions among diverse peers and diversity related coursework. Like Nelson Laird, Gottfredson et al. find a positive relationship between student outcomes and diversity experiences.

Despite these gaps, Schmidt's narrative is generally accurate. Reporting on these cases for the *Chronicle*, he relies on sources from all sides and does well in conveying much of the drama. Readers interested in other accounts of the litigation might consult relevant chapters in Jeffrey Toobin's *The Nine*, Bloomberg reporter Greg Stohr's account, *A Black and White Case: How Affirmative Action Survived Its Greatest Legal Challenge*, and Barbara A. Perry's *The Michigan Affirmative Action Cases*. My article, *You've Got to Have Friends: Lessons Learned from the Role of the Amici in the University of Michigan Cases*, offers observations drawn from my participation in the litigation process, centering on the role of the amici.

The book's other focus is to describe the state of affirmative action and college admissions at selective colleges and universities in general. Schmidt attacks what he perceives as the inequity of selective college admissions. While one may debate the uniformity of admissions practices across the myriad of public and private institutions, the amici represented in the Michigan cases suggest that in many respects competitive admissions processes have converged, recognizing of the importance of achieving racial and ethnically diverse student bodies.

In *Color and Money*, Schmidt eloquently articulates a plea for broader economic access. Schmidt looks at the income distribution of students in America's most selective colleges and universities and asserts that " . . . poor and working class kids must traverse a daunting obstacle course to get to a selective college while rich kids stroll along a moving sidewalk" (p. 63). The goal of providing greater access is shared by higher education leaders. In the wake of the Michigan cases, both public and private universities have taken steps to ease the path of access for those less well off. On the admissions side, some colleges and universities have made more explicit their interest in finding out about applicants from disadvantaged backgrounds. To recruit and yield more economically disadvantaged students, public universities have instituted programs such as the M Pact at Michigan and the Carolina Covenant. Many public and private institutions have initiated efforts to curb the loan burden of needy students. The recent economic downturn has made the question of affordability all the more pressing for many American families.

Yet, as Schmidt recognizes, college admissions respond to the applicant pool; the eventual student body is affected by the yield and retention efforts. In perhaps the authoritative book on this topic, *Equity and Excellence in American Higher Education*, Bowen, Martin A. Kurzweil, and Eugene M. Tobin capture the complexity of the admissions pool in their discussion of "college preparedness." In their study of nineteen academically selective colleges and universities (including Oberlin), the authors show that the percentage of applicants in the lowest quartile of family incomes who are accepted is not significantly lower than applicants accepted in the higher quartile. The authors conclude that first generation students and lower income students were "heavily underrepresented" in the student bodies at these institutions but the percentages of these students at the various stages (application/admission/yield) do not vary much (p. 98–101). One explanation is that that many lower income students are not applying to selective colleges. Caroline Hoxby and Christopher Avery (2009) find that, in a study of 21,000 high-achieving, low-income students, over 60 percent applied only to relatively noncompetitive public institutions. Hoxby and Avery's work reinforces the notion that lower income students are simply not applying to the elite schools.

Why aren't more low income students applying to the selective colleges and universities? There are many possible explanations. As Bowen, Kurzweil, and Tobin discuss, elevating the level of college prep may be crucial—improving public high schools, and prioritizing counseling and mentoring (such as with test-taking skills) within public high schools. There may well be an information gap, both as to the existence of generous financial aid packages and as to the interest of elite schools in recruiting such students. To be fair, some admissions policies may operate to discourage lower income applicants—as Bowen, Kurzweil, and Tobin point out, consideration of legacy admissions, athletic ability, and early decision applications can operate to the disadvantage of lower income, first generation students. Daniel Golden's prize-winning study of legacy admissions, *The Price of Admission: How America's Ruling Class Buys Its Way into Elite Colleges—and Who Gets Left Outside the Gates*, provides a particularly graphic account of the role of privilege.

For students of color, race conscious considerations (where permitted and/or utilized) may provide an advantage. Schmidt's emphasis on race or ethnic considerations is undermined by the evidence showing however, that these considerations affect relatively few admissions decisions at the most selective colleges and universities because the applicant pool of students of color is so small. Bowen and Bok's *The Shape of the River* made this point clearly: in an analysis of five selective universities, Bowen and Bok find that " . . . even if white students filled all the places created by reducing black enrollment, overall white probability of admission would rise by only one and a half percentage points." While eliminating racial or ethnically based affirmative action would have relatively little benefit for majority students, the leaky pipeline for students of color creates the potential for drastic reductions in the admission of racial and ethnic minorities should affirmative action be eliminated. A recent study by the Carnegie Mellon Tepper School of Business provides evidence that a universal ban on affirmative action in higher education could reduce the minority population at highly selective colleges and universities by 35 percent.

Schmidt describes what many believe to be an educational and societal challenge, but the emphasis in his title and throughout his book falls short of providing solutions. In his closing chapter, Schmidt asks about peace and justice, suggesting that a social revolt may be in the making if America's leading colleges do not offer broader access. He is undoubtedly right to advocate for policies that promote access by less affluent students, but these policies can and should be explored along with ongoing efforts to promote racial and ethnically diverse student bodies.

Educators, policymakers, and others disturbed by the picture painted by Schmidt would do well to focus as well on the preadmissions stage. The questions we should be asking are: How do we as a nation promote educational excellence at the primary and secondary levels? How do we encourage those high achieving secondary school students to compete for admission to our best colleges and universities? How do we make higher education affordable at a time when economic pressures are weighing heavily on both public and private institutions? How do we ensure that less advantaged students can succeed at the most competitive college sand universities? Finding solutions to these important questions ultimately should unite, rather than divide, us regardless of race or ethnicity.

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K Law and Economics

The Economic Structure of International Law. By Joel P. Trachtman. Cambridge and London: Harvard University Press, 2008. Pp. xii, 354. $55.00. ISBN 978–0–674–03098–5.

JEL 2009–0160

In *The Economic Structure of International Law,* Joel Trachtman, a Professor of Law at the Fletcher School, sets out to create a social scientific analysis of the structure of international law based on economic principles. In relying on game theory and on transaction cost analysis in the tradition of Coase, Williamson, and North, his analysis is consistent with work in political science over the past quarter-century on international cooperation.

States are interpreted as rational actors seeking to achieve their preferences, which are not necessarily interpreted as purely based on materialistic gain. In other words, the theory assumes utility-maximizing behavior without specifying the content of the values being maximized.

Trachtman's economic analysis of law is liberal because it takes individual preferences as fundamental both methodologically and normatively. In his view, "the only valid source of preferences—of values—is individuals" (p. 1). His analysis differs, however, from a public choice analysis that assumes public officials to be necessarily purely self-interested in ways that make their preferences deviate from those of the public. On the contrary, for Trachtman they may be highly accountable or they may "have been so educated or socialized as to have internalized the community's preferences as their own" (p. 19). Trachtman is also critical of those law and economics scholars who oppose mandatory rules in order to let individuals exercise their own preferences, since he believes that individuals may best express their preferences through government.

Trachtman focuses on the structure of international law rather than on particular sets of rules in different substantive areas. Central to his analysis is the allocation of authority, or jurisdiction. At what level, and to what entities, do states assign authority? The analogy is with property rights among individuals in domestic society. Although one might have thought that law and economics would imply the desirability of clear entitlements, Trachtman argues that if transaction costs are high, "muddy" specifications could be more desirable.

After explaining his commitment to an economic analysis of law (in chapter 1) and to a focus on jurisdiction (in chapter 2), Trachtman devotes the next three chapters—the core of his analysis—to sketching how a jurisdictionally oriented economic analysis of law would explain customary international law (CIL), treaty making, and international organizations. The final two chapters discuss linkage among issues (and therefore, organizations), on the one hand, and adjudication, on the other. These chapters are probably of more interest to legal scholars than to economists or political scientists. Questions of adjudication come down largely to whether adjudication relies, and should rely, principally on rules—specified ex ante—or on standards, which provide general guidance but do not specify required conduct in advance.

With respect to CIL, Trachtman cogently refutes the argument made by Jack L. Goldsmith and Eric A. Posner's 2005 book, *The Limits of International Law,* to the effect that CIL does not affect state behavior. Trachtman identifies the major flaw in the Goldsmith–Posner argument, which is to contrast behavior motivated by self-interest with behavior affected by international law. What Goldsmith and Posner fail to recognize is that "legal obligation and self-interest are not mutually exclusive categories" and that "there is no question that law can affect behavior *through* self-interest" (p. 113, italics in original). In other words, institutions, including legal institutions, can alter incentives and therefore the preferred strategies of self-interested players. Trachtman sketches a verbal model of CIL, using noncooperative game theory, in which CIL provides focal points for players in Prisoners' Dilemma (PD) games: "CIL rules may serve as equilibrium selection devices that provide a greater possibility for a stable equilibrium" (p. 117).

In his analysis of treaty making, Trachtman uses cooperative as opposed to noncooperative game theory. He thereby assumes that treaties represent binding contracts—an assumption that many political scientists, aware of the difficulty of enforcing treaties in world politics, would be reluctant to make. Yet the results of adopting one approach or the other may not differ as profoundly as the difference in assumptions might seem to imply. Using noncooperative game theory, PD games generate a *compliance* problem: players make agreements (which entail relatively low costs under these assumptions), then renege when doing so is more beneficial than compliance. Using cooperative game theory to analyze similar situations generates an *adherence* problem. Facing the choice between joining an enforceable treaty that produces a public good and not doing so, players will often fail to adhere to the treaty in the first place. As a result, there may be fewer treaties with smaller memberships but higher levels of compliance. Nonadherence may be more transparent than noncompliance, but the overall level of cooperation may not be profoundly affected. The core problem—of creating incentives for players to contribute to the production of public goods, through reciprocity, reputational incentives, or by persuasion—remains essentially the same under either set of assumptions, as long as the players are states with autonomous decision-making power who cannot be coerced into joining treaty regimes.

Finally, Trachtman's explanation of international organizations follows the political science literature, also building on Coase and Williamson, which has emphasized the role of transaction costs in affecting whether international institutions are created and the powers delegated to them. The major difference between his work and the earliest work along these lines in political science, during the 1980s, is simply that Trachtman emphasizes formal organizations more than the broader concept of international regimes—complexes of rules, norms, organizations, and decision-making procedures. Even here, Trachtman follows recent trends in political science and law, particularly in the work of Kenneth Abbott and Duncan Snidal. In this chapter, he usefully applies his comparative transaction costs analysis to two important shifts in international organizations: the Single European Act that transformed European Community policies in the 1980s and the Dispute Settlement Understanding that produced a means of binding adjudication in the World Trade Organization during the 1990s.

One of the limitations of Trachtman's analysis, about which he is admirably frank, is that it treats states as unitary actors, not exploring relationships between domestic politics and adherence to, or compliance with, international law. Yet we know that democracies behave differently in international relations from autocracies; that common law and civil law states vary systematically in their ratification of international treaties, and that domestic political coalitions are often crucial in determining how states behave toward international treaties and organizations. During the past twenty years, the political science literature on international cooperation has begun to unpack the "black box" of the state, and it would be useful for the social scientific analysis of international law to follow this trend.

The Economic Structure of International Law could profitably be read in conjunction with Andrew T. Guzman's 2008 volume, *How International Law Works: A Rational Choice Theory*. Trachtman and Guzman share a common ambition: to develop a general theory of how international law affects state behavior, using rational choice theory. They seek to understand why states make agreements, the role of treaties, and each author devotes a chapter to customary international law. The core of each of these books is a refutation of the Goldsmith–Posner view of CIL. Yet in other ways, these two books make different arguments and rely on different strands of literature. Guzman emphasizes more than Trachtman the role of reputation. As he summarizes his theory, "Through an exchange of promises, states and their representatives make explicit pledges of reputational capital, establish reciprocal commitments, and identify situations in which retaliatory actions may be taken" (Guzman, p. 183). Trachtman mentions reputation briefly, and Guzman alludes to jurisdictional issues, but it is striking how two highly knowledgeable legal scholars, both relying on political–economic theories of cooperation, emphasize different aspects of these theories and rely on different lines of work. Trachtman, as noted above, emphasizes Coase, Williamson, and North, but does not mention important work on reputation by political scientists such as Beth Simmons and Michael Tomz; Guzman cites Coase but there are no citations in his volume to Williamson or North.

The Economic Structure of International Law does not create major new theoretical arguments, present formal mathematical models, or present the results of sustained empirical research. Nevertheless, in the field of international law it represents a significant step forward. Much work by legal scholars remains loosely argued and oriented more toward textual interpretation and advocacy than toward rigorous explanation; and work by economists has often adopted unduly narrow assumptions about the motivations of policymakers, the prevalence of material interests, or the incompatibility of law and self-interest. Neither political scientists nor economists have known enough about law to show how a rational–institutional analysis would relate to various technical rules and specific practices of international law, as Trachtman does. It is impressive that Trachtman, who is thoroughly learned in the law, is also highly competent in the relevant portions of economics and political science. *The Economic Structure of International Law* should help to set a standard for the systematic use of social science in the analysis of international law.

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Global Price Fixing. By John M. Connor. Second edition. Studies in Industrial Organization, vol. 26. Berlin and New York: Springer, 2007. Pp. xvi, 503. $199.00. ISBN 978-3-540-34217-5. *JEL 2007-0584*

Perusing the press releases from the U.S. Department of Justice Antitrust Division (DOJ) or the European Commission (EC) from virtually any month in 2008, one encounters headlines such as these:

- "LG, Sharp, Chunghwa Agree to Plead Guilty, Pay Total of $585 Million in Fines for Participating in LCD Price-Fixing Conspiracies" (DOJ, Nov. 2008)
- "Major International Airlines Agree to Plead Guilty and Pay Criminal Fines Totaling More Than $500 Million for Fixing Prices in Air Cargo Rates" (DOJ, June 2008)
- "Commission Fines Car Glass Producers over €1.3 Billion for Market Sharing Cartel" (EC, Nov. 2008)
- "Commission Fines Wax Producers €676 Million for Price Fixing and Market Sharing Cartel" (EC, Oct 2008)

Similar announcements, each one a record of cartel enforcement activity, have been made regularly since the mid-1990s, triggered by a change in U.S. leniency policy and an increase in the resources devoted to cartel enforcement, including international cartels.

Attitudes toward cartels and cartel enforcement changed in the years following the revelations of an Archer Daniels Midland executive about ADM's participation in international price fixing conspiracies in lysine and citric acid, among other products.[1] In 1993, the DOJ revised their corporate amnesty policy so that amnesty from all U.S. criminal penalties would be automatically granted to the first firm to confess, as long as there was no preexisting investigation of collusion in the industry. Over a decade of increasingly aggressive antitrust enforcement against price fixing, we have seen higher fines as well as moves toward criminalization and private enforcement actions in countries where these options for responding to antitrust offenses had not previously been allowed.

In 2008, the DOJ boasted of increased fines, increased prison sentences, and an active slate of ongoing investigations:

> By all measures, the Division's cartel enforcement program had a banner year that broke new ground. Total prison sentences—the single most effective deterrent—*more than doubled the* previous record. The average sentence imposed on Division defendants reached a new record of 31 months. Fines were the second highest in Division history, and new case generation remains strong with more than 135 pending grand jury investigations (Thomas Barnett 2008).

Some economists believe that cartels are largely short-lived and ineffective: that cartels once formed will quickly perish under the weight of the ever-present incentive to cheat. Others believe that enough cartels last long enough and are effective enough to cause significant social welfare loss. Whichever side one takes in this debate, one cannot deny that cartel activity continues, and we are spending increasing resources globally to deter and prosecute these cartels.

John Connor's book, *Global Price Fixing*, an updated and revised second edition, is an in-depth look at the organization, mechanisms of operation, behavior, and effects of three famous

[1] Kurt Eichenwald, "Three Sentenced in Archer Daniels Midland Case," *N.Y. Times*, July 10, 1999, at C1.

international cartels in the citric acid, lysine, and vitamins industries. Empirical researchers often focus on "international cartels"—those with members from two or more countries. Connor focuses on "global cartels," which he defines as "conspiracies that bridge two or more continents" (p. 5). The cartels in citric acid and lysine inaugurated the renaissance of prosecution of international cartels in the mid-1990s. The vitamins cartel, following a few years later, was for government antitrust enforcers "the greatest catch in antitrust history" (p. 360). In terms of the cumulative size of penalties imposed, the convicted vitamins cartel members "were in absolute monetary terms the most heavily sanctioned defendants in the history of antitrust law. From 1999 to 2005, the defendants paid about $5 billion in fines and settlement payouts, of which more than 80 percent resulted from U.S. government and private legal actions. Moreover, 20 heavy individual criminal sentences were imposed on the managers of the cartels" (p. 432).

The scope of the book is wide-ranging, addressing the economics of cartel operation for these three in-depth cases, but at the same time offering an overview of the policy issues as well as commentary on recent developments. Not surprisingly, there is much to bring up to date because "[m]uch has happened in the field of cartels since the first edition was written in 2000" (p. viii). Although the organization of the book is substantially the same, the focus has broadened. This is particularly notable in the introduction. While the first edition introductory chapters had lengthy discussions of Archer Daniels Midland and U.S. antitrust law, the second edition has substituted topics such as the escalating prosecution efforts worldwide. This includes discussions of the number of foreign corporate defendants in U.S. criminal cartel cases, private lawsuits in Canada and Europe, and new antitrust laws and enforcement activities in Japan and Korea.

The citric acid, lysine, and vitamins sections of the book cover industry background, the organization and functioning of the cartel, and the economic effects of the cartel (primarily on prices and trade). Although the reader will find the same rich institutional detail as in the original edition, they will also find updated tables and charts and many new citations to current work. Many of these updates draw from Connor's own scholarly writings between 2001 and 2007 on topics including global antitrust prosecutions, optimal deterrence, and cartel overcharges.

Chapter 11, for example, on "The Vitamins Conspiracies," has a new introduction based on allegations made in U.S. civil suits. This chapter in particular is substantially longer than in the first edition and draws on Connor's own prodigious research. In later chapters, we learn of developments regarding private suits in Canada, Australia, and the United Kingdom. Chapter 17 has a section on the fate of individual conspirators, with updates (see pp. 464–66), as well as highlights of how the vitamins industry has restructured in light of government prosecutions and private litigation in numerous countries (pp. 458–59). In 2003, for example, Hoffman–La Roche "for 70 years the proud global leader in the vitamins industry . . . decided to sell its entire Vitamins and Fine Chemicals Division" (p. 458).

Connor does not hold back on criticism of current anticartel policies. He is a long-standing proponent of higher penalties and generally stronger enforcement: "Because sanctions are so weak outside North America, global cartels are under-deterred" (p. 436). He laments that the average prison term imposed between 1995–2005 was less than two years even though U.S. antitrust law allows for sentences of up to ten years (p. 471). Still, he recognizes pragmatic issues: "the limited resources of the federal antitrust agencies make avoiding all but a few trials each year virtually necessary" (p. 471). He argues that this necessitates allowing private lawsuits outside the United States.

Policymakers in the cartel arena are currently focused on cooperation between antitrust authorities across jurisdictions, increased emphasis on settlements (that is, plea agreements, following the U.S. model), and increased fines and criminal sanctions. As Neelie Kroes, European Commission for Competition, said in an April 2008 speech: "Cartels are happy to work across national borders, and so must we be." *Global Price Fixing* continues to be an invaluable reference for those with a deep interest in the economics of cartels, and a very readable and accessible narrative on the stories behind three famous contemporary international cartels. Given the apparent continued proclivity of firms to conspire to fix prices and allocate markets, these stories will continue to be relevant for researchers and policymakers for years to come.

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O Economic Development, Technological Change, and Growth

Job Creation and Poverty Reduction in India: Towards Rapid and Sustained Growth. Edited by Sadiq Ahmed. Washington, D.C.: World Bank; Los Angeles and London: Sage Publications, 2007. Pp. 350. ISBN 978-0-7619-3651-0. *JEL 2008-0608*

India has been one of the fastest growing economies in the world, with an annual growth rate of about 6 percent during the 1980s and 1990s, and close to 8 percent over the past couple of years. It is the fourth largest economy in the world in terms of purchasing power and is projected to reach low-middle-income status by the end of this decade, if present rates of growth continue. That's the bright side of the story. The dark side concerns the high rates of poverty and increasing inequality in this country. India is home to between 230 and 280 million poor people. This represents about one fifth of the world's poor, living on less than one dollar a day. Inequality has been on the rise: the richest 1 percent of the population saw a 71 percent increase in real income between 1987 and 1999 (Abhijit Banerjee and Thomas Piketty 2005). This book of collected articles examines the sources and sustainability of such high rates of growth in relation to its implications for poverty reduction with focus on the vulnerabilities awaiting ahead.

The first chapter sets the stage for the rest of the book by examining the evolution and structure of India's long-term growth over the past five decades and identifying emerging constraints to sustained growth. The main goal of this chapter is to establish that the liberalization period is associated with high growth and that high growth leads to a reduction in poverty; the causality in both cases is assumed. The argument is mostly based on contrasting the growth and development indicators across states and industries over two phases of development. The low-growth 1950–80 period is characterized as inward-looking with reliance on command-and-control, while the high-growth 1980–2004 period is marked by an outward focus and greater reliance on market incentives.

The link between liberalization, growth, and poverty reduction is also central to the remaining chapters of the book. Each chapter focuses on a particular constraint to sustained high growth, describing the past and present policy environment and discussing the ways in which the constraint might be eased. The second and third chapters analyze the fiscal policy constraints facing the federal and state governments, respectively. The next two chapters focus on the reforms in the private sector (chapter 4) and the financial sector (chapter 5) that are needed to improve the investment climate and attract domestic and foreign investment. Chapter 6 focuses specifically on India's poor infrastructure, a recurring theme in each chapter, as a detriment to sustained growth and emphasizes the limited ability of the government to address this problem due to the sharp fiscal deterioration experienced after the mid-1990s. Chapter 7 analyzes the agriculture sector, which accounts for more than one half of total employment in India. The last chapter on labor markets focuses on the sources of the low employment elasticity of growth and emphasizes the importance of labor market flexibility.

The detailed description of various components of the Indian economy, both past and present, as well as the comprehensive picture of the policy environment, will provide useful background to readers unfamiliar with India's recent growth experience. The discussion of the challenges that must be faced in sustaining high growth rates will appeal to anyone interested in growth-oriented policies. However, it is somewhat disappointing that the book contains surprisingly little information on the incidence and magnitude of poverty itself. The reader, who might be unfamiliar with the Indian economy, is left to dig into the extensive list of references to find out about the evolution of even some basic indicators of living standards for different segments of the population. While the poverty indexes are useful indicators of purchasing power, it would have been helpful to supplement these with other measures of well-being such as nutritional levels, life expectancy, education and health outcomes, crime rates, and access to public services.

Although not emphasized in the book, even the evolution of simple head-count measures of poverty in India during the postliberalization period seems to be much in dispute. This period has been described in terms ranging from "unprecedented improvement" at one end to "widespread impoverishment" at the other (the "great Indian poverty debate"). Angus Deaton and Jean Dreze (2002) suggest that the reality is somewhere in between: poverty reduction during the 1990s appears to be mostly in line with the previous rates of progress in the 1980s. Since the main emphasis of the book is on growth-enhancing policies as a means of poverty reduction, the explanations for why the high growth rates in the postreform period have not resulted in faster poverty reduction is at the heart of the argument. This discussion is mostly left to the last chapter on labor markets, which I found extremely informative. This chapter provides an extensive description of labor market outcomes of workers in different sectors and states as well as a detailed description of the policy environment. In addition to the standard discussion on wage and employment levels, the authors also consider job quality, particularly as it relates to informality, and the lack of labor market options for workers with low and medium levels of education. The authors convincingly argue that promoting employment growth in manufacturing and increasing the share of the formal sector are necessary. While I agree that enhancing labor market flexibility and investment in education should be at the top of the policy agenda, these are by and large medium to long-term solutions to an urgent problem. Besides, given the large share (90 percent) of the informal sector in total employment, it is not clear how effective further reforms in these spheres will be. Government policies that are more directly pro-poor such as social safety nets and employment insurance to reduce the immediate vulnerability of the poor to income and employment risk, deserve some further consideration.

Although the large (and increasing) inequality within and between various groups is briefly mentioned in most of the chapters, an extensive discussion of this issue does not get the detailed treatment that it deserves. While there is little disagreement over the positive effect of growth on poverty reduction, there is a lively debate on the merits and costs of alternative policy choices in achieving high growth. Even if one were to agree that liberalization policies in the 1990s were instrumental in achieving the high growth rates (as most do), it is possible that rising inequality could, in the terminology of this book, constitute another "constraint" on growth. Apart from being a moral concern, rising inequality could be detrimental to further growth by decreasing the support for reform and possibly leading to social unrest by aggravating redistributional imbalances between groups of different ethnic, religious and regional backgrounds (Ravi Kanbur 2007). I would have been interested to see a more detailed discussion of the sources of the increase in income inequality (across states as well as across other characteristics such as skill level, urbanicity, and occupation status in terms of formal versus informal sectors). What policies have been put in place to address inequality, and what policies might be appropriate in the future?

Overall I found this volume very interesting and informative. Nonetheless, I think that a more balanced discussion of the other side of the Indian growth experience, the lower than expected rate of poverty reduction and the distributional consequences, would have given the reader a more complete picture of this fascinating economy during interesting times.

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When Things Fell Apart: State Failure in Late-Century Africa. By Robert H. Bates. Cambridge Studies in Comparative Politics. Cambridge and New York: Cambridge University Press, 2008. Pp. xiv, 191. $60.00, cloth; $19.99, paper. ISBN 978–0–521–88735–9, cloth; 978–0–521–71525–6, pbk. *JEL 2008–1031*

In his new book, entitled *When Things Fell Apart: State Failure in Late-Century Africa*, Robert H. Bates explores the causes of the politically motivated violence that has plagued many African nations in the postcolonial period. Specifically, he aims to better understand why citizens form militia groups that lead to civil wars and strife. Rather than studying the motivations of those who create or join these militias, which has most recently been the focus of the literature, he instead explores why leaders would undertake actions that would likely lead to conditions that encourage the formation of these rebel groups. He also recasts the role of economic forces in creating these conditions. For example, he moves away from the traditional argument of poverty as a contributing factor of a state failure and instead focuses on the condition of public finances within the country as the instigator.

After providing a brief review of the literature, Bates delves straight into his core theoretical arguments. Using a fable to illustrate his claims, he proposes that three main factors determine the probability of state failure: the level of public resources available, the rewards from predation, and the elite's discount rate. Leaders make decisions based on these factors as to whether they will use their power to protect citizens from outside harm or to prey upon their citizens' wealth. The same leader who perhaps starts off by choosing to shelter his citizens may become a predator given exogenous changes to these three main factors (and thus changes to his incentive structure). Consequently, citizens will naturally form rebellious groups if their leader chooses to exhibit predatory behaviors, and a cycle of violence will ensue. In the remainder of the book, Bates provides the reader with substantial qualitative and quantitative evidence to support his main theories.

I must admit, the book left me deeply saddened for a variety of reasons. First, many of the "state failures" discussed in the book sowed the seeds for violence that is still ongoing today, with many of these conflicts showing no signs of resolution in the near future. Second, as I was reading through the cases presented, I was again reminded how much history tends to repeat itself. The examples painstakingly described by the author closely resemble current events such as the economic and political breakdown in Zimbabwe, the politically motivated assassination of Benazir Bhutto in Pakistan, the violence that erupted during the last election in Kenya, and so forth. Why have we not learned what can be done to prevent future misfortunes? Alternatively, can anything be done to actually prevent them? Or are we simply doomed to consistently repeat ourselves based on the fact that certain economic and political conditions, as described by Bates, are outside anyone's control and thus will repeatedly arise despite our best efforts? Third, while the book provides a deeper descriptive understanding of the complex socioeconomic conditions that create a situation ripe for violence, it also reminds the reader of how many more questions remain. Why, for example, did a democratic system become sustainable in India or the United States but often led to one-party states in Africa? Why did one-party political systems lead to such devastating consequences in many African nations, while a one-party state in China (granted, by no means perfect) heralded in unparalleled levels of economic growth? In general, how much do we really understand regarding the interaction of political systems, economic growth, and violence?

This is an intriguing book about the types of political and economic conditions that lead to political violence. Anyone interested in political economy and African development would find this book highly informative. The thoughtful case studies nicely illustrate the theories posited by the author as to the rise of militia groups. For those interested in a more quantitative approach to these questions, the appendix provides a carefully presented analysis of a panel dataset that includes about forty-five African countries over a period of twenty-five years. However, the book left me craving the policy take-away that would help us prevent these types of tragedies from occurring again. Quite frankly, this is less a failure of the book and more about the enormity and complexity of the topic that this book addresses. In sum, while this book advances our understanding of these issues, it also points to a need—and provides direction—for further research in this area.

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The Decline of Latin American Economies: Growth, Institutions, and Crises. Edited by Sebastian Edwards, Gerardo Esquivel, and Graciela Márquez. A National Bureau of Economic Research Conference Report. Chicago and London: University of Chicago Press, 2007. Pp. viii, 418. $85.00. ISBN 978–0–226–18500–2. *JEL 2007–1517*

The economic performance of Latin America in the last century and a half has been lacking, in particular relative to its northern neighbors—Canada and the United States—over the same time period. What explains Latin America's relative economic decline? This question has long haunted economists and historians. However, there is yet not an agreed-upon answer.

A vast amount of economic research has raised to prominence the view that countries' economic performances are largely determined by the institutional arrangements that govern their economies. This view supports the relevance of pursuing a comprehensive research agenda aimed at understanding how the institutions that have prevailed in Latin America have been different from those present in other countries—and how those differences have evolved over time. Such an agenda should include as essential elements not only a thorough description of institutional dissimilarities across countries and over time but also the identification of factors that have caused—and may still cause—institutions to be different, and the study of their consequences.

The articles in this book, written mostly by economic historians, fill scattered pieces of this broad agenda. Their themes, approaches, and country coverage are considerably heterogeneous. The papers analyze a wide variety of institutions ranging from tax systems and contract enforcement to labor and banking regulation, with a special focus on the 1870–1930 period. Some papers look at institutions in specific countries while others perform statistical analysis based on cross-country data. In general, more emphasis is placed on studying how institutional differences affect various outcomes than on characterizing, in detail, the differences across countries in institutional settings or in identifying their causes. The book does not achieve—nor attempts—to provide a unified explanation of Latin America's relative economic decline. However, by fleshing out a number of specific links between institutions and economic outcomes, it has the virtue of bringing to the fore the importance of comparing Latin America's economic history with that of other countries through the lenses of their prevailing institutions.

One of the main distinguishing features of Latin America's economic history has been its salient lack of stability, manifest in high inflation and recurrent financial and currency crises. Five of the ten papers in the book deal with this issue. In "Financial Crises, 1880–1913: The Role of Foreign Currency Debt," Michael D. Bordo and Christopher M. Meissner investigate econometrically the extent to which the proportion of foreign debt held in foreign currency makes a country more susceptible to financial (i.e., debt, currency, banking) crises. They find that it only does so when "financial institutions" are not sound—although the paper does not abound in details about specific differences in financial institutions across countries. In "Sudden Stops and Currency Drops: A Historical Look," Luis Catào shows that strong and abrupt reversals in the direction of capital flows ("sudden stops") were pervasive previous to World War I and, as it has been the case more recently, that they were spurted by hikes in central bank discount rates in the core capital exporting countries. Using econometric evidence for the period 1871–1913, he also shows that even though the reversals were indiscriminate across countries, they generated currency crises only in countries with a high procyclicality of fiscal policy and money supply—the latter driven by loose regulation of bank of issues, institutional obstacles to loan recovery, and high credit elasticity to cyclically sensitive collateral values.

Successful institutional arrangements are often those adequately shaped to fit local environmental conditions. To what extent can some institutions substitute for others to achieve similar outcomes in a different environment? The second two papers in this first group of five deal with this issue. In "Related Lending: Manifest Looting or

Good Governance? Lessons from the Economic History of Mexico," Noel Maurer and Stephen Haber document the prevalence of related lending in the Mexican banking sector during the dictatorship of Porfirio Díaz (1876–1911) as an efficient outcome to circumvent weak institutions to enforce private contracts and high costs of obtaining information about potential borrowers. Over that period, textile mills entered the banking industry as a way to secure financing otherwise unavailable. The authors argue that related lending did not result in capital misallocation. The textile sector, the main recipient of related lending, was not less profitable than other sectors; recipient mills were more profitable than nonrecipient ones; and since banks were profitable, there was no obvious expropriation of minority bank owners. Despite efficiently substituting for the lack of contract enforcement and high information costs in securing financing for textile mills, related lending was still unable to channel funding for other sectors, probably hampering their potential development. Similarly, in the case study "Establishing Credibility: The Role of Foreign Advisors in Chile's 1955–1958," Sebastian Edwards describes how a government can substitute its lack of political credibility to implement a stabilization plan with a nationally visible team of foreign advisors. In this case, successful substitution was short-lived as the stabilization plan finally failed, less than three years after implementation. A final paper in this first group is "The True Measure of Country Risk: A Primer on the Interrelations between Solvency and the Polity Structure of Emerging Markets, Argentina 1886–1992," where Gerardo della Paolera and Martin Grandes construct a measure of country risk that takes into account the fiscal interdependence between the national and subnational entities.

"Inequality and the Evolution of Taxation: Evidence from the Economic History of the Americas," by Ken Sokoloff and Eric Zolt, is the most complete among the studies in the book in terms of addressing the research agenda described earlier. First, it makes a thorough comparison of tax institutions across countries in America (North versus South); local (municipal or provincial) tax revenue relative to tax revenue raised by national governments was substantially higher in the United States and Canada than in Latin American countries. Second, the paper identifies the consequences of this asymmetry, most notably the much earlier spread of locally financed public primary schooling in North America. Third, it considers the potential causes of tax structure dissimilarity. It proposes that the great levels of income inequality in Latin America implied that financing for a public school system would need to come from the elites. However, since the elites were not to be the main beneficiaries of broadening school access, they did not favor policies aimed at achieving it.

Another broad institution addressed in the book is labor regulation. Aurora Gomez-Galvarriato, in "The Political Economy of Protectionism: The Mexican Textile Industry, 1900–1950," develops a case study of one single textile firm in Mexico: CIVSA. Based on access to the company's records, the paper documents that, while this firm's labor productivity resembled that of similarly sized textile firms in other countries (e.g., United States, United Kingdom, and Japan) at the beginning of the twentieth century, labor productivity in CIVSA fell largely behind after the Mexican Revolution. The combination of strong labor unions coupled with the interest of relatively less productive firms resulted in strong restrictions on capital investment that hindered productivity growth.

Two papers in the book deal with the consequences of commercial policy. In "Some Economic Effects of Closing the Economy: The Mexican Experience in the Mid-Twentieth Century," Gerardo Esquivel and Graciela Marquez use historical data, in particular the rise in Mexican import protection in 1947 due to the widespread imposition of import licenses and quotas, to test predictions of international trade and economic geography models. They find empirical support for the Stolper–Samuelson theorem, a key component of the Heckscher–Ohlin theory of trade, but do not find support for the predictions of more recent economic geography models. In "Before the Golden Age: Economic Growth in Mexico and Portugal," Pedro Lains argues that, contrary to the prevailing wisdom, industrialization in Mexico and Portugal started in the interwar period rather than after World War II, helped by the protection conferred by a depreciated real exchange rate.

I keep my final comments for the first paper in the book, "When Did Latin America Fall Behind?" by Leandro Prados de la Escosura. This paper constructs new data on the historical evolution of aggregate economic output for a large set of countries to make a provoking point. While Latin America fell behind the United States in the nineteenth century, its economic performance did not lag, at least after the mid-century, the performance of current OECD countries. This finding indicates that the common assessment of Latin America's retardation as occurring in the nineteenth century might be based on the use of an inappropriate yardstick; the economic performance of the United States was the exception, not the rule. On the contrary, it was in the last two decades of the twentieth century when the per capita income gap between Latin America and OECD countries widened most dramatically—and even more so the income gap relative to success cases of economic development such as those of East Asian countries. These findings suggest that Latin America's current relative backwardness should not be primarily blamed on the institutions that have prevailed in the postindependence period—and their persistent effects over time—but rather on economic and political forces less strongly tied to the past. Which is somehow at odds with the spirit that seems to have motivated the book.

This is a book that should appeal a wider audience than readers specifically interested in Latin America. Readers with a broader interest in economic development and long-run growth will find this collection of papers original and informative, at points intriguing, and overall thought stimulating.

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P Economic Systems

Land in Transition: Reform and Poverty in Rural Vietnam. By Martin Ravallion and Dominique van de Walle. Washington, D.C.: World Bank; Houndmills, U.K. and New York: Palgrave Macmillan, 2008. Pp. xii, 203. $30.00, paper. ISBN 978–0–8213–7275–3, cloth; 978–0–8213–7274–6, pbk. *JEL 2008–1091*

Most economists are well aware of the dramatic changes in China's economy since 1980, but relatively few are aware of the equally dramatic changes in Vietnam's economy. Vietnam began to replace its planned economy with a market economy in the late 1980s and, as in China, these policy changes were followed by a sustained period of rapid economic growth. Both countries began by dismantling their large, inefficient state farms and providing almost all rural households with individual plots of land, and in both countries agricultural output rapidly increased after this policy change. Yet in both countries some observers are worried that the return to the private sector is leading to a much more unequal distribution of income and, more generally, that a significant proportion of the population has not benefitted from the economic reforms. This has in fact happened in China (e.g., Ximing Wu and Jeffrey Perloff 2005), but thus far it is not clear whether this is also happening in Vietnam.

In this book, Martin Ravallion and Dominique van de Walle examine the impact of the sweeping changes in agricultural land policies in Vietnam that started in the late 1980s and continue to this day. In some ways, Vietnam's move toward more market-oriented policies has gone further than in China; for example, Vietnam essentially legalized the buying and selling of agricultural land in the 1990s, while in China this has not yet occurred (although a law has recently been drafted to do so). Another advantage of studying the case of Vietnam is that, since 1992–93, there have been periodic nationally representative household surveys that collect a wide variety of information, including very detailed information on households' agricultural activities. For those reasons, this book is of interest not only to economists and other social scientists who study Vietnam but also to those who are interested in China or in other countries that have replaced their planned economies with more market-oriented economic policies.

The book consists of eight chapters. After a brief introductory chapter, chapter 2 presents the historical context and, to the extent possible, the policy debates that occurred within the Vietnamese government regarding if, and how, the move to market-oriented policies in agriculture would be implemented. The data used, four nationally representative household surveys implemented in

1992–93, 1997–98, 2002, and 2004, as well as a panel survey on the impact of rural roads that was conducted in six provinces from 1997 to 2003, are described in chapter 3.[1] The next four chapters use the data to examine four distinct questions concerning Vietnam's rural land policies since the late 1980s. In the rest of this review, I will summarize the findings of the book, and my assessment of how well the book succeeds in answering the important questions that it has raised.

Before turning to the main contribution of the book, the data analysis in chapters 4–7, I would like to highlight a few aspects of the first three chapters. The first chapter is a well written introduction to the book, and is particularly valuable in its comparisons of China and Vietnam. Chapter 2 provides a very useful description of the historical context, highlighting differences between the northern and southern areas of Vietnam. It also provides a very useful exposition about how land was distributed to households in the late 1980s and how restrictions on the use of land were relaxed in the 1990s, again with interesting comparisons to China. Finally, chapter 3 provides a useful description of the data used, although it occasionally provides too much information in the sense that the detail should have been moved into the later chapters where the need for the detail becomes clearer.

The first policy question addressed by the book, in chapter 4, is how did the initial distribution of land in the late 1980s affect the distribution of household welfare and the efficiency of agricultural production? More specifically, the chapter examines whether the actual distribution of land, which could be interpreted as an attempt by local (commune) authorities to maximize an implicit social welfare function, is close to the distribution of land that would prevail if commune leaders were attempting to maximize total income (total consumption).[2] (While it would have been very interesting to examine the distribution of household welfare before and after the distribution of land, this is not possible due to the lack of household survey data before the early 1990s.) One way to examine whether the actual distribution of land was similar to what would prevail if the goal was to maximize total income (which equals total output) would have been to estimate a household agricultural production function and check to see whether the marginal product of land is similar across different households within the same commune. This is not an easy task and the authors instead propose another route that requires only data on household consumption, land holdings, and household characteristics. More specifically, the authors develop a relatively simple model that allows them to test the above hypothesis by comparing reduced form regressions of land holdings on a wide variety of household characteristics with reduced form regressions of consumption per capita on the same household characteristics. One could argue, as the authors do, that this is a more general test of efficient land allocation because it could also account for use of agricultural land in nonagricultural production activities.

While this test is intriguing, I do have two worries about applying it. First, it requires a particular functional form for a household's consumption as a function of its allocated land and the abovementioned household characteristics; some robustness checks using other functional forms would have been more convincing. Second, even with the detailed data that they have, there are potentially serious problems of omitted variable bias and attenuation (measurement error) bias, which could lead to incorrect inferences. Using this approach, Ravallion and van de Walle reject the hypothesis that the actual distribution of land is also the consumption–income maximizing distribution of land. While I do not disagree with this result, for the simple reason that the entire land distribution process was very political and allowed a fair amount of local discretion, the above worries reduce the confidence I have in it. I think the result would have been more convincing if they had also estimated production functions and compared households' marginal productivity of agricultural land. While production function estimates also require assumptions, this would have been a good robustness check of their results.

[1] As a matter of full disclosure, I was deeply involved in the design, and much of the analysis, of the four nationally representative household surveys.

[2] Much of the material in this chapter is from Ravallion and van de Walle (2004), just as much of the material in chapter 5 is from Ravallion and van de Walle (2006) and much of the material in chapter 6 is from Ravallion and van de Walle (2008).

Finally, chapter 4 also presents evidence that the allocation of land in the early 1990s was quite equitable; this is an important finding, but it can be shown without making strong assumptions.

Chapter 5 investigates whether the government's introduction of land markets in 1993 led to a more efficient allocation of land, in the sense that households whose production activities had a relatively high marginal productivity of land acquired additional plots of land from households with a relatively low marginal productivity of land. My main comment here is similar to my comment on chapter 4; the "natural" way to test this would be based on estimates of households' marginal productivity of land: households in the panel data that had higher (lower) marginal productions would be likely to buy (sell) land between 1993 and 1998. Instead, the authors use the same model in chapter 4 to test whether the inefficiencies detected in 1993 had become smaller in later years, and additional functional form assumptions (see p.105) are needed to obtain testable hypotheses. Their results indicate that land transactions after 1993 led to a more efficient distribution of land, which is quite plausible, but the assumptions needed to apply their methodology may leave many skeptics unconvinced. As in chapter 4, more robustness checks would have been useful.

Chapter 6 presents what, to me, is an especially interesting question: Did the introduction of land markets lead to a less equitable distribution of land, giving rise to a poor, landless class that has little choice but to subsist by selling their labor to those who managed to acquire relatively large parcels of land? This is an issue that can be addressed, to a large extent, by simply examining the data without making assumptions that may be questioned, and much of this chapter does exactly that. Data from 2004 show that, on average, landlessness is more likely for better off households in rural areas, which contradicts the hypothesis that introducing land markets would result in a poor, landless class. The one exception to this is in the Mekong Delta, where the evidence is more ambiguous and offers some support to the claim that introducing land markets will lead to a poor landless class (although even here the poverty rate is falling over time). The general result that the introduction of land markets in Vietnam did not lead to increased inequality and poverty is an important result with direct implications for China (as well as other formally socialist economies); Ravallion and van de Walle's results on this issue are quite convincing, but more research is needed to understand more clearly the mechanisms that led to this result, and well as in variation in this result across different regions in Vietnam.

Rural credit issues are examined, albeit briefly, in chapter 7. Vietnam has established many programs that are intended to assist poor rural households, including credit programs, but in general these programs are not very effective at reaching the poor. This is shown using mainly descriptive analysis based on both the nationwide surveys and the panel data from six provinces. The reasons why these programs are ineffective is an important topic for future research.

In summary, this book provides a very useful analysis of the consequences of Vietnam's rural policies that ended state farms and returned to household farming that includes rights to buy and sell agricultural land. While much of the research has appeared in journal articles, combining it into a single volume will be useful for those who have not yet seen those articles. While I do have some reservations regarding the analysis in chapters 4 and 5, the book provides important findings and raises many interesting questions regarding privatization of agricultural production that will be of interest both to those interested in Vietnam and to those interested in China's vast rural areas. Ideally, this book should be read not just by economists but also by policymakers and noneconomist staff in international development agencies. Unfortunately, the plethora of models and estimation details in some chapters will lose many in the noneconomist audience (though Vietnamese, and hopefully Chinese, students and academics will value the details). Future research on the rural economy in Vietnam, and in China, should build on what is in this book, and try to make it accessible to a wider audience.

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Z Other Special Topics

The Marketplace of Christianity. By Robert B. Ekelund Jr., Robert F. Hébert, and Robert D. Tollison. Cambridge and London: MIT Press, 2006. Pp. x, 355. $29.95. ISBN 978–0–262–05082–1. *JEL 2007–0812*

If a sign that a subfield in economics has come of age is having its own *JEL* code, then the Economics of Religion has arrived (*JEL* code Z12). It even has its own professional organization—the Association for the Study of Religion, Economics, and Culture (ASREC). The economics of religion is concerned with two fundamental issues: How do economic and demographic variables influence religious dogma, practice, and behaviors? How do religious variables influence economic and demographic behavior? A wide range of topics have been, and are being, addressed in the literature motivated by these two questions. In *The Marketplace of Christianity,* Robert Ekelund Jr., Robert Hebert, and Robert Tollison (hereafter EHT) make an important contribution to this new, and rapidly growing field.

While the focus of the EHT study is Christianity, and in particular the Roman Catholic Church and the half-millennium from the Protestant Reformation to Christianity today, it has implications for understanding other religions. Moreover, it has implications for several subfields beyond the economics of religion—including economic history and industrial organization. Indeed, EHT show that applying the principles of economics to understanding how the thousand year monopoly of the Roman Catholic Church was challenged by the Reformation and how both the Catholic Church and the emergent Protestant denominations responded, is a fascinating market structure story.

In addition to the extensive endnotes, the book includes a very useful chronology of Christianity from its beginnings to the present, an extensive glossary of terms, and an index. The exposition is very clear, and the explanations of economic concepts and principles and of Christian doctrine and politics are sufficiently detailed that the book is accessible to those not versed in either, or even not versed in both.

The first substantive chapter (chapter 2), the Economics of Religion, is a review of the history of economic thought in this area. It is primarily a discussion of Adam Smith's writing in *The Wealth of Nations*. Smith offers the first discussion of religion from an economic perspective and focuses on how competition makes both individual clerics and churches more responsive to the concerns and interests of the congregants and, hence, more efficient. Smith has a particularly favorable view toward the Scottish Presbyterian Church because of the greater control of the congregants (including in choosing their pastor) as it was a "bottom up" (congregationalist) as distinct from a "top down" (hierarchical and authoritarian) institution. This is an excellent discussion of Smith's thoughts on religion, summarized in a single sentence in chapter 7 on the demise of the Roman Catholic Church monopoly: "Adam Smith recognized that sixteenth-century Protestants helped promote a supply-side revolution that amounted to a massive tax cut in terms of money and time committed to salvation seeking" (p. 161).

Economists then essentially ignored the topic for two centuries (Max Weber being an exception, with a chapter of his own in this book). Yet, in contrast to the excellent exposition of Smith's views on religion, the discussion of the modern period is wanting. EHT indicate that Gary S. Becker's pioneering work in applying utility maximizing behavior based on full prices and full incomes to topics that had been considered outside the realm of economics (Becker's household production of Z-Goods) influenced the important work of Corry Azzi and Ronald G. Ehrenberg (1975), and even more so the path breaking work of Laurence R. Iannaccone (1988, 1991, 1992, 1998). The richness of their contributions, however, especially Iannaconne's, does not come through. It is not clear why the tone in discussing this exciting literature is downbeat. Nor is there a discussion of the extensive literature in the Sociology of Religion on religious monopolies versus pluralism (see, for example, R. Stephen Warner 1993, 2005).

Chapter 2 closes with EHT indicating their goal: "to explain institutional change using the tools of contemporary economics . . . our analysis is firmly grounded in the neoclassical economic principle of self-interested behavior. We apply this principle to institutions" (p. 36). In their framework, churches are viewed as firms that respond to changing constraints. They recognize the two way causation—changes in constraints (prices, trade-offs, and incomes) alter the nature of institutions (churches, religions), and that changes in institutions can themselves alter the nature of the constraints.

EHT view religion as a "credence good," that is, a good whose value (utility) is difficult if not impossible to measure before or after consumption. Its value is taken on faith. It is also a "public good" (e.g., provides a set of rules for society), a "private good" (e.g., provides comfort), and a "club good" (i.e., a good whose benefits are greater if one is part of an active group). It is, therefore, a multifaceted good, the demand for which is downward sloping (chapter 3). Reflecting the central premise of Christianity in a belief in the afterlife, EHT write that: "one way of coping with existential dread is to establish the credibility of utility in life after death. A common theme of most religions, therefore, is the promise of an afterlife" (p. 49). The authors believe the concept of salvation makes up the core element of the market for Christianity, but it may be less central or irrelevant for other religions or belief systems.

EHT write: "If we use wage rates to proxy income, the literature suggests that there is a mild negative relationship between income and religious participation suggesting that religion generally is an inferior good" (p. 57). Yet their measure of income would include both a price effect (i.e., higher value of time) and a "pure" income effect (price of time constant). Participation (measured by time) may fall with a rise in income if the substitution effect of a wage increase dominates the pure income effect, even when religion is a superior good.

Gutenberg's development of the movable type printing press is given by EHT as but one example of how technological change, and the change in relative prices, influenced the course of religion in Europe. It resulted in a decline in the cost of publishing pamphlets and books (and not just the Bible), increasing the productivity of literacy, thereby encouraging the spread of literacy and facilitating the widespread dissemination of criticism of the Roman Catholic Church. Between 1517 and 1520, "Luther's thirty publications sold over three hundred thousand copies in the vernacular" (p. 81). The church's effort to thwart the spread of "heretical" ideas led to censorship and book burning. While the Protestant Reformation may have taken place even in the absence of the printing press, EHT believe that this technological innovation facilitated its emergence and spread and, hence, the decline in the Roman Catholic Church monopoly over Christianity in Western Europe.

The medieval Roman Catholic Church is viewed by EHT as a contemporary corporation, with a CEO (the Pope), with upstream directors of various functions (the curia and cardinals), with geographically dispersed downstream divisions (local bishops, parish priests, and monks) that collect revenues from selling services (p. 94–95). The main church product was spiritual redemption, and the Roman Catholic Church offered this through a continuous price system. The medieval church gave structure to the demand curve for salvation though the development of doctrines and rules of purgatory, the distinction between venal and moral sins, indulgences, confessions, penance, and other church innovations. By setting different prices for different customers based on their income, the church sought to be a price discriminating monopolist (first-degree price discrimination). The entry of competitors seeking to weaken the Roman Catholic monopoly was thwarted by manipulation of doctrine and by penalties, ranging from the mild to excommunication and death. The rules became complex, the prices charged became very high. Existing potentially competitive religions (Eastern Orthodox Church, Judaism, and Islam) were marginalized by various means.

EHT contend that by raising the price it charged for monopoly services the Catholic Church encouraged individuals to seek a cheaper religion, and for the civil authorities that relied on the Church for legal services to find a cheaper (local or country-specific monopoly) supplier. Europe was ripe for the emergence of a lower priced competitor. Protestantism, the theme of chapter 5, offered salvation at a lower price in terms of money and time, and was more congregational and less authoritarian in its organizational structure. It emerged both to satisfy the interests of monarchs (England and Scandinavia) and from the activities of religious entrepreneurs (e.g., Luther and Calvin). Those whose demand for religion was most sensitive to the Roman Catholic Church's monopoly pricing were the first to break away.

The Roman Catholic Church was not passive in its response to this competition (chapter 6). The Counter-Reformation, the Church's response, included lowering the "full price" (monetary and practice), improving service to parishioners, doctrinal changes that were largely nominal rather than fundamental (Council of Trent), educational programs, the Inquisition, and, of course, wars (e.g., the Thirty Years War). The Inquisition was introduced to raise the price of practicing Protestantism and to suppress the other emerging threat, the spread of science and rationalism, which was also enhanced by the spread of literacy and the falling price of publishing. Yet, Protestantism was also enhanced by the failure of the Roman Catholic Church to act sufficiently against church corruption (including nepotism) and the increasing concentration of Church power in the Italian clerics.

EHT devote much of a chapter to Max Weber (chapter 8). They discuss his views on Protestantism and economic growth, that is, that the economies of the Protestant countries grew more rapidly because Protestantism encouraged hard work, savings, and investment. They place greater emphasis, however, on the difference in the price (money and time) of salvation in the two religions. By lowering the full price, they argue that Protestantism freed up resources that were put to use in expanding economic activity. They acknowledge, however, that the empirical support of this hypothesis is, at best, very weak. The most interesting part of this chapter is the Appendix on the economic role of the medieval cathedral, and the comparison of the heights of cathedrals. However, their suggestion that the Catholic cathedrals' unused capacity served as a mechanism for discouraging potential entry by competitive religions seems like a stretch (pp. 225–26).

The final chapter (chapter 9) skips the centuries between the Reformation/Counter-Reformation to bring the reader to a discussion of issues facing contemporary Christianity. EHT contend that an analysis of the implications of the effect of church doctrine and practice on the "full price" of participating in a Protestant denomination can explain the multiplicity of denominations in the contemporary world. This is documented through discussions of divisions (perhaps schisms) among Baptists, Methodists, and Anglicans. These differences in "full prices" arise from differences in doctrine, ritual, religious policy, social/political policy, and organization. The "full price" for adherents includes not merely money prices and time inputs, but also includes lifestyle differences regarding: sex and sex roles, clothing, dancing, food and beverage consumption, modern medicine, technology, association with persons of other denominations, etc. As a result, "a multiplicity of forms of Christianity exist and are an economic function of income, education, factors affecting risk preference, and other shifters . . . " (p. 249). It is a marketplace of intense product differentiation.

Among American Catholics, church attendance has declined, EHT argue, because unchanged church doctrine is increasingly at variance with the views of Catholics in the developed countries that have changed with their higher levels of income and education and knowledge of science and, hence, their changing world view. The cost of being a practicing Catholic has increased.

In the EHT view, the greatest divide within the Protestant and Roman Catholic churches relates to the issues of sex, including sexual behavior and identity (e.g., premarital sex, homosexuality) and procreation (e.g., birth control, abortion). These issues are dividing Christianity within the developed countries, and between Christians in the developed and less-developed countries. It is because of sharp differences in views on these issues and doctrinal rigidity that schisms have emerged in some Protestant denominations and EHT predict a schism (or several schisms) in the Roman Catholic Church. These schisms, responding to heterogeneous demanders, increase the extent of product differentiation in this market.

EHT discuss the "culture wars" between science and fundamentalist/conservative Christians (including some Protestants and Roman Catholics) in the United States. Various battles are waged over the teaching of evolution, stem cell research, when life begins (abortion), and sexual orientation, among other issues. What they see as emerging is an "individualized Christianity" or a "cafeteria style" Christianity where people select which parts of dogma they accept and practice, and which they reject. This is consistent with the rising number of individuals who, while not atheists or agnostics, indicate that they are not members of any particular organized denomination. It would have been interesting if EHT had more of a discussion of secularism as an alternative form of "religion."

These schisms are beneficial. Quoting Andre Suares, a French poet, EHT write: "There are no heresies in dead religions." Then they continue: "Stated in economic terms, heresy and schism will be constant factors in emerging Christianity because heterogeneous demands for new forms are a function of factors—income, education, or science—that are in constant flux" (p. 268). These ideas seem to be particularly relevant in the current period where religious fundamentalism and liberalism/individualism are clashing to various degrees in all the world's major religions. The application of microeconomic theory that is so successfully applied here to one major development in Christianity can, in principle, be applied to these other religions as well.

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Measuring the Value of Culture: Methods and Examples in Cultural Economics. By Jeanette D. Snowball. Berlin and New York: Springer, 2008. Pp. xi, 230. $109.00. ISBN 978–3–540–74355–2. *JEL 2008–0747*

Cultural economics is a burgeoning field. Practitioners currently question whether existing economic tools suffice to research the arts and the cultural industries or whether the special conditions of this field compel economists to rethink their tools and introduce new concepts into economics. Jeanette Snowball explores the applicability of existing tools and ventures into some new directions in this well written, lucid, and compact study of valuation studies in the field of the arts. It is an important book for anyone interested in valuation studies and especially for those who study the worlds of the arts.

The valuation of cultural goods poses a great challenge as it takes place, for a major part, outside the sphere of the market. Without subsidies, grants, and donations, a lot of art projects would not occur. This means that both individuals and organizations (like governments) appreciate art products over and beyond the value that the market attributes to them. People value cultural goods for the option of enjoying them (option value) or just for knowing that they exist (existence value) or for the value they may have for later generations (bequest values). The similarities with a good like nature is striking and so it should not be surprising that cultural economists borrow the techniques that the economic research into natural resources has generated.

Snowball provides a thorough review, discussion, and evaluation of economic impact studies, the applications of the contingent valuation method, and the willingness to pay method. In addition, she gives a tentative appraisal of the choice experiment method, tentative as this method is still relatively new. A student of each of these methods will benefit greatly from her discussion. She covers a great deal of applications. The reader will develop an appreciation for the difficulties that any researcher who tries to assess the value of culture faces.

Snowball starts with economic impact studies. Politicians are often eager to hear what contribution a cultural project, such as a museum or a festival, makes to the local economy. They love economic numbers that indicate the economic relevance or irrelevance of the project. Snowball's survey points at the pitfalls of such studies. The assessment of the indirect impact—the generation of economic activity due to employment in the cultural sector and visitors from outside the region—and the multiplier effect that can be derived from the indirect impact (the multiple effect of a dollar or euro invested in a cultural project on the local economy) proves to be especially dubious.

Because of doubts about economic impact studies, economists have embraced contingent valuation and willingness to pay methods. As any economist will readily acknowledge, these methods have the advantage that they have consumers or users of cultural goods determine their value, rather than that an arts council does so. The idea is basically that, where markets function inadequately, we try to create a market by asking people what they would pay if they had the option to consume the good. This gets us into the technology of surveys. The design of surveys proves to be critical. Even if the problems of design have been addressed—such as the issue of how much information to give to respondents and how to articulate alternative options—we are left with the issue of seduction. Art producers know all too well that their business is about seduction and persuasion and, therefore, about changing the preferences of people. The producer of modern dance tries to seduce people to like modern dance even if they thought that they did not care for such an art form. The question is, therefore, how willingness to pay studies can do justice to the power of persuasion.

Snowball provides an extensive and exhaustive discussion of the criticisms and defense of the contingent valuation and willingness to pay studies. She discusses a few South African studies in detail. Those who are about to set up their own willingness to pay study will find this book an excellent source. They also will learn about the relatively new choice experiment method where respondents are asked to value attributes of a good, rather than the good itself. Such a study allows for more differentiation and does justice to the particular qualities of a cultural good.

Because all of these studies demonstrate the willingness to pay for cultural goods even among nonusers, the conclusion is warranted that cultural goods generate significant positive externalities and nonmarket benefits. This could be an argument for government subsidies of the arts, but not necessarily so as the third sphere, or the sphere of donations and gifts, can be an alternative source of finance.

Whether the current state of knowledge suffices for the valuation of cultural goods remains to be seen. We still seem to be in need for measurements that show the cultural returns of festivals, museums, and other cultural goods so that we can hold arts producers and politicians dealing with the arts accountable.

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