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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

Macro Mayhem: A Dia Fenner Economic Thriller. By Michael L. Walden and M. E. Whitman Walden. Lincoln, Neb.: iUniverse, 2006. Pp. vii, 115. $10.95, paper. ISBN 978–0–595–38000–8.
JEL 2006–1246

Micro Mischief: A Dia Fenner Economic Thriller. By Michael L. Walden and M. E. Whitman Walden. Lincoln, Neb.: iUniverse, 2006. Pp. ix, 155. $12.95, paper. ISBN 978–0–595–38879–0.
JEL 2006–1247

Using fiction to instruct in economics and other disciplines can be effective when emotional tags and an engaging plot boost student interest and understanding. Moreover, sympathy with characters can also heighten moral imagination, which is why Adam Smith advocated its use. Successful novels of this genre tackle a focused subject and explore it from several angles, allowing student comprehension to grow in depth and maturity. Examples from the Enlightenment include Rousseau's *Emile* and Voltaire's *Candide.* In the modern era, Russ Roberts's *The Choice: A Fable of Free Trade and Protectionism* (third edition, Prentice Hall 2007) has become a classic for providing insights on international trade.

Less successful are encyclopedic works that spread themselves thin trying to cover all the concepts of a macro or micro course. This is the approach taken by the husband–wife team of

Walden and Walden. Michael Walden is a Professor of Agricultural and Resource Economics at North Carolina State University and Whitman Walden is an educator. The protagonist in both novels is Lydia (Dia) Fenner, a recent Cornell Ph.D. with a specialty in macroeconomics.

In the first book, *Macro Mayhem* (which can be read out of sequence), Dia is a senior economist in the Treasury Department, responsible for devising fiscal policy to counteract a cyclical downturn. Somewhat implausibly, fresh out of graduate school and after two weeks on the job, she ends up in the middle of a confrontation between the Treasury Secretary and Congress over an economic stimulus package. This provides the plot line as Dia navigates between her economics training and the political demands of the job. Dia's fiscal policy proposal seemingly is prepared overnight (like many term papers)—without reference to data collection or econometric projection.

Macro Mayhem ambitiously tries to touch on most macro principles concepts, including Keynes, the Depression, money creation, trade deficits, budget deficits, inflation, the Phillips curve, the liquidity trap, unemployment, full employment, outsourcing and several micro subjects like the economics of wine production and prices of housing and gasoline. What the authors are attempting to show is that economic issues and insights pervade daily life.

Unfortunately, the treatment of so many topics is necessarily superficial, akin to name-dropping of concepts. The lack of realism is sometimes confusing. In presenting exchange rates, the authors use the British pound and dollar, but select a price of two pounds per dollar, which will unnecessarily confuse students if they attempt to compare this to actual market prices over the last few decades (which are approximately the inverse). The Open Market Committee is incorrectly stated to be composed of Fed Board Governors and the regional bank presidents (rather than the Board, the New York Fed president, and a rotating group of four of the remaining eleven bank presidents). In discussing tax cuts, the authors make no distinction between marginal and average tax rates.

The book does a good job, however, of addressing the shortcomings of fiscal policy for fine-tuning the macro economy. The authors provide no guidance whether to use the book as a teaser to stimulate student interest at the beginning of the course or as a review at the end. Either approach would work, but the book does not stand on its own without a textbook to provide analytical substance. Both books provide a brief list of annotated readings for some chapters.

By *Micro Mischief,* Dia has left the Treasury Department and the politics of Washington to assume an academic post at a small college. A fellow faculty member in botany believes he has discovered how to turn kudzu, the ubiquitous weed plant, into oil and seeks a large government subsidy for its production. The kudzu innovation appears to threaten oil prices and leads to murder. As Dia solves the mystery, she explores a vast array of micro issues such as scarcity, opportunity cost, demand and supply, price signals, pricing cobwebs, elasticity, price discrimination, pay equity, fixed and variable costs of production, economies of scale, public choice, health care, and numerous others.

Addressing so many concepts in a short novel means the treatment is typically perfunctory. In some instances, it is problematic: the authors conflate self-interest with greed, confuse a movement along the supply curve with a shift in supply, equate business sense with economic sense (without noting the difference between private and social costs and benefits), and profess that the "dismal science" gets its name from its focus on scarcity and trade-offs (rather than Thomas Carlyle's attack on economic reformers opposed to slavery). The authors use an example of differentiated goods and discuss the dimensions of heterogeneity in product and service—yet imply that this represents perfect competition (rather than monopolistic competition).

As with *Macro Mayhem,* the authors successfully convey the insight that economic concepts are pervasive in life and yet poorly understood by otherwise educated persons. The micro book does a good job of illustrating the difference between innovation per se and useful innovation measured by market potential for profit. Unfortunately, the impression is given that serious economic analyses of public policies can be accomplished over a weekend, without substantial data collection and analysis.

The micro text has a better chance than the macro of engaging student interest and learning because the concepts are less abstract. The idea of motivating students through fictional constructs is a good one. Future Dia Fenner mysteries will hopefully develop depth on a focused subject.

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B Schools of Economic Thought and Methodology

Adam's Fallacy: A Guide to Economic Theology. By Duncan K. Foley. Cambridge and London: Harvard University Press, Belknap Press, 2006. Pp. xv, 265. $25.95. ISBN 978–0–674–02309–3.
JEL 2007–0030

This book addresses a question of critical contemporary importance: Can we divide the world into an economic sphere, appropriately driven by self-interested motivations, and the rest of life, in which other-regarding values are morally important? Duncan Foley argues in this book that we cannot. As a vehicle for exploring this topic, Foley takes the reader on an excursion through the history of economic thought. The "Adam" in the book's title refers to Adam Smith, and particularly to the passages in Smith's writing suggesting that the invisible hand of markets might convert private self-interest into public good. Foley uses the term "theology" to emphasize that a dualistic separation of economics and values is a matter of faith and belief, rather than of fact or science.

Adam's Fallacy takes the reader through much material from a course Foley has taught on the "Theoretical Foundations of Political Economy." The historical figures discussed include Smith, Thomas Malthus, David Ricardo, Karl Marx, William Stanley Jevons, Carl Menger, John Bates Clark, John Maynard Keynes, Thorstein Veblen, Friedrich von Hayek, and Joseph Schumpeter. Smith and Marx get their own chapters; the others get much shorter consideration, in some cases only a few pages. The writing style is geared to an upper-undergraduate-level student, with graphs and equations contained in appendices. In this quick romp through history, readers are presented with brief treatments of such topics as marginalism and Pareto allocations, and extended treatments of the labor theory of value, surplus value, capital accumulation, class conflict, and the value of gold. As Foley states in the preface, "This is my own take on economics, and exploits the great figures in the history of political economy shamelessly for my own ends. Be warned" (p. xii).

And warned we should be. While the discussion is highly intelligent, serious scholars of the history of economic thought will find many points to disagree with in this pedagogical, agenda-focused recounting of personalities, theories, and events. More casual readers may also find cause to wonder on many occasions where the theories of Ricardo or Marx end and the views of Foley or other contemporary economists begin.

But this book is not meant to be a scholarly tome in the history of thought; the review of history is meant to support Foley's point, and do so for a broad audience. He wants to refute the idea that economic life exists in a sphere that is morally separate from the rest of life and our other-regarding ethical concerns. In some sense, his review of the history of thought does contribute to this project, at least by showing that economists' work over the centuries has encompassed more than the narrow price theory and optimization methods that form the core of the mainstream discipline today. Distribution, population, the division of labor, and the evolving forms of capitalist organization have, Foley shows, been central concerns at other times in the discipline's history. But in another sense, the historical review contradicts and weakens Foley's own main argument. Foley presents two distinctly different—and incompatible—views about the nature of economic life, and his adherence to certain historical doctrines leads him to less-than-satisfactory responses to contemporary moral challenges.

On the one hand, Foley draws on a rather open-ended philosophy of economic systems. He argues that the "laws of the market"—which are (by "Adam's fallacy") supposed to turn private greed into public good—are not nearly so lawful as often supposed. "There is no escaping the moral relevance of weighing the good and the harm in each case" of economic decision making, Foley writes (p. 226). Rather, he says, "The fallacy lies in thinking that there are universal principles that short-circuit this process" (p. 226). "As we give up Adam's Fallacy, we can also give up the conceit that there are specific laws of economics parallel to natural laws" (p. 225), he claims. And because "capitalism is . . . constantly evolving," he says that "it seems rash to posit the existence of immutable economic laws" (pp. 203–04). He writes approvingly of Marx's insight that "social reality is determined by human action [and] is subject to historical change" (p. 91). When writing from an open-systems point of view, Foley rejects the idea that there are universal laws that drive economies, independent of our morally significant human actions, and seemingly creates a place for discussion of moral choices at the heart of economic life.

In distinct counterpoint to this view of openness and evolving systems, however, Foley more often, in this book, repeatedly insists that capitalism has a "logic" or "laws" that are, at base, intransigent and unchanging. He writes, for example, that

> The organization of the social division of labor through commodity exchange and wage labor *systematically* inverts the ordinary logic of human relationships. The *logic* of the commodity system assumes the *universal* assertion of self-interest in opposition to others. . . . [T]he reality of commodity exchange and its *laws* tends to defeat moral action. Thus Adam's Fallacy becomes a real and *inescapable* part of the experience of life (p. 85, emphasis added).

He is quite passionate in his belief that capitalism imposes "antagonistic, impersonal, and self-regarding social relations" (p. 2) on society, which lead to "the systematic imposition of costs on those least able to bear them, and the implacable reproduction of inequalities" (p. 3). He decries the "inequality, poverty, and the social disruption that capitalism wreaks on a world scale" (p. 212). Thus, the review of the history of the labor theory of value, surplus, and exploitation—topics much out of fashion in contemporary economics and politics—are integral to Foley's argument. Foley writes disparagingly of notions of market socialism which focus only on legal institutions and distribution (p. 205). Such notions, he believes, do not address what seem to him to be the central issues: the appropriation of surplus value from workers by capitalists and the alienation inherent in the production of commodities for the market (pp. 103, 227).

In the final chapter of the book, Foley seeks to tell readers what attitude we should take toward the world in which we find ourselves. Given Foley's passionate stance toward class exploitation, one might expect some equally passionate calls to action in his closing arguments. He is, however, surprisingly circumspect, detached, and ambivalent. He does not advocate an alternative to capitalism and the exploitation he believes it brings. Rather, he recommends only the decidedly more moderate policies of "continuing political and regulatory intervention to keep the pursuit of self-interest from running off the rails." In such intervention, he includes (but does not elaborate on) "institutions such as central banks, social security, and antitrust authorities" and "new institutions of regulation and control of aggregate demand, competition, and environmental impact" (p. 225). The attitude he prescribes seems to be that, while we crucially need to understand that ethics is a part of economics, we need to also resign ourselves to living morally ambivalent lives within an inherently unethical, but inescapable, capitalist wealth-creation machine.

But has not Foley created his own version of the economics/values dualism he criticizes? While in his open-system moments, Foley writes that we cannot "short-circuit" the process of making moral choices, in his "logic of capitalism" moments he reinstates a view that all capitalist choices fall intrinsically into one moral category: exploitative. Yet Foley's dogmatic belief in the "original sin of class exploitation" (p. 218) might be considered to be as much a matter of belief as the "theology" he attributes to Smith. Certainly, people feel exploited as wage earners when they work in conditions of less-than-fair wages, abuse, or disrespect for their identity as human beings. And people often feel alienated from their work when they feel it does not make a contribution to society, and alienated from society when the gap between rich and poor widens to a chasm. But the doctrine that production for markets *inevitably* creates morally problematic exploitation and alienation is not an empirical observation, but rather a throwback to Marx, and to Foley's own Marxist training. Considerable research in organizational behavior, management, economic sociology, psychology, and ethics point in the direction of more situationally specific ethical evaluations of contemporary workplaces and markets: some are oppressive and destructive of community, while others—even within capitalist societies—may be supportive of human flourishing. By ignoring his own insight that "social reality is determined by human action" (p. 91), Foley's arguments end up being rather unlikely to appeal to readers outside the rather limited audience of his fellow believers.

Foley's argument is also made less strong by its focus backwards, on history, and its consequent neglect of pressing contemporary problems. The book is very sketchy on issues of globalization, development, and the environment, and neglects issues of gender and race entirely. His belief that "[l]arge areas of the world and their people will very likely follow the path blazed by Western Europe, North America, and Japan, a path of industrialization, urbanization, and movement . . . to higher-productivity industrial and post-industrial production" (p. 223) is quite optimistic, in light of the contemporary politics of trade and debt. In spite of considerable evidence from science about the threats of global climate change, Foley largely dismisses environmental concerns through adoption of optimistic forecasts about agricultural productivity (pp. 60, 81) and an assertion that "Global industrial capitalism . . . is already mobilizing to . . . a void an environmental catastrophe" (p. 223). Women barely get a mention, and then only as people who "choose childbearing patterns" (p. 58), in spite of the fact that women's ability and right to make such choices is an area of considerable economic and ethical debate. Racial and ethnic discrimination get no mention at all. To the many people concerned with economics and values who feel that these issues are some of the most important we face today, this book offers scant guidance.

The central project of this book is an important one. Too many government policies and private actions these days are based on the belief that self-interest and even overwhelming greed are morally acceptable within the economic sphere. Challenges to naïve neoliberalism and dogmatic allegiance to unfettered markets are urgently needed. Foley's purpose may have been more successfully accomplished, however, had he more thoroughly pursued the notion of open systems that appears in parts of the book, and been less wedded to a doctrine of class conflict. His passionate call to move beyond self-interest might then have led to more elaboration of what it could mean for economic actors at all levels to consider the "moral relevance of weighing the good and the harm in each case" (p. 226). *Adam's Fallacy* is a noble attempt to deal with the interface of ethics and economics. But by remaining wedded to its own version of theology, it unfortunately fails to take us all the way to where we need to go.

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D Microeconomics

New Foundations of Cost–Benefit Analysis. By Matthew D. Adler and Eric A. Posner. Cambridge and London: Harvard University Press, 2006. Pp. x, 236. $49.95. ISBN 978–0–674–02279–9. *JEL 2007–0045*

With Americans' faith in government nearing an all-time low, Matthew Adler and Eric Posner's *New Foundations of Cost–Benefit Analysis* is a well-timed prescription for increasing efficiency and transparency in government decision making.[1] The authors argue that government agencies should perform cost–benefit analysis (CBA) in determining whether to fund certain projects and describe methods to make such analysis low-cost, accurate, and free of political pressure. Amidst charges that the Environmental Protection Agency (EPA) too hastily okayed the air at ground-zero and that the administration censored statements by the Surgeon General on global-warming and stem-cell research, a book that explores the role of agencies in government could not arrive at a better moment.

The first three chapters define and provide moral foundations for CBA. Consider a proposed government project, defined broadly, such as building a neighborhood park or funding a certain type of cancer research. Agencies must determine each person's "compensating variation," the dollar amount (negative or positive) that would make him as well off as he would be in the status quo. If the cost of the project falls below the "sum of the CVs," then CBA would argue for funding the project. The authors use graphs and figures clearly to provide a fine introduction to those unfamiliar with the topic and, for those more initiated, a lively defense of CBA on philosophical grounds.

The book goes well beyond textbook CBA, and gives readers a historical and practical perspective on the topic. Only after executive orders by the Reagan and Clinton administrations requiring CBA on projects of sufficient cost did agencies have to play by these rules. Economists may ponder how decisions could possibly have been made before but, in other fields, CBA is a more recent and controversial development. And, as the authors acknowledge,

[1] See http://www.galluppoll.com/content/?ci=27946.

CBA is not free—conducting surveys and researching potential survey biases cost money (the authors are especially convincing about the challenges of using so-called "existence values" in determining people's value for preserving environmental wonders, such as the Grand Canyon, that they may never visit). Given the practical challenges of data collection, the authors suggest that CBA should only be performed for projects with potentially large costs and benefits and discuss when instead to use rules of thumb, making the book more than just an academic exercise.

The authors address many potential arguments against CBA with skill and rigor but three objections at least partially survive their defense: first, that political considerations will bias agencies' CBA; second, that CBA, an inherently utilitarian exercise, can produce morally repugnant results if based on the preferences of a prejudiced electorate; finally, that CBA cannot adequately deal with equity issues.

In chapter 4, "Political Oversight," the authors rightly warn that political concerns could threaten CBA. But the principal–agent model they develop speaks only to a very narrow example of politicization: the president (principal) seeks to maximize social welfare, and delegates responsibility to an agency (agent) that is ideologically committed to its regulatory mission even at the expense of social welfare. The agency has specialized knowledge and thus can sometimes convince the president to approve its recommendation. "One of the main virtues of CBA," the authors write, "is that it enhances the transparency of regulation, so that citizens and elected officials can monitor agencies and discipline those that go awry." They thus model CBA as reducing the informational asymmetries between the president and the agency, transferring power to the president.

Such presidential goodwill may strike some readers as overly optimistic, but the authors do not seem troubled: "What if the president and the OMB are captured by industry . . . ? If so, then CBA, by strengthening the president's grip over the agencies, may enable him to reduce overall welfare for the benefit of industry. This strikes us as no more than a theoretical possibility . . ."

For those not entirely convinced that this possibility remains merely theoretical, the courts may represent a more effective check on agency behavior. The authors cite the asbestos-regulation case *Corrosion Proof Fittings v. EPA* (1999) as an example in which the courts rejected an agency's CBA. The court criticized the EPA for, among other mistakes, "discounting only the costs of the regulation and not the benefits," "using an unreasonably high valuation for life," and "calculating costs and benefits over a short period . . . rather than the life of the regulation." Judicial rather than presidential or congressional oversight could enjoy important advantages because of judges' relative political insulation; the *Corrosion* case provides a fascinating example of the role courts might play in checking agencies' behavior.

But the authors devote only two pages to this idea. They speculate that "[a]s long as courts can adequately review cost–benefit analyses, this is an attractive route to take. However, we do not know whether courts have this capacity." It is a bit disappointing that two such talented legal scholars do not dig a little further—their assessment of courts' "capacity" in this respect could guide economists and other scholars who seek to model political processes but for whom familiarity with the courts is not a comparative advantage.

The second objection to CBA is its inherently teleological basis, and the authors attempt to distance themselves from pure utilitarianism by introducing their notion of "weak welfarism," which they lay out in technical terms. "[T]he morally required choice . . . in a given choice situation, depends on the balance of $\{W, F_1, F_2 \ldots, F_m\}$, where W is overall welfare [and] the F_i are other considerations." Agencies should use CBA to calculate "raw" welfare W, but policy should not be based on raw W alone. W has moral weight (as the authors convincingly argue, "[a] choice that benefits someone and harms no one is morally required or at least morally commendable"), but total dependence on W could devolve into a brutish utilitarianism that morally requires someone's death if it provides sufficient enjoyment to others. How do we achieve a balance?

The authors offer a solution that should appeal to economists: division of labor. "[A]gencies should have the minimal task of determining the effect of projects on overall well-being, and different political actors—Congress, the courts—should enforce moral commitments. No one asks agencies to decide whether to implement such 'projects' as abortion legalization and capital punishment. In the political arena, controversial issues are resolved not by CBA but instead by political and moral debate through which people find common ground." I began to envision economists busily calculating a project's raw W, sending it to another part of the pin factory where "different political actors" process raw W—inspect it for morally reprehensible preferences, add in some F_is—and soon an optimal policy comes rolling off the assembly line.

But how does the political process actually enforce moral commitments? How, for example, do we define F_i's that guarantee the rights of groups that the majority dislikes? "Objective badness would be determined by Congress or the OMB at an abstract level. We could imagine, for example, guidelines holding that preferences that are based on animus against racial minorities, women, and homosexuals would not be counted."

But could we not imagine something else instead? Can we realistically expect the political branches to police this fine line? Could a politician that implemented Fis that protected minorities but dissatisfied a majority of voters win reelection? The authors cannot address this scenario because they provide no theory of voter behavior and instead focus on the interplay between agencies and the president (and occasionally Congress and the courts) without ever formally tying their actions to voters' preferences. Thus, in formulating the set over which one must maximize, $\{W, F_1, F_2 \ldots, F_m\}$, the F_i's come from outside the model, which may disappoint readers who accept and understand basic CBA but had hoped to find more rigorous reconciliation of CBA with abstract concepts such as rule of law, natural rights, and equality (the very F_i's the authors treat as exogenous).

The final objection I discuss is CBA's wealth bias. The authors acknowledge that CBA's conversion of utility into dollars discounts the preferences of the poor, as a single dollar translates into greater utility gains or losses for them due to the decreasing marginal utility of wealth. They discuss a possible solution—distribution-weighting, which would weight the compensating-variation values individuals give by some inverse function of wealth—only to reject it. As an example, they consider the decision of whether to build a park in a rich or poor neighborhood, concluding that distribution-weighting would lead to the incorrect decision to put the park in a poor neighborhood, driving up the cost of rent and encouraging landlords to convert apartments to condominiums and sell to the rich. They also warn that providing more amenities to the poor will disincentivize work and savings.

First, placed-based (as opposed to people-based) redistribution has long fallen out of favor with urban economists and policymakers because of exactly the capitalization the authors describe (although recent empirical work questions whether price increases due to the arrival of amenities actually hurt the poor).[2] By picking a place-based project, the authors have set up a feeble straw-man; the decision, say, to fund research for diseases disproportionately affecting the poor would have better illustrated the concerns regarding the redistributive powers of agencies. Second, I find the authors worry about disincentives misplaced. The current evidence on the effect of even direct taxation on taxable income or the intensity of labor-market participation remains mixed, and yet the authors worry that the rich will work and save less because the poor enjoy nice *parks.*[3]

My criticism along these three lines, however, should not lead potential readers away from a fine treatment on an important and complex topic. In general, the book provides a clear description of CBA and economists will find little with which to disagree, though some may wish that the assumptions or implications of CBA had been further challenged. If assigning this book to economists is akin to preaching to the choir, then its final recommendations will leave us quite content as we walk home from church. First, agencies should submit their CBA analyses to peer review (I imagine such review would require economists). Second, agencies should have to use standardized valuations (e.g., they must use a uniform value of statistical life; surely economists will contribute in setting the uniform value). Finally, "[a]nother approach that we recommend is to improve the culture of agencies, to make their staffs more friendly to economic analysis and CBA. Requiring agencies to hire more economists and statisticians

[2] See Jacob L. Vigdor (2007) for a review of the theoretical argument that revitalization "prices out" the urban poor. Empirically, Vigdor finds that increases in prices related to revitalization are often small in comparison to residents' willingness to pay for such amenities, suggesting that the poor may well benefit from urban renewal.

[3] See Jon Gruber and Emmanuel Saez (2002) and the papers cited therein.

and to give them administrative authority may help improve agency culture in this way."

Amen.

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E Macroeconomics and Monetary Economics

Boom–Bust Cycles and Financial Liberalization. By Aaron Tornell and Frank Westermann. CESifo Book Series. Cambridge and London: MIT Press, 2005. Pp. 186. $32.00. ISBN 0–262–20159–3. *JEL 2006–0893*

Introduction

In the late 1980s and early 1990s, under the auspices of the Washington Consensus, a number of developing countries embarked on the path to financial liberalization. Following the East Asian crisis, however, these policy initiatives have come under heavy scrutiny and become the subject of heated debate in policy and academic circles around the world. Proponents of financial liberalization claim that, by allowing capital to flow from rich to poor countries, financial liberalization generates economic growth. Critics of the policy argue that international portfolio equity and debt flows are myopic and flee emerging economies at the first sign of trouble. The crises that ensue impose massive costs, making financial liberalization a hard choice to justify. In the aftermath of these crises, the Washington Consensus has given way to "Washington Confusion" about financial reforms in the developing world (Dani Rodrik 2006).

Boom–Bust Cycles and Financial Liberalization by Aaron Tornell and Frank Westermann is a timely and balanced contribution to the highly polarized debate. The authors argue, quite sensibly to my mind, that the view that financial liberalization is detrimental to economic development ignores the fact that liberalized capital markets may have played an important role in financing growth in developing economies. With this starting point in hand, the authors embark upon an ambitious journey to demonstrate that financial liberalization is a critical ingredient in the recipe for economic growth. At the same time, their analysis attempts to reconcile this with the view that, if not done right, financial liberalization can be accompanied by financial fragility and the occasional crisis. This is no mean feat.

The first part of the book presents a description of stylized facts about macro-variables related to financial liberalization, growth, and crises. The authors begin by documenting a surprising fact—most of the fastest growing developing economies in the last two decades have also experienced lending booms and busts. In comparison to the postwar business cycles in the G-7 nations, the large fluctuations experienced by developing countries with open capital markets and strong credit growth resemble "the Roaring Twenties followed by the Great Depression." By contrast, countries with more stable credit growth have exhibited, on average, the lowest growth rates. Paradoxically, the authors argue, factors that contribute to financial fragility appear to be a source of long-term growth as well.

A natural question thus arises. How can we explain the positive link between long-run growth and the severity of the boom–bust cycles in developing economies? Tornell and Westermann argue that a strong credit channel is a key determinant of both long-run growth and the observed large fluctuations in such growth in economies that undertake financial liberalization. This is the subject of the remainder of the book.

The second part presents a theoretical framework to explain how credit market imperfections prevalent in the liberalizing economies can account for the observed empirical patterns. The authors then go on to examine the normative question of whether financial liberalization is a good idea even if it leads to financial fragility. The final part provides microeconomic evidence of the credit market imperfections that drive the results arising from the theoretical framework.

Sectoral Asymmetries and the Transmission Mechanism

Tornell and Westermann make an important point—fluctuations and crises in emerging markets are better understood as sectoral phenomena. Their goal is to disentangle these sectoral effects to examine transmission mechanisms that generate boom–bust cycles in emerging markets. They argue correctly that aggregate data mask these patterns.

The authors focus on a fundamental asymmetry between the tradable and nontradable sectors to make their case. While the tradable sector can finance itself on international financial markets, the nontradable sector has restricted access to international finance before liberalization. Financial liberalization provides firms in the nontradable sector with access to international capital. The easing of credit constraints allows the nontradable sector to grow. As a result, the demand for the tradable sector goods also goes up and an economic boom results.

To explain economic busts within the same framework, the authors argue that, in emerging markets, institutional problems abound. Importantly, in the nontradable sector, contract enforceability problems ("bad markets") and systemic bailout guarantees ("bad policy") lead to excessive credit risk taking and currency mismatch problems (dollar denominated debt and local currency revenues). To show this, they consider the impact of an adverse domestic interest rate shock. The tradable sector is insulated from the shock because it has continued access to international capital markets. In the nontradable sector, however, net worth constraints kick in and are exacerbated by the adverse impact of currency mismatch on firm balance sheets. Credit dries up, the nontradable sector contracts, domestic demand for tradable goods falls, and we have a bust.

A key aspect of the amplifying mechanism in *Boom–Bust Cycles and Financial Liberalization* is that adverse domestic interest rate shocks are exacerbated by currency mismatches on nontradable firm balance sheets. Nontradable firm assets are denominated in the local currency, whereas their liabilities are denominated in a foreign currency. It is worth noting that foreign currency-denominated liabilities suggest that these firms are borrowing at some international interest rate with, perhaps, a premium attached.

While the adjustment mechanism described in the text is successful in explaining booms, it is, perhaps, a little less successful in delivering busts. The mechanism that delivers the credit boom and an increase in economic growth relies on domestic firm access to international capital markets. The credit bust and decrease in economic growth, in contrast, are triggered by an increase in the domestic lending rate that has an adverse effect on the nontradable sector's net worth, credit constraints kick in and the sector contracts. Following financial liberalization, how do domestic interest rate shocks affect firms (tradable or nontradable) that are borrowing at foreign-currency-denominated interest rates?

Relying on the domestic interest rate to trigger the bust suggests that there is some capital market imperfection that restricts nontradable firm access to international capital markets. If, for instance, domestic banks tap into international capital markets following financial liberalization and provide credit to nontradable firms at a domestic interest rate (and not a foreign currency-denominated rate), then, following a domestic interest rate hike, net worth constraints kick in for nontradable firms, the nontradable sector contracts and so on. In this case, however, a dilemma arises. It is hard to explain a currency mismatch on nontradable firm balance sheets if these firms' borrowings are denominated in the local currency. Alternatively, if nontradable firm debt is foreign currency denominated and at an international borrowing rate albeit with a premium, and capital-markets are open, a currency mismatch can be generated but not a role for the local currency-denominated domestic lending rate.

In a situation where domestic banks can lend at both a domestic lending rate and a foreign-currency-denominated rate (as is the case in many emerging markets), it would be useful to know what the authors have in mind when it comes to a shock to the domestic interest rate. This is particularly important since the domestic interest rate shock is such a key part of the adjustment mechanism that triggers the bust. It is also worth noting that, following financial liberalization, the differential between the local-currency-denominated lending rate and the foreign-currency-denominated rate, is the expected devaluation risk.

In East Asia, for instance, domestic banks borrowed heavily from international banks on a short-term basis and lent to domestic firms, to the extent that domestic banks lent to nontradable firms, and took on a fair amount of exchange rate exposure since firm borrowings were dollar-denominated while their revenues were in the local currency. Domestic banks also had a maturity mismatch (short-term borrowing and long-term lending) on their hands. During the crisis, foreign investors pulled out of domestic stock markets, foreign banks stopped rolling over the short- term loans, and exchange rates collapsed.

With restricted access to international capital markets, domestic interest rates rose, exacerbating the currency and maturity mismatch on the banking sector's balance sheets, credit dried up, and the nontradable sector suffered disproportionately during and in the aftermath of the crisis. Note that it is the real exchange rate devaluation that increased the domestic currency value of foreign currency-denominated debt and weakened nontradable firm balance sheets, resulting in borrowing constraints. The tradable sector, on the other hand, suffered less to the extent that it had continued access to international capital markets. In fact, theory suggests that tradable firms become more profitable if the sector gains a competitive advantage from the exchange rate devaluation.

Equity and Debt Market Liberalizations

It would have been helpful if the authors had clarified the definition of financial liberalization more explicitly. Financial liberalization encompasses a wide range of policy measures, ranging from policies that put an end to domestic financial repression in the form of interest rate ceilings to the removal of barriers to international capital flows through various forms of capital account liberalization. The relationship between different financial liberalization measures and their impact on the credit channel proposed by Tornell and Westermann is not obvious.

To see this, observe that while equity and debt market liberalizations fall under the umbrella of financial liberalization, they can act upon the expansion of credit and economic growth through very different channels. The empirical analysis in *Boom–Bust Cycles and Financial Liberalization* uses equity market liberalization dates as an indicator of changes in openness. However, the theoretical model the authors offer relies on international debt markets, domestic borrowing constraints for nontradable firms, and domestic interest rate shocks to generate the booms and busts in credit and economic growth. Thus, the theoretical link between the adjustment mechanism and equity market liberalizations is not immediately apparent.

Equity market liberalizations allow foreign investors to hold and trade stocks on domestic stock markets. Inflows of foreign capital following the liberalization of domestic stock markets drive down the cost of capital for all firms in the economy by driving down the domestic interest rate. As the cost of equity falls, capital becomes cheap and investment goes up leading to economic growth, so an economic boom can be explained within this framework. Following equity market liberalizations, however, the cost of equity is pinned down by international benchmarks—based on a world risk-free rate and an international risk premium rather than the domestic interest rate (Rene M. Stulz 1999). It is not clear where a domestic interest rate shock that drives the bust according to Tornell and Westermann figures in this picture.

Conclusion

In conclusion, I found the book to be an extremely informative read and one that provides a unique perspective on the controversies surrounding financial liberalization in developing countries. The authors do a service to the profession by documenting key stylized facts to inform the debate. In doing so, the authors make the important point that countries that undertook financial liberalization grew faster than those that did not. The authors pursue a laudable goal in trying to disentangle the asymmetries in the sectoral responses to financial liberalization and generating both booms and busts within a unified theoretical framework. I recommend this book as a must-read for graduate students and researchers in international and development economics.

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F International Economics

Measuring the Restrictiveness of International Trade Policy. By James E. Anderson and J. Peter Neary. Cambridge and London: MIT Press, 2005. Pp. xvii, 320. $37.50. ISBN 0–262–01220–0. *JEL 2006–1301*

Despite the substantial reductions in tariff and other barriers to international trade that have been negotiated and enacted over the past sixty years since the inception of the General Agreement on Tariffs and Trade (GATT), distortions to international trade continue to exist. Indeed, there have been recent worrying signs that perhaps the post–World War II trend to free trade may not only have stalled but reversed. The lack of progress on the latest Doha Round of trade negotiations being organized by the World Trade Organization (WTO), which replaced GATT, is of particular concern. There has also been a spate of preferential trade agreements (typically called free trade areas but often stopping far short of free trade), which embody discriminatory trade policies that go against the most favored nation concept of applying the same tariffs to imports from each nation. Given this continued persistence of substantial trade barriers, the book by James Anderson and Peter Neary on the measurement of the restrictiveness of international trade policy is apposite.

Anderson and Neary propose two main measures of the restrictiveness of international trade policy for an economy, which (in the first instance) is assumed to be perfectly competitive and to have duties imposed on its imports. The first is the trade restrictiveness index (TRI), which the authors define as the uniform ad valorem tariff rate that, if applied to all imports, would achieve the same level of welfare (in a single consumer model) as the initial equilibrium with an arbitrarily given tariff structure. An advantage of this TRI concept is that it is intuitively appealing, with the restrictiveness of tariffs being reduced to a single tariff rate that could be actually used as a trade policy instrument. The authors argue that this is an appropriate measure if welfare is the primary concern and that existing measures of average tariff rates with import shares as weights are deficient in important respects.

The second main measure of trade restrictiveness is dubbed the Mercantilist trade restrictiveness index (MTRI), designed to appeal to trade policy negotiators, policy analysts, and advisors for whom market access and trade volumes are the variables of interest rather than consumer welfare. In this case, the value of a country's imports (the goods subject to tariffs), where (importantly) the valuation of the import quantities uses world prices, is taken as the measure of market access. The MTRI is defined by the authors as the uniform tariff rate that, if applied to all imports, would result in the same level of market access (world price value of imports) as occurs in the initial equilibrium.

The book is devoted to explaining and developing the theoretical properties of these two concepts, arguing the advantages of them over existing measures in the literature, extending their applicability to a range of more general situations than considered in the introductory chapters and, importantly, discussing how the concepts can be applied to real world economies.

The book is divided into three parts. The first part comprises just one chapter designed to provide a nontechnical discussion of the two trade restrictiveness indices. It does this through partial equilibrium supply and demand curve diagrams for two goods, supplemented by some graphs of numerical simulations. This chapter effectively provides the essence of the arguments to come later in a duality-based, general equilibrium analysis that continues throughout the rest of the book. Those wanting only partial equilibrium analysis will have to put the book down at the end of this chapter.

Part 2, comprising eight chapters, develops the theory of the measurement of trade restrictiveness in considerable detail. The authors devote the first chapter of this part to a review of the theory of tariff policy in a small open economy with a single consumer. They develop the duality technology (expenditure and revenue functions) that have become accepted tools used by trade theorists and that are used throughout the book, and summarize the main results concerning the welfare effects of radial tariff reductions and the concertina tariff reform (whereby the largest ad valorem tariff is reduced).

The TRI and the MTRI are defined and analyzed in the next two chapters. Although each concept is defined as a solution of an equation for a uniform tariff rate, the authors show that the concepts, in rates of change form, can be expressed as a weighted average of tariff rates. They stress that the weights are not the import share weights used in the calculation of the weighted average tariff rate that is commonly applied in the literature, since they account for substitution in both production and consumption as a result of the tariffs. In contrast, the usual formula typically gives low weight to goods with very high tariffs since they are likely to have small import volumes. Therein lies the main criticism that Anderson and Neary direct at the standard, accepted measure of trade restrictiveness.

The next chapter provides results that are likely to appeal to many policy analysts. In this chapter, the authors show that the rate of change in the TRI can be completely described as a function of changes in just two "sufficient statistics." These are the generalized average tariff rate, which measures the level of the tariff structure, and the generalized variance, which measures the dispersion of tariff rates around the generalized tariff rate. The weights are, not surprisingly, provided by the elements of the net substitution matrix. (It is worth noting that the welfare effects of *any* tariff reform can be expressed in terms of the two tariff change measures, that is, the change in the generalized average and generalized variance of tariff changes.)

The remainder of part 2 is concerned with showing how the two indices can be developed in alternative economic environments. Given the importance of import quotas, two chapters are devoted to them. Again, the theory of the welfare effects of quota reforms is surveyed (a very valuable exposition in its own right) and then the TRI is applied. Other environments covered are the case of large countries, external economies of scale, monopolistic competition, and a multicountry world.

Those empirically and policy oriented readers who got the essence of the theoretical properties of the TRI and MTRI from part 2 and are keen to see applications in part 3, entitled "Applications," will be perhaps a little disappointed. Most of part 3 comprises more theory, albeit theory concerning applications. Apart from chapter 15, the overall flavor of part 3 is one of extensions of the theory with some illustrative applications, rather than one of detailed applications to real world situations for practitioners and applied policy analysts. To get into the actual applications, the reader must first digest considerable theoretical preparations. Nevertheless, applications are included—to Mexican agricultural policy, U.S. agricultural policy, the multi-fibre agreement, U.S. dairy quotas, and to twenty-five countries in chapter 15.

Part 3 comprises six chapters, each devoted to a particular extension or issue regarding the application of the TRI and MTRI measures. Chapter 12 deals with distortions other than tariffs and quotas on trade, including import subsidies and export taxes as well as tax distortions in the domestic goods and factors markets. Chapter 13 then turns attention to what the authors call alternative reference points. By this is meant that the essence of the TRI and MTRI measures can be constructed to focus on objectives other than utility, such as sectoral factor incomes or value added, which leads to a excursion into effective rates of protection. In chapter 14, Anderson and Neary point out that the TRI and MTRI can be defined as quantity measures rather than price (tariff rate) measures that occupy the rest of the book. They develop their quantity measures, using distance functions, and relate the resulting TRI to Debreu's well known "coefficient of utilization," which measures the distance of an economy from the boundary of the efficiency set. In this context, the quantity version of the TRI is a uniform reduction in existing quotas that yields the same utility as some initial equilibrium with a reference set of quotas.

Perhaps the most useful of the chapters of part 3 is chapter 15, in which the authors apply the standard versions of the TRI and MTRI to computable general equilibrium models for twenty-five different countries and compare these results with calculations of the average level of tariffs. The model, common in structure to all countries, comprises a household that consumes a nontraded good and finished imported goods according to a constant elasticity of substitution (CES) utility function, and a production sector that uses a CES aggregate of domestic factor and imported inputs to produce the nontraded good and exports according to a constant elasticity of transformation technology. There are both tariffs and quotas on imports, with revenues being distributed to the consumer as a lump sum. As readily admitted by the authors, this application is subject to many data and specification limitations and so the results should be regarded as being illustrative rather than definitive. Nevertheless, this application is very useful for putting the theory into a realistic empirical context. Moreover, conveniently for readers and would-be users, the authors have made the documentation and the Excel spreadsheet that computes the solutions for the model available on the web.

The main conclusions drawn from the results presented are that the TRI substantially exceeds the MTRI, which, in turn, usually exceeds the weighted average tariff rate (with import shares as weights), and that there is a fairly strong rank correlation between the TRIs and the average tariffs. While this latter outcome may give comfort to users of the standard weighted average tariff measure, what is striking is that there are many instances where the rankings are not consistent and the TRI is unusually high relative to the MRTI and the average tariff rate. Thus, a puzzling (unanswered) question arises as to why countries such as Indonesia, Austria, Malaysia, Morocco, and Finland, for example, have particularly high TRI measures compared to their MTRI and average tariff measures. Another important aspect of the results is that the rank correlations become much lower for year-on-year changes in the trade restrictiveness indices. In summarizing the evidence provided by the calculations and analysis of the trade restrictiveness measures for the twenty-five countries, the authors conclude, with caveats, that "the conventional measures are likely to be very seriously misleading in practice."

The book is well written and well structured. Nevertheless, I do have two comments about it contents. Firstly, the book perhaps tries to include too many topics. My feeling is that the main message could have been presented in a more streamlined way, possibly at a cost of being less comprehensive in coverage. For example, though they are relevant, I wondered about the need to delve into gravity models, implicit separability and aggregation, and effective tariffs. Readers can, of course, self select the chapters to read. Most readers will probably be satisfied with chapters 1–8, which set out the theory for tariff and quota policies and the appropriate TRI and MTRI measures for these, and then jump to chapter 15, which provides applications to twenty-five countries. The remaining chapters can then be read on a needs basis. Secondly, I would have liked the application part to have also included a chapter on the detailed application of the book's methods to one country. This would have helped the reader get a feel for how to deal with a variety of applied and theoretical issues in the context of how to actually set up the model structure, obtain the necessary data, compute the trade restrictiveness measures and to interpret them for a real open economy.

Overall, Anderson and Neary have provided the profession with an excellent account of the issues and problems in measuring the restrictiveness of international (and other) trade policies employed by countries. They are very critical of the standard measure obtained as the weighted average of tariff rates, since it does not take account of substitution in production and consumption arising from the trade distortions. The authors' TRI and MTRI measures do take account of these and provide easily interpreted scalar measures (uniform tariff rates) that are well founded in economic theory. While the theoretical case seems clearly in favor of the authors, practitioners may be less persuaded due to the cost of theoretical correctness compared to the ease of computing the traditional weighted average tariff rate. Computation of the TRI and MTRI requires detailed quantitative knowledge of the substitutions in production and consumption coming about because of the distortions, and this requires knowledge of the structure of the technology and preferences at least over the relevant subsets. That is, these measures are informationally demanding. To date, computable general equilibrium models of open economies (such as used by the authors in their several applications) typically impose very restrictive structure—both functional form and parameter values—rather than having the structure come from detailed empirical evidence. However, the appropriate response is not to revert to the weighted average tariff (though it doesn't perform poorly in some respects, it seems), but to direct effort to providing the required empirical estimates of disaggregated preferences and technology sets.

The book will appeal most to those international trade economists who have a very good grounding in modern trade theory based upon general competitive equilibrium and the principles of duality. The measures the authors propose can best be developed and analyzed in such a framework. Certainly all graduate students in international trade should be exposed to this book, providing as it does the background on the welfare economics of tariff and quota reform before developing the TRI and MTRI measures of trade restrictiveness and finally some empirical applications. Trade policy practitioners will also benefit from this book, and should find that the payoffs from working through the essential theoretical parts are worth the effort. Overall, the authors have convincingly argued the case that quantitative measures of trade restrictiveness should be based upon sound economic theory. It remains for practitioners to further develop the empirical evidence to improve implementation of these measures and for them to become part of the information base for policy reforms.

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Multinational Firms, Innovation, and Productivity. By Davide Castellani and Antonello Zanfei. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2006. Pp. xii, 249. $100.00. ISBN 978–1–84542–198–4. *JEL 2007–0092*

When business writers and consultancies advocated their management-by-buzzword recommendations of the 1980s and 1990s, expatriats at multinational firms were mocked for management by helicopter: expats would be flown in by surprise, swirl up much dust upon landing, and be lifted out for replacement before the dust had settled. In their monograph, *Multinational Firms, Innovation, and Productivity,* Davide Castellani and Antonello Zanfei don't shy from buzzwords either, of the economic sort in their case. Multinational firms (MNFs) are "asset-seeking" or "asset-exploiting" or both, they form "double networks" to realize their "exploration potential," and they "bridge innovation systems" through their "embededness" against "cultural resistance," the "liability of foreignness" and the "incompatibility of knowledge." But Castellani and Zanfei emphasize the long-lived links that persist after the expat dust has settled. Castellani and Zanfei point to knowledge transfers and innovations that MNFs might be uniquely fit to pursue and implement across their locations.

In the first chapters, Castellani and Zanfei march through the topic in bold steps. Select theories serve as landmarks for orientation, and potentially conflicting evidence is cleared from the way, mainly to arrive at the authors' descriptive notion of MNFs as double networks: internal networks of affiliated companies and external networks of suppliers, clients, and cooperation partners. Given the unavoidable omissions under the authors' fast moves, this reader expected them to head toward a similarly bold ultimate thesis as a reward. Along the way, an implicit proposition seemed to emerge: that today's MNFs tend to seek otherwise inaccessible or unexploited foreign knowledge assets in order to integrate the innovation throughout their multinational networks to their own competitive advantage. Starbuck's integration of local hot-drink specialties and sweets from around the world into its global standard menu might be the emblematic example—admittedly outside the authors' focus on manufacturing. But the authors stop short of such a clear stance. Instead, Castellani and Zanfei stress firm-level diversity as an overwhelming feature of the data and document, for instance, how productivity responses vary across Italian manufacturers depending on their export status, their contractual cooperation with local firms, and their ownership by foreign or Italian MNF parent companies. It is unifying theory behind the heterogeneity, however, that this reader most longed for. How much foreign direct investment is asset seeking and under what conditions do MNFs choose what form of network?

Stimulating discussion awaits the reader in part 2 of the monograph with a synopsis of alternative strands of research into firm-level heterogeneity and its relationship to market structure and international integration. Recent economic research emphasizes, as a cause of multinational expansion, the heterogeneity in firm-level productivity—productivity being the economist's common proxy to what management researchers would call a source of competitive advantage (Richard R. Nelson 1991). Castellani and Zanfei contrast this idea with the viewpoint that a firm's pursuit of competitive advantage is an outcome of its exposure to competitors' internationalization strategies and improved access to foreign markets. Castellani and Zanfei consequently appeal to the economist that "one should consider that firms' international involvement can further reinforce their advantages and hence contribute to generating heterogeneity" (Castellani and Zanfei 2006, p. 86). To broaden the concept of competitive advantage, Castellani and Zanfei use innovation measures alongside with, and as distinct from, productivity in most of their empirical exercises in part 3. Data limitations, however, do not allow the authors to follow through on the theoretical view that firm-level productivity and its distribution endogenously respond to the competitive environment.

Congressman Charles Rangel, the ranking Democrat on the House Ways and Means Committee, recently joked that U.S. legislation encourages MNFs to send "everything but their mailboxes overseas." Mailbox communication maybe the reason why merely 13 percent of the managers at MNFs in 1997 found that their companies were "adept at transferring knowledge held by one part of the organization to other parts" (Rudy Ruggles 1998, p. 81). Castellani and Zanfei, in contrast, provide evidence that at least Italian MNFs are getting better at the knowledge transfer.

The economic rationale by which MNFs enter contractual relationships with suppliers, clients, and cooperation partners, and by which they choose to own affiliates, are closely related to our understanding of the boundaries of the firm. Much research lies ahead to clarify the emergence of firm boundaries, but existing theories of transaction costs, contractual imperfections, and property rights offer valuable guidance. Recent models of MNFs use this guidance, identify frictions to production and exchange, and show how firms can alleviate the frictions through contracts and property-rights assignments. A strength of Castellani and Zanfei's exposition is the integration of many strands of literature, ranging from business surveys and case studies of management strategies to the economic literatures on international trade and investment. Surprisingly, however, none of the usual explanations for firm boundaries—Benjamin Klein, Robert G. Crawford and Armen A. Alchian (1978), Oliver E. Williamson (1985), Sanford J. Grossman and Oliver D. Hart (1986), or Hart and John Moore (1990)—makes it into the monograph's discussion or reference list. Its otherwise broad view on many literatures notwithstanding, the monograph leaves out the drama of case-oriented business books and the anecdotes of journalistic work. Instead it traces economic thought on firm-level competitive advantage to its early origins and presents an informed assessment of empirical evidence on productivity and innovation at multinational firms and their local competitors.

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Global Imbalances and the Lessons of Bretton Woods. By Barry Eichengreen. Cairoli Lecture Series. Cambridge and London: MIT Press, 2007. Pp. xiv, 187. \$26.00. ISBN 978–0–262–05084–5. *JEL 2007–0485*

The international monetary system, with its often implicit rules of the game governing how nations interact, continually evolves through time. Nevertheless, history remains particularly important for understanding today's huge global imbalances. Economists have failed rather dismally to construct convincing theoretical models of why the international dollar standard has led to seemingly endless U.S. current account deficits that are sustained by the seemingly endless willingness of the rest of the world to acquire liquid dollar assets.

In his engaging and often insightful book, Barry Eichengreen draws parallels with the last major period of dollar predominance during the Bretton Woods of fixed dollar exchange rates, beginning in 1950 until its breakdown with the Nixon shock in 1971, and with sterling's predominance before 1914. He also carefully points out the differences between these two periods and the present one—often with illustrative data and summary figures. The book is divided into four chapters:

1. Global Imbalances and the Lessons of Bretton Woods
2. The Anatomy of the Gold Pool
3. How to Exit a Currency Peg: Japan and the End of the Bretton Woods Period (with Mariko Hatase)
4. Sterling's Past, Dollar's Future

Eichengreen notes that, through most of the Bretton Woods period of fixed dollar exchange rates, the United States ran current account surpluses. He also makes the often- overlooked point that today's large current account deficits may well be endogenous to the way in which the dollar standard now works. When most new issues of treasury bonds can be easily lodged with foreign central banks, the U.S. federal government's borrowing constraint is softened. Fiscal deficits generate no political pain because interest rates stay low so as not to squeeze out domestic private borrowers. Nevertheless, Eichengreen remains fundamentally pessimistic over whether high U.S. deficits can continue without eventually triggering a run on the dollar that forces a fundamental change. But, being a good economic historian, he carefully qualifies any predictions.

As a reviewer, however, I would be remiss—not to say utterly boring—if I did not dissent from some of his historical analogies. He sees the ultimate failure of the London gold pool to prop up the fixed-rate dollar–gold exchange standard of the 1960s as a portent for the absence of collective foreign support during a crisis in the current phase of the dollar standard—sometimes called Bretton Woods II. He treats Japan's exit from the fixed-rate dollar standard in 1971 as "successful," with useful lessons for China exiting from its current pegged dollar exchange rate. And in several places he theorizes implicitly and incorrectly about the role of the exchange rate in the adjustment process. Let us discuss each issue in turn.

Analyzing the long-neglected London Gold Pool of the 1960s is the most novel part of the book. Under the Gold Pool, created in 1961, Belgium, France, Germany, Italy, the Netherlands, Switzerland, the United Kingdom, and the United States agreed to regulate the London open-market price of gold to help the United States maintain its commitment, under the Bretton Woods Agreement of 1945, to sell gold to foreign central banks or other official institutions at $35 per ounce.

Eichengreen explains why such a cartel-like pool was necessary. In the post–World War II era of very rapid economic growth, the demand for official exchange reserves also grew rapidly—and initially, in the 1950s, demand was largely satisfied by foreign governments voluntarily accumulating dollar bank accounts or U.S. Treasury bonds. However, by 1965, official dollar reserves owned by foreigners exceeded the U.S. government's gold stock, thus creating the famous Triffin dilemma. Any individual foreign government might prefer to hold on to its dollar reserves because they bore interest and were highly liquid for international transacting. However, if other governments bought gold from the U.S. Federal Reserve at $35, and so exhausted the Fed's stock as to drive the United States off its gold convertibility commitment, the market price would jump above $35. Unless the foreign government in question also joined the rush to buy gold, it could forgo capital gains on its existing gold stocks as well as on new purchases.

To mitigate this dilemma, Eichengreen characterizes the London Gold Pool as a collective agreement among central banks to constrain gold conversions by its European members. If the London free market price of gold tended to rise above $35, as when newly mined gold was in unusually short supply, then, using the Bank of England as their agent, members of the gold pool would sell gold—with pro rata shares assigned more or less by economic size—to drive the open-market price back down again. And if "excess" gold appeared on the London market, which would threaten to drive the price below $35, the Bank of England would buy gold and distribute it to the cartel members using the same pro rata shares. As long as confidence in the mechanism was sustained, the incentive of any cartel member—or other central banks outside the cartel—to "defect" by demanding gold from the U.S. Federal Reserve at $35 per ounce was minimized.

But not eliminated. Each participating government could, outside the London market, still use dollars to buy gold at $35 directly from the U.S. Federal Reserve Bank. And as dollar holdings of foreign governments continued to grow, they eventually did just that. Eichengreen clearly documents the gold rush that began in 1967 through

1 April 1968, when the gold pool was dissolved. By that time, the U.S. gold stock had fallen to just $11 billion, which was less than half of what it had been in 1950. Afterwards, the U.S. government became much more restrictive with official conversions and the free-market price of gold rose well above $35. This foreshadowed the complete closing of the U.S. gold window in August 1971, when President Nixon also demanded that the dollar be devalued against the currencies of all the major industrial countries.

Eichengreen sees the ultimate failure of the Gold Pool to rescue the fixed rate dollar standard of the 1950s and 1960s as a harbinger of the fragility of the current version of the dollar standard. Emerging markets in Asia and Latin America peg—albeit softly—to the dollar while building up large stocks of dollar exchange reserves. “Still, the Gold Pool reminds us that governments seeking to prevent a fall in the currency issued by a country that is their principal export market face a problem of collective action. . . . and sustaining cooperation can be problematic” (p. 71).

Against Eichengreen’s historical analogy, however, the dollar–gold exchange standard of the 1960s had flaws, both legal and conceptual, not present in today’s version of the dollar standard. First, from the Triffin dilemma, the American commitment to convert dollars into gold at $35 per ounce gave speculators, including some central banks, a one-way bet that the future price of gold would be greater than $35. In effect, gold became superior to the dollar as an international store of value. At best, the London Gold Pool could only defer an inevitable run on the dollar. In contrast, under today’s pure dollar standard, there is no obviously more fundamental store of value into which speculators can fly.

Second, accepted macroeconomic doctrine is different today. In the 1950s into the 1970s, most economists suffered from the great Phillips curve delusion. If an economy systematically accepted more inflation, they believed that it would run with systematically lower unemployment. In practice, when inflation in the United States began edging up in the late 1960s, the U.S. government in general, and the Federal Reserve Bank in particular, was conceptually inhibited by fear of rising unemployment from taking decisive action to disinflate. Instead, to restore America’s international competitiveness, they called for the dollar to be devalued—to which President Nixon acquiesced in August of 1971 while continuing with an expansionary monetary policy. The great inflation of the 1970s, with high unemployment, is sad testimony to the effect of the Phillips fallacy on public policy.

After gold was virtually demonetized in 1968, the Phillips curve delusion also foreclosed the option of continuing with the system of fixed dollar exchange parities. For such a pure dollar standard to work without a gold base, confidence in the center’s commitment to price level stability is paramount—but not so in the inflationary 1970s.

Another aspect of the Philips curve delusion was the idea that each country might have a different preferred trade-off between inflation and unemployment. If Britain preferred to run with high inflation and low unemployment, whereas Germany wanted to reduce inflation at all costs even if it meant living with higher unemployment, why should they be bound together under a regime of fixed exchange rates that inhibits them from carrying out their differing domestic policy objectives? Now when respectable central banks strive for price level stability, such arguments seem naïve. However, they didn’t seem so before the collapse of the Bretton Woods dollar parities.

The present form of the dollar standard—where countries in Asia and Latin America “softly” peg to the dollar to anchor their national price levels—has somewhat more robust doctrinal underpinnings than the fixed rate dollar standard of the 1950s and 1960s. (Europe and the small countries to its East that are more on a euro-centered regime.) The peripheral countries can now have more confidence that the center country is at least striving for price-level stability—even if it doesn’t always manage it. And if the United States does succeed in achieving virtual price stability in the dollar prices of tradable goods on a worldwide scale, then most peripheral countries are willing to take this as a norm for their domestic price-level targets. Indeed, because countries in Asia with large current account (saving) surpluses finance them by building up dollar claims domestically, they are loathe to permit exchange appreciations that would immediately impose large capital losses on the domestic holders of dollar assets, and eventually impose economy-wide deflation—as in Japan twenty years ago.

Thus Eichengreen's view that, in the new millennium, collective action will be required to prop up the foreign exchange value of the dollar—and, in crisis situations, that such collective action is likely to be found wanting, given the ultimate failure of the London Gold Pool in the 1960s—is too pessimistic. As long as the U.S Federal Reserve keeps the purchasing power of the dollar fairly stable, and the dollar is widely used in invoicing foreign trade and as a clearing currency among banks, each peripheral country in Asia and Latin America has incentives to stabilize its dollar exchange rate without acting collectively. Dollar exchange reserves remain attractive because there is no one-way bet on the future rise of the price of gold or any other more fundamental international asset. So the current pure dollar standard is *financially sustainable* although it may yet prove *politically unacceptable* in the United States itself.

The current threat to the dollar standard comes from political factions within the United States wanting to devalue the dollar, not from foreign governments threatening to withdraw their support for the dollar as Eichengreen would have it. Why should the United States, with a virtually unlimited line of easy credit from the rest of the world, find the current situation to be politically unacceptable? The problem is that the process of transferring resources from the rest of the world creates tensions within the American economy itself.

What is the transfer mechanism? In order to transfer real resources from the rest of the world (apart from surplus-saving oil-producing emirates) to cover America's saving deficiency, the United States runs very large trade deficits in manufactures from surplus-saving industrial economies such as China, Japan, smaller ones in East Asia, and Germany. This real transfer of manufactures needed to cover the shortfall in American saving speeds the contraction in employment in U.S. manufacturing beyond the natural rate of decline experienced by other mature industrial economies.

The upshot is a protectionist backlash in the United States, particularly by members of Congress with manufacturing constituencies. Instead of blaming America's own deficient saving, which makes foreign borrowing necessary, American politicians incorrectly blame "unfair" foreign trading practices—undervalued currencies, substandard labor practices, dumping of subsidized exports in American markets, and so on. Rather than any general collapse in America's credit line with the rest of the world as Eichengreen would have it, this protectionist backlash in the United States is the serious threat to the world economy in general, and to the dollar standard in particular.

Currently, this backlash is taking the form of U.S. Congressmen fashioning legislation to force China—and perhaps even Japan again—to appreciate its currency. However, contrary to a widely held belief within the economics profession, devaluing the dollar is itself no panacea for correcting the savings (trade) imbalances across countries. Unlike what the old and familiar elasticities model of the balance of trade would suggest, having creditor countries like Japan or China appreciate against the dollar would have no predictable effect on their trade surpluses. In effect, their savings surpluses (or the American saving deficiency) need not be corrected if the dollar is devalued. But at several points in his book, Eichengreen theorizes (implicitly) that appreciations of the Asian currencies are necessary and inevitable to reduce their export surpluses.

Although failing to "correct" global trade imbalances, any such major change in the dollar's nominal exchange rate could create serious *monetary* imbalances in the world economy: deflation in the appreciating countries or inflation in the United States. The trade-off between the two is somewhat arbitrary, but the effect of nominal exchange rate changes on the "real" exchange rate eventually washes out.

Eichengreen spends a full chapter on what he considers to be Japan's successful exit in 1971 from the Bretton Woods system of fixed dollar parities—with possible lessons for China at the present time. Both countries, Japan in the 1950s and 1960s, and China 1994 to 2005, had been enomously successful by using fixed dollar exchange rates as anchors for their domestic monetary policies during parallel eras of extremely rapid economic growth and incompletely liberalized capital markets. But Eichengreen takes it as self-evident that exiting to an independent national monetary policy—for Japan then, and China now—is to be preferred. Because of outside American pressure, exiting is not (was not) a free choice in either case. But we can back cast historically to reexamine the consequences for Japan.

Japan suffered a loss of monetary control in 1971–73, along with the other industrial countries, because of a massive outflow of capital from the United States from anticipated depreciation of the dollar. Similarly, China faces large inflows of hot money at the present time. But, for the rest of the 1970s, the upward "float" of the yen (there was another big forced appreciation in 1977–78) insulated Japan somewhat from the unwanted high inflation in the United States. However, because Japan had been forced into appreciating, this was a lucky accident.

The big problem for Japan came in the mid-1980s with the Plaza Accord which forced another big appreciation of the yen *after* the U.S. price level had been more or less stabilized. Expectations of further (forced) appreciations then contributed to the bubble economy of the late 1980s, and, by the mid-1990s, pushed short-term interest rates toward zero, the dreaded liquidity trap. Strong deflationary pressure then developed as the bubbles burst and the yen continued to appreciate through 1995. The upshot was Japan's "lost decade" of 1992 to 2002, during which its trade surplus remained large despite the very high yen. So Japan's exit from the fixed exchange rate regime ultimately brought economic disaster.

What should the authorities have learned from this unfortunate episode? If credible, a system of stable nominal exchange rates against the center country's currency protects creditor countries on the periphery from being exposed to unwanted mercantile pressure to appreciate from the United States—which is in thrall to false theory that devaluing the dollar is the way to "correct" the U.S. trade deficit. Because repeated appreciations of the renminbi will fail to reduce China's (saving) current account surplus, China may eventually be forced down the same deflationary road as Japan was.

But my quibbles with some of Eichengreen's conclusions should not detract from the fact that this excellent book is a must read for economists and historians of how the international monetary system has evolved through time. The book is accessible to a wide variety of readers, and should be particularly valuable to young economists whose up-to-date technical training often obliterates historical perspective.

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U.S. National Security and Foreign Direct Investment. By Edward M. Graham and David M. Marchick. Washington, D.C.: Institute for International Economics, 2006. Pp. xxvi, 190. $17.95, paper. ISBN 0–88132–391–8.

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Indirect economic policy measures are used to address national security concerns when investment in direct security measures is financially or otherwise infeasible. Indirect measures can include restrictions on trade, foreign investment, and various types of economic activity. An obvious problem here is that policy formulation is quite often captured by protectionist lobbies. To restrict competition, a domestic firm may lobby against the takeover of a competing firm in the same industry by a foreign firm which is much stronger economically and technologically. This is especially a problem when national security is very loosely defined, and when politicians become actively involved in the evaluation of the security implications of allowing any kind of trade or international investment flows. This is exactly the point made by the book on U.S. national security and foreign investment by Edward Graham and David Marchik.

The book by Graham and Marchik focuses on the national security impact of foreign direct investment (FDI) that has taken the form recently of mainly mergers and acquisitions. It also looks at how such mergers and acquisitions are regulated by the government, and analyzes in detail the problems associated with the current institutional set up for the evaluation of the security implications of individual FDI projects. Furthermore, the book evaluates the changes in the current set up that have been proposed and are being attempted by the U.S. Congress. It also proposes reforms in the existing institutions and policies. Graham and Marchik argue that this entire issue is very important due to the positive economic effects of FDI through the good, high paying jobs it creates and its positive effects on R&D, technological spillovers, and growth. The authors do a good job of documenting all this through tables and figures showing basic statistics and through a survey of existing econometric research on this subject. They thus argue that limiting FDI will have adverse economic effects and, therefore, prohibition of any merger or acquisition or any kind of Greenfield investment should be based only on very solid national security grounds backed by hard evidence.

In this book, the authors start by tracing the history of FDI in the United States and show how it has changed from being primarily Greenfield to recently being mainly mergers and acquisitions. They argue that the national security concerns associated with FDI have also changed as a result. According to the authors, during World War I, the main concern was the existence of German assets in certain key, strategic industries. The U.S. government seized the assets of German firms (operating mainly in the chemical, radio broadcasting, telecommunications, and transport machinery industries) and of U.S. citizens of German origin. This was done with the help of the Trading with the Enemy Act (TWEA). The assets seized were transferred to American companies such as Dupont and General Electric. The TWEA was invoked again during World War II. As the type and the size of FDI have changed, so has our nature of security concerns associated with FDI. With the United States clearly being the most advanced technologically, the authors feel that the fear now is about the acquisition by foreigners of U.S. technology and technical knowledge, especially in strategic industries. Specifically, there is fear regarding acquisition of such know-how through acquisition of American firms by Chinese firms, most of which are state-controlled. These fears are not unfounded as the personal computers section of the IBM has been recently taken over by Lenovo, and there was a failed attempt by China's oil corporation to take over Unocal, an American oil company. The fear is that indirectly, the Chinese government (still Communist) will acquire control over American physical and intellectual assets. Apart from the fear of aquisition of U.S. assets by Chinese companies, another fear, according to the authors, is of course the fear of the Middle East as exemplified by the uproar over the proposed acquisition by Dubai World Ports (DWP) of Peninsular and Oriental Steam Navigation Company controlling six U.S. ports. To deal with situations of this sort, the United States has in place an institutional set up brought about by the Exon–Florio amendment to the Defense Production Act of 1950, passed during the last years of the Reagan presidency. As clearly explained in the book, this amendment allows the president to block a transaction on the basis of "credible evidence" that it "threatens to impair the national security," where "national security" is not defined but is open to interpretation by the twelve-member Committee on Foreign Investment in the United States (CFIUS) that reviews foreign investments. The book describes the CFIUS process in fairly great detail. Also, the process is beautifully summarized in table 2.1 of the book. According to the authors, the problem with the process today is its excessive politicization with attempts by politicians and domestic lobbies to politicize it further. The authors argue that it is this politicization which prevented the acquisition of Peninsular & Oriental by DWP, despite clear evidence furnished by the latter that security was not threatened and despite DWP agreeing to all the conditions imposed on them including close monitoring. The authors also provide another piece of evidence on excessive politicization: the takeover of the Massachusetts based Norton by British Tire and Rubber was opposed by the Congress, which changed its stance when a French firm proposed the same acquisition but at a much higher price.

The current membership of CFIUS does not include senators or congressmen. The U.S. Congress has been attempting for a while to change the composition of CFIUS to bring in greater congressional oversight to the CFIUS process. At the same time, as explained in detail in the book, the Congress has also been trying to go beyond what one might understand by just "national security." There have been suggestions to also include "economic security," to which the authors seem vehemently opposed. The authors are also completely opposed to any change in composition of the CFIUS. They, however, are in favor of adding protection of critical infrastructure, as defined by the Department of Homeland Security, in the CFIUS evaluation of projects. In addition, they argue in favor of greater transparency in the CFIUS process, including greater disclosure of information to the Congress. Furthermore, in their opinion, the threshold ownership of 10 percent defined as "foreign control" is very low and needs to be raised. One important recommendation they make is the setting of uniform international standards in evaluating national security implications of FDI projects.

The book is well written and very lucid. The economic analysis is very simple minded. There is no sophisticated theory or rigorous econometric work in the book. The important thing to note is that the book has a strong message: Allow free international investment flows unless there are genuine national security concerns, and for that to happen minimize politicization and congressional oversight of the CFIUS process. The authors also argue that we should not treat Chinese acquisitions of American firms any differently from acquisitions by firms from other countries. Otherwise, what the United States preaches in support of free trade, free foreign investment, and free markets to the rest of the world will lack credibility. The authors, however, forget that the United States also tries to promote democratic values throughout the world. Acquisitions of American firms by Chinese firms controlled by their Communist government may come in the way of spreading the message of democracy. Besides takeover of private firms by firms, that are at least partially if not fully state controlled, is certainly not consistent with the practice of free markets and free foreign investment, and of capitalism. It is important to note here that we should not always judge the quality of each and every outcome solely by its impact on economic growth. In a world where there are environmental problems, security challenges, and widespread inequality and poverty, we have to go beyond per capita incomes in judging the merits and demerits of economic policies and institutions. Due to the presence of these numerous trade-offs, people are sometimes better off sacrificing growth for a better and more secure environment and for a world with more economic and political freedom. And so the authors do not have a rebuttal in their book to the following potential criticism: How can our government allow the takeover of private firms by firms controlled by another government that does not care about political and economic freedom? I certainly sympathize with the authors' concern regarding the capture of economic policy making by protectionist lobbies. But I also think the situation here is more complex than what they describe.

Next, I also think that the book is too much about what should be done rather than what the authors feel is most likely going to happen. We all know that economic policies are seldom welfare maximizing, and that is why there is now a fairly large literature on endogenous policies, where this endogeneity is explained by political-economy forces. In this set up, outcomes can be changed only by an agenda setter (in this case the president) who changes the process of decision making, especially through a change in the sequence of moves by the various actors in the process or through some changes in the overall team of actors.

Overall, this book is an outstanding contribution. It is the first book I have seen on the subject of FDI and national security and in my opinion, is a valuable addition to the new literature on the economics of national security. While the book will be of special interest to people in policy circles, it should also be read by everyone who is doing or planning to do rigorous research on economic issues related to national security. The book is full of ideas. As mentioned above, I find the economics in this book to be fairly straightforward and simple minded. At the same time, I also do not find any flaws in any of the economic reasoning in the book. While I have criticized the book for not going into the details of possible future outcomes and focusing more on what should be rather on what will be, I think the authors have done a great service by writing this book as it would educate politicians and anyone else involved in policy making about the adverse effects of restricting FDI, especially when it is done to please special interest groups and is not done on genuine national security or other welfare grounds. While international economists, including those in academia, have a lot to learn from this book about the history of the link between FDI and national security and how it has been handled in the United States, economists working specifically in this area need to read this book to gain familiarity with the institutional details pertaining to this FDI–national security link and to get to know the important unanswered questions on this subject.

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How to Spend $50 Billion to Make the World a Better Place. Edited by Bjørn Lomborg. Cambridge and New York: Cambridge University Press, 2006. Pp. xxi, 183. $45.00, cloth; $12.99, paper. ISBN 978–0–521–86679–8, cloth; 978–0–521–68571–9, pbk.

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The development discourse is so dominated by people who know the answer ("free trade," "industrial policy," "education," "incentives," "people's participation," etc.) that it is always refreshing to read something that starts by asking a question. Bjorn Lomborg started by inviting a group of experts to write a paper outlining their view of how best to spend 50 billion dollars to make the world a better place. Then he found some other experts to critique those papers. Finally he managed to persuade a panel consisting of Nobel Prize winners and other distinguished economists to evaluate these alternative views and put them all together into the Copenhagen "Consensus."

Unfortunately, by far the most important thing that I got by reading the book that came out of all of this is an understanding of the many ways such an exercise can go wrong. The problem, it turns out, goes back to the very beginning: We want to ask experts, but experts in what? Somebody has to pick the areas of expertise, but based on what? I assume that what happened is that the editor picked. The things that he picked are mostly standard—who would argue with infectious diseases, better nutrition, education, global warming, civil peace, better governance or water. The remaining items in the list—trade, migration, and subsidy policies—are more controversial, but that in some ways is all the more reason to try to get experts to argue about them.

What is less clear is why the list ended where it did: what about micro-credit, clearly the flavor of the year? Why not decentralization? Or women's empowerment? Or roads? Or for that matter population policy, less fashionable these days, but still a huge issue in parts of Asia and Africa?

Perhaps this is carping: any list has to end somewhere. But the focus on market friendly reforms and changes in governance, combined with an emphasis on health and education to the exclusion most other things, smells of a rather specific political position, associated, for example, with what is sometimes called augmented Washington Consensus. This would not be a problem per se—after all, politics is everywhere—but it conflicts slightly with the tone of scientific objectivity that the introductory chapter aspires to: We start to worry that the rabbit has already gone into the hat.

The worry deepens when one bites into one of the chapters. The chapter on subsidies and trade barriers begins: "Eliminating Government Subsidies and trade barriers has clear economic benefits." I do not think of myself as a friend of *dirigisme,* but the absolutism in that sentence worried me: you mean people should pay market prices for AIDS medicines and school meals in public schools should be priced at cost? I read on, hoping that this was merely an opening salvo, to be followed by a more nuanced discussion. Alas not. The author quickly gives an undergraduate textbook exposition of the principle of comparative advantage, and then goes on tell us that "economies that have liberalized their trade have enjoyed an average 1.5% increase in annual GDP growth compared with the pre-reform rate." Possibly true, but so what? The author presumably wants us to count this as evidence for the benefits of trade liberalization, but this is no more than a correlation, not a proof of causation. Could it not be that the benefits come from the conditions that allowed the country to liberalize, or other policies implemented during the liberalization episode? But the author is not about to let this kind of quibbling to get in his way: The paragraph ends on a high note—"Free trade is a necessary, but not sufficient, condition for sustained economic growth." But then how about China and India, still among the more closed countries in the world?

It is therefore no surprise that he goes on characterize opponents of free trade as narrow special interests and misguided NGOs. Perhaps this is what got to be little too much for Arvind Panagariya, no protectionist himself, and in his (otherwise very friendly) opponent's remarks on this chapter, he points out that there are cases where there may be a lot of losers from trade liberalization.

Given all this, it is hard to have much confidence in the author's estimates of the gains and losses from freeing trade. He says nothing about how the particular demand and supply elasticities that are at the heart of how the gains get calculated, were arrived at. Nor is there any mention of the various reasonable worries one might have about the identification issues involved in estimating those elasticities. Finally, the losses are simply set, essentially by purely authorial decree, to be one-third of the gains. There is no mention of mechanisms for identifying or compensating the losers.

The point here is not to attack free trade, which is almost always the right benchmark in discussions of trade policy, but to capture the tone of this chapter. The author seems to be more a cheerleader than an expert—indeed given that his cv does not list any paper published in a top ten (top twenty?) journal in the last fifteen years and precious few before that, I did wonder how he got to be chosen to be an expert.

There are chapters in the book that are not at all like this one. The chapter on "Hunger and Malnutrition" is excellent, especially in its discuss of prenatal and early childhood issues and in identifying the key micronutrients. The chapter on education by Lant Pritchett and the response by Paul Schultz, provides a wonderful summary of the reasons why there is something of a crisis in educational policy today, without necessarily suggesting a clear way forward. The idea, developed in the essay on civil wars, of pouring resources into a country in the immediate aftermath of a civil war in order to prevent a relapse, is definitely intriguing and perhaps worth an experiment, though there are obviously many reasons why it might not pan out.

Most of the other chapters in the book were however much more along the lines of the chapter on trade, pushing some fairly standard policy options without offering any new insights into why these options ought to be chosen or even a discussion of why the evidence is strong enough to warrant spending billions of dollars on them, followed by a cost benefit calculation that seems to pull numbers out of nowhere. The chapter on water, for example (which is actually one of the better chapters), blandly asserts that "future (infrastructure) projects must be community managed," without citing any evidence that demonstrates the importance of community management. By contrast a recent NBER paper by Michael Kremer and Alix Zwane that actually reviews the existing evidence concludes that "there is little evidence that providing community-level rural water infrastructure substantially reduces diarrheal disease or that this infrastructure can be effectively maintained." Similarly, the proposal on HIV/AIDS seems to have entirely missed the mounting evidence (quite of bit of which comes from the work of my colleagues in the Abdul Latif Jameel Poverty Action Lab) that we do not really know how to get people to behave in ways that would reduce the transmission of HIV (though, of course we know how people *ought* to behave in order to prevent transmission).

Yet the proposal on HIV/AIDS was ranked the highest by the panel and the one on community managed water sources was seventh (trade liberalization was third). The micro-nutrients initiative, my personal favorite, was second, but the idea of improving maternal nutrition to help reduce the number of underweight children being born, which also seemed promising, did not make it.

The point, once again, is not to attack specific ideas or choices. Any attempt like this, by its very nature, will get some of the answers wrong . What is troubling is that there is so little done, it seems, to get them right. While the entire discourse is one of evidence and scientific method, no one here seems to be particularly interested in actually taking a hard look at what the data has to say. We are shown the trappings of a process of enquiry and learning, but is it anything more than an elaborate ritual that helps us get back to known answers?

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This is a memorable book, more for the extraordinary process of its writing than for its often humdrum argument. Impatient with the failure of the "conflicting demands of the media, the people, and the politicians," which result in "political decisions (that) seldom take into account a comprehensive view of the effects and costs of solving one problem in relation to another," Bjørn Lomborg, the Director of the Copenhagen Consensus Center, has invented a mechanism "to make the world a better place," with a(n imaginary) one-time budget of $50 billion. This process involves three circles of economists. In the outer circle, at presumably the lowest level of skill, are twenty "prominent researchers" who serve as a Greek chorus of commentators for the second circle, composed of "eight distinguished economists" (though there are actually thirteen represented here, so perhaps five are not so distinguished?) whose job it is to make global betterment "challenges" in ten areas (one of which is mysteriously missing from the book). The inner and most-highly skilled circle is an "expert panel" of "eight top economists, including four Nobel laureates," comprising Jagdish Bhagwati, Robert Fogel, Bruno Frey, Justin Lin, Douglass North, Thomas Schelling, Vernon Smith, and Nancy Stokey. These "experts," as they are repeatedly referred to in the book, serve as jurors, who take the challenge material and the comments of the chorus, and use rigorous benefit cost analysis to rank each of the challenges, which they did with something very close to consensus. According to the best proposals, and in order of priority, "we" should spend $27 billion to fight HIV/AIDS, $12 billion to fight malnutrition and hunger by providing micronutrients, $1 billion to liberalize trade, and the remaining $10 billion to fight malaria. All three of the proposals to do something about global warming were rated as "bad," indeed at the very bottom of the list; also dismissed as either "fair" or "bad" were two proposals to make it easier for migrants to move from poor to rich countries.

There is no description of the process of selection of any of the economists in any of the circles. The drumbeat repetition of the term "experts" only draws attention to the absence of those Nobel laureates who are best known for their thinking on issues of economic development, such as Amartya Sen, Robert Solow, or Joseph Stiglitz, not to mention James Mirrlees, none of whom are strangers to benefit–cost analysis. The two outer circles are also notable for the distinction of the names that were excluded. This exercise explicitly claims legitimacy for its results from the process by which it was conducted, from the distinction of those involved, and from the fact that they reached consensus. But without details of the process for selecting the participants, the fact of a consensus means very little. Certainly there is no consensus among economists that action to mitigate global warming is a "bad" investment, as the recent vigorous debate around the Stern report bears witness. It is hard to imagine that Sen, Solow, Stiglitz, or Mirrlees would have signed on to a benefit–cost analysis that settled the *ethical* question of how to treat unborn generations using the market choices of those currently alive, and certainly not to one that appears not to understand that doing so might require argument. The benefit–cost analysis that was central to development economics in the 1960s has been discarded today, but after reading the Copenhagen Consensus, one can only conclude that it used to be done a great deal better in the past.

Most of us think that the world would be a better place if it contained less poverty. Yet none of the challenges involves poverty reduction directly. Education, often thought to be a prime mechanism for poverty reduction, is dismissed, largely because we don't know what works, as is well-argued in the intelligent challenge paper by Lant Pritchett. (As if we did know what works for HIV/AIDS prevention, for reducing child malnutrition, or for reducing the "incidence" (meaning prevalence) of civil wars.) Nor are growth policies included—presumably because they are not projects but policies—but that doesn't stop the number three priority being the elimination of subsidies and trade barriers, which isn't a project either. Indeed, since the book excludes political costs from consideration, free trade is a free good, with an infinite benefit to cost ratio. But then why is allowing more migration so far down the list, when there is wide agreement that it would be far more effective than trade liberalization in reducing poverty? Perhaps precisely because it works better than trade liberalization and is thus even more politically unpalatable. Yet political costs are supposed to be ignored. It is hard to make any coherent sense of this project, even on its own terms.

Why did old-fashioned cost–benefit analysis, of the Dasgupta, Marglin, and Sen or of the Little and Mirrlees varieties fall into disfavor? One reason was their simplistic view of the political economy of development, that governments were benevolent social welfare maximizers. Another reason was that those analyses focused on the valuation process (using shadow prices, or social rates of discount) but hardly at all on the positive questions of what projects actually did. The profession of development economics has become much more skeptical about both of these assumptions. One tribe of development economists, represented best by Peter Bauer in the past and Bill Easterly today, thinks that social-welfare maximizing governments are about as plentiful as hypogriffs, and that in the reality, external assistance is at best useless and in many cases actually undercuts good governance. Another tribe is less skeptical about the possibility of doing something through foreign aid, perhaps especially for health, but recognizes that we have no idea what works and what does not. To remedy this, they are pursuing an extensive program of experimentation in an attempt to find good models to scale-up. These tribes represent two of the major lines of thought in development today. One of the problems of having "experts" who are experts in something else is that they appear to be unaware of the field to which they are putatively providing definitive solutions. This applies mostly to the inner circle; several of the challengers and their critics note both the uncertainties about mechanisms and the issues of political economy—though several sport the sort of unjustified certainty that comes only with the thickest of skins—but the warnings do not find their way into the consensus, perhaps because the terms of reference prohibited it.

I would be the last to claim that development economics of the last fifty years has been a success story, nor to argue that it is not refreshing to have smart if unknowledgeable people pitch in. But there is surely something to be learned, if only from our mistakes. Like Rosenstein-Rodan in the 1950s, and Jeffrey Sachs in 2000, Lomborg is trying to fill a "financing gap," as if money were the only thing preventing the elimination of communicable diseases. This is what smart people who have never thought about it before are likely to come up with, just as Rosenstein-Rodan did before there was any literature or experience to guide him. Sachs, to his credit, has a detailed plan, albeit one that many of us think is both hopelessly ambitious and hopelessly naïve. Lomborg's Consensus does not even identify the "we" who are to spend the $50 billion, although it certainly shares Sachs' confidence in the usefulness of social engineering by well-meaning outside experts. My own opinion is that if this Copenhagen "Consensus" had been subjected to its own benefit–cost protocols it would never have been undertaken, an outcome that might, indeed, have made the world a (slightly) better place.

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This one is different. The "Copenhagen Consensus" was elaborated during one conference convened in Copenhagen, where papers were presented on a number of topics, presumably selected by Bjorn Lomborg, the director of the "Copenhagen Consensus Center," himself. The authors of these papers are perfectly respectable people but it is difficult to judge what they said or did because the chapters of this book present only very condensed forms of them. Judging from those chapters, though, the papers were not particularly heavy on the rigorous evidence that the introduction, with its token gesture towards the importance of "fact and knowledge," promises us. All of them conclude with some sort of cost–benefit analysis that miraculously appears from nowhere. Eight top economists (Jagdish Bhagwati, Robert Fogel, Bruno Frey, Justin Lin, Douglass North, Thomas Schelling, Vernon Smith, and Nancy Stokey) then "rank" the different proposals. These top economists include four Nobel Prize winners, but zero development economists (unless Lin counts as a development economist since, after all, he works in China), zero public finance economists, zero labor economists, zero health economists, and zero education economists—in short, any economist who might have been thought to have informed views on the subjects in question. This ranking is given considerable weight. Sure enough, being good economists, they choose the proposal with the best cost–benefit ratios as reported in the authors' report, perhaps the only thing they could do given the situation they were hemmed in.

Unsurprisingly, control of HIV–AIDS is their top pick. "Spending assigned to this purpose would yield extraordinarily high benefits, averting nearly 30 million new infections by 2010. Costs are substantial, estimated at $27 billion." Since these very highly esteemed people are convinced that spending $27 billion dollars will reduce the number of new infections by 30 million, the implication is that the reader should be convinced, too. But on what basis? The chapter on HIV–AIDS has no references to specific studies on the prevention methods that are supposed to be so effective (and is really not very specific on how the control of HIV–AIDs would be achieved), but I happen to have worked on this issue and, to my knowledge, there is very little rigorous evidence on effective prevention strategies in Africa. The reviews that do exist of prevention among adolescents show very mixed impacts, though some strategies are more effective than others. As it turns out, I agree that we need to spend money on prevention of HIV–AIDS in Africa but not because we pretend we know what works, just because it seems the problem is so serious we have to try. But we still have a considerable amount to learn about what works and what does not, and we should not pretend otherwise. To have a vague, unrigorous cost benefits analysis that no reader could judge on her own gravely rubber-stamped by four Nobel laureates condones the bad habit of blithe and rash decision-making while in ignorance: it is not just counterproductive but also dangerous.

Their second pick is less outrageous. They propose to spend money to improve the availability of micronutrients in developing countries. There actually is a considerable amount of good evidence that micronutrient deficiency reduces productivity and health in developing countries and that it can, in principle, be addressed. This chapter on hunger is one of the best-written chapters in the book. It is precise, concise, and specific. It also rests on very solid literature. However, less is known on *how* these micronutrients should be made available. If one is to spend $12 billion on this effort, one will need to be a little more concrete about what form that spending should take. The authors of the chapter rightly mention there are several routes (fortification, tablets, etc.). It is, however, not clear that these strategies are enough. To give an example, in India, while iron tablets are routinely available, iron deficiency anemia is prevalent, and one must find other ways to make iron available. Flour fortification is a common way, but not everybody buys flour on the market. To reach the poor, one must find a way to fortify flour at the local level. A randomized experiment is currently under way in Udaipur district, Rajasthan, to test the effectiveness of this approach.

In the face of such wooliness, there seems little point in going on and on. One can only concur with the introduction of the book, which states that “political decisions should not be made arbitrarily but should be based on fact and knowledge.” It is also a good idea to present these elements of “fact and knowledge” to the general public and let them decide. Alas, this book is not it. In one sense, it is heartening to see that someone thought he could get fame and glory—and did get it, since Lomborg was named by Time Magazine as one of the most influential people of 2004—by selling something in the name of “rigorous evidence” to fight the world’s greatest problems because this suggests there may be a demand for such evidence. On the other hand, it is somewhat depressing to see what passes for evidence in this book, especially in light of the recent efforts to rigorously evaluate the impact of development policy around the world. That the authority of our best economic theorists and historians should be called to judge empirical propositions that are not particularly well argued is an unedifying spectacle. It seems like a waste of everybody’s time. One must hope that a more serious work will come along soon to replace this before evidence-based policy becomes another tired buzzword.

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G Financial Economics

The Econometrics of Individual Risk: Credit, Insurance, and Marketing. By Christian Gourieroux and Joann Jasiak. Princeton and Oxford: Princeton University Press, 2007. Pp. xii, 241. $75.00. ISBN 978–0–691–12066–9.
JEL 2007–0509

The book aims to bring together econometric models and applications to individual risks as a coherent collection and to form the basis of a new discipline in econometrics. The focus is on econometric modeling applied to a range of problems in insurance and finance. Its major emphasis is on econometric modeling of individual risks along with some varied applications that are used more as illustrations. The text uses empirical examples based on real life data to illustrate the application of the topics covered but this is not the main focus of the book. This is an area that has generated increased interest because of data availability at the individual risk level, particularly in the areas covered of credit, insurance and marketing. Much of the data in these areas is confidential to organizations because of the commercial sensitivity or private nature of the information.

The authors, Christian Gourieroux and Joann Jasiak, are well qualified to write the book, both having research backgrounds in the discipline and with experience in writing graduate level texts in their areas of specialization. Readers should have a solid mathematical probability and statistics background at least at the level of advanced undergraduate in order to benefit from the text. A basic background in econometrics would be useful but not essential. As indicated by the authors, the early chapters of the text could be used for a Masters course for students with a quantitative background and previous exposure to mathematical probability. It would suit students in a range of disciplines including economics, finance, actuarial science, marketing, and even management provided they had the appropriate quantitative background. The book could also be used for a PhD course for students with an econometrics background who were interested in risk modeling related to credit scores and other applications of models for individual risks including insurance risks.

Chapter 1 provides a brief introduction to the nature of the risks to be covered in the text. This includes probability of occurrence of a loss, or the frequency of loss, the time to a loss, or related duration variable, and the severity or amount of loss. Dichotomous qualitative variables take the values zero or one corresponding to whether or not a claim or loss has occurred or has been reported. Frequency corresponds to a count variable, and duration and severity are continuous variables. The score is the major object of study which is a risk index to be used to assess default probabilities based on individual characteristics. This process is referred to as segmentation, particularly in marketing.

Chapter 2 covers the dichotomous qualitative random variable taking the value 0 if an event of interest occurs and one otherwise. This definition ensures the score is decreasing in the risk of the event occurring. Basic concepts of conditional probability are introduced before consideration of risk prediction based on classification of individual risks into good and bad or high and low. The use of the likelihood ratio with a multivariate normal distribution for the score conditional on covariates is used as an example to illustrate the assumption of linear discriminant analysis. The logit and probit models are then introduced and compared with discriminant analysis. The linear discriminant model is shown to be a special case of a dichotomous logit model. A short section on risk heterogeneity concludes the chapter. This chapter would have been more valuable if it had emphasized the basic assumptions of the models and discussed how to test if these assumptions hold. Examples illustrating what happens with these models if the assumptions do not hold would have provided an important warning of the importance of model diagnostic tests. Econometric models are usually developed to handle situations involving real life data when standard model assumptions do not hold. Highlighting the strengths and limitations of models should be an important matter for a graduate text in the area.

Estimation methods, including statistical properties of maximum likelihood estimation, are covered in chapter 3. The maximum likelihood estimation for a logit model and the linear discriminant model is covered, followed by hypothesis testing procedures including the Wald test, the Likelihood ratio test, and Lagrange Multiplier test. Finally applications of the models to using financial ratios to predict company failure and mortgage credit assessment are discussed.

Chapter 4 discusses the monitoring of the performance of the score estimation using the performance and selection curves as a functional measure of goodness of fit. An example of the use of these curves for consumer credit scoring is then given that discusses the detection of structural changes in time. This example appeared to be out of place since the chapter had not covered time changing models. The use of these performance curves is referred to as richness curves in marketing. They were proposed as a better method of determining goodness of fit than standard measures such as R^2. How well a model can discriminate between different risks does not necessarily reflect in the R^2 of the model. These techniques are not conventional in econometrics and deserved a more thorough theoretical justification.

Chapter 5 discusses loss frequency models or count data models. The obvious example is for claims frequency in insurance. The Poisson regression model along with allowance for heterogeneity leading to the negative binomial model is covered. Poisson regression is used to model claims intensity as a function of individual risk characteristics or covariates. Maximum likelihood estimation is then summarized. Allowance for heterogeneity using a gamma distribution for the heterogeneity factor is introduced giving the negative binomial regression model. A bonus-malus insurance scheme is then analyzed by considering distribution of the heterogeneity factor and its impact on pure insurance premiums. A semiparametric model for the heterogeneity factor is introduced for the case where the gamma distribution is not justified. A summary of risk factors for rating individual automobile risks is provided as the last section. Some of the issues in applying Poisson regression including qualitative variables are mentioned such as the need to remove one category for model identification purposes. This would have been a good place to introduce generalized linear models and the application to insurance data. These models provide a more general modeling framework and are used particularly in insurance for modeling the variables that are covered here in this example.

Chapter 6 covers models for duration variables such as time to death of an individual in a life insurance contract or time to default on a loan. Pareto regressions as well as accelerated and proportional hazards models are covered. The chapter starts with a brief coverage of basic concepts of survival distributions including the survivor function and hazard rate. The concept of duration dependence of the hazard function is introduced along with some standard survival distributions, in particular, the exponential, gamma, Weibull, and log-normal. The exponential regression model that assumes hazard rates are constant in time but vary across individuals based on their characteristics is introduced. Following the modeling of count data, the model with gamma heterogeneity in the individual hazard rate is then covered leading to the Pareto regression model. The impact of this on duration dependence is analyzed. Accelerated hazard models and the proportional hazard model are briefly covered. The final section considers mainly prepayment analysis for car loans. With the current interest in longevity risk and aging populations, examples of analysis of mortality and factors that impact mortality would have been useful to include. There is also publicly available data. The area is also of interest to marketing and other business students as well as the actuarial and financial disciplines.

Models for nonhomogeneous samples and partially observed variables are discussed in chapter 7. The first part of the chapter considers endogenous stratification and the second part the allowance for truncation and censoring of data. The main problem that is addressed by the use of stratified sampling is when the proportion of risky individuals in the population is relatively small. To avoid a large sample size, the use of stratified sampling can be used. Maximum likelihood estimators based on endogenous stratification are not consistent. Truncation and censoring are important concepts in survival analysis. Brief coverage of the log-likelihood function for the exponential model allowing for truncation and censoring is given. The issue of competing risks is briefly mentioned. The censoring and truncation of data deserves more coverage since they are often found in empirical data in this area. The chapter finishes with coverage of selectivity bias since usually the data observed have been selected through a screening process and only successful individuals are observed. A model for bias correction is introduced.

Chapter 8 covers transition models based on homogeneous Markov chains. A brief coverage of Markov chain properties begins the chapter with a two-state space illustration provided. Maximum likelihood estimation is briefly covered along with the use of a dichotomous qualitative model for the transition probabilities. Time varying transition probabilities are considered. Revolving consumer credit and corporate ratings are considered as examples.

Chapter 9 introduces the polytomous logit model to handle the situation where there are multiple scores such as is the case for risks from both default and prepayment for lenders to individuals. An example is given of the cases where the default risk occurs early versus later in the loan term. A term structure of default is discussed as is an allowance for severity in the score. The case of competing risks of default and repayment is then considered. The polytomous logit model is then introduced along with a marketing example to assess consumer choices for the introduction of a new product. The impact of multiple scores is then discussed in the case of profit and utility optimizing decisions. An issue of multiple scores is collinearity and the use of singular value decomposition as a dimension reduction technique is covered along with the related statistical inference. An example is reviewed covering the impact of individual characteristics on asset portfolios and asset allocations.

Models to allow for serial dependence in longitudinal data, the case of panel data, are introduced in chapter 10. The compound Poisson process is introduced as a model for marked point processes with time-independent events and marks. Serial dependence is then introduced through autoregressive processes for panel data. Issues of heterogeneity are considered. An application to consumer credit cards completes the chapter. Panel data is a very important modeling technique in empirical studies in many areas including finance, insurance and actuarial science. Although it is a major area on its own, it would have been useful to have provided a broader coverage of these models and the data modeling issues they are designed to address.

The final chapter, chapter 11, considers bank management of credit risk based on the use of value at risk. There is a brief discussion of Value at Risk and how it is used in bank market risk and properties of risk measures are reviewed. Credit risk for portfolios of credits is then considered. The determination of the profit and loss distribution for the portfolio is derived for the maturity horizon of the loans and for shorter horizons. The factors that impact default rates are discussed using macroeconomic covariates. Corporate bond portfolios including default correlation is then introduced with the case of transition matrices and a dynamic probit model.

The book is extremely comprehensive in its coverage of models in the area of credit and insurance risks at the individual level. The common theme is the quantification of risks including models for frequency or probability, timing, and severity. The analysis is mostly for individual risks with the exception of the final chapter that also considers corporate bonds. There is no reason that the techniques covered need to be confined to individual analysis since they have applications to other situations where explanatory factors are used to assess probabilities or scores.

A great way to explore the topics covered in this text is through using actual data to assess the use of the various techniques and to learn the limitations of the methodology. Data sets used in the examples were not provided by the authors, mainly because of confidentiality reasons. This lack of availability of data is a great pity. Without the opportunity to use the datasets to reproduce results and explore other issues, the text becomes more of a theoretical overview of key techniques.

There are other applications of the modeling that would also be worth exploring in more detail. Insurance risk models for rating individual risks use a range of techniques but the use of generalized linear models has become almost standard. Similar methods are increasingly applied to individual risk characteristics and loss reserving. The analysis of individual data for the detection of fraud in banking and insurance is an important area and a wide variety of data mining techniques are being used. Decision trees, neural nets, and genetic algorithms are among them. Although these modeling approaches would not normally be considered a central focus of econometric modeling, they are designed to address similar issues and are actively used in applications.

A challenge with a text such as this is to provide the best balance between theory and application. It is also a great challenge to cater to a cross disciplinary audience in terms of applications. An interesting question is whether it would be better to have a solid coverage of the theory in the first part of a text and then to focus on applications with separate chapters for a range of disciplines including economics, finance, marketing, insurance, and actuarial science in the latter part? This would allow different disciplines to use the text for students in a specialist research class or to cover the theory in a common class. This is often what happens in universities in any event. It is a great benefit if research students from a range of disciplines can be brought together to study common underlying techniques but this can sometimes be limited by the interests or background of students completing research where the focus is the applications rather than the theory.

Overall this is very well written text. It is concise and covers a very broad range of models suitable for application across a number of disciplines but mainly in finance and to some extent in marketing. Its conciseness can be considered as both strength and a weakness. Diagnostic tests and the use of actual data to illustrate some of the modeling challenges in the area could have been given more attention. Beyond that the book fills an important gap and will be of interest to researchers and graduate students in a number of disciplines. Its breadth of coverage of techniques is unique and the quality of exposition exceptional.

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H Public Economics

Social Security and the Stock Market: How the Pursuit of Market Magic Shapes the System. By Alicia H. Munnell and Steven A. Sass. Kalamazoo, Mich.: W. E. Upjohn Institute for Employment Research, 2006. Pp. ix, 171. $40.00, cloth; $18.00, paper. ISBN 978–0–88099–291–6, cloth; 978–0–88099–290–9, pbk. *JEL 2007–0128*

In this short book (roughly 150 pages), Alicia Munnell and Steven Sass explore whether investing in stocks can help solve the financing problems faced by the Social Security system in the United States. Both authors are experts in the field of pensions and retirement and, in recent years, they have done extensive joint research at the Boston College Center for Retirement Research. In this book, they examine the experiences of three nations that have tried adding equities to the investment mix of their Social Security systems in the recent past—the United Kingdom, Australia, and Canada. The authors have done a good job of summarizing those experiences and extracting some valuable lessons for the United States.

The book begins by alerting the reader to the basic fact that investing in the stock market is *not* a magical cure for what ails the U.S. Social Security system. Munnell and Sass clearly state "introducing equities into the Social Security program, by itself, will not significantly reduce the burden on future generations of providing for a greatly expanded elderly population." They point out that a shift from bonds to stocks in the Social Security Trust Fund's portfolio does not automatically increase aggregate saving or the productivity of capital investment. If aggregate national saving is shifted toward riskier investments, any increase in the expected rate of return would be accompanied by an increase in aggregate risk.

Notwithstanding this warning, Munnell and Sass believe there is a valid case for including stocks in the Social Security program. They claim it improves intergenerational risk-sharing, diversifies Social Security's funding base, and could help reduce the program's financing shortfall. But the risks and costs of equity investment must be addressed. The book's main focus is on whether it is better to carry out a national policy of investment in equities through a centrally managed trust fund—the Canadian approach—or through a system of individual self-directed accounts as in the United Kingdom and Australia. After a careful review of the evidence, they conclude that the approach taken by Canada offers the most promise for the U.S. Social Security program.

This book is a good piece of applied economic analysis. There is not a single equation in the book and it hardly uses any technical jargon. I recommend it to anyone with an interest in this area, especially to policymakers.

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I Health, Education, and Welfare

Poverty and Discrimination. By Kevin Lang. Princeton and Oxford: Princeton University Press, 2007. Pp. xiii, 407. $60.00, cloth. ISBN 978–0–691–11954–0. *JEL 2007–0557*

Lyndon Johnson launched the "War on Poverty" in the United States in 1964. Over forty years later, poverty remains a pressing social issue. In 2005, one in eight Americans lived in poverty and, although there have been large increases in average income in recent decades, the proportion in poverty has remained relatively constant. In addition, poverty rates are significantly higher for African Americans and Hispanics, raising the question of whether racial discrimination might play a role.

Making sense of the areas of poverty and discrimination can be difficult for students. A full understanding involves knowledge of trends for a number of statistics. It involves acquiring information on a large number of specific public policies, how they have evolved over time, and understanding what economic theory predicts about the effects of these policies. Finally, it requires knowledge of the empirical evidence on these subjects, as well as a way to evaluate the quality of that evidence. In *Poverty and Discrimination,* Kevin Lang provides the information necessary for that understanding in a single place.

This ambitious book synthesizes a great deal of information. Lang makes it intelligible to a broad audience, including upper-level undergraduate and masters-level students, as well as individuals involved in policy making. However, what is equally important is his goal to "help [the reader] distinguish the good research from the rest." The book, while providing evidence, also provides the reader with the conceptual tools necessary to evaluate that evidence.

Part 1 of the book looks at the issue of poverty. Starting in chapter 2, Lang focuses on defining poverty and identifying the poor. He carefully discusses the complexities associated with official definitions of poverty, then uses both the official poverty definition and a number of alternate indicators to provide a picture of the poor in the United States along the dimensions of race, age, family structure, and urban status. The chapter then touches briefly on extreme poverty, homelessness, and food insecurity, and moves on to describe the dynamics of poverty spells.

In chapter 3, Lang describes the evolution of federal poverty policy in the United States. He provides evidence on the relative importance of a number of different poverty programs between 1970 and 2000, with attention to both the type of assistance as well as how well the programs actually target the poor. The chapter then focuses on the shift away from cash grants toward in-kind transfers such as Food Stamps, housing programs and Medicaid, as well as toward wage subsidies for workers like the Earned Income Tax Credit (EITC). Lang describes clearly how the work incentives differ between the Aid to Families with Dependent Children (AFDC) program and the EITC. The chapter discusses the existing evidence regarding the effects of the EITC on labor supply and then moves on to consider why policymakers may want to choose in-kind transfers instead of cash grants, despite possible losses of utility to the recipients.

Chapter 4 presents trends in poverty in the United States over the past forty-five years, followed by an empirical exercise that aims to show how much of the trends in poverty can be explained by a variety of factors, including male earnings, male earnings inequality, earnings for low-skilled female workers, government transfers, and the number of female-headed households. Lang shows that changes in economic factors such as wages have played a large role in the evolution of poverty over time, but also shows that economic growth does not always affect the entire income distribution and therefore may not always reduce poverty.

Given the importance of wages and wage inequality to the trends in poverty discussed in chapter 4, chapter 5 moves on to a discussion of wage inequality, as well as a number of potential explanations for its growth in recent years. It then covers a number of labor market policies, beginning with minimum wage laws and moving on to a history of job training programs. It provides evidence from evaluations of job training and welfare to work programs, and then ends with a discussion of employer-based subsidies.

Chapter 6 pulls together a number of different topics that are related to both family formation and child poverty. It begins with a summary of changes in family structure over the past half-century, discussing increases in nonmarital births and declines in marriage. Lang then goes on to present a number of potential explanations for these trends. The chapter moves on to discuss the features of the welfare system that may have contributed to changes in family structure. It provides an excellent discussion of teen childbearing, clearly explaining the methodological issues involved, and how economists have developed strategies for estimating causal relationships. The chapter concludes with a consideration of children and poverty more broadly, covering the effects of growing up with a single parent, intergenerational transmission of poverty, and a number of policies aimed at children of various ages, from preschool programs to public health insurance programs such as Medicaid and SCHIP.

Chapter 7 describes the spatial concentration of poverty in the United States. It first discusses the literature on "neighborhood effects" and the difficulties of identification in this literature, given the endogeneity of neighborhood choice. Lang then moves on to present research on the effects of the Gautreaux and Moving to Opportunity programs, which randomly removed low-income individuals from high-poverty areas. The chapter concludes by providing information on a number of different community development projects.

Chapter 8 focuses on education and education reform, and begins with a short discussion of the relationship between education and earnings. Lang then goes on to provide an extensive discussion of high stakes testing, and how its effects differ depending on our model of the educational production function. Next, the chapter covers the research on the effects of decentralization and competition on school quality.

In chapter 9, Lang turns to the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA)—the major welfare reform bill that was passed in 1996. The chapter first makes the case for welfare reform by revisiting the family structure concerns raised in chapter 6 and by adding a discussion of the work disincentives generated by the AFDC program, as well as public concerns about welfare dependence and intergenerational transmission of welfare dependence. Lang then discusses the elements of PRWORA and the empirical evidence on its effects. Again, he provides a clear, intuitive discussion of the difficulties associated with identifying these effects, particularly given that welfare reform was enacted at the federal level with very little variation either across states or over time.

In part 2, the final third of the book, Lang turns to discrimination, with a primary focus on racial discrimination. Chapter 10 provides an excellent and clear description of various theories of discrimination. Lang carefully outlines the predictions of each theory of discrimination with respect to the resulting racial composition of work forces and racial gaps in wages, and also provides a careful discussion of the conditions under which we should (and perhaps more importantly, should not) expect discrimination to be eliminated by market forces.

Chapter 11 discusses the existing empirical evidence on racial discrimination in the labor market. It begins with a presentation of trends in black–white earnings differentials and then evaluates a number of potential explanations for the decrease in the black–white wage gap over time. Lang also covers the difficulties associated with measuring discrimination empirically, explaining the limitations of both audit studies and wage regressions.

Chapter 12 explores the black–white test score gap and the role played by both segregation and affirmative action in the education system. Chapter 13 covers the corresponding evidence on racial discrimination in consumer markets. The most attention is paid to housing, with a discussion of residential segregation and its effects, as well as spatial mismatch. It then moves on to cover discrimination in credit and a number of other markets and concludes by discussing evidence on discrimination in the judicial system.

The last chapter in part 2 turns from discrimination based on race to discrimination based on sex. It presents information on the male–female earnings gap and provides a number of different explanations for these differences, including human capital, occupational segregation, and discrimination. Lang discusses the role played by both marriage and childbearing in these differences, examining both marriage premia in wages and motherhood wage penalties. The chapter covers research on discrimination based on sexual orientation, and concludes with discussions of the role of public policies such as comparable worth.

Overall, *Poverty and Discrimination* provides an excellent introduction to the study of these two important topics. It pulls a great deal of information together in one place. The book is accessible to readers at a number of different levels. Those relatively new to quantitative analysis will benefit from Lang's effective use of graphs to convey intuition, and from the review of probability and statistics found early in the text. More sophisticated students can turn to the appendices of many chapters to find a more technical analysis. The book will also serve as an extremely valuable reference volume for researchers in the fields of poverty and discrimination.

In addition, Lang attempts to instill in the reader an understanding of how difficult it is to answer many of these important policy questions. He is able to explain in an intuitive manner the methodological difficulties in identifying causal effects that have been at the forefront of much of the recent economics and program evaluation literature in these areas. Students and policymakers who read this book will come away from it with an enhanced ability to think critically about research findings.

However, one of the weaknesses of the book stems directly from the ambitious goals of the author. Because the book attempts to cover such a wide breadth of topics, it ends up feeling uneven in places. A great deal of attention is paid to some topics that might be traditionally considered less important in a book on poverty (e.g., high stakes testing). Yet the discussion of welfare and welfare reform is scattered across a number of different chapters. A unified discussion of the features of the system and the reasons why it eventually led to such fundamental welfare reform would be useful. In addition, the link between the two sections of the book is relatively weak. While some of the discussion of empirical techniques and methodological approaches in the first part of the book are returned to in the latter part, the two sections could largely stand alone.

Those minor criticisms aside, *Poverty and Discrimination* fills a gap in the literature by compiling a vast amount of information, both theoretical and empirical, in one place. It will serve both of Lang's target audiences (students and policymakers) well, in particular because it pays such careful attention not only to what we know, but to how we know it.

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The Economics of Infectious Disease. Edited by Jennifer A. Roberts. Oxford and New York: Oxford University Press, 2006. Pp. xii, 386. $110.00, cloth; $57.50, paper. ISBN 978–0–19–851621–7, cloth; 978–0–19–851622–4, pbk.
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According to the most recent statistics of the World Health Organization, infectious diseases account for about 26 percent of the global burden of disease and an even greater share of the disease burden in the developing countries of South Asia and Sub-Saharan Africa (http://www.who.int/topics/global_burden_of_disease). With the worldwide HIV/AIDS epidemic showing little signs of abating and oft expressed concerns, sometimes bordering on panic, about the avian flu, SARS and multidrug resistant tuberculosis, infectious health conditions and interventions to address them will continue to loom large on the radar of policymakers and the general public. The edited volume by Jennifer Roberts, *The Economics of Infectious Disease,* is, therefore, a welcome addition to efforts to bring an economic perspective to bear on the task of addressing infectious health conditions. Indeed, as the experience with the worldwide AIDS epidemic and the recent scare with regard to the traveler with drug-resistant tuberculosis pointedly suggest, issues surrounding the spread and control of infectious disease are of interest not only to clinicians and biomedical researchers, but also to public health specialists, legal experts, ethicists, political scientists and economists.

An extensive body of existing research suggests that economic analyses can contribute to our understanding of infectious health conditions and toward developing effective interventions. They do so in at least three different ways. Firstly, such analyses can help highlight the significant economic implications of policy inaction. Economists have estimated the negative macroeconomic implications of disease, as illustrated by studies of the impact of HIV/AIDS, SARS, the influenza epidemic, and malaria on the Gross Domestic Product (GDP) or GDP per capita of nation states (David E. Bloom and Ajay Mahal 1997; John Luke Gallup and Jeffrey D. Sachs 2001; Emma Xiaoqin Fan 2003; Elizabeth Brainerd and Mark V. Siegler 2003). In addition, considerable work has been directed toward assessing sector- and region-level impacts of infectious diseases, including the transport, agriculture, health, and insurance sectors. An especially productive line of inquiry has been the impact of infectious disease on the economic well being of households, driven primarily by the fact that increased morbidity among affected members leads to greater out-of-pocket health care expenses, as well as foregone incomes because of the time spent away from work by the sick and caregiving members of the household (Max O. Bachmann and Frederick L. R. Booysen 2003).

Secondly, the focus of much of economic analysis on factors that guide the behavior of agents, and correspondingly, the key proximate role of individual behaviors in the spread of infections means that economists can contribute much to the understanding of the "roots" of infectious disease. These roots sometimes have explicit financial characteristics. For instance, concerns for economic survival may lead individuals infected with a specific disease to migrate from a region where a condition, say tuberculosis, is more common, to another where employment opportunities are more accessible, but the pool of individuals susceptible to the disease much greater. Physicians, concerned about liabilities from malpractice lawsuits or the potential loss of patients who are not prescribed antibiotics, may adopt liberal prescription practices leading to the emergence of microbes that become resistant to antibiotics over time. Financial concerns need not be the only consideration, however. When economists speak of "roots," they refer more generally to the calculus commonly ascribed to agents, of undertaking actions whose net benefit (not necessarily financial net benefit) is perceived by them to be the highest among alternative courses of action. An individual may choose not to get vaccinated against some infectious condition if the psychic cost of the pain and inconvenience in getting vaccinated exceeds the potential gain from prevention to the individual. Similarly, injecting drug users may choose to use unclean needles if cleaning injecting equipment is time consuming and thereby worsens withdrawal symptoms.

Finally, economists are concerned about policy interventions to address these epidemics. Here, the adoption of an economics lens requires making the case that the spread of infectious disease, or ways to control them (such as vaccination), are potentially associated with some form of market failure, be it externalities, public good characteristics, information asymmetries, or inequity; and further that interventions by public sector organizations, while potentially plagued by incentive problems, are worthwhile in the net. Economists have also developed a set of tools to rank the desirability of alternative policy interventions, be it in the health or other sectors. These evaluation methods, referred to as cost–benefit and cost-effectiveness analyses (CEA), have been extensively discussed in the literature. Whereas cost–benefit analysis compares the monetary value of all benefits and costs associated with alternative interventions, CEA assesses interventions in terms of costs incurred per unit of beneficial outcome achieved which, in the case of health interventions, can be the such as the number of infectious disease cases averted, or the numbers of lives saved.

How well does the book transmit these ideas to noneconomists working in the field of infectious disease? And are there research and policy questions, not yet fully appreciated by professional economists that the book directs our attention to? Indeed, the introductory chapter of the *Economics of Infectious Disease* lays out two key objectives for the book: "to introduce economic analysis and its application to those who are involved in infectious disease and its control; and to introduce economists to infectious disease and the challenges that it raises" (p. 1). I argue below that, all things considered, this collection, while certainly valuable, provides a somewhat lopsided introduction to infectious disease specialists concerned about the different ways in which economic analyses can be brought to bear in their own subject area. To the professional economist wishing to work on the subject of infectious disease, the book has potential value as well. This value arises, not so much from the rigor with which topics related to infectious disease are addressed, but in its directing the reader toward subtopics that economists have hitherto paid little attention to and modes of analyses that have been underutilized thus far, particularly in the context of developing countries.

How effective is the book as an introduction to the use of economic analyses of infectious disease? In some respects, particularly in the description of methodologies used for carrying out CEA and cost–benefit analyses of interventions and the application of these methods to undertaking actual comparisons of alternative interventions, it is rather good. Chapter 2, by Catherine Goodman et al., for instance, assesses the use of CEA in arriving at a choice of first-line malaria drugs—chloroquine versus sulfadoxine-pyrimethamine (SP)—in Tanzania. The analysis sheds light on the key bits of information required for undertaking a careful CEA of these options, including the likelihood of increased drug resistance in future periods, the cost of second-line antimalarial drugs, the formulation used, clinical efficacy of drugs (including side-effects), the time-frame of analysis and the like. Lilani Kumaranayake et al., in chapter 4, consider the case of assessing cost-effectiveness of HIV-related interventions, and emphasize in particular the importance of accounting for the (dynamic) impact of interventions on secondary infections. In chapter 5, Richard Grieve explores ways to better account for technological improvements in making cost-effectiveness comparisons of alternative interventions to treat Hepatitis C infections. Specifically, he makes the point that when long-lasting treatments are compared, cost-effectiveness comparisons must somehow be able to account for technological changes that lower the cost of a given treatment type over time (e.g., by way of improved methods of administering medicine, more effective substitution of cheaper for more expensive medical personnel, etc.).

Chapter 8, by Rosalind Plowman, is a useful introduction of the methods of CEA as applied to assessing alternative interventions to address hospital acquired infections (HAI). A key issue highlighted in this chapter, as well as in chapter 6, authored by Nicholas Graves and Diana Weinhold, is the methodological difficulty of isolating the impact of HAI on extra hospital stays and health costs, an important intermediate step in undertaking CEA. Specifically, they point to the "two-way" relationship, whereby HAI contributes to increased lengths of hospital stays and increased lengths of stay potentially increases the risk to the patient of contracting HAI. The authors point to instrumental variable methods and assessing differences in hospital stays among otherwise matched patients to address this problem. This chapter also provides a useful survey of the economic evaluation literature as it pertains to hospital acquired infections.

CEA is not very useful if the objective is to arrive at a monetary estimate of the beneficial impact of interventions, particularly if one seeks to assess the desirability of health sector interventions with interventions in say, the infrastructure sector; or to compare gains in terms of reduced morbidity resulting from interventions addressing different health conditions. With this monetary perspective, chapters 9 and 10 focus on methods for eliciting individuals' willingness to pay (WTP) for a flu vaccine and treated bed nets, respectively, in order to arrive at a monetary estimate of their beneficial impact. Chapter 10, authored by Mrigesh Bhatia and Julia Fox-Rushby, is particularly informative from the viewpoint of the careful attention paid both to the design of questionnaires for obtaining survey based estimates of WTP, as well as the actual execution of the survey. Their empirical analysis focusing on WTP for treated bed nets among Indian village households also sheds light on some of difficulties that arise in using WTP estimates from survey responses. Chapters 13 and 16 include useful discussions on the application of cost–benefit analyses to food safety interventions and include a quite informative assessment of various U.S. government food safety regulations, including the Hazard Analysis and Critical Control Point Systems, and various requirements aimed at reducing the incidence of salmonella in shell eggs and of listeriosis.

Overall, one could not help but feel that there was an excess of chapters devoted to economic evaluation techniques. I found the book rather less illuminating when discussing elements of behavior that underpin infectious disease and its control, i.e., the "economic roots" of infectious disease. To be sure, the introductory chapter to the volume refers to a number of economic issues relevant to the spread and control of infectious disease: market failure associated with externalities, the public good nature of interventions against infectious disease, and the role of informational asymmetries and transactions costs in the emergence and functioning of organizations that respond to infectious disease. However, the analysis is rather hurried and simplistic and reads like a listing of topics and unlikely to be readily deciphered by a newcomer to economic analysis, unless backed up by further reading. I was also struck by some inaccuracies, for instance, the statement that ". . . Buyers and sellers in perfectly competitive markets can both 'walk away' if the product or price is not to their liking" (p. 8), a definition that suggests that the perfectly competitive paradigm is no different from almost any other market setting.

There are exceptions of course. Chapter 3, by Ramanan Laxminarayan, is a fascinating discussion on optimal strategies toward drug resistance. Specifically, the author interprets the issue of provider prescription/use of antimicrobials and associated resistance as a standard economic problem of the overuse of open access natural resources. He explores incentives faced by health care providers to overprescribe, relative to socially optimal levels, including market-driven pressure from patients and the potential risk of malpractice liability. The chapter also highlights incentives that drive pharmaceutical manufacturers, including the demand from the animal industry, as well the relationship between patent length and search for new antimicrobials. The discussion on patents in the chapter also demonstrates another key feature that is a hallmark of economic analyses: that well-intentioned policies may well have unintended consequences. Specifically, lowering the length of patents in an effort to restrict monopoly profits and high prices of drugs may lower incentives for pharmaceutical manufacturers to promote careful prescription of existing drugs, and correspondingly increase incentives for them to develop and enhance demand for new drugs, even at the cost of increased drug resistance.

Two of the chapters explore applications of the tools of new institutional economics to infectious disease investigation and control. Azermeen Jamasji-Pavri, in chapter 14, uses the principal–agent framework and transactions cost perspectives for studying organizations and individuals involved in the investigation of infectious disease outbreaks. In chapter 15, Pauline Allen and Bronwyn Croxson examine contracts between health departments and NHS trusts in the United Kingdom in terms of their efficacy in addressing infectious disease. Both chapters conclude that hierarchical governance structures were likely to be more effective than "arms-length" contracts in the investigation and control of infectious disease. If I had one concern, it was that the tools of economic analysis in these analyses were not well integrated with the context of infectious disease and the discussion a bit too jargon-heavy to be fully appreciated by noneconomists. One is left with the feeling that more could have been done to leverage the rich economic literature on organizations to think about appropriate governance structures for the effective investigation of the causes of infectious disease outbreaks, or their control.

How effective is the book in introducing economists to research and policy issues raised by infectious disease? At some level, this is a moot point, since questions related to infectious disease have attracted the attention of leading economists for some time (Dagobert L. Brito, Eytan Sheshinski, and Michael D. Intriligator 1991; Tomas J. Philipson and Richard A. Posner 1993; Michael Kremer, Rachel Glennerster, and Heidi Williams 2006). That said, there are at least two areas where the book can inform research in infectious disease economics. Firstly, the book contributes by its implicit appeal toward a better balance in economic research on infectious disease. There is little doubt that, excepting work on economic evaluation of interventions, usually published in public health journals, a significant amount of attention in what is usually termed "mainstream" economics literature has been directed towards HIV/AIDS. In particular, the chapters in this book point to a set of issues beyond HIV/AIDS, many of which are potentially quite significant in terms of their health and policy impacts, including infections acquired in hospital settings, the linkages between infectious disease and trade policy, the challenge of drug resistance, the ties between economics, animal health and human health, and finally food safety.

The second contribution of the *Economics of Infectious Disease* is the attention it directs to an area that could benefit from considerably more research: namely, the application of the tools of contract theory in economics to study the organizational response to infectious disease. In doing so, the work presented in the book sheds new light on the potentially important role of governance structures in the efficient delivery of health services related to infectious conditions, preventive or curative. All this is timely, particularly in the context of developing countries, where the organization of health services financing and delivery has attracted some attention, particularly around vertical provision of individual infectious disease prevention programs versus the horizontal integration of programs directed at different diseases. More generally, the need to go beyond the availability of financial resources and to paying greater attention to the organization of health services in poorer countries of the world is also one of the main lessons emerging from the 3×5 ARV treatment initiative of the World Health Organization. It is also a return to the roots of the discipline, given that issues of information and organization have a long history in health economics, dating back to at least the work of Kenneth J. Arrow (1963).

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J Labor and Demographic Economics

Chutes and Ladders: Navigating the Low-Wage Labor Market. By Katherine S. Newman. New York: Russell Sage Foundation, Cambridge and London: Harvard University Press, 2006. Pp. xii, 405. $35.00. ISBN 978–0–674–02336–9.
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In her book, *Chutes and Ladders: Navigating the Low-Wage Labor Market,* Katherine Newman traces the career paths of nearly three hundred mostly black and Hispanic individuals who were either employed at, or had unsuccessfully applied to, one of four fast food establishments in Harlem between 1993 and 1994. Newman never reveals the identity of these fast food establishments, but she tells us that they are from the same national chain, which she gives the fictitious name Burger Barn.

These Burger Barn employees and rejectees also formed the basis of Newman's 1999 book, *No Shame in My Game: The Working Poor in the Inner City.* In *Chutes and Ladders,* Newman extends the work of this earlier book by conducting two waves of follow-up interviews, first in 1997 and then in 2002, to examine whether the strong economic conditions of the late 1990s translated into career advancement for these initially disadvantaged workers. Newman's main message is one of surprised but cautious optimism. Yes, many of the workers in her sample remain poor, but a handful also experience substantial wage growth (by 2002, a few are even earning over $70,000 a year). Newman argues that these success stories offer insights into how individuals in "dead-end jobs" are able achieve economic security. Nonetheless, it is hard to know what lessons to draw from such small, nonrandom sample. Indeed, most of Newman's policy recommendations, which appear in chapter 9, are taken from other studies.

From a methodological standpoint, Newman's book proceeds along two dimensions. The first is a detailed ethnographic study of the Burger Barn sample. She traces the lives of "high-flyers" like Kyesha and Jamal whose earnings increase dramatically over time and "low-riders" like Kevin and Vanessa whose employment prospects remain poor. Although Newman's focus is on career paths, she also reveals relevant aspects of individuals' personal lives and family backgrounds, and while the people Newman examines are not necessarily representative of the low-income population in the United States, their stories are inherently interesting and offer a rare glimpse into the lives of the working poor. Through her ethnographic analysis, Newman shows us that the people in her sample actively strive to improve their labor market prospects, are cunning in their approach to social welfare programs and, despite the many obstacles they face, are not necessarily helpless victims of the low-wage labor market.

To augment her ethnographic analysis, Newman quantitatively examines wage and employment patterns for the Burger Barn sample. In addition, she draws upon data from the Survey of Income and Program Participation (SIPP) to verify whether the empirical patterns she observes hold for a nationally representative sample of low-income workers in the food service industry and for other time periods. Unfortunately, while Newman's ethnographic analysis is richly detailed and informative, her quantitative analysis is poorly executed, inadequately explained, and provides misleading support for the cautious optimism she expresses throughout the book.

First, for both the Burger Barn and SIPP samples, individuals were included in the sample partly because of their disadvantaged economic position.[1] Thus, regression toward the mean is likely to explain some part of their wage growth. Second, both the Burger Barn and SIPP samples include a large fraction of relatively young workers. In 1993, for example, 35 percent of Newman's Burger Barn sample is between the age of 15 and 19, and these individuals are likely to see

[1] For the Burger Barn employees and rejectees, the disadvantage is self-evident, and for the SIPP sample, individuals had to be at or below 1.5 times the poverty level for that individual's family at some point while the individual was employed in the food service industry.

their wages grow simply because of life-cycle wage growth.

Sample attrition also is likely to bias Newman's analysis. On page 71, Newman tells us that, in 1997, she and her research team collected data on a random sample of slightly more than a hundred of the original Burger Barn subjects. In the next paragraph, however, Newman admits that the resources available to find the subjects were limited and that attrition was high. This is especially troubling given, for example, the disappearance of men from the sample. In 1993, 47 percent of the sample is male but, by 2002, this number had dropped to 35 percent. Newman never explains why the gender composition of her sample changes so dramatically, but one hypothesis is that men who are incarcerated are hard to track down. If so, Newman's analysis would fail to capture this negative labor market outcome.

For both the Burger Barn and SIPP samples, Newman also makes the mistake of assuming that wage growth for those who are employed is the same as wage growth for those who are unemployed. For example, on page 153, Newman writes, "Twenty-two percent of the people we began following in 1993 realized wage gains in excess of five dollars per hour in real terms by the time of our follow-up in 1997." Evidently, this statement is based on a chart on page 73, which reveals that, *for those who were employed in both 1993 and 1997,* twenty-two percent saw their wages go up by more than five dollars per hour in real terms. It turns out, however, that only about 45 percent of the 1997 sample was employed in both 1993 and 1997. Thus, the people who experienced this level of wage growth made up less than 10 percent of the 1997 sample. It is, of course, convenient to assume that wage growth for the employed is the same as wage growth for the unemployed because we don't observe wages for the unemployed. Nonetheless, since individuals with a strong labor force attachment experience faster wage growth than those who are often unemployed, this assumption is likely to bias Newman in the direction of finding more wage growth than there actually is.

Finally, the book contains a number of sloppy mistakes. On page 72, for example, Newman tells us that in her Burger Barn sample the median wage in 1997 is $7.49 an hour in 1993 dollars. Then, in a footnote on the same page, she tells us that the median wage in 1997 is also $7.49 an hour in 1997 dollars, suggesting (counterfactually) that there was no inflation between those years. Taken as a whole, these sorts of errors make it hard to trust the empirical analysis

None of this is to say that Newman's main message is wrong. Indeed, evidence suggests that there is considerable economic mobility in the United States. Nonetheless, from a quantitative perspective, the facts and figures that Newman presents in her book should be taken with a healthy dose of salt. For those interested in insightful stories about the low-income population, Newman's book has much to offer. For those interested in hard facts about economic mobility in the United States or new insights into policies directed toward the low-income population, Newman's book is disappointing.

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L Industrial Organization

The Baseball Economist: The Real Game Exposed. By J. C. Bradbury. New York: Penguin, 2007. Pp. 336. $24.95, cloth. ISBN 978–0–525–94993–0. *JEL 2007–1005*

Diamond Dollars: The Economics of Winning in Baseball. By Vince Gennaro. Hingham, Mass.: Maple Street Press, 2007. Pp. xiii, 253. $24.95. ISBN 978–0–9777–436–3–6. *JEL 2007–1010*

Sports economics books seem to be plentiful these days. The 2007 season brought two new monographs: *Diamond Dollars: The Economics of Winning in Baseball* by Vince Gennaro and *The Baseball Economist: The Real Game Exposed* by J. C. Bradbury. Each attempts to tackle a distinct area within the grasp of economic analysis.

Diamond Dollars is the more ambitious of the two. Gennaro assays nothing less than an uncovering of the mysteries of the baseball economy and purports to offer a model for small market teams to be successful. Gennaro covers a wide range of issues, from the relationship between winning and revenues, to the value of players, the dynamics of player development, a strategy for assembling a winning team and building a team brand.

Along the way, Gennaro reveals a basic understanding of the economics of baseball and, although not a formally trained economist himself, a reasonable understanding of rudimentary microeconomic analysis. He addresses many disparate topics, but his core concept is the "win curve." While there is nothing particularly novel here, Gennaro's presentation puts a different spin on the marginal revenue of winning. Basically, he argues that the relevant range of wins per season is between 70 and 105. Below 70 or above 105 wins, he asserts, there is no relationship between winning and revenues. Within the range, there is a sweet spot between 86 and 91 wins that is the threshold for making it to the postseason and, consequently, it is within this sweet spot that there is the highest incremental return to winning. Making it to the playoffs, according to Gennaro, has its biggest impact by increasing a team's revenues in succeeding years. For teams below 70 wins, Gennaro's thesis is that winning a few extra games brings them essentially no new revenue, meaning they have little incentive to spend on free agents, whether or not they receive transfers from the wealthy teams.

So far, so good. But here Gennaro hits a brick wall. His text suggests that he has subjected his theory to rigorous modeling and testing, yet he provides the reader with next to no information on how he has executed his empirical analysis. The few bits of explanation he does provide fail to inspire confidence.

For instance, Gennaro apparently has done some regression analysis where revenue is the dependent variable and team wins in the current and previous years are two of the arguments. We are not informed what the other independent variables are, nor are we told whether the relationship is tested linearly, nor for what years it is tested. Curiously, Gennaro does state that he has weighted the current and lagged team wins equally and that he has run a regression separately for each team to capture its distinct win curve. It is not clear how many years of data he has in each team regression or why he did not instead use team dummies and interactive variables in a pooled regression to identify these effects.

Furthermore, Gennaro estimates his revenue data. Here he makes some reasonable assumptions, but he also misses his target on some sources of revenue. Despite these problems, Gennaro intrepidly forges ahead, glibly making specific claims about various teams' win curves. Unfortunately, most of the rest of *Diamond Dollars* suffers from similar weaknesses.

Detailed problems appear throughout the book. Here is a small sample of these difficulties. Gennaro does not adequately source his discussion. The few footnotes and citations he does provide often confuse or obfuscate the matter. He claims that he has developed a model to evaluate players' productivity, but he neither explains the model nor does he cite the pioneering work of Gerald Scully in this field or its lineage. Later in the book, Gennaro appears to assert authorship of the idea that baseball's revenue sharing would be more effective if it were based on the value of a team's market, rather than on a team's revenue. Yet this position has been amply discussed in the professional and journalistic literature. Indeed, it is even incorporated into MLB's latest collective bargaining agreement, which Gennaro entirely misses.

Gennaro also makes inaccurate claims about financial data. For example, he states that the NFL "generates over $2.5 billion in annual broadcast revenues" and that "about 80 percent of the NFL's $5 billion in revenue is shared" (p. 4). The correct numbers would be over $3.3 billion, about 70 percent, and over $6 billion. On the next page, he writes that "$40 million from MLB's central fund" is distributed equally to each team annually. The actual number in the last two years has been under $30 million. Such errors are not catastrophic. They do, however, become problematic when one tries to make detailed claims about a team's revenue or a player's value.

Gennaro devotes an entire chapter to teams' player development systems. Here too there are broad generalizations and data inaccuracies that mar his discussion. He estimates that an average team devotes $12 million yearly to its player development system (scouting, signing bonuses, operation of minor league affiliates.) The actual average is almost 50 percent higher. In a footnote, however, Gennaro states that his estimate excludes international scouting and bonuses, perhaps accounting for the lower number. Yet, it is curious why he would exclude the international aspect of player development, when today almost half of all minor leaguers come from outside the continental United States.

One of the book's concluding chapters is entitled "A Strategic Approach to Assembling the

Roster." Here Gennaro uses his hidden statistical method to find that the ratio of player productivity to player salary reveals that per dollar of output pitchers cost 14 percent more than position players and that left-handed pitchers cost 17 percent more. The inference is that team general managers should be particularly cautious in overpaying for these positions. The important question that Gennaro does not address is that if his estimates are correct, why doesn't the market correct itself over time? And if the market does correct itself, what is left of his policy recommendations? The unstartling punch line of this chapter is that low-revenue clubs can be successful by depending less on free agents and more on developing players out of their own farm system.

In his chapter on building a team brand, Gennaro offers a host of broad sweep generalizations and arbitrary weights to make standard, prosaic recommendations common to the marketing literature. One of his more remarkable statements is found on pages 193–194. To wit: "A team that tries to get through a season with a roster filled with fringe players and minor league prospects is testing the patience of its fans and risking its credibility. Even the 56-win Kansas City Royals had proven major leaguers on their 2005 squad. Matt Stairs and Terrence Long, along with their star player Mike Sweeney, complemented a host of youngsters. For all their faults in the 2005 season, the Royals maintained the credibility and trust of their fans by not ridding themselves of Sweeney's $11 million contract. For the 2006 season, the Royals loaded up on mature talent with Reggie Sanders, Doug Mientkiewicz, Mark Redman, and Mark Grudzielanek, giving hope to the K.C. faithful." This worthy team won 62 of its 162 games in 2006.

Gennaro's closing chapter has some gems on the stadium economic impact literature. On page 238, he writes: "The stadium dialogue seems to quickly degenerate into a 'he said-she said' by 'greedy owners' and 'ungrateful municipalities' about who gets the most benefit from a new ballpark and consequently, the appropriate mix of public versus private funding. The two sides seem to argue so vehemently out of self-interest, citing the results of previous parks, selectively choosing the data that fits their points of view, that they neglect to get at the key issue. 'Do stadiums enhance the local economy?' may be the wrong question. The right question is *'Under what conditions can stadiums enhance the local economy?'*" He goes on to claim that, if a team plays more day games, it will help local commerce more and, therefore, cities should bargain in their lease agreements to have teams play more day games. His discussion here is either disingenuous or completely ignorant of the academic literature on the subject of economic impact.

In the end, Gennaro's monograph misses its mark. His treatment of baseball's economic system offers little of academic value, of general reader interest, or team management assistance.

J. C. Bradbury's *The Baseball Economist* is a collection of wide-ranging essays on different elements of the game that the author believes are amenable to economic or statistical analysis. The quality of these essays is sharply uneven, with the stronger contributions related to evaluating on-field performance. His topics range from why there are no left-handed catchers, to valuing the worth of a pitching coach, whether having a strong on-deck batter actually helps the current batter or not, why there are more hit batsmen in the American League, a game theory treatment of the game's steroid problem, assessing the role of entrepreneurial behavior in the front office, an evaluation of the competitive balance issue, an introduction to sabermetrics, how much a ballplayer is worth, and whether MLB is a monopoly. Below I consider some of the collection's high points and low points. The latter, unfortunately, are more plentiful.

My favorite Bradbury entry convincingly attacks a longstanding baseball myth—a strong on-deck batter helps the current batter. Bradbury uses data from 1984–92 to test this relationship and what he comes up with is the following: for every 100 points higher is the OPS (on-base percentage plus slugging percentage of the current batter) of the on-deck batter, the current batter will walk 2.6 percent less, will have a 1.1 percent lower batting average, and will hit 3.0 percent fewer home runs. He controls for the OPS of the current batter, the game situation, the ballpark, and other factors. Hence, Bradbury concludes that a strong on-deck batter does create an incentive for the pitcher not to walk the current batter, but it also encourages the pitcher to add focus and make tougher pitches in the strike zone, thereby lowering the current hitter's average and power. While these relationships are statistically significant, their actual impact is very small. His conclusion appears solid and one wonders how long it will take before Joe Buck, Tim McCarver, Jon Miller, et al. catch on.

Bradbury's chapter on whether it pays for a manager to argue with the umpire is provocative, but unsatisfying. He describes the phenomenon of a manager protesting a call as rent-seeking behavior. The manager attempts to bully the ump, so that the ump will remember the unpleasant experience and will think twice before making another close call against the manager's team. Bradbury suggests that sometimes the managers succeed in this endeavor, although the statistical evidence he presents is too weak to support his claim. It is rent seeking because there is no net gain, no output increase, just a transfer of marginal calls from one team to another. Meanwhile, the fans, according to Bradbury, have their utility lowered because they have to spend a few extra minutes at the game due to these fits of managerial distemper. Well maybe, but it is also possible that the fans enjoy managerial protests both because they are amusing and because it vicariously vents their own frustration at bad umpire calls. As for a few extra minutes at the ballpark—hey, it's baseball.

In another interesting essay, Bradbury assembles evidence that Leo Mazzone is actually a pitching coach who makes a positive difference. Bradbury compares pitchers performance under Mazzone with their performance before and after working with him. The evidence appears to support superior skills for Mazzone. Bradbury also explains Mazzone's philosophy and method. Mazzone has now been pitching coach for the Baltimore Orioles for two years. Perhaps it is time for corroboratory evidence.

From here the book goes downhill. In one chapter, Bradbury discusses the presumed advantage that big city teams have. He concludes that "the advantage appears to be slight and virtually meaningless" (p. 80). His argument here is sloppy. Bradbury's simple regression finds that variance in city size accounts for 40 percent of the variance in win percentage over a period of years. This seems to indicate a rather substantial impact of city size. Further, the author fails to consider the interactive effect of city size and a team owning its own regional sports channel (RSN), the number of large corporations in the market, or the size of MLB's assigned team television market—three factors that would have reinforced the effect of city size. Along the way, Bradbury misapprehends the functioning of the amateur draft and overlooks the unequalizing effect of the posting system with Japanese baseball.

In his chapter on what makes for an effective front office, Bradbury singles out two desiderata: having a good team on the field and having a low payroll. Of course, all teams would like that combination, but no team can win every year on a modest payroll. A franchise is a business and there are more factors behind whether or not it will be a successful organization, including effective promotional efforts, good relations with and involvement in the host community, charitable activities, new investments, and profitability, among others. Bradbury then attempts to quantify team rankings by adding up the value produced by all the players on each team and subtracting team payroll, yielding his "net value of team play." Every team has a positive net value except his lowest ranking team, the Yankees with a negative net value of $29.8 million during 2003–05. This anomalous result comes from the wrongheaded methodology he uses to estimate player marginal revenue product (to be discussed below) and from the narrow definition he adopts to identify team success.

His chapter on steroids in baseball employs game theory to model the choice that a player makes whether or not to indulge. He argues that when every player chooses to use steroids it is a Nash equilibrium. This result, however, appears to depend on his arbitrarily chosen values for the supposed productivity gain and the health costs (only $500,000) from indulgence. Bradbury's analysis ignores the enormous uncertainty that surrounds this choice for players.

Another chapter, "Scouts and Stat-Heads," provides a generally useful introduction to sabermetric analysis, but Bradbury gets a bit starry-eyed over the presumed power of sabermetrics. On page 147, for instance, Bradbury writes: "Old ways and old scouting methods may disappear, but the end result is a good one for the fan: better and cheaper baseball." No team is contemplating the elimination of traditional scouting methods, nor is one likely to in the future. New statistical methods have been employed to supplement, not supplant, traditional scouting.

Bradbury's essay "How to Judge a Hitter or a Pitcher" also overplays the sabermetric hand. Baseball analysts frequently refer to a statistic called hitting with "runners in scoring position" (RISP). It is meant to represent how effective a clutch hitter a player is. Bradbury says folderol: "The problem is that hitting with RISP is not a skill . . . but a statistical anomaly" (p. 155). "If hitting with RISP is something a hitter can purposely alter, I have a hard time believing he is holding something back in non-RISP situations" (p. 156). There you have it—there is no such thing as clutch hitting. This is an awfully linear, materialist view of the world where a player's emotions and his state of physical depletion over a 162-game season play no role. Further, Bradbury is being inconsistent. In his chapter about the on-deck batter, he asserted that a pitcher can ramp it up and pitch more carefully and effectively to the current batter when a strong batter is on deck. So, in Bradbury's world, pitchers can focus and pitch in the clutch, but hitters can't turn the same trick. Later in the chapter, Bradbury endorses the proposition from *Moneyball* that on-base percentage (OBP) is three times as important as slugging percentage (SLG). He arrives at this outcome by running a multiple regression of runs on batting average, OBP and SLG. The coefficient on OBP is almost three times that on SLG. The problem here is not only that the arguments are collinear and the coefficients are less reliable, but that SLG (it counts a homerun as four hits, a triple as three, etc.) is a much higher number than OBP. The coefficient, therefore, will necessarily be smaller on SLG. If elasticity is used instead of the estimated coefficient, OBP is 1.8 times greater than SLG.

Bradbury also discusses the assessment of pitching skills in this chapter. The main argument here is that a pitcher's ERA from one year to the next is highly variable, but that a pitcher's walks, strikeouts, and home runs allowed are more stable over time. The inference is that ERA depends more on outside factors, such as a team's fielding prowess, and, hence, is a poor measure of the inherent skills of a pitcher. While there is something compelling to this logic, it seems caution is in order. First, a pitcher's skills may actually vary from year to year, along with his ERA, as other factors change, such as, his ballpark, his pitching coach, his bullpen, his team's offense, the angle of his arm slot, his confidence level, etc. This variability does not mean that the skill is spurious. Second, if all we consider is strikeouts, walks and home runs, what are we saying about sinkerball pitchers who induce groundballs or pitchers who throw fastballs with movement or offspeed pitches that induce weak swings and popups? Didn't Bradbury already write that, with a strong on-deck batter, pitchers can pitch more effectively within the strike zone?

Next, Bradbury offers a chapter on the worth of a ballplayer. He gets off to a bad start here by misrepresenting the functioning of the players' market and the terms of the collective bargaining agreement. He then misspecifies his team revenue function, leaving out RSN ownership, the number of large corporations in the host market, the size of the team's assigned television territory, among other factors. But the fatal problem is that Bradbury's methodology unwittingly identifies a player's *average* revenue product, not his *marginal* revenue product. By his reckoning, all of a team's revenue is attributed to the players, leaving nothing left over for front offices expenses, stadium expenses, minor league operations, or profits. Given this misstep, it is not surprising that Bradbury finds players at all levels (under reserve, arbitration eligible, and free agents) are paid less than what he estimates they are worth.

Bradbury then moves on to the baseball product market, asking "Is MLB a Monopoly?" Bucking all scholarly analysis and legal decisions on the question, Bradbury writes "I'm not sure MLB is a monopoly" (p. 201). Then, in a comedy of errors, he explains his ambivalence. He writes that Judge Kenesaw Mountain Landis' decision that baseball was not interstate commerce gave the game its antitrust exemption and that "the Supreme Court has upheld the Landis decision on several occasions" (p. 203). Here Bradbury is confusing two cases. The first was a suit brought by the owners of the Federal League teams in January 1915 against baseball's reserve clause. This case went before Judge Landis. Landis, however, never issued a decision. The parties settled at the end of the year. The second was a litigation brought by the owners of the Baltimore Terrapins of the Federal League because they did not believe that the terms of the settlement were fair to their franchise. After losing in district court (where Landis played no role), baseball appealed the decision and won. The Terrapins then appealed before the Supreme Court, where the case was heard in April 1922. A few months later the Supreme Court decided in baseball's favor and the antitrust exemption was born.

Bradbury then distorts the record further by asserting "At the heart of the argument that MLB acts like a monopolist is the existence of the antitrust exemption" (p. 205). He cites no sources for this claim, because there are none. Each team sport league is a monopolist because it is the sole producer of its product and has no close substitutes. The NFL has no blanket exemption and it is a monopoly; likewise the NBA. Bradbury then writes referring to the NFL, NBA, NHL, and MLB that "each of these enjoys some antitrust exemptions for collective bargaining with labor unions. . . ." Here, of course, it is not an exemption granted to the leagues, but a general statutory exemption granted to all labor unions by the Clayton Act of 1914. Bradbury continues "There is no strong evidence that the antitrust exemption provides any monopoly privileges to MLB other than protecting it from expensive lawsuits" (p. 208). While the value of baseball's exemption today is not what it used to be, there is still a good case to be made that MLB's minor leagues and perhaps its amateur draft could not exist in their present form were it not for the exemption.

Bradbury's last essay argues that the market for top-level professional baseball in the United States is contestable. If this were true, then the earlier question about whether or not MLB is a monopoly might be moot. Here Bradbury makes two points. First, if there is an aspect of the industry that is not a natural monopoly and, hence, constitutes an artificial barrier to entry, it is the subsidies from local governments that teams receive for the construction of their stadiums. But, he avers, this is not really an issue because "the public does not seem averse to subsidizing major sports teams from leagues other than the dominant existing league" (p. 220). It is clear that Bradbury has never been involved in starting a new or nondominant league. His notion that politicians are not averse to providing subsidies to teams from these upstart leagues is just plain wrong. Second, Bradbury goes on to argue that MLB's market is contestable. He does this by discussing the emergence of the American Association in 1882 and the American League in 1901. He further adduces what he erroneously calls the "Central League" (real name: the Continental League) forcing baseball to expand the number of its teams in 1961. Leaving details aside, the difficulty with Bradbury's claim is that the industry's economic structure today is very different from what it was 57 or 120 years ago.

Bradbury, then, whiffs in his effort to expand his analysis beyond the narrow confines of the baseball diamond. After a promising beginning, *The Baseball Economist* fails to expose the real game.

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Lectures on Antitrust Economics. By Michael D. Whinston. Cairoli Lecture Series. Cambridge: MIT Press, 2006. Pp. xii, 249. $30.00. ISBN 978–0–262–23256–2. *JEL 2007–0599*

Whinston's elegant volume, derived from lectures given at Torcuato University in Argentina, drills into three important topics in competition policy: collusion, mergers, and exclusive contracts. Its coverage of both theoretical and empirical work on these topics is thorough and up to date. At the end, the reader is left hoping for a successor volume on other important topics, notably predatory pricing, tying, bundling, vertical mergers, and vertical price fixing.

The volume is laudably free of the narcissism that infects many books derived from invited lectures. Not only is the work of many other theorists given full weight, but the author reports extensively on empirical work, notwithstanding his firm placement in the tribe of theory.

Whinston apologizes for his focus on economics and his limited treatment of antitrust law, but I find this a strength. For one thing, the law is gradually shifting toward the principle that an antitrust case is a demonstration that a specific intervention in a market improves social welfare. To prevail in a challenge to a merger, for example, the government needs to demonstrate that customers would be better off without the merger than with it. Modern courts are losing their single-minded devotion to the formulaic approach of defining a relevant market, measuring market power within that market, and only then considering the effects of conduct challenged as harmful to competition. In place of that rigid formula, modern courts would like to know by how much the conduct has raised prices or diminished product quality. Whinston provides sophisticated guidance to economists involved in this process. The audience for the book is the well-trained specialist in modern economic theory—you won't get far in this book unless you can handle most of the end-of-chapter problems in Mas-Colell, Whinston, and Green's *Microeconomic Theory.* Lawyers may want to hire a member of that fraternity to guide them through the material in Whinston's volume.

The central message of the book is that modern competition economics is way, way more complicated and ambiguous than you thought. Even the most alert student of the literature in this field will find a number of surprising "ah, but no" propositions here. Surely it is a good idea to prevent horizontal rivals from talking to each other. *Ah, but no.* The leading theory of the successful cartel posits that the cartel will punish cheaters by reverting to low-profit competitive prices. If cartel members are in touch with each other, they can renegotiate after cheating occurs, to avoid inefficient mutual profit losses. In fact, they cannot resist the temptation to renegotiate, as they lack any way to commit to carrying out the threatened punishment. But cheaters, knowing that the punishment is an empty threat, cheat away and the cartel fails. Barbara McCutcheon is responsible for this point.

Punishment for antitrust violations often takes the form of monetary damages. This is the exclusive sanction from civil antitrust proceedings and is increasingly the way that the Justice Department formulates monetary penalties in price-fixing cases (though, oddly, not in other government antitrust cases). By setting out quantitative models and econometric methods for measuring the effects of conduct that harms competition, the volume provides extensive help to those who measure damages. In early pages, though, it has a wonderful *ah, but no* insight: If the victims of price elevation know that they will recover damages for the amount a cartel raises prices, their willingness to pay rises by the amount of the damages. This enables the wrongdoers to set even higher prices. Whinston gives a full analysis of the resulting equilibrium, considering the multiplier relating damages to price elevation (usually three) and the probability that the misconduct will be detected and punished. He does not go on to the next step, which is to alter damages principles to pay most of the damages to the government rather than the victims, though this point has been made by others in the related context of punitive damages. The puzzling reluctance of the government to take serious money away from serious violators, apart from price-fixing cases, diminishes the payoff to government involvement in antitrust enforcement.

The first of the three topics in the book—and the one with the most surprise value—is price fixing. Whinston writes that this chapter ". . . covers what is undoubtedly the most settled area of antitrust. Here I try to unsettle the discourse a bit, suggesting that economists know less about price fixing than they think" (p. 3). He starts with the familiar proposition that our leading framework for thinking about collusion cannot distinguish tacit from explicit collusion. The framework of Nash equilibrium describes an equilibrium but often says nothing about how the participants got to the equilibrium. Antitrust law condemns overt agreements among rivals to cut output and raise price but is less clear about tacit collusion. Within the modern economic view of antitrust—that economists should demonstrate that a particular feasible intervention in a market serves the interests of the public—a prohibition of tacit collusion may fail the test of feasibility. How are we to formulate instructions to firms to avoid tacit collusion? Lawyers—and lamentably many antitrust economists—say that firms should be limited to "competing on the merits," but as Whinston argues convincingly, we don't know how to write the manual of permissible conduct to implement this proposition.

One of the clearest signs of the advanced nature of the book is that Whinston presumes knowledge of modern dynamic oligopoly theory in his discussion of price fixing. Before you pick up this book, be sure you have mastered the basic idea of that theory: High prices are an equilibrium because all sellers know that, should one defect and take away more than their share of the market by setting a lower price, the others will respond by setting low prices in subsequent periods. The notion of a trigger strategy, at the core of modern theory, is not mentioned anywhere in the chapter or the book, because prospective readers know it by heart. This is Economics 257, not 202, and especially not 101.

The theory of "cheap talk" tries to deal with the central question of how communication among rivals might help them achieve the benefits of tacit collusion. Whinston's verdict on cheap talk is skeptical. He does not believe that theories of this type have delivered much so far, though he observes that economists, including himself, generally believe that communication facilitates tacit collusion. Theories to confirm this common-sense belief have eluded economists to date.

Would-be cartel members face substantial obstacles to gathering the information they need to run an effective cartel. Whinston reviews a body of research that treats this as a revelation problem of the type first considered by James Mirrlees in the context of the problem the government faces in trying to determine an individual's ability to pay for tax purposes. Recent work by Susan Athey and Kyle Bagwell elucidates solutions for revealing cost. Whinston notes a paradox—this information may serve the public interest because it may enable a cartel to allocate output to the efficient producer.

Eliciting information about the information of greatest importance in operating a cartel—adherence to the cartel's agreement on prices or quantities—is a particular challenge to the success of a cartel. A good fraction of the evidence on the incidence and effects of cartels arises in government procurement auctions; Whinston discusses the literature on this point late in the chapter. The sunshine philosophy of government unfortunately aids collusion by solving some or all of the cartel's problem of obtaining reliable information about the actual conduct of putative cartel members. In private business-to-business procurement, buyers go to elaborate lengths to keep the terms of the agreement with the winning bidder secret from the losers. A widespread practice is the off-invoice discount. Only a handful of top executives in the buying and selling companies know the true terms of the transaction. A rival who is able to gain access to an invoice to check adherence to an implicit or explicit agreement about prices will get the impression that the cartel is working, when in fact buyers are paying less than the cartel price. In many industries, the only reliable information available about rivals is their productive capacity. Whinston does not consider cartel theory under this information limitation, but it would be a useful addition to the modern theory of collusion.

Whinston reviews empirical work on the benefits of breaking cartels and other interventions against price-fixing. The general tone of this commentary is that the measured benefits seem fairly small, though definitely detectable. Effects in nonauction markets are almost entirely in single digits. The biggest effect—40 percent—is in a government sewer-construction auction. In a notorious nonauction setting, the market for the animal-feed ingredient, lysine, Whinston's figure 2.4 challenges the reader to find effects associated with the formation and elimination of the cartel. The plaintiff's expert in the civil litigation found an elevation of 18 percent, but the figure suggests that the estimate may have been the subject of vigorous dispute.

The chapter on mergers is generally skeptical of the current practice of estimating their effects using static oligopoly models. In static models, a merger that does not lower marginal cost must necessarily raise prices. Oliver Williamson introduced the proposition—highly influential in merger policy today—that one must look for efficiencies of mergers that lower marginal cost to identify the ones that are good for customers. *Ah, but no,* teaches Whinston. A merger raises the payoff from cheating on a cartel, especially among the sellers not involved in the merger. Thus the merger may preclude an effective cartel equilibrium. In a dynamic setting with trigger strategies, the effect of a merger on prices is ambiguous when it has no effect on marginal cost. Whinston does not come back to this point in his extensive later review of agency procedures for evaluating mergers. The prevailing view among enforcers is that increased concentration makes collusion more likely, so they add a factor for the "coordinated effects" to the "unilateral effects" measured by a one-shot oligopoly model, usually Bertrand. Dynamic oligopoly models have not entered merger-enforcement practice yet. The main reason is the great diversity of equilibria in dynamic models.

The merger chapter spends far more effort than is merited on the formulaic process laid out in the *Merger Guidelines* of the FTC and the Justice Department. As a practical matter, sponsors of a merger gain more traction at the agencies from a direct demonstration of a favorable or neutral effect on prices than they do by defining a relevant market and measuring the change in concentration in that market, following the recipe in the *Guidelines.* This is visible in Whinston's discussion, where the analysis needed to apply the market-definition principles overlaps substantially with the analysis needed to measure the unilateral effects of a merger. Soon, the *Guidelines* will read, "The FTC and Justice

Department review proposed mergers by estimating the effects of the merger on the prices and other characteristics of all products affected by the merger." When this advance occurs, Whinston can take part of the credit.

After slogging through the twenty-two pages devoted to the *Guidelines*, I encountered section 3.5, "Breaking the Market-Definition Mold," with great relief and satisfaction. Here Whinston turns to the methods that economists use in practice to evaluate mergers and the findings that are more likely to influence the agencies in modern merger disputes. The two of most interest are merger simulation and event studies in the stock market.

The obvious defect of the market-definition approach is that it takes a binary in-or-out, weed-or-flower, stand on what we consider parameters, the cross-elasticities of demand between other products and a product affected by a merger. A merger simulation model includes all the products with sufficiently large cross-elasticities (positive or negative) to have significant roles in the calculations. Are SUVs in the same market as compact cars? That is a conundrum for market definition, but a merger simulation would probably include a small positive cross-price elasticity, capturing the small but discernable substitutability of the two kinds of vehicles.

A limited amount of evidence based on comparison of merger-simulation predictions of price changes with actual postmerger prices changes is sobering, as Whinston demonstrates in table 3.1 for airline mergers. The correlation is rather lower than the sponsors of merger simulation could wish for.

Although Whinston ultimately comes down in favor of merger simulation as the best practical alternative, he reminds the reader that the assumptions of the models used in practice are fairly strong. Because they are not dynamic, they cannot deal with collusion supported by trigger strategies and thus miss any changes in collusion that result from a merger. The agencies supplement the findings of merger simulation models with more informal consideration of the coordination effects that those models omit.

Event studies play a role in the quantitative analysis of mergers. These studies measure the changes in stock prices of merging companies, direct rivals not involved in the merger, and customers, that occur when the surprise of an intended merger becomes known to traders. The change in the combined value of the merging companies measures the joint effect of reduced competition and efficiencies, so it has no direct role in merger evaluation. The perplexing number of merger announcements that result in a decline in the combined value, such as HP–Compaq, raises interesting issues, not mentioned by Whinston. The two danger signals in the stock market are increases in the stock prices of rivals—presumably signaling their benefit from reduced competition—and decreases in the stock prices of the customers who will be paying higher prices.

Whinston notes that the statistical power of event studies may be limited because the stock market is noisy, so random variations in stock prices from unknown sources may confuse detection of merger effects. A finding of no significant effect on a stock price has no strong meaning—the effect may be buried in the noise. But a finding of a significant effect is just that—one that is unlikely to be the result of random noise. Many controversial mergers have had effects on stock prices with p values below 0.01.

Whinston concentrates on a deeper problem: an event study measures the impact of *all* of the information in a merger announcement, not just the effects that concern evaluation of the competitive effects of the merger. He observes that traders may infer that rivals will benefit from the same alteration in the economic environment that caused a pair of firms to merge. Their stock-price increases are not pure signals of diminished competition from the merger.

Students of merger enforcement can learn a lot from the limited number of court trials of merger challenges that have occurred (most of the time the merging companies call it off if an agency announces a court challenge). Whinston discusses the trial of the proposed merger of Staples and Office Depot only in the context of econometric studies supporting rival market definitions—office superstores against all office-supply retailing. The parties also introduced evidence about the effects of increased competition when a new superstore opened in a particular market. Notwithstanding any ideas about in-or-out market definition, a convincing showing that competition reduced prices in superstores would support the proposition that the merger would raise prices in those markets where Staples and

Office Depot competed before the merger. The court disallowed the merger.

A more recent merger trial, on the Justice Department's challenge to the merger of Oracle and PeopleSoft, has important lessons as well, but is not included in Whinston's discussion. The products at issue were software packages sold to large businesses. A customer buys a set of packages in what amounts to an informal auction, first qualifying several potential suppliers and then soliciting repeated bids until the bidding stops, as in an English auction. The Justice Department engaged a leading auction economics specialist to re-run the actual auctions under the assumption that PeopleSoft and Oracle coordinated their bids rather than acting as rivals. He found important price elevation in those instances where the two companies bid against each other, especially where there was little involvement of other potential suppliers. The court found the government's case unconvincing, not because it departed from the relevant market formula but because it did not go far enough in restating the environment under the hypothetical merger. The court approved the merger, despite the court's other finding that Oracle had failed to demonstrate any efficiencies from the merger.

Both merger trials demonstrate that the agencies and the courts actually put a good deal of weight on analyses that tackle the central issue: What will a merger do to prices?

Whinston's last chapter, on exclusive contracts, has rather a different character because this topic is where he has made most of his many contributions to competition analysis. The *ah, but no* propositions come in layers. The Chicago School (now seen as oversimplifiers even at Chicago and certainly at Northwestern, twenty-seven miles north) analyzed exclusivity as a paid-for element of a purchase. Under the assumption of no externality, the purchaser and seller will bargain to the socially optimal combination of price and exclusivity. Where exclusivity is observed, there must be some efficiency payoff that enlarges the pie enough so that the seller comes out ahead, even after paying the customer for exclusivity.

Ah, but no_1 say Aghion and Bolton in an important 1987 paper that is the starting point for Whinston's analysis. By signing a contract that requires buyers to compensate the seller for lost profit if a buyer decides to buy from an entrant, the buyers and incumbent seller can take away any prospective profit from an entrant. Microsoft used such a contract with computer makers prior to the 1995 consent decree with the Justice Department. Inefficiently little entry will occur. Chicago is wrong because there is no intrinsic joint benefit to exclusivity. *Ah, but* no_2 says Whinston, because this analysis makes the unpleasant assumption that the buyer and seller commit to the action in advance, despite the mutually profitable opportunity to renegotiate once entry has occurred. The renegotiation—as so often!—vitiates the power of the threat.

Ah, but no_3 follows right on: Whinston's work with Ilya Segal adds increasing returns, with the implication of negative externalities across buyers. In this setting, the seller can purchase exclusivity and its attendant barrier to entry at little or no cost. As Whinston states, "The protection of competition is, in a sense, a public good" (p. 143). Under some conditions, exclusivity may not cost the seller anything.

The reader may not find Whinston's ensuing discussion of the intricacies of modern exclusive dealing theory as fascinating as he does, and may want to sample selectively from the many variants he discusses. His focus is entirely on exclusive contracts and he does not go the additional step to study vertical integration by merger.

The book ends with an interesting discussion of the limited empirical research on the consequences of exclusive contracts. Event studies of legal changes suggest that customers are harmed by changes that permit more exclusivity. In beer distribution, where exclusivity is required in some states and banned in others, exclusivity results in slightly higher prices and substantially higher quantity, suggesting that exclusivity causes some reduction in competition by raising barriers to entry and a lot more sales effort by exclusive distributors.

Anyone who has passed Economics 202 and has a practical or theoretical interest in modern competition issues will benefit enormously by spending time with Whinston's excellent book.

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Government Failure versus Market Failure: Microeconomics Policy Research and Government Performance. By Clifford Winston. Washington, D.C.: Brookings Institution Press, Washington, D.C.: American

Enterprise Institute for Public Policy Research, 2006. Pp. xi, 130. $39.95, cloth; $16.95, paper. ISBN 978–0–8157–9390–8, cloth; 978–0–8157–9389–2, pbk.

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When policymakers spot a market failure or, in some cases, imagine that there is some market failure even if there is not, they often assume that government intervention will necessarily improve things. Unfortunately, that is not often the case, as many government programs fall short using the same yardstick one might use to assess the performance of markets. In his classic essay, Charles Wolf Jr. used the term "nonmarket failure" to characterize public policies that also underperform on efficiency grounds. In my textbook with Joseph E. Harrington Jr. and John M. Vernon, we coined the term "government failure" to characterize these failures of government policy, and Clifford Winston uses this label as well.

In this thoroughly researched and well-written book, Clifford Winston takes the government failure concept and explores the wide range of insights that can be derived from examining the systematic failures of government policy. His analysis cuts a broad swath through the entire domestic policy area, ranging from regulatory policy to antitrust efforts and public expenditure programs. Because of the breadth of scope, the treatment of some topics is often relatively brief. In many passages, every new paragraph reviews another failure of government policies. Despite the somewhat breathless pace of the book, it functions superbly as a summary of the key contributions by economists across an extremely wide range of policy domains.

Winston's treatment focuses on what most economists would recognize as a generally accepted efficiency goal: "To what extent does a market failure policy improve social welfare?" (p. 11). While this objective may be compelling to economists, the controversy surrounding the nominations of the directors for the U.S. Office of Management and Budget Office of Information and Regulatory Affairs during the Bush administration suggests that many interest groups remain strongly opposed to a meaningful comparison of policy benefits and costs.

Chapter 3 begins a series of case studies of government policy failures. This chapter focuses on antitrust policy, which is perhaps the most well-established policy area that he considers. Winston notes that many antitrust actions have not enhanced competition significantly or decreased prices, with the possible exception of the breakup of AT&T. For empirical assessments such as this, Winston is on firm ground.

The more controversial claims pertain to mergers, in which Winston observes that the federal authorities have often attacked mergers that would have enhanced efficiency and might have benefited consumers as well. This claim may be true. Unfortunately, a retrospective policy assessment such as this is much easier to make than it would be at the time of the merger approval decision. The cost savings from mergers are often quite speculative, as are the effects on competition in the market. While the direction of the effects is often clear, the magnitude of the consequences from shifts in the HHI index is less well-established.

A major benefit of Winston's comprehensive analysis is that many of the insights generalize to efforts that are high on the current policy agenda. His detailed critique of agricultural price supports is particularly timely given the current rush to subsidize ethanol, which is the questionable but much acclaimed solution to our energy dependence and pollution problems.

In chapter 4, Winston turns his attention to the more contemporaneous social regulations. This entire body of regulatory policy has had quite mixed performance, with costs per expected life saved well out of line with any reasonable benefit–cost balance. Indeed, this imbalance is so great that many of these regulations may be counterproductive, in that the opportunity costs from profligate expenditures may lead to more increases in risk than the risk decreases that result directly from the regulation. Thus, if the regulations had not been issued, the savings in costs to consumers and the public generally would have had a more beneficial effect in reducing risk than the regulations themselves.

Winston does not focus on the more widely discussed cost-per-life-saved estimates or risk-risk analyses, but instead addresses a series of case studies designed to provide general lessons for social regulation policies. In the context of labeling, he observes that hazard communication efforts of various types can play a productive role and can be effective. For example, the dolphin-safe tuna labeling, which was subsequently standardized through the Dolphin Protection

Consumer Information Act of 1990, was a major success for eco-labeling. On the other hand, labeling regulations can have a counterproductive effect. For example, John Calfee and I view cigarette advertising regulations as having an unintended effect of discouraging the introduction of safer cigarettes.

A classic regulatory failure is with respect to the drug approval process for new drugs. Winston's review indicates that many economists have long argued that the Food and Drug Administration (FDA) strikes an inappropriate balance between type I and type II errors. The agency is much too concerned with the risks of approving a harmful drug as opposed to the risks associated with failing to approve a beneficial drug. Moreover, current pressures on the FDA are in the opposite direction. The most vocal critics urge more black box warnings for dangerous drugs, not an expedited approval process.

How one can change this policy emphasis is unclear because the identified lives associated with the harmful, bad drug that is approved often loom much larger than the statistical lives that would be saved by approving a beneficial drug. Identifiable victims such as those harmed by a new drug, or a well established constituency such as AIDS patients who can benefit from expedited approval of AIDS drugs, fare well in the political process. However, the prospective beneficiaries of heart medicines may not yet know that they belong to the class of people who will benefit from drug approvals.

The analysis of automobile and transportation related regulation is especially strong. Clifford Winston explores the many government policies that address externalities, such as the Corporate Average Fuel Economy standards. His conclusion that the most influential factors in promoting the purchase of fuel-efficient cars have been the rising price of gasoline and profit-maximizing incentives stemming from the market provides a valuable lesson for current policymakers who are grappling with the task of selecting the most effective policy option to limit greenhouse gas emissions.

The book then turns to the public production area, for which Clifford Winston has been a leading contributor on a wide variety of topics. Winston is an advocate of congestion tolls to make more efficient use of roads and decrease the delays in traffic. As the London experience suggests, these policies have some promise inside the Beltway and in large urban areas such as New York. But for parts of the country where traffic congestion means being stuck behind a slow truck in a no-passing zone, higher gas taxes might be a better solution to our broader transportation policy problems.

Throughout the book, Winston flags policy areas, indicates possible solutions, and gives a guide to the pertinent literature, but does not explore individual policy areas fully. The book aims for breadth rather than depth on particular topics. Winston's volume delivers an incredible wealth of case study vignettes but is not a substitute for more in-depth analysis of the particular topic areas.

Nevertheless, readers will be rewarded with an astounding array of interesting economic results. For example, with the exception of BART in the San Francisco Bay Area, Winston concludes that "every U.S. transit system actually reduced social welfare" (p. 70). Less surprising is his critique of the U.S. Postal Service, although the extent to which the Postal Service is inefficient or is being adversely affected by cream-skimming by Federal Express and United Parcel Service is not examined within the context of this relatively brief treatment.

What are we to make of this rather daunting array of government failures? Clifford Winston offers a variety of theories and explores the role of interest groups. What I found to be particularly valuable was his table that compiled the welfare costs of market failure policies. By giving tallies of the costs across different areas of government intervention, we can get a sense of what the big losers are and where we should put our emphasis in terms of policy reform. Although some of the cost estimates are gross costs, often because net cost figures are not readily available, many of his cost figures are deadweight losses. Commodity price support programs, tariffs and quotas, inefficient pricing and investment in highways, and inefficient pricing and investment in airports all impose quite staggering losses on the economy, well in excess of $10 billion each.

Winston's perceptive analysis also emphasizes three reasons other than these multi-billion dollar price tags for why his concerns about government failure are of fundamental policy importance. First, because government efforts to address market failures comprise a major portion of what the government does on the economic front, the poor performance of these diverse government interventions constitutes a substantial drag on economic growth. Second, as Winston's thorough critique makes clear, empirical evidence on the performance of government policies provides a sounder guidance for structuring our policy approach than the ideological arguments that often dominate such debates. Popular commentators and economists engaged in policy debates would benefit from drawing on the empirical lessons regarding the performance of policies to address purported market failures rather than assuming that all government policies will be effective, regardless of how ill-conceived or mismanaged these policies might be. Instead of relying on ideology, Winston's book provides a framework for reframing current policy debates in an empirically based, scientific manner. Third, *Government Failure versus Market Failure* highlights policy areas where more empirical research is needed. Winston's excellent volume concludes with a compelling general plea for more policy-oriented economics research.

Even if some well intentioned policymakers are armed with the wealth of case studies documented in this book, they will find that bad government policies are difficult to alter. Once policies are in place, constituencies in favor of the status quo resist change. For example, once companies have complied with costly government regulations, they often want those requirements to continue as a barrier to the entry of competitors. Reform is much more likely to be successful in altering new initiatives on the policy agenda.

Reform advocates will find in this book a manageable checklist of economic insights to apply to policy design. Winston's policy recommendations draw on mainstream economic principles, combined with a keen empirical sense of the types of policies that have proven to be successful. He advocates efficiency tests for antitrust policies, opposes agricultural price supports and quotas, supports deregulation in many economic regulation contexts, and supports emissions trading to address environmental externalities. Advocates of current failed government policies are usually well aware of these more efficient policy approaches. But the concerns that drive policy often bear little relation to the naive and hopeful notion that the government maximizes social welfare. Although reforming government policy will not be a simple task, economists and policymakers who are searching for ways to improve social welfare will find this book to be a valuable starting point.

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Personnel Economics in Imperfect Labour Markets. By Pietro Garibaldi. Oxford and New York: Oxford University Press, 2006. Pp. xvii, 258. $99.00, cloth; $45.00, paper. ISBN 0–19–928066–5, cloth; 0–19–928067–3, pbk.
JEL 2006–1079

This book is an attempt to consolidate what we know about Personnel Economics by focusing on *Personnel Economics in Imperfect Labor Markets.* Even on the first page of the book, the author is clear about this mission. In particular he notes that "The view of personnel economics analyzed in this book is based on two key properties of . . . labour markets: labour markets are imperfect and jobs are associated to [sic] rents; labour market institutions interact with personnel policies. Notably, wages are partly set outside the firm–worker pair (minimum wages and collective agreements are widespread)" and "job termination policies are affected by a sizeable and binding employment protection legislation." This is a worthy goal and the idea for writing a book that focuses on imperfect labor markets is a very good one.

The book contains thirteen chapters. In the first, "Personnel Economics and Non-Competitive Labour Markets," the author outlines the book by nicely describing that market imperfections create some interesting complications in personnel economics that are absent in perfect labor markets. This includes some motivation with basic statistics from the OECD on differences in union density and employment protection legislation in a set of six countries and the

European Union. The second chapter, "The Optimal Skill Ratio," discusses two main issues: the hours/employment trade-off and the skill composition of the labor force. This introduces ideas of isocost lines and isoquant curves and a bit of calculus. It also describes the idea of wage compression and introduces some data across five countries. Chapter 3, "The Hours-Employment Tradeoff," includes a detailed discussion of the difference between variable and fixed costs and derives a formal solution to the employment-hours trade-off. It goes on to show, from a theoretical point of view, what might happen in the presence of an overtime premium and the potential effects of reducing the workweek. The chapter ends with a nice discussion of the reduction of the workweek in France in 1982.

Chapter 4, "Temporary or Permanent?," is a discussion of trade-offs between open-ended and fixed-term contracts. In particular, the chapter examines three separate issues. First is firm choice of either temporary or permanent contracts. Second is combining temporary and permanent contracts. Finally, the author examines hiring a temporary worker and using a temporary help firm as an intermediary. The chapter ends with some data from the OECD on the incidence of temporary workers by country over time. In chapter 5, "Selection in Recruiting," the author discusses contingent contracts, signaling, and the simple schooling model.

The next three chapters concern compensation. Chapter 6 is titled "Optimal Compensation Schemes: Foundation." This chapter introduces concepts of indifference curves and examines a simple principal–agent model, makes comparisons between risk-neutral and risk-averse agents, and sets the groundwork for subsequent chapters. In chapter 7, "Pay for Performance with Wage Constraints," the author introduces the idea of workers with heterogeneous abilities. This chapter also describes performance pay with a two-tiered wage system and Edward P. Lazear's (2000) Safelite Glass example of one firm switching from salaries to piece rates. The third compensation chapter (chapter 8), "Relative Compensation and Efficiency Wage," introduces additional ideas such as tournament theory and efficiency wages.

Chapters 9 and 10 focus on training. Chapter 9, "Training and Human Capital Investment," introduces the ideas of benefits and costs of education, discounting, and general versus specific capital. Chapter 10, "Training Investment in Imperfect Labor Markets," builds on chapter 9 by considering firm-sponsored training with specific focus on the temporary help industry.

In "Job Destruction" (chapter 11), the author covers issues of "firm-initiated" separations (including a discussion of wage cuts), imperfect labor markets, labor hoarding, and the effects of "firing taxes." Chapter 12, "Further Issues in Employment Protection Legislation," examines employment protection legislation including taxes and transfers. The book concludes with a chapter on "Teams and Group Incentives." This includes a discussion of the "one over N problem," remedies for compensating teams, and an example from a firm called Koret, in the garment manufacturing industry that changed its compensation scheme from individual piece rates to group piece rates. This is based on a paper by Barton H. Hamilton, Jack A. Nickerson, and Hideo Owan (2003).

If this were the only book that a newcomer to the field were to read about Personnel Economics, there is quite a lot of value added within the pages. However, there are a number of things that surprised me about the book. I expected the book to cover more new ground. There is quite a bit of overlap with Lazear (1995), Lazear (1998), and a set of undergraduate labor economics textbooks such as Ronald G. Ehrenberg and Robert S. Smith (2006) and George Borjas (2004). Garibaldi's book seems written on roughly the same level as the others. It uses some calculus and is more theoretical than Lazear (1998), Ehrenberg and Smith (2006), or Borjas (2004), although it is less technical than Lazear (1995). It is also written from a European perspective, and it is this last bit that sets it apart most.

The book reads less like a book than like an effective set of (mostly) independent lectures. There are few transitions between chapters. A particularly stark example of this occurs at the start of chapter 11 (p. 187). After just having completed a discussion of training in imperfect labor markets, the author jumps straight into a chapter on "Job Destruction." Both subjects are important and worthy of discussion. However, a connection between these chapters would improve the book. There are few individual conclusions to chapters, many of which end quite abruptly. The book also lacks a concluding section to tie everything together.

Given the title of the book and its introduction, I was expecting more of a focus on imperfect labor markets. Chapter 1 nicely outlines some of the potential imperfections in the labor market, such as minimum wage legislation, union density, and employment protection legislation. Given that the author is employed by a European university, I expected to see a steady diet of "imperfection details." In fact, many of the chapters, although nicely done, were largely about Personnel Economics in *perfect* labor markets. The main chapters on compensation (6–8) are largely standard discussions of the economics of personnel. This is also true of the first chapter on training and human capital investment, chapter 9.

While the book does a fine job covering theoretical ideas, I expected more new empirical examples. Some of those examples, e.g., Lazear's (2000) classic on Safelite Glass, are very good. There are other places where the text would have benefited from a more detailed discussion of well-known literature. For example, in chapter 5 during the discussion of productivity versus signaling models of education (p. 80), some of the empirical literature in this area could have been mentioned (e.g., Thomas Hungerford and Gary Solon 1987 and Kevin Lang and David Kropp 1986).

I also thought that most of the chapters would be improved by more citations, although chapter 11, "Job Destruction," is a counterexample in that it has many nice citations. An example appears in chapter 1 during the discussion of the minimum wage (p. 6) as a classic imperfection in the labor market. There is not a single citation to the vast literature on the minimum wage. Another example appears in the section on tournaments (pp. 133–51), where there is no citation to two classics in the field (Lazear and Sherwin Rosen 1981 and Rosen 1986). Further, in several instances, even when specific previous work is discussed, the proper citations are not always provided. One example is in the discussion of Safelite Glass (pp. 121–22) where Lazear (2000) is listed in the footnotes to two tables but never, as far as I know, in the text. This is also true in the discussion of teams in the Koret garment case (pp. 223–31). Hamilton, Nickerson, and Owan (2003) is listed in the references on page 243, but this paper is never mentioned in the text, even though the author devotes nine pages to the paper. In a discussion of nonrecurring fixed costs, we are told "The best estimate of these cost is the one obtained with Israeli data, and shows that the typical hiring costs is approximately two weeks of paid salary" (p. 28). This is a perfect spot for a citation, though none is provided.

I thought the book could have profited from some careful editing. This never really made any of the arguments incorrect, but it was frustrating. An example occurs on page 2 where we are told "sections 1.2 and 1.3 briefly discuss the nature of the labor market institutions." In fact, this discussion appears not in sections 1.2 and 1.3 but in sections 1.3 and 1.4. Another example occurs on page 20 where the author refers to the "top panel" of figure 2.4. In fact, there is no top panel but only right and left panels. Other examples include things like "We now analyze in some details the idea . . ." (p. 74) or "which we assume to be described by this simply relationships" (p. 75). In the description of wage compression, there is a nice table detailing "wage productivity premia in different countries"(p. 21). However, the author provides neither the data source nor the years. There are also a few instances where, instead of writing "chapter" or "book" the word "report" (p. 203) appears, as if these chapters have been cobbled together from other sources. Hamilton, Nickerson, and Owan (2003) is listed in the references under the first author's first name "Barton," instead of his last. George Baker is listed as the only author of the well-known Baker, Michael Gibbs, and Bengt Holmstrom (1994) paper on page 242. The preface (p. ix) claims there are fourteen chapters and there are only thirteen! These and related problems may have occurred between writing and printing the book but could have been easily avoided with a careful reading.

Personnel Economics in Imperfect Labor Markets is not perfect. I think it could use more careful editing, more examples, more emphasis on imperfections in the labor market, more transitions and better writing, and more institutional detail. On the other hand, it is an up-to-date introduction to Personnel Economics and is a useful introduction to many of the main ideas in the field. Those interested in moving into or learning about Personnel Economics should add it to their reading list.

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N Economic History

Soldiers to Citizens: The G.I. Bill and the Making of the Greatest Generation. By Suzanne Mettler. Oxford and New York: Oxford University Press, 2005. Pp. xvi, 252. $30.00. ISBN 978–0–19–518097–8. *JEL 2007–0228*

The generation of Americans born in the 1910s and 1920s, who grew up during the Great Depression and came of age during World War II, has been noted for it's members' high levels of civic involvement. Sometimes even referred to as the "civic generation," these men and women epitomized the ideals of American democracy through their intense involvement in politics and civic organizations during the post-war era. What made this generation different from more recent generations who are more disengaged from public life? To what extent was this generation's commitment to the public good shaped by their experiences of the G.I. Bill? These questions are the focus of an intriguing new book by Suzanne Mettler.

In order to address these questions, Mettler has extensively surveyed over fifteen hundred members of the World War II generation, and conducted in-depth interviews with twenty-eight of them. Her efforts culminate in a central thesis: that the G.I. Bill had an overwhelmingly positive effect on veterans' civic involvement and that this effect was due to more than the bill's impact on individuals' educational attainment. In particular, much of the program's power lay in its inclusiveness, its generosity, and the rules and procedures by which it was administered.

The G.I. Bill is widely regarded as one of the most significant education policies to have taken place in modern America. Signed into law on June 22, 1944, it provided unprecedented educational aid to all returning veterans who had served for at least ninety days or had been discharged early because of disabilities acquired during service. Most veterans were eligible for all four years of benefits, provided they began their schooling before July 1951. Since the conscription rates of (male) cohorts who came of age during World War II were near 80 percent, the vast majority of men who were in their early twenties at the war's end were entitled to G.I. benefits. The bill also made very generous financial provisions, providing full tuition, books, and supplies toward virtually any institution of higher education in the country, as well as a monthly stipend that varied by family size. Previous studies have estimated that this cash allowance was worth 50–70 percent of the opportunity cost of not working.

The program's inclusiveness and generosity made a strong impression on the men and women whom Mettler interviewed. They also stand in contrast to many of today's social programs, which are more narrowly targeted and have experienced real declines in value. Examples of such programs include the minimum wage, food stamps, cash welfare, Pell grants, and unemployment insurance. Previous researchers have argued that individuals' attitudes about government and their participation in the political system are affected by their experience of social programs, and that the level of resources that are distributed is likely to influence civic engagement by both directly affecting individuals' well-being and by affecting their sense that government is for and about people like them. Individuals' routine, day-to-day encounters with government may also influence their attitudes. Whereas the G.I. Bill described by Mettler receives an A+ on all three counts, today's social programs would be unlikely to receive such high marks. Mettler asserts that this may be part of the key to understanding why World War II cohorts were so much more involved in civic life then men and women are today.

The book begins with an overview of the history of the G.I. Bill, which credits the American Legion for the bill's generosity and inclusiveness. Mettler argues that the bill was not simply an extension of New Deal policies, but an extension of a long tradition of compensating soldiers for performing the "ultimate act of participatory citizenship." Importantly, a common rhetoric surrounding the G.I. Bill's passage was that it would enable veterans to become more fully engaged in the civic activities that were thought to be key to a successful democracy in the postwar era.

One of the pleasures of reading this book is the personal accounts that bring Mettler's thesis to life. She draws heavily from these accounts to make the case that World War II had a profound effect on the men and women who served, and laid the groundwork for the leadership roles that they assumed in the postwar era. Nearly all of her interviewees describe their experience in the armed forces as life-transforming, even those who did not serve in combat (the vast majority). For many of these individuals, military service brought them into working relationships with a more diverse group of people than they would have interacted with otherwise, and took them to places all over the world. Furthermore, these experiences took place during key formative years—just as they were maturing from late adolescence into early adulthood. Mettler argues that these experiences provided many with the leadership skills and self-esteem that formed the groundwork for the active role they took in civic life in the postwar era. World War II veterans were thus poised to take advantage of the opportunities offered to them through the G.I. Bill.

And take advantage they did. Estimates of G.I. bill take-up rates range from nearly 30 percent for the oldest cohorts of eligible men to over 60 percent for those born at the end of the 1920s. Several high quality studies have already documented that the G.I. Bill had a substantive effect on the educational attainment of World War II cohorts. Mettler's own empirical analyses of the G.I. Bill's effects on education and other measures of social mobility are the weakest part of the book, and do not measure up to economists' concerns about causality. Most of the analyses are based on comparisons of G.I. bill users to nonusers, and are subject to standard omitted-variables criticisms. Nevertheless, the correlations make a compelling story, and the veteran's descriptions of the program's effect on their own lives add richness to our understanding of its impacts.

Notably, their memories of the G.I. Bill go beyond the fact that it opened doors to better careers by increasing their schooling. They overwhelmingly associate the G.I. Bill with positive experiences of government: most felt that it was administered fairly and efficiently, that the benefits were generous and inclusive, and that as G.I. recipients they were treated with respect. These positive experiences may have had important effects on their later degree of civic engagement. Furthermore, the bill's inclusiveness—both in terms of the number of men who qualified and the socioeconomic groups that were affected—suggests that if it did have an impact on individuals' notions of government, then those impacts would likely have been far-reaching.

Most of the book focuses on the experiences of non-black men, but Mettler suggests that the G.I. Bill also played an important role in mobilizing black veterans' participation in the civil rights movement. Amidst an otherwise highly segregated America, the G.I. Bill was an equal opportunity program. Its educational opportunities provided black men with skills and resources that probably enhanced their ability to be political activists, but after making use of these opportunities, black men returned to a labor market that was as discriminatory as ever. The juxtaposition of these experiences may have been an additional motivator toward political action: Mettler finds that, relative to black men who did not use G.I. benefits, black beneficiaries were more likely to be involved in civil rights protests, marches, and demonstrations and, relative to white beneficiaries, they were much more likely to join organizations that challenged the system.

Soldiers to Citizens is an absorbing read. It takes on an interesting, but little explored, question that is relevant to current discussions about political apathy and the role of government. Although much of the evidence presented in the book would be considered correlational by the standards of an applied microeconomist, the evidence is layered together in such a way as to make a persuasive, or at least thought provoking, case. Economists are likely to view the collection of voices synthesized in this book as a nice complement to studies of the causal impacts of the G.I. Bill generated by our own discipline.

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From the Corn Laws to Free Trade: Interests, Ideas, and Institutions in Historical Perspective. By Cheryl Schonhardt-Bailey. Cambridge, Mass. and London: MIT Press, 2006. Pp. xiii, 426. ISBN 978–0–262–19543–0.

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This book covers a broad array of arguments and evidence surrounding Britain's repeal of the corn laws. As the author notes in her introduction, this pivotal event remains puzzling. Despite their seeming dominance within political institutions, landowners rejected protection for a key agricultural product, grain, raising questions for both political scientists and economic historians. Why did an elite give up its economic privilege? Did landowners cave in to public pressure building up outside electoral politics? Were they swept up in the rising wave of liberal ideology? Or were they conceding economic interests in order to hold onto their continued political privileges? Did the same causal forces play out in the House of Lords as in the Commons?

Over the years, scholars have generated a variety of arguments and elaborated various forms of supporting evidence. Schonhardt-Bailey proposes that most existing arguments are at best incomplete—with a few so weakly supported they can be dismissed. The best explanation for repeal, she claims, can be found in the interaction of interests, ideas, and institutions. To persuade us of this claim, the book masterfully pushes existing arguments and methods to their limits.

The book consolidates not only previous research by economic historians and political scientists examining this episode from different perspectives, it looks at the complexities of repeal more thoroughly than one could possibly find anywhere else. Schonhardt-Bailey demonstrates not only command over these competing arguments, economic models, political institutions, and electoral politics, but also over the historical details so critical for evaluating competing claims. In order to show that ideas, interests, and institutions each played a part, Schonhardt-Bailey demonstrates innovative ways to measure the importance of material and nonmaterial forces. By structuring analyses in ways that separate yet build off competing and complementary claims, she skillfully creates opportunities to weigh when and where ideas came into play, or why some interests appear to matter while others did not. This also allows her to integrate discussions of political institutions—not only electoral processes but also the interaction of cabinet with both houses of Parliament.

The work is exemplary for political economists because it illustrates ways to bring current discussions about interests and ideas into a single framework. To investigate the impact of ideas, she uses computer-based content analysis to assess which themes dominated debate both within Parliament and portions of the press. This helps convert vague claims about the relevance of particular ideas (or broad ideologies) into measurable indications of MPs' (and voters') concerns, hopes, and fears. While the evidence gives us an overview of the all the debates we have never had before (and identifies some surprises), Schonhardt-Bailey's interpretation of the data can be contested. She found evidence of specific patterns stressing political and economic concerns, reflecting certain ideas. Yet, one gets the impression MPs were debating the obvious: the anticipated effects of repeal. Liberals and Anti-Corn Law League members painted a positive picture, while most Conservatives argued repeal would hurt not only landowners but the economy more generally, as well as causing consequent political and social upheaval. Their positions surely reflect their understandings of trade and its role in the domestic political economy—so no one would argue ideas did not matter. Our questions concern when and to what degree ideas influenced the outcome, compared to other causal forces.

Scholars have been particularly puzzled by why Peel's followers shifted their stance on trade so abruptly. Different types of ideas provide potential explanations. Some have suggested Peel and his followers changed their views because they bought into liberal conceptions; this has little supporting evidence, however. Ideas about the role representatives were intended to play have also been raised. In this approach, MPs have conflicting tasks: represent the narrow interests of their constituents, or the interests of society more broadly. In a time before modern political parties had developed, these cross-pressures could be severe—and movements such as the Chartists and the Anti-Corn Law League built up the pressure for MPs to consider their broader responsibilities. Using analysis of voting patterns in Parliament, Schonhardt-Bailey suggests repeal sparked a deeper division among Tories than we may have previously recognized.

Among the institutional factors, Schonhardt-Bailey examines why the House of Lords accepted repeal. This aspect of repeal is the least studied and least understood. By looking at the issues raised within the press and the content of speeches given in the Lords, as well as the written protests lodged there, Schonhardt-Bailey makes a convincing claim that ideas influenced the outcome in the Lords, but not in the way you might think. Ideas—liberal arguments on political economy in particular—appear not to have swung support to Peel so much as frightened the non-Peelite Conservatives. These insights on the way interests, ideas, and institutions influenced each other give us a fresh perspective on the way repeal unfolded.

While the work is magisterial in its sweep and in the way it pushes existing arguments to their limits, those limits exist. Repeal continues to attract our attention (as do other flip-flops in trade policy, including American tariffs between 1930 and 1934 or Imperial Germany's between 1890 and 1902) because interest-based models struggle to account for such rapid fluctuations. These difficulties arise from the way political economists construct analyses. We privilege economic models. We typically start by identifying interests prior to politics, using one of several traditional economic models to describe the domestic distribution of the gains from trade. We then examine whether conditions gave organizational advantages to one set of interests rather than the other, to see which would have greatest political voice. After that, political institutions enter, determining which voices were more likely to be heard when policy was decided.

However, the way we order these elements may not integrate economic and political elements well. These historical episodes puzzle us because the economic models lead us to expect preferences to be stable over the short-run. Scholars interject ideas and institutions into explanations of repeal because (in Schonhardt-Bailey's words, p. 32) interest-based arguments "offer no clear rationale for why Peelites suddenly reversed their position." Alternatively, this conclusion may reflect how shallow our understanding of economic interests in trade is—or how our traditional approach discounts politics. For example, the analysis of the content of debates mentioned above could be used in an entirely different way. Politics in representative government typically hinge on the ability to persuade. Persuasion matters most when cleavages are in flux. Much of the puzzle here depends on how sharply defined one believes trade-based cleavages were.

There is very little discussion of the latent sector-based division within British agriculture in the 1830s and 1840s. Did farmers raising livestock necessarily share interests with grain growers? Moreover, several factors could have driven agrarian interests to shift between 1841 and 1846. They escape attention because older models of trade-based interests do not include them. A micro-level perspective would focus more on how farmers were adjusting to changes in Britain's location in the international economy. Taking advantage of relative abundance in capital and labor required switching away from land-intensive production in grain—but also altering many aspects of rural life. Sticking with economic interests, a transition in production required investments. Levels of general economic activity surely determined whether individuals believed such investments were worthwhile. In the election of 1841, held during an economic downturn, Tories promised farmers protection because most farmers feared taking on the risks of adjustment. Once the economy recovered, and those risks receded, Peel began discussing repeal. Yet the economic cycle doesn't get mentioned here—as it rarely gets mentioned in any discussions of the politics of trade.

Assumptions from basic economic models also cause us to isolate changes in trade from other policy dimensions. In this case, the tariff on grain is not linked to the rest of Peel's legislation. Peel did not offer a straight-up reduction of the tariff on grain, as Cobden or his followers had continually proposed. Instead, Peel spread reduction of the grain tariff over several years, reduced tariffs on certain agricultural inputs immediately, and included funding for loans for agricultural improvements. In these ways, Peel exposed the sector-based split in agriculture. These aspects of Peel's legislation are only mentioned once (p. 171), and treated as mere concessions rather than as integral facets to adjustment of agricultural production. Peel's legislation made investment monies available. The funds were completely lent out (at market, not subsidized, rates). How does this fit with the idea landowners already had capital? In fact, many of the biggest landowners could not easily borrow, because entailment prevented them from using land as collateral. How many members of the House of Lords borrowed some of these funds for drainage, hedges or construction of barns?

Our expectation that interests change only slowly also encourages us to ignore evidence of interests before and after policy changes. Again, this is an issue for political economists studying trade generally, reflecting the limitations of our current approaches. Here, the analysis focuses almost entirely on the lead-up to repeal, ending with its passage. Since much of the puzzle is driven by the assumption landowners acted against their material interests, it is worth considering repeal's impact. Schonhardt-Bailey has done extremely difficult and interesting work examining the extent to which landowners may have earned income off industrial investments, with the idea that these investments allowed them to accept lost income from land rents. Yet British agriculture's performance after repeal was much more mixed—some rents rose. Interest-based arguments may have more mileage, if we move beyond traditional assumptions.

This book is an excellent contribution for anyone interested in the political economy of trade. It presents innovative techniques for modeling and measuring the interaction of ideas and interests (applicable to other historical episodes), it captures the institutional complexity of the case, and provides new ways to appreciate how ideas framed political discussions. One cannot find so much material, from so many different perspectives, handled so deftly, in any other single place. Yet for all that, this will undoubtedly not be final say on repeal, for certain aspects of this case will surely continue to puzzle us.

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O Economic Development, Technological Change, and Growth

Transforming the Development Landscape: The Role of the Private Sector. Edited by Lael Brainard. Washington, D.C.: Brookings Institution Press, 2006. Pp. viii, 143. $18.95, paper. ISBN 978–0–8157–1124–7. *JEL 2007–0242*

By any account, the private sector collectively is the largest producer in all but a few economies in the world. Indeed, as the authors of *Transforming the Development Landscape* cite, 90 percent of jobs in developing countries are in the private sector (p. 31) and private investment and remittance flows from developed to developing countries have grown much larger than official aid flows (p. 17). Thus the private sector is at once the object of development as well as its greatest potential financier.

Of course, private actors left unto themselves might not end up reducing childhood mortality or improving the plight of marginalized citizens. The challenge, then, is to guide some private economic activity toward positive social ends. This edited volume, containing contributions by a spectrum of authors from academics to practitioners, lays out a number of ways in which this might be done. The private sector is variously treated as (1) a recipient of development assistance, (2) a source of funds or philanthropy, and (3) an implementer and purveyor of know-how.

Encouraging the Indigenous Private Sector

Warrick Smith of the World Bank's Private Sector Development group writes how countries can achieve a favorable "investment climate," a policy environment that promotes entrepreneurship and a vigorous private sector. Many of the chief concerns of firms in developing countries have to do with the macro policy environment (p. 33) as well as the hidden costs—like poor infrastructure, corruption, and crime (p. 34)—of operating in emerging markets. Smith offers no one-size-fits-all solution, noting the variation in obstacles facing entrepreneurs as well as the political difficulties of reform.

Two chapters examine small and medium-sized enterprises (SMEs) in developing countries. Alan Patricof, a U.S. private equity mogul, and Julie Sunderland, a development consultant, advocate SME "enterprise funds." These funds would recreate the roles played by venture capitalists in rich countries, giving small dynamic companies an equity boost at a stage in their development where high-priced debt could stifle growth. Given the "poor returns and many business failures" of private sector endeavors in funding developing-country SMEs (p. 78), Patricof and Sunderland suggest they should be funded by donors willing to accept low rates of return.

Brown University's Ross Levine examines the question of whether aid agencies should subsidize SMEs at all. While sympathetic to the importance of SMEs, Levine comes down against subsidies: in the very economies where the SME sector is weak, political barriers and an entrenched business elite would serve to undermine any efficiency gains of the subsidies by directing them toward the wrong SMEs.

Reaching into Deep Pockets

Timothy Freundlich of Calvert, a socially conscious financial services agency, argues that rich-country capital should be allocated not only to maximize financial returns but also to raise social value. Of course, individual investors may not be willing to sacrifice personal financial return and instead prefer to free ride off others' social investing. Here, Freundlich notes that some blended-value investing (i.e., Ben & Jerry's over Philip Morris) does just as well as the benchmarks; he advocates lower-return—and more socially aggressive—investing for foundations and those individuals who in the rest of their lives pursue altruistic activities.

Brookings' David de Ferranti notes the plethora of recent ideas to finance development activities. Some of these involve improving the efficiency of aid delivery, for example the "advanced purchase commitments" that provide a market for the development of tropical-disease vaccines by prefunding a prize for the biotech company that comes up with a cure. Others are simply new sources of revenue for development, such as a French proposal for taxing jet fuel. Rajiv Shah and Sylvia Mathews of the Gates Foundation describe two of these ideas, which pertain to global health and in which the Gates Foundation has taken a leading role, in more detail.

Enlisting Help in the Fight

Multinationals and first-world businesses can, by applying their know-how or even during their daily operations, have a positive developmental impact. Larry Cooley, a development consultant, describes one aspect of the ubiquitous development-speak term, "public–private partnerships." He focuses on the relationships between multinational corporations and aid agencies; these groups increasingly work together to improve the developmental impact of the corporations in their emerging-market operations. It is noteworthy that the private sector partner is not just a source of funds, but often the implementer.

Lael Brainard of Brookings and Vinca LaFleur of CSIS argue that firms should "build social values into core business strategies" (p. 17) through a so-called corporate social responsibility approach. They describe how a mixture of corporate philanthropy, positive lobbying, and core business strategy can improve the bottom line, make a positive difference in development, and make employees happy. Harvard's Jane Nelson notes the limits of this approach: demands on business leaders are "complex and at times contradictory" (p. 42) and businesses, in the absence of good government, can achieve little in the way of poverty reduction. That said, she offers a set of prescriptions for businesses to have a positive impact on development.

Having Your Cake and Eating It Too?

The overall tone of the volume, consistent with many business/development circles, is that profitability and positive social externalities are one and the same. Of course, this can't quite be true—if it were, the book would not have to be written in the first place. Subtler messages contained in the different chapters are that the divergence between profit and doing good may be small; that even a small fraction of business and financing activity in the first world could have a large impact in the developing world; and that donor and philanthropist finance can be used to close these gaps. Development practitioners, corporate social responsibility executives, and academic economists wondering how the policy engaged think about harnessing the private sector would find these messages informative.

Regardless of what firms and aid agencies do in the United States or Europe, sustained poverty reduction can only occur when private sector activity in developing countries supports dignified living standards. This is a different notion than the requirement that firms in the developing world "do good" instead of merely doing well. As we move ahead, we need to be careful that we manage our good intentions and refrain from imposing additional development burdens on these already-fragile, emerging-market firms.

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Accelerating the Globalization of America: The Role for Information Technology. By Catherine L. Mann. With Jacob Funk Kirkegaard. Washington, D.C.: Institute for International Economics, 2006. Pp. xxx, 237. $20.25, paper. ISBN 978–0–88132–390–0. *JEL 2006–1481*

The book examines the benefits of globalization for American workers and firms, highlighting the role of information technology—both as an industry which is itself rapidly globalizing and as a driving force behind the trans-nationalization of economic activity. Written by Catherine Mann, a professor of economics at Brandeis University and a senior fellow of the Peterson Institute of International Economics, the book argues that globalization and information technology (IT) are radically transforming patterns of production, employment, and trade around the globe, requiring important changes in U.S. economic policies to exploit its full potential. Although the book is primarily oriented to policymakers, researchers, and students interested in IT will also value its comprehensive overview of IT-related issues.

The basic argument of the book can be laid out as shown in figure 1. The root causes of the IT revolution are said to be the development of new, more powerful IT technologies and the transfer of production of IT goods to low-labor-cost countries. These two changes are driving down costs of IT goods, which in turn is said to provide a powerful stimulus to business investment due to the highly price elastic nature of demand. Higher IT investment in turn stimulates more IT investment due to network effects: the more other firms are adopting production methods intensive in IT, the higher the returns are to investing in it, so that IT investment booms have powerful feedback effects. The adoption of IT-intensive methods in turn boosts productivity and incomes, which further promotes demand for IT goods and services, encouraging entry into IT-related fields. Mann points to an evolving global division of labor, wherein developed countries tend to specialize in the design of IT hardware and software and production of IT services (which are intensive in skilled labor), and developing countries tend to specialize in production of IT hardware (which is intensive in semi-skilled work). These trends spur further transnationalization of the IT industry and surging trade in services. The book argues that these trends are unambiguously positive for the U.S. economy, but that there are short-term problems to be navigated. Of special importance are the need to make sure that the distribution of skills within the work force matches what is needed to realize the full potential of IT, and the need to reduce barriers to international trade in services.

While the book provides a valuable compendium of information on the IT industry, two aspects of its argument diverge somewhat from scholarly research on the IT industry. First, much of the discussion and evidence presented in the book centers on IT hardware, with hardware said to be "a model for the global evolution of IT services and software" (Mann 2003, p. 1). However, the relevance of the paradigm for hardware to the other segments hinges importantly on the potential importance of globalization in driving the chain of events described above: If globalization has been the key driver behind the observed price declines that have spurred investment in hardware, then continuing globalization of software production and services could well generate similar effects; but if innovation has been the main driver, it is more difficult to argue that the hardware paradigm applies to software and services.

Indeed, much recent research suggests that technological innovation has been the more critical factor in explaining price declines for IT hardware. In the U.S. semiconductor industry, the capital-intensive tasks—like etching circuitry on silicon wafers—are typically done in U.S. "fabs," while it is the labor-intensive tasks—packaging and testing—that are increasingly done abroad. While transnationalization does let firms save on

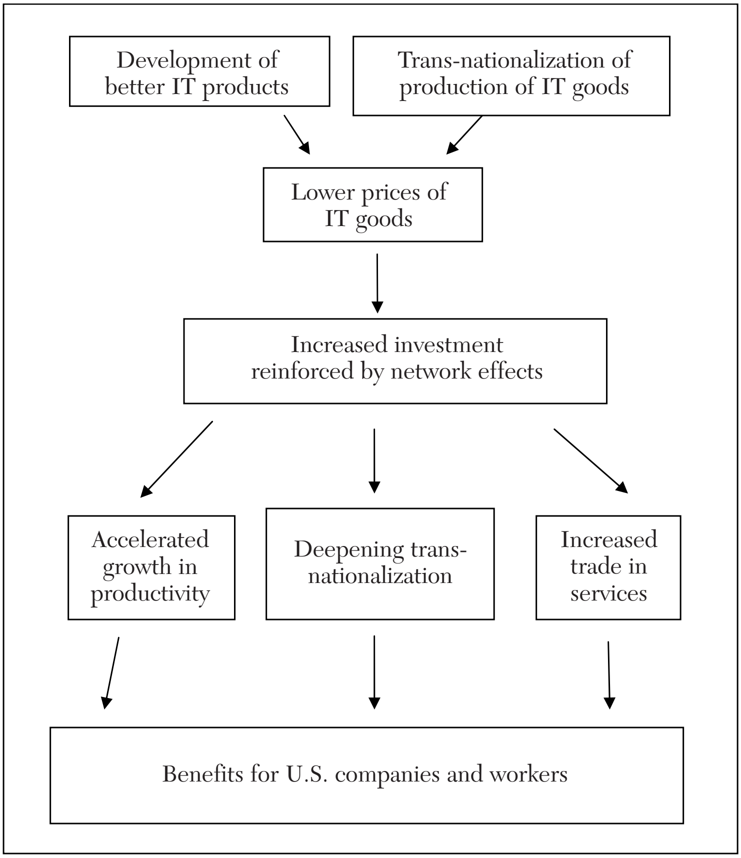

Figure 1. Schematic Representation of Economic Changes Related to the IT Industry

costs, the highly capital-intensive nature of semiconductor production means that these savings are not large relative to the total costs of production. Instead, empirical studies typically find that the declines in indexes of hardware prices discussed in the book are almost entirely due to measured quality changes resulting from product innovation.[1] For example, Ana Aizcorbe (2006) and Kenneth Flamm (2007) show that virtually all of the declines in price indexes for Intel's microprocessor chips can be attributed to quality improvements. Thus, additional price effects from globalization of hardware would appear to be second order.

[1] See Ernst R. Berndt and Neal J. Rappaport (2001) for a review of the empirical literature on computer prices, and Mark Doms (2005a) and John Van Reenen (2006) for recent discussions of these issues for communications equipment and network servers, respectively.

In contrast, the software and services subsectors are much more labor-intensive, which may make transnationalization of production more important in putting downward pressure on prices in these subsectors. Working against that, however, is the custom nature of software: the bulk of software spending in the United States is "own-account" (software developed in-house) or "custom" (contracted out to software developers), so that IT software and services lack the general-purpose technology aspect of IT hardware. Similarly, in arguing that IT itself contributes to acceleration of globalization, the effects here have much more to do with IT hardware than with software or services; although the latter have certainly facilitated the conduct of business on a global basis, it is the hardware aspect that has been strongly influential in driving down communications costs. At the end of the day, even if one argues that software and services have the potential to generate the same sorts of effects described above for hardware, this would require innovation at a pace that is not seen in the available data.[2]

A second issue concerns the book's portrayal of the stimulus provided by IT to business investment and productivity as likely to be long-lasting, whereas there is considerable uncertainty about this.[3] The book characterizes spending on IT hardware as price elastic, where a one percent drop in IT prices will generate a more-than-one percent increase in nominal investment. While high levels of price responsiveness were clearly evident during the 1990s, there is some question as to whether they remain so high today. According to Doms (2005b), the 1990s were a unique period of time when a broad array of innovations was introduced: most notably, the internet, advanced operating systems, and search engines that facilitate the use of the internet for business purposes. Doms suggests that, as these innovations become widely diffused, the rate of investment in IT products is likely to slow: "Now when IT products are introduced, firms respond by upgrading their existing IT technology, but the boost from firms delving into IT has likely been lost" (p. 4). How long that could take is an open question. It could be that the U.S. market is fairly mature, so that the first-order effects have already been seen; this is potentially consistent with the fact that the 1990s pick-up in growth of labor productivity seems to be dwindling down. However, to the extent that one views IT hardware as a general-purpose technology, the historical evidence on such technologies—notably, steam power and electricity—suggests that there can be long lags before potential contributions to productivity growth are fully realized. Thus, how much additional stimulus to investment and productivity growth should be expected is not at all clear.

A final point is that, although the book underlines the importance of new policies "...to ensure that the U.S. remains first in innovation, business transformation, and education and skills," it does not go much beyond the usual general ideas of providing support for workers who lose their jobs due to technology and/or trade, and creating ways for workers to continually upgrade their skills in line with the needs of new technologies. While it is hard to disagree with these ideas in principle, it is not clear in practice what types of new human-capital policies would be effective in keeping the supply of IT labor in line with demand; the idea of "lifelong learning"—wherein people need to constantly update their skills to stay up with the ever-moving state of the art—seems sensible, but perhaps too costly relative to the benefits as a general workforce strategy. To be sure, the policy questions here are complex and answers to them are not straightforward, but one wishes the book offered somewhat more in the way of original thinking on how best to approach them.

[2] Available price indexes for software show slower price declines (slower quality increases) than those for hardware. This seems true of the evidence for prepackaged software (see Marc Prud'homme, Dimitri Sanga and Kam Yu 2005; and Jaison R. Abel, Berndt and Alan G. White 2003), as well as the (admittedly thin) evidence for custom software (Scott Goldfarb et al. 2006).

[3] See John Fernald, David Thipphavong, and Bharat Trehan (2007) for a recent review of the arguments.

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Q Agricultural and Natural Resource Economics · Environmental and Ecological Economics

Delivering on Doha: Farm Trade and the Poor. By Kimberly Ann Elliott. Washington, D.C.: Institute for International Economics, Washington, D.C.: Center for Global Development, 2006. Pp. xiii, 148. $22.95, paper. ISBN 978–0–88132–392–4. *JEL 2007–0329*

This book develops the background and justification for a specific set of recommendations aimed at advancing the agricultural negotiations under the World Trade Organization's Doha Development Agenda (DDA). Indeed, the final chapter lays out detailed recommendations for a DDA package deal. The book was released midway through 2006, when there remained some hope that these negotiations would be successfully concluded. However, one year later, the DDA negotiations have withered on the vine. As the 2007 August recess arrived in Geneva, the *Bridges Weekly Trade Digest* reported that "prospects for salvaging an accord in the Doha Round of global trade talks remain dim." And the major area of contention is the very set of agricultural policies which are at the subject of this book. Elliot's contribution provides an important synthesis of the issues at hand and the facts behind the debate over the benefits of an agriculturally-oriented WTO development agenda. As such, it remains a useful read—particularly for those seeking a contemporary, well-written introduction into the rather arcane subject of agricultural trade policy and its impacts on developing countries.

It is useful to view this book in the context of its sponsor institutions: The Institute for International Economics and the Center for Global Development. These are two think-tanks seeking to advance a multilateral, outward-looking agenda in Washington. They are noted for well-written analysis by leading economists on contemporary topics. In the preface to this book, Nancy Birdsall and C. Fred Bergsten, directors of these two institutions, note that they view this book as a follow-up to the 2004 volume by William R. Cline on the same subject. In that book, entitled *Trade Policy and Global Poverty,* Cline suggested that world poverty could be reduced by 500 million people if global trade liberalization were undertaken—and that this would be largely driven by higher agricultural prices lifting the incomes of the rural poor. This was a headline-grabbing number and it was used to advance the argument that the DDA could potentially have a very big impact on the stubborn problem of global poverty. However, this work was subject to considerable skepticism and subsequent research has produced far lower estimates (e.g., Thomas W. Hertel and L. Alan Winters 2006). Therefore, it is interesting to begin by examining the present author's position on this controversial point.

Kimberly Elliot is far more cautious than her colleague in her assessment of the potential impacts of agricultural trade reform on poverty. Indeed, after providing a brief motivation for why agricultural trade and poverty are so closely linked, she concludes her opening chapter by arguing that "a Doha agreement on agricultural liberalization is not enough to ensure that the poor will benefit" (p. 9). Indeed, unlike Cline, she emphasizes the fact that "many countries . . . lack the capacity to respond effectively to changing conditions in global markets and will need assistance if they are to take advantage of these changes. . . . Rich country liberalization is thus only part of the answer . . ." (p. 10). This balanced analysis of the problem is characteristic of the entire book, which focuses more on the evaluation of existing research on this topic than on the reporting of new findings. As such, it is a breath of fresh air on this subject from two institutions which had previously committed themselves to the defense of a single number in the trade and poverty debate.

Chapter 2 of the book reviews the broad facts surrounding rich country agricultural policies. These facts are rather well-established in the literature, but Elliot reviews the main points concisely. Chapter 3 draws on recent experiences with reform in the United States and the European Union in order to evaluate the prospects for future reforms. She closes this chapter by expressing some skepticism as to whether the leadership in France and the United States will have sufficient political will to implement significant reforms. With the benefit of an additional year of political history, it appears that this skepticism was justified. As I write this review, the U.S. Congress has failed to renew the Trade Promotion Authority needed to enable the U.S. Trade Representative to negotiate multilateral and bilateral trade agreements in good faith. In addition, the U.S. House of Representatives has passed a 2007 Farm Bill which fails to reform agricultural policies—despite the golden opportunity provided by record-high commodity prices. Cutting the individual farm subsidy cutoff to $1 million of income ($2 million for couples) was the boldest move that Congress was able to muster in light of the large number of contested rural seats in the House of Representatives. This is hardly likely to impress developing country trade negotiators concerned that they are facing unfair competition from rich farmers in rich countries.

Chapter 4 of the book evaluates the opportunities for developing countries to take advantage of rich country reforms. Here, Kimberly Elliot emphasizes the product composition of developing country agricultural exports, noting that the heavily protected, temperate products exported by rich countries represent only a portion of developing country exports, with the fastest growing category of trade—fruits, vegetables, facing much lower tariffs. This suggests that the gains from reform may be more limited than some previous authors have suggested. Of course, one is left wondering to what degree the high levels of support provided in rich countries over the past forty years have discouraged developing countries from competing in the market for temperate products. Elliot concludes the chapter by highlighting the importance of Sanitary and Phytosanitary Standards in international agricultural trade. Developing countries often lack the capacity to respond to these standards, which are increasingly being imposed, not just by rich country governments, but also by supermarkets and other private sector buyers. Thus this is an area ripe for development assistance or so-called “aid for trade.”

Chapter 5 is titled “The Devil in the Doha Details” and this is quite appropriate in the context of the WTO negotiations. Developing countries were dismayed when most of the promised agricultural gains from the previous multilateral agreement (the Uruguay Round) failed to materialize—owing to the ability of sophisticated policy makers to exploit the loopholes inserted into the agreement by savvy negotiators. Arcane issues such as the choice of base period from which to reduce protection, the inclusion of guarantee prices in subsidy measurement, and the choice of prices used to convert specific tariffs, all conspired to remove most of the teeth from the Uruguay Round Agreement on Agriculture. This time around, developing countries have been more vigilant of language proposed by the rich countries—but they have also begun emulating the rich countries. This has involved seeking to create loopholes in areas such as special products, which would permit developing countries to exempt a percentage of their products from formula tariff cuts, and the special safeguard mechanism which would permit them to levy additional tariffs on food products in cases where imports surge. Here, the author misses an opportunity to teach a fundamental lesson of economics. If the DDA really aims to reduce poverty, then its focus should be on giving the poor access to food at world prices. Allowing developing countries to raise tariffs on staple food products—at the very moment when that food is most scarce—is very likely to be a formula for raising, not lowering, poverty. Indeed, the absence of more aggressive developing country participation in the agricultural part of the Doha agenda has contributed to a package that is less poverty-friendly that it might otherwise be (Hertel et al. 2007).

The book concludes with a list of rather sensible recommendations for a Doha Package Deal in the areas of export competition, domestic support, market access, monitoring, special and differential treatment and aid for trade. Given the many good intentions mobilized on behalf of the DDA, it is a pity that these negotiations have stalled. The fact is that advocacy of more liberal trade, as promoted by this book and others like it, has failed to overcome the special interests that dominate trade policy. Of course, if agricultural policies are not reformed in the present environment of high prices and international pressure, then it is unlikely that such reform will come in the near future. From the point of view of global welfare, this is most unfortunate. However, the persistence of such policies bodes well for continuing sales of this book!

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Environmental Economics for Tree Huggers and Other Skeptics. By William K. Jaeger. Washington, D.C. and London: Island Press, 2005. Pp. xv, 281. $45.00, cloth; $22.50, paper. ISBN 1–55963–664–5, cloth; 1–55963–668–8, pbk. *JEL 2007–0360*

Anyone who has ever taught environmental economics to students of an environmental studies program will be familiar with the almost instinctive hostility environmental scientists and environment activists have toward economics. The entire conceptual framework of economics, which most economists see as a towering intellectual achievement, tends to be rejected as biased, corrupt, and morally bankrupt. To the economics professor this can be more than a little disconcerting and is likely to get the class off on the wrong foot, possibly never to regain any firm footing.

Professor William Jaeger's text is aimed at exactly these types of students: "Tree Huggers and Other Skeptics." The text acknowledges students' skepticism up front and immediately sets about to convince the skeptics that economics has something important, even necessary, to bring to the solution of environmental problems.

Like most economists trained in the last quarter-century, Jaeger takes for granted the proposition that noncommercial, nonmarket goods, services, and institutions are as much part of "the economy" and the subject matter of "economics" as are markets, businesses, money, and prices. Economics is simply the realm of scarcity-constrained choice, trade-offs, and incentives. Its analytical tools are applicable far beyond the market realm, including the world of environmental capital, goods and services.

The book is organized into three major sections. The first emphasizes the applicability of the economists' "tools of the trade" to almost any situation where choices have to be made and trade-offs confronted. This naturally leads into a discussion of the values associated with the natural environment and the costs associated with either using or protecting those environments. The importance of marginal value (demand) and marginal cost (supply) are introduced and used throughout the text. Market failure, discounting the future, economic growth and development, and the gains from trade are also discussed in separate chapters.

The second section focuses on the role of institutional arrangements in helping us use scarce resources efficiently, including environmental resources. Alternative property rights schemes are presented as institutional "rules of the game" that have strengths and weaknesses depending on the character of the resource at issue. Alternative approaches to environmental policy are discussed in that context including "command and control," economic incentives, and tradable usage rights. The approach is mainstream economic, with the conventional preference for market mimicking approaches, but with a healthy skepticism of ideological "free market" solutions.

The third section explains why it is important to try to give environmental values quantitative economic expression. It then proceeds to explain the various tools economists have developed to do so, both revealed preference and stated preference approaches. Because this involves quantifying the value of things that are not usually given expression in monetary terms, Jaeger includes a discussion of where economics and economic value stand within the realm of ethics or morality.

The book ends with a discussion of the limitations of economics when it comes to environmental policy and protection and the pursuit of human well being. While openly admitting that economics has serious philosophic and conceptual limitations and cannot provide an exclusive or dominant environmental perspective, Jaeger insists that economic science has something vitally important to bring to the development and implementation of effective and efficient environmental policy.

This text should help avoid *some* of the instinctive hostility of many environmental activists to economics simply because it begins with a clear and unambiguous embrace of the legitimacy of environmental values and protection. It appropriately restates many of those environmental concerns in conventional economic terms. But that may also be its undoing.

Jaeger, like most contemporary economists, clearly understands why economic science must look beyond markets, commercial business, and the world of monetary transactions. But popular economic dialogue suggests no such expansive economic view. The "economics" section of the local newspaper is still the business section and most students entering college think "economics" and "business" are the same thing. For that reason, Jaeger's tactic of beginning by showing that economics easily embraces environmental concerns is likely to trigger the very skepticism he is seeking to undermine. Environmental activists will see it as an extension of a profane "business mentality" to a "sacred" non-market realm, simply because they mistakenly equate economics with commercial business.

In my experience, this confusion has to be met head-on at the very beginning by doing two things. First, a direct effort has to be made to distinguish economics as an analytical social science from the promotion and perfection of commercial business firms and financial markets. Second, the legitimate realm of economics has to be defined at the start so that students can see that there are relatively bright boundaries that limit the reach of economics. Economics is *not* a social theory of everything. This is important because economists use phrases such as "environmental values" which can sound like they are referring the philosophic or metaphysical realm when they are doing nothing of the sort. That is, noneconomists hear economists making much more sweeping claims of expertise and knowledge than the economist intends. That can be disfunctionally offputting in an instructional setting.

In terms of defining the legitimate realm of economics so that it sounds more like a pragmatic social science and less like an expansive philosophic worldview, I would suggest at least the following clear distinctions be made at the start of instruction:

- The realm of economics is limited to settings where trade-offs are considered culturally and ethically appropriate. There are very important settings (spirituality, intimacy, honor and loyalty, basic rights, the lives of individuals, etc.) where this is not the case.
- Economic values are *relative* values stated in barter or trade-off terms, not intrinsic values. Economists do not deal with the "real" (e.g., metaphysical) value of things. This allows economists to sidestep many important but noneconomic philosophic concerns.
- Economic values are *incremental* values, dealing only with small changes from the status quo. In general, they are not *total* values or values involving changes that cannot be looked upon as "incremental" such as the death of an individual, the extinction of a species, or revolutionary or catastrophic developments.
- Economics is not very good at dealing with the very long term, say 50 or 100 or more years out, although it may have valuable contributions to make about how to cope with the uncertainties and risks of the long term. The changes in human values, the evolution of human institutions, the importance of time-dependent paths, the problems of multi-generational equity, and the dominance of gross ignorance and uncertainty about the distant future render many of our tools of questionable reliability for long-term decisions.
- Even though *equity* has always been a central component of public economic dialogue, analytical economics has largely sidestepped that broad set of popular "economic" concerns in order to avoid ideological and philosophic disputes to which economics as a social science could contribute very little.
- Economics is self-consciously amoral. It accepts peoples' preferences as they are. It does not evaluate the spiritual or ethical

> direction in which those preferences and human institutions, including markets, may carry a society or humanity as a whole.

Jaeger makes many of these points in his closing chapter on the limits of the economist's approach. That I think is too late. By then, many environmental studies students will already have been turned off by what they misinterpret as an unattractive, biased, and overreaching social philosophy that is far removed from scientific analysis.

Taken together, these distinction are likely to lead some students to conclude that economics has very little to contribute to the really important issues that dominate our intellectual and emotional lives. But that is the point: To present economics as a set of finely crafted tools that are both safe and useful in attacking some specific and limited but important problems. That is the way chemistry, physics, and biology present themselves.

For introductory classes where we are trying to get students intrigued with the power and quirky logic of the economist, a different tack altogether may be called for, one that intrigues the student with counterintuitive, even humorous, results, applying economic logic in unlikely settings. In some ways this is easy because economics, as a social science, is easily entangled in the full complexity of human lives. But we ultimately have to be careful not to overreach and go beyond our role as social scientists offering analytical tools for the understanding and solution of a particular set of social problems. Although it may be good for our egos to believe that we can offer a social "theory of everything," the scientific method demands that we reign in those egos and offer with our instruction a heavy dose of humility about the limits of economics as a social science.

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R Urban, Rural, and Regional Economics

Don't Call It Sprawl: Metropolitan Structure in the Twenty-First Century. By William T. Bogart. Cambridge and New York: Cambridge University Press, 2006. Pp. xii, 218. $24.99, paper. ISBN 978–0–521–86091–8, cloth; 978–0–521–67803–2, pbk. *JEL 2007–0373*

This is a nontechnical book on trends in metropolitan sizes and structures. It assumes no knowledge of economics on the part of the reader, and develops in its pages the few economic concepts it employs. Urban sprawl is the ostensible subject of the book and the author devotes a chapter to alternative definitions. He never settles on a definition and the book could easily have been written without the concept. A nonpejorative term would be better for serious analysis. The key issue is of course whether and if so how private competitive urban markets might result in excessively low population and employment densities; if not, what government actions cause excessively low densities.

The first four chapters document the increasingly dispersed nature of employment of suburban subcenters or "trading places" as the author chooses to refer to them. He emphasizes and documents the relationship between specialized production and trading between sub-centers and worker/consumers.

Trouble begins in chapter 6, on zoning. He emphasizes that the purpose of zoning is to limit externalities, although no serious analysis is presented other than an anecdote about separating manufacturing from residences. However, manufacturing is by now a small percentage of employment and has been migrating either to the peripheries or outside metropolitan areas for decades. Most zoning simply limits densities of office employment and residences. These activities do not provide significant negative externalities. More important, a sequence of fine papers in the last quarter century has provided evidence that residential density controls are the result of lobbying by property owners to limit housing supply in unusually desirable neighborhoods of metropolitan areas. If housing cannot be built up, it must be built out and I would refer to such controls as a cause of sprawl.

Chapter 7 on congestioin is similarly confused. The author does not distinguish between crowding and congestion of transportation facilities. Crowding is desirable on transportation facilities where land is valuable for the same reason that dense development is appropriate in such places. There is an extensive literature on congestion pricing. Bogart correctly emphasizes that commuting distances have increased during recent decades but commuting times show no trend. In fact, the dramatic suburbanization of both residences and employment would have resulted in substantial decreases in commuting times if zoning did not force excessive segregation of jobs and residences and if governments did not dramatically underprice road use.

Chapter 8 concerns local government. The author appears to favor tax base sharing within metropolitan areas, apparently based on equity considerations. Tax base sharing must be mandated by state or federal governments since it results in gains to central city governments and losses to suburban residents and government. Since the United States is almost the only industrialized country that is dominated by fragmented local governments, I always feel that international comparisons would benefit discussions on this issue. This chapter also includes a brief discussion of public education and its financing. The author's main concern is with racial segregation in schools, at least after he discusses measures of segregation. The discussion is disparate; for example there is no clear distinction between measures to reduce segregation within and between school districts. Nor is voluntary segregation distinguished from involuntary segregation. Finally, no relationship is discussed between zoning and segregation, although density controls in high income areas have the effect of segregating low income residents within and between local government jurisdictions.

Chapter 9 summarizes the author's outlook. The author's first suggestion is that trade among trading places should be encouraged, but there is no suggestion how governments could improve upon private markets in that respect. The second summary view is that the distinction between urban and rural is blurred. So what? The author clearly likes car-based travel. So do I. He worries about very young and very old nondrivers. People who are too young or too old to drive also have trouble with public transportation; it is not clear to me where this leads or that inaccessibility by the young and old is the most important problem with our car-based transportation system. A final issue is how much land will be "consumed" by urban population growth in the next few decades. Of course, people do not "consume" land, they develop it. Everybody must live somewhere, and metropolitan densities exceed those in small towns and rural areas. I don't know what the issue is here.

Who is this book intended for? The author does not say. It must be either undergraduates in a general urban survey course or the general reader. In either case, the book is worth the reader's while. It is well written and thoughtful. It will not be valuable to urban scholars.

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Green Cities: Urban Growth and the Environment. By Matthew E. Kahn. Washington, D.C.: Brookings Institution Press, 2006. Pp. vii, 160. \$44.95, cloth; \$18.95, paper. ISBN 978–0–8157–4816–8, cloth; 978–0–8157–4815–1, pbk.

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This is a relatively nontechnical book about urbanization and environmental conditions. The title, *Green Cities,* is merely pandering to popular jargon. The book assumes almost no knowledge of economics and can be read by most college students.

Chapter 2 begins the discussion with measures of urban environmental quality. Unfortunately, Kahn first presents an "ecological footprint" quiz. Such quizzes are attempts to persuade readers that traditional notions of high living standards are misguided. Authors of such quizzes want us to be vegetarians living in small houses and making predominant use of public transportation. The quizzes and the book in general take no account of the fact that the usual radial fixed rail public transit system is a major cause of what they choose to call "sprawl" or of the fact that construction and operation of fixed rail underground systems are energy inefficient. The remainder of the chapter is a sensible discussion of benefits and costs of urban environmental quality. Kahn's uncritical use of metropolitan single family home prices as measures of environmental quality ignores supply side constraints imposed by geography and by government constraints on dwelling construction. The chapter ends with a proposed "green city index" (not calculated), which would not measure much that economists might recognize.

Chapter 3 discusses the "Urban Environmental Kuznets Curve," which purports to show that pollution increases with income per capita at low-income levels and decreases as income rises beyond a modest level. The chapter presents a good survey of determinants of the Kuznets curve and of technical and government measures to shift the curve. The chapter concludes with trivia that establish nothing. One is death and smoke per day during the "killer smog" in

London in early December 1952 (I lived in Birmingham, England, at the time, which was probably worse than London, and sometimes could not see across a busy street at noon time). There is no comparison with usual December death rates and no discussion of cause and effect, which have been much studied. A final piece of trivia is a graph of number of articles that mention pollution in the *New York Times* from 1979 until 2001. What is the point?

Chapter 4 discusses individual choices to become more "green" as time passes and as income rises. Kahn shows remarkable examples of "green" consumption increases as income increases despite "free rider" problems. The suggestion that Hollywood celebrities be shown driving "green" cars is at the nonsensical level of TV advertising.

Chapter 5 discusses government actions that have led to greener cities. The author recognizes that, despite the examples of the previous chapters, the external diseconomy nature of polluting discharges inevitably results in socially excessive pollution. Elaborating on the discussion in the environmental Kuznets curve chapter, he presents evidence that higher income and better-educated citizens are more likely to favor government environmental protection policies. I have doubts whether some of the pro-environment congressional votes that he has studied would have done more good than harm taking account of the costs of the proposals studied. Successive sections discuss government actions related to air, water and solid waste policies. The discussions are elementary but basically competent. The worst part of the chapter is the discussion of urban land management. Kahn obviously likes greenbelts, since they limit the "footprint" of the city they surround. They are typically accompanied by stringent limits on housing density, as in his favorite example of Boulder, Colorado, and mostly serve to raise owner-occupiers' home prices, excluding moderate income and minority people. More important, those excluded must live somewhere else. If large urban areas did not have such exclusionary zoning, densities would be much greater in large metropolitan areas than in small areas and settlements elsewhere, thus economizing on overall "footprints." The entire discussion of zoning is misconceived, despite much literature that shows that metropolitan zoning is mostly intended to exclude low income and minority residents. The author loves cobblestone streets (p. 89). I am unaware of any benefit–cost analysis.

Chapter 6 focuses on population growth and the environment. Kahn tries to show that large metropolitan areas are bad for the environment. For U.S. data, the conclusion is false. Los Angeles, his favorite example, has dramatically reduced air pollution during recent decades even as its population has grown. The conclusion is also doubtful in many developing countries, mainly because larger metropolitan areas are the primary locations of income growth and income growth provides more resources and more educated residents, both of which lead to reduced pollution. Bangkok, Dhaka, and Jakarta, all of which I know well, have dramatically higher environmental quality than they had a few decades ago, although their growth has been rapid. Dhaka is a good example. Although desperately poor, death rates from flooding and gastrointestinal diseases have decreased in recent decades, mostly because of modest improvements in seasonal flooding. Health would be much better among Dhaka's poor if the corrupt government did not make it virtually impossible for the poor to become illegal residents and refuse to provide any services (education, health services, water, waste disposal, etc.) to illegal residents. The most odious statement in the chapter is an Internet quote from one Jason Din Alt to the effect that immigration to the United States threatens ecological disaster (p. 106). How many illegal Mexican immigrants to California suffer worse environments there than at their places of birth? Why include such stuff?

Chapter 7 focuses on the environmental cost of U.S. sprawl. His definition of sprawl (p. 110) includes all suburbanization regardless of cause. A pervasive claim throughout the chapter is that sprawl increases commuting. However, sprawl includes suburbanization of both residences and jobs. If all jobs are concentrated in the central business district and residences are spread around the CBO, clearly commuting is maximal. If jobs and residence suburbanize, as they have, then it is possible for nearly all workers to live close to their work places. That this is not the pattern in U.S. metropolitan areas results from government underpricing of transportation and from government land use controls that artificially lower densities and that force excessive separation of employment centers from residences.

Incidentally, excessive density controls also induce commuters to use cars instead of buses because the diversity of workplace–residential combinations makes it impossible to provide enough riders to make buses profitable. (Fixed rail systems for suburb-to-suburb commuting are unthinkable.) Of course, government underpricing of road use and excessive density controls make the developed area of a metropolitan area excessive. To refer to sprawl as "land consumption" (p. 121) is a misnomer. The term is "land development." To refer to increased open space surrounding dwellings as decreased "open space" is also a curious use of words (p. 122). In conclusion, Kahn favors "smart growth" policies to create "sustainable" metropolitan areas. These are pejorative terms and have no place in a serious book. Smart growth means greater government controls to users of the term. I have already argued that government controls are a major source of excessive suburbanization in U.S. metropolitan areas.

Chapter 8 concludes the book. Almost all the book's data pertain to U.S. metropolitan areas and almost all show substantial improvement during recent decades. Kahn appears to me to be grudging in admitting this conclusion. He rapidly turns to developing countries, which have had only incidental importance in the book. Finally, the last pages of the book are devoted to the global climate, which has hardly been discussed in earlier chapters. The only data introduced on global environment in this concluding chapter are from the International Panel on Climate Change. Although that source is hardly the worst study of the subject, it is odd to introduce its pessimistic forecasts as unquestioned facts so late in he book.

Kahn has written many fine papers on environmental issues. This book brings together much useful work by him and others, and introduces new data and analysis. For this reviewer's tastes, it caters excessively to environmental populists.

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Z Other Special Topics

Getting Your Way: Strategic Dilemmas in the Real World. By James M. Jasper. Chicago and London: University of Chicago Press, 2006. Pp. xv, 234. $28.00. ISBN 978–0–226–39475–6.

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James Jasper's book is best described, using his own words, as "a sociology of strategy" (p. xiii) and therefore is not a book aimed primarily at economists. His compilation of strategic dilemmas and his emphasis on the subjective nature of strategy is designed to enlighten the sociological community with respect to the depth and breadth of the permeation of strategy throughout society and throughout social interactions. The book is ultimately an attempt to encourage sociologists to accept and to embrace the need to consider strategy as an important cog in explaining the machinations of social behavior. Such enlightenment is a goal that economists can champion, although they may find this book provides few truly new approaches to the analysis or application of strategy.

Getting Your Way is divided into six major chapters, along with short introductory and concluding sections. The six chapters are devoted to discussions of various aspects of social life that play a role in defining, organizing, and directing strategic decision making. The chapters flow fluidly through the initial, intermediate, and ending stages of strategic situations faced in social life. Every chapter also contains one or more of Jasper's "dilemmas," each aimed at specifying a particular strategic trade-off associated with an identifiable social interaction or set of interactions.

The first main chapter considers "starting points," ways in which individuals decide to enter or are entered into strategic interaction, and three dilemmas associated with this process. Next comes a discussion of "threats" which are identified as a primary motivator for engaging in strategic behavior; one additional dilemma appears in this chapter. Following threats comes an examination of "goals," or motives, for those who engage in strategic interaction. Within this chapter are nine dilemmas individuals and groups may face in identifying and pursuing goals in all (not just strategic) situations. Once goals have been articulated, one must have a means to work toward achieving those goals. This is the focus of the subsequent chapter on "capacities" that considers the skills and resources available or necessary to use strategy well. A further ten dilemmas appear in this chapter. What follows capacities is "players as audiences," along with seven more dilemmas. In this fifth chapter, Jasper "emphasize[s] the cultural interpretation and emotional reactions at the heart of all strategic action." (p. 118) Having identified the need to fully understand players, the book closes its presentation of dilemmas with a chapter on how to understand the "arenas" in which strategy takes place and with a final seven dilemmas related to this topic.

Although this book may be a tour de force within the field of sociology (which I honestly cannot assess in either direction), it is unlikely to find a great following among a more technically minded group like economists. Jasper is a story teller. His anecdotes and examples, both from personal experiences or observed behavior and from historical events, particularly from ancient Rome, are terrific. His taxonomy of dilemmas drawn from the anecdotes and examples, however, is overdone. The dilemmas themselves have (generally) cute names but few are memorable, some are truly opaque, and many require such lengthy explanation—running to one-third or one-half of a page—that they quickly lose whatever force they had to influence strategic thought. Further, the dilemmas from various chapters overlap those that appear in other chapters; they intertwine with each other and cannot easily be separated into clearly definable groups (and they are back- and forward-referenced as well). Finally, there are just too many of them (thirty-seven in all) for the set as a whole to be truly useful. A reader looking for a structured presentation of a well-defined set of real world strategic dilemmas will be disappointed.

Jasper's approach throughout the book is meant to show the importance of the social aspects of strategy in contrast to what he views as the overly mathematical methodology adopted by game theorists. Unfortunately, he does not appear to realize that the set of chapters he presents bears a striking resemblance to the basic building blocks used by those same game theorists. Although cutting edge game theory remains mathematically beyond the grasp of many economics students, and perhaps even their professors, applied game theory (sometimes referred to as games of strategy[1]) is more accessible and takes an approach similar to that which Jasper champions. When thinking about strategic games at a very applied level, one would generally start by fully describing an interaction, including among other things its initial conditions (starting points), the actions (capacities) available to players, the preferences (goals) of those players, the rules of the game (arenas) in which they are playing, and the potential for strategic moves (threats) to be made. (Notice that these descriptors cover all six of Jasper's chapters.) Then one would also have to think carefully about levels of information and knowledge across players, how players view themselves and each other, whether and how social norms might play a role in the game, etc. in order to specify a satisfactory payoff structure for a particular strategic interation. In fact, in applying game theoretic tools and concepts to strategic situations faced in everyday life, researchers consider many of the "social" issues that Jasper argues are missing from game theoretic models. Those researchers, because they are economists, tend not to focus, perhaps, on the social aspects of the interaction but they do not ignore them. Although it may be grossly true, as Jasper says, that "strategic interaction cannot be reduced to mathematical models" (p. 172), most game theorists would argue that strategic interaction certainly can be guided by the predictions that emerge from those models.

For a book that advertises itself as a tome devoted to *Getting Your Way,* this piece does not actually provide any specific advice to help you truly get your way. As such, the title is somewhat deceptive. Jasper unabashedly admits in his preface that he is not writing a "how-to" (p. xiii) and in his conclusion that his "aim has not been to provide lessons to practitioners" (p. 176). But, as a result, his book lacks a clear, core message beyond "social aspects matter to strategy" or perhaps more accurately "strategy matters to social interaction." This message, while worthwhile, may prove relatively old hat among economists.

References

Dixit, Avinash, and Susan Skeath. 2004. *Games of Strategy*. Second edition. New York: W. W. Norton.

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[1] See, for example, Dixit and S. Skeath (2004).