A General Economics and Teaching


JEL 2004–0833

There’s a new microeconomics on the block, and it’s not the microeconomics you were taught in school. The new microeconomics takes seriously that many markets and contracts are incomplete, that agents are differentially informed, that much that is pertinent to their interactions is not verifiable or admissible in a court of law. While those first elements would shock no one trained in the past thirty years, the new microeconomics goes much further, allowing that people sometimes display social preferences such as concern over fairness, a desire to reciprocate when treated well, and a desire to punish when taken advantage of. More radically, still, this new microeconomics takes institutions as not only critical, but variable and scarce, and it treats their evolution and selection as a central problem of economics. Indeed, this new microeconomics sometimes takes preferences or institutions as the variables to be explained, modeling selection.

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.
of agent types and institutional outcomes under relevant evolutionary pressures.

In giving this microeconomics what is perhaps its first textbook-style and textbook-length treatment, Samuel Bowles makes only modest claims for the completeness of the new paradigm. In the introduction, he cites J. S. Mill’s famous 1848 howler: “Happily, there is nothing in the laws of Value which remains . . . to clear up; the theory of the subject is complete.” Writes Bowles in contrast: “This book conveys no such reassurance. Our understanding of microeconomics is fundamentally in flux. Little is settled. Nothing is complete.” (p. 19). In the concluding chapter, where he summarizes key elements of the “evolutionary social science” he sees emerging, he readily admits that there “is no unified paradigm of this name, but rather a disjointed set of approaches, many of which are rather rudimentary” (p. 478). Such modesty is becoming, and in some respects apt, but should not give the wrong impression: there are already many promising tools in Bowles’s paradigm-building kit.

The book is divided into three parts. Part 1, “Coordination and Conflict: Generic Social Interactions,” introduces game theory, presents elements to be used in later chapters, and discusses methodological issues. Chief among the latter are the nature of preferences and the two-way relationship between preferences and institutions, a subject of much discussion in part 3. Chapter 4, on coordination failures, discusses common property, public goods, and other coordination problems, and considers how social preferences sometimes help to solve them. Chapter 5, “Dividing the Gains to Cooperation,” introduces Nash and other bargaining models and discusses the conflict between cooperation and competition that looms large throughout the book.

Part 2, “Competition and Cooperation: The Institutions of Capitalism,” treats the more traditional topics of microeconomics in distinctive and sometimes novel ways. Chapter 6, “Utopian Capitalism: Decentralized Coordination,” lays out the Walrasian general equilibrium model and carefully discusses its limitations. Exchange under more realistic conditions, such as incomplete information about quality, is analyzed in chapter 7. Chapters 8 and 9, among my favorites in the book, discuss labor and credit markets, elaborating the “contested exchange” framework developed by Bowles and frequent collaborator Herbert Gintis. Like its somewhat more staid cousins, the Shapiro–Stiglitz unemployment model and the Stiglitz–Weiss credit market model, the contested exchange approach depicts a world in which markets don’t clear and prices determine effort or quality. For Bowles, the terms of the exchange, rather than being settled by a handshake, are worked out through time under the influence of “short side power.” Chapter 10 discusses why the institutions of a capitalist economy are as they are, e.g., why firms controlled by capital-providers hire workers rather than workers organizing firms and hiring (renting or borrowing) capital.

Part 3, “Change: The Co-evolution of Institutions and Preferences,” is the least traditional part of the book. As its title suggests, the aim is to understand how preferences shape institutions, how institutions shape preferences, and how both could have emerged from human biological and cultural beginnings. Economic history, growth, and development provide examples for discussion, and the text takes lengthy excursions into the preagricultural world of 100,000 to 11,000 years ago. Why study the coevolution of institutions and preferences? Because, argues Bowles, they’re of fundamental importance to real world economic and social outcomes. He begins the book with the puzzle of why the lush Bangladesh and wealthy Moghul India of the fourteenth and fifteenth centuries sank into poverty while the relatively poor Europe of that period grew, along with some of its colonial offshoots, to be the economic leaders of today’s world. The answer, he suggests, is the “emergence and diffusion of a novel set of institutions that came to be called capitalism [which] brought about a vast expansion in the productivity of human labor in Europe and not in Bangladesh [or India].”

Capitalism is not, argues Bowles, simply the substitution of fully defined property rights and contracts for custom, community, and state. Rather, it’s a dynamic mix of new, yet never contractually complete, arrangements, with long-present human motivations ranging from greed to reciprocity and vengeance, bolstered by effective states, legal systems, and communities of the workplace, neighborhood, and interest group. Understanding the system’s emergence and functioning requires not only the old invisible hand
model and its protagonist, *Homo economicus*, but also models of how trading partners establish trust, why workers respond positively to job rents, and why people participate in civic and political organizations. And for this, one needs to know how reciprocity, the tendency toward “altruistic punishment,” and other “social preferences,” could be supported by a human gene pool that from a simplistic evolutionary standpoint seems inconsistent with survival-of-the-fittest principles.

Although Bowles’s work helps to set out a research agenda that will take decades to explore, the book offers many of the necessary tools, a number of interesting starts, and ample food for thought served up with rich perspective on the histories of both the ideas and the substantive questions at issue. It may not become next year’s core graduate micro text in most universities. But whether as a companion to a more conventional text, or as an entrée in its own right, it’s an intellectual meal to relish.

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JEL 2004–0008

The authors have previously written a graduate level econometrics textbook entitled *Estimation and Inference in Econometrics*, which was published in 1993 by Oxford University Press. The present book covers many new topics and is at a slightly less advanced level. Because of the adjustment in the level, the authors occasionally refer to their older book for more details or more precise derivations of results. Therefore one wonders whether both books will remain competitors in the market of graduate level econometrics textbooks. If the older book disappears, it may also reduce the value of the present one.

The present book assumes a good bit of prior knowledge in statistics and econometrics. Although the prerequisites are often briefly reviewed, this is done at a level which will not be easily accessible to students without some background knowledge. As the title suggests, the book focuses on the theory and methods and not on applied econometrics. Given the lack of empirical examples except in the exercises, it is clearly helpful for a student to approach the book with some background motivation.

The first ten chapters and thus almost two thirds of the book cover the foundations by focusing on important methods of inference for econometrics. After an introduction to the linear regression model (chapters 1 and 2), estimation by ordinary least squares (chapter 3), hypothesis testing (chapter 4), confidence intervals (chapter 5), nonlinear least squares (chapter 6), generalized least squares (chapter 7), instrumental variables (IV) estimation (chapter 8), the generalized method of moments (GMM) (chapter 9), and maximum likelihood (ML) estimation (chapter 10) are treated. Some of the material in these chapters is quite standard but the presentation is often a bit nonstandard, which makes the text unique and interesting as a textbook. For example, there is a long discussion of the geometry of the linear regression model. There may be students who prefer that to a purely algebraic treatment. Also, confidence intervals are introduced via tests. This way some common misconceptions about confidence intervals may be avoided more easily than with a “classical” introduction. On the other hand, understanding how to interpret and use confidence intervals does not necessarily become easier.

A big plus of the treatment in the present volume is the introduction of simulation and bootstrapping methods at a very early stage. These important tools are presented here by experts with admirable insights. I like especially the discussion of bootstrap confidence intervals in chapter 5. A small problem in this chapter is perhaps the unfortunate example for the delta method on page 206, where the asymptotic properties of an estimator of $\theta = \theta^2$ are to be inferred from those of an estimator of $\theta$. The delta method can be used unless $\theta = 0$. This case is also a very nice example for the bootstrap interval given in (5.54) to be unsuitable because it will contain the true parameter value with probability zero whenever the confidence level is less than one. Unfortunately these problems are not discussed or pointed out by the authors and the example is presented as one for rather than against these methods. Generally, the exposition is quite insightful, however, and even advanced methods like the method of simulated moments (chapter 9) become accessible.
The statement of the central limit theorem in terms of a probability limit rather than the more common convergence in distribution on page 149 is nice because it makes some asymptotic arguments more intuitive and easy to explain. On the other hand, it might be helpful for some readers to see a discussion of the relation to the more common definition of convergence in distribution.

Another special feature of the present text is that the authors introduce method-of-moments estimation early on and thereby make the discussion of IV estimation and GMM more natural or at least easier to motivate. In this context, a special feature of the book is also the discussion and use of the theory of estimating functions. Moreover the authors emphasize the use of artificial regressions. These features give the book its special flavor which may well appeal to some students more than a standard approach to IV and GMM estimation, for example. In addition to the general methods, a wide range of modern topics such as heteroskedasticity and autocorrelation consistent (HAC) covariance matrix estimation is discussed.

Assumptions and mathematical or statistical tools are usually introduced or reviewed where they are needed. This strategy of presenting them has the advantage that they are easy to motivate. Also the assumptions needed for the derivations are often just stated somewhere in the text or are just referred to as regularity conditions which are not stated. The results are usually not displayed as theorems or propositions. The drawback is that I sometimes had trouble keeping track of the conditions under which the derivations are done. Also it can be difficult to find a particular result in the book. Therefore, I can only agree with the authors’ self assessment in the preface that “this book is intended more as a text than as a reference . . .” (p. xi).

While generally the explanations and derivations are quite well done, there are occasional exceptions where I found the presentation unclear or misleading. Take for instance the discussion of the asymptotic theory of the HAC covariance matrix estimator on page 363 which is clearly problematic. Another problem occurs on page 161, where the authors infer from consistent estimation of an individual error that the whole error vector of a regression model whose dimension depends on the sample size is estimated consistently. Despite these caveats, the first ten chapters provide a very good foundation for departing to other econometric topics some of which are considered in the last five chapters.

Chapter 11 covers microeconometric tools such as logit, probit, tobit and duration models. Systems of equations including seemingly unrelated regressions and nonlinear models are discussed in chapter 12. Stationary time series models, including distributed lags, error correction models, and a very brief introduction to vector autoregressions, are considered in chapter 13. Chapter 14 is then devoted to nonstationary time series with unit root testing and cointegration. Finally, chapter 15 discusses a wealth of model checking tools in addition to those that were considered already in earlier chapters. These range from specification tests based on artificial regressions to tests of nonnested hypotheses and nonparametric methods. Also a brief section on model selection criteria is included. Given the need for intense model checking in applied work, the last chapter is clearly an asset for an econometrics textbook.

In summary, the book is in my view a very good choice for a graduate level course in econometric theory for those who like the authors’ style. It has some particular strengths which make it attractive. One of them is the emphasis on modern simulation methods throughout the text; others I have mentioned above. One advantage I have not mentioned previously is the rich set of exercises. Some of them require challenging algebraic derivations, some guide through simulation studies and others offer empirical illustrations. Solutions to selected exercises and other supporting material for the book are available at a website.

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two other essay collections, *Out of the Margin: Feminist Perspectives on Economics* (eds. Edith Kuiper and Jolande Sap, 1995, London and New York: Routledge) and the encyclopedic *Elgar Companion to Feminist Economics* (eds. Janice Peterson and Margaret Lewis 1999, Cheltenham, UK: Edward Elgar), would provide one with a thorough overview of the major issues and topics treated by the still-nascent area of feminist economics, both as originally conceptualized in the late 1980s and as developed over the ensuing decade and a half.

Marianne A. Ferber and Julie A. Nelson’s introduction summarizes the development to date of feminist economics institutions. This progress is substantial and includes formation of a healthy flagship organization, IAFFE (International Association for Feminist Economics), IAFFE’s journal, *Feminist Economics*, and a growing list of publications that can be recognized as explicitly feminist economics. Ferber and Nelson also assess how much of an impact feminist economics has made upon the economics mainstream (some—but awareness lags).

Sociologist/honorary economist Paula England follows with an essay that expands upon her piece on “the separative self” in the earlier volume, addressing the dichotomy between separative and soluble self as well as the use of dichotomous thinking in economics. After critiquing the lack of allowance for altruism, taste formation, and empathy in mainstream economic models, she cites increased interest in the modeling of bargaining within marriage, endogenous tastes, and the study of care work as promising countertrends within economics.

England and Nancy Folbre then combine to consider contracting for care, a topic that Folbre in particular is noted for developing. They suggest the usefulness of theory regarding various contracting problems, including missing and incomplete markets and monitoring and enforcement problems, in considering why the level of care provided in societies may be suboptimal. They also critique the “traditional masculine emphasis on individual choice, rational choice, and measurable results” (p. 75) for deflecting economists’ attention away from the issues of how and how much care service is provided.

Julie A. Nelson continues the separative vs. soluble discussion from the England paper and from her own earlier work, using this framework to address the modeling of the firm by economists. Nelson draws on a range of insights from both mainstream economists and economic sociologists in critiquing the way in which the firm is treated in much of economic modeling. This discussion is not overtly gender-related, but nonetheless continues her critique of economic thought regarding the blindness that stems from overuse of gender-linked dichotomies.

Lisa Saunders and William Darity Jr. consider the symmetries and asymmetries in treatment of gender and race in economics. They argue for greater explicit recognition of the relationship between feminism and antiracism, while at the same time arguing that exclusionary mechanisms need not operate similarly on the bases of gender and race.

Lourdes Benería contributes an essay on “Economic Rationality and Globalization.” This contribution, incorporating as it does broad discussions of current globalization patterns, Polanyi’s work in analyzing earlier market formation trends, Davos man, and gender norm creation, is overly ambitious given the space constraint and wavers in focus.

Myra Strober discusses how mainstream economics constructs have been used in the education literature and how this has been in many ways problematic, for instance in narrowing the view of the role of education down to an emphasis on earnings-related skills and in focusing on increasing efficiency and choice rather than on increasing funding to schools. Strober thus identifies the colonization of a multifaceted field by neoclassical economics as problematic when no offsetting heterodox economic perspective serves as balance (much as when Beckerian analysis was used to analyze marriage and family-related topics).

Rebecca M. Blank and Cordelia W. Reimers, in contrast to the rest of the authors, explicitly identify as “economists who are feminists” rather than as “feminist economists.” As such, they ponder the way in which feminism affects their topic selections and emphases within their empirical public policy-oriented research agendas, and advocate a marginalist approach to incorporating feminist-motivated theoretical innovations into economic modeling.

S. Charusheela and Eiman Zein-Elabdin consider the relationships between feminism, postcolonial thought, and economics. This essay serves as a useful primer for economists who
want to begin to understand the terminology and concerns of their university colleagues who are steeped in postcolonial (often by way of postmodern) thought. The literature that explicitly ties economics to postcolonial thought is small, but likely to grow substantially, in intersection with development studies and thereby with development economists, in the near future.

All the contributors think that economics should be more open to critiques from within and without economics and more open to subject matter that may be currently more of a preoccupation within other disciplines. But they vary in willingness to depart substantially from mainstream modeling, in high correlation to the degree to which their own training was neoclassical vs. heterodox. For good or bad, although this book goes a long way in helping the reader understand what feminist economics is (and is not), the definition of feminist economics, and the answer to whether or not the critiques of mainstream practice that it entails are unique among the heterodox economic traditions and related social sciences, is still not cast in stone.

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JEL 2004–0016

This book is a twenty-three chapter-long spear that aims to kill the woolly mammoths that are courses on African economic development. Unfortunately, the book’s aim is not true. The chapters are repetitive, disconnected, the editing shockingly bad, and there are significant absences (little on gender or debt, for example).

Chop the book up, and the teacher will have some darts—not suitable for woolly mammoths, but capable of bringing down smaller prey. Among these are several fine contributions that would be useful in undergraduate courses. Chapter 5, for example, is an excellent survey of growth theories and their application to Africa by Marcel Fafchamps. His argument is that both neoclassical and endogenous growth theories miss the point: the pecuniary externalities of agglomeration are the real game in town. This seems sensible. Unfortunately, this shift in perspective yields no silver bullet for policy; Blaise Compaoré of Burkina Faso and the three dozen other strongmen on the continent will receive no answer to the question central to Fafchamps’s article: How long to wait until agglomeration externalities are played out in China and what to do in the meantime?

Paul Collier in chapter 8 reprints a stimulating summary of the relation between ethnic diversity in Africa and economic and political outcomes. He reiterates a controversial finding: ethnic diversity only matters when decoupled from democracy. Multiethnic societies that are democratic perform fine. The chapter also includes a fine review of theories of ethnicity in political action. This literature is hobbled by a lack of attention to the problem of scale and to the technology of identity. Kin, clan, and tribe are important identities everywhere in the world. Collier is at pains to note that they are effective identities for structuring economic transactions in an environment of limited information and repeated interaction. Being tall or liking salty food would not be useful identities for solving the social problem of who to trust. Effective identities are relatively immutable, geographically encompassing, and easily verified. Being tall misses the second property, while liking salt misses the third. But the identity that matters for the national stage, referred to as “ethnicity” in most African contexts, which generates the ethnic markers that corral voters into blocs, is plainly neither immutable nor verifiable. A vast literature shows precisely how mutable national-level ethnicity is. So does it make sense to treat the two kinds of identity—local tribe and national ethnicity—as the same thing? Local identities can coexist, rather, with national-level ethnicities. The question of the production of national-level ethnicity—its endogeneity—is left undiscussed. This is unfortunate, because it may well be that democracy and dictatorships produce different kinds of national ethnicities. One thinks of the colonial period, when ethnicities were produced quite deliberately by authorities for reasons of both administrative convenience and Machiavellian preservation of illegitimate regimes.

Some of the other chapters in the book are quite adequate. Chapter 3, by Nnadozie, is a decent introduction to concepts and measurement of growth. Chapter 7 by N. Vink and N. Tregurtha on poverty is a reasonable quantitative assessment of the quality of life of South
African farm workers. Mario Azevedo offers a good survey of health issues on the continent in chapter 9, and Azevedo and Nnadozie in chapter 10 present a nice introduction to education issues. Surveys on financial development, by Léonce Ndikumana, and globalization and development, by Richard Mshomba, will be useful to students in introductory courses. Femi Babrinde's introduction to regionalism in chapter 19 is silent on empirical estimates of the cost of trade diversion in the African context, an absence that considerably reduces the value of an otherwise decent essay.

Collier's chapter on ethnicity is best read after a straightforward chapter 11, by John Quinn, that reviews the evidence on democracy and development. Quinn's chapter is pitched at the right level for this volume, and students will then be ready for the more challenging chapter by Collier. The opposite is true for the dynamic panel GMM estimates of Kwabena Gyimah-Brempong's chapter 12 on political instability and for the muddled summary by E. Wayne Nafziger of a project exploring inequality and conflict in Africa. Nafziger's outcome variable, humanitarian emergencies, is not well-defined in the chapter, so it is difficult to read without going to other papers by the author. The chapter seems to be simply a direct “lifting” of a discussion of regression coefficients, but without any of the regression results presented, leaving the reader with an impression of arbitrary pronouncement that this or that factor matters or does not matter. The sketches of real conflicts scattered through the text add to the impression of a hasty cut-and-paste document.

These last two are not the only bad chapters in the volume. Oddly, the introductory chapter by the editor is among the least useful. Nnadozie attempts to motivate the study of African economies, but in very confused and hesitant way. A section on “principal messages” begins with a brief discussion of the W. A. Lewis model, continues with a summary of recent cross-country growth regressions, and then arrives at the principal message that there is no principal message because studies of African economic development are in flux, are complex, and are confused. Surely the goal of an introduction should have been to resolve, simplify, and clarify.

Following the vein of bad chapters, chapter 6 on population growth is marred by an inability to clearly specify the difference between causation and correlation between growth in population and growth in income per person. An awful chapter on land tenure and agricultural development includes a bizarre section lauding Zimbabwe's growth in maize production, with nary a nod to the daunting land tenure issues that have helped turn Zimbabwe into a basket case. A chapter on trade and development reprints seven pages of tables giving per year, per country, data on trade flows, in micro-font sizes. These and others suffer from poor writing, disorganization, inappropriate discussions of technicalities, and lack of focus. A closing chapter by a Cambridge, MA “brain trust” of James Deessenberry, Arthur Goldsmith, and Malcolm McPherson fails to deliver originality. Instead, the authors survey all of the policy failures on the continent, and conclude that if the policy failures are reversed, growth will follow. How pat is that?

The book is intended for students, but the instructor should be very wary of assigning this book. One editorial mistake was to have each chapter begin with a list of “key terms.” The first “key term” of the book is an infelicitous term: African dummy. What kind of a book oriented to students who do not know regression analysis starts out by highlighting a negative coefficient on a dummy variable? Many of the key words for many other chapters are nonsensical. In some chapters the writing is quite bad. Should a student be exposed to such poor writing?

If I seem caustic, it is because the publisher, Academic Press, an imprint of Elsevier Science, seems here to be in the business of duping a well-meaning public. Around the world, courses on African economic development will be taught with greater frequency. Should Elsevier cash in on this trend by selling an amoebic product? Yes, one of the editorial lapses is letting an author refer not to anemic economic growth but to amoebic growth . . .

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JEL 2004–0853
Lance Taylor's new book, Reconstructing Macroeconomics, offers an impressive structuralist critique of modern macroeconomics and builds the case for restoring an encompassing structuralist perspective on contemporary macroeconomic issues. This critique rejects two core tenets of mainstream economics, the role of methodological individualism, as articulated in the representative agent, and the reliance on a supply-side determination of output, in favor of an approach which emphasises social relations between broad groups (often “workers” and “capitalists”), views macroeconomics as being driven by the dynamic evolution of these social tensions, and sees output determined on the demand side (the Keynes–Kalecki principle of effective demand). This book is concerned with challenging the former and promoting the latter.

The book's origins lie in Professor Taylor's lecture course in advanced macroeconomics at the New School in New York. In style and structure it reads as such, which is both a strength and a weakness. However, it is much more than a textbook: it is a highly individual essay on the history of economic thought, but also the best introduction to the canon of structuralist economics I have read. And, as the cover reviewers stress, it is written in Taylor's distinctively witty, provocative, and deeply intelligent style.

In terms of structure, the book inverts the “structuralist proposals and critiques of the mainstream” of the subtitle. Chapter 1 sets the stage with an introduction to the social accounting matrix (SAM) framework used throughout the book, using it to place class-based conflicts at the heart of the national accounting system (even though SAM methods are widely used outside the structuralist camp). The next five chapters represent the “critiques of the mainstream.” These are all excellent chapters covering: price determination (chapter 2); money, interest rates, and inflation (chapter 3); and the characterization (and determination) of aggregate demand, consumption and investment (chapter 4). It is here that Taylor throws down the gauntlet, arguing that: “the essentials of the principle of effective demand thus appears to have survived sixty years of intense scrutiny. Moreover, strong attacks to a greater or lesser extent have misfired . . . Say's Law is not true . . . and effective demand is the valid approach to macroeconomics” (p. 172).

The development of this theme starts with a discussion of alternative macroeconomic closure rules and how they shape perspectives on growth (chapter 5); building up eventually to a full-frontal assault on monetarism and new classical economics in chapter 6. Typically, these chapters start from the SAM laid out in chapter 1 and then introduce a few simple finger exercises based on alternative closure rules so as to highlight basic insights which form the link into the broader critique to follow. Taylor's mastery of mainstream macroeconomics, and his ability to place ideas within a compelling historical narrative, make these chapters highly readable and thought-provoking, and contribute to a compelling critique. For example, the first ten pages of chapter 3 on money is one of the best short histories of monetary thought one will find anywhere.

The second half of the book turns to the “structuralist proposals” of the title, developing, in particular, the ideas foreshadowed in chapter 4. The main elements of Taylor's structuralist synthesis are contained in chapter 7 (on effective demand and the idea of the “Distribution Curve”); chapter 8 (on structuralist finance and money), and chapter 9 (on cycles). All three chapters are, however, very hard work, especially for the reader (especially the graduate student) schooled in mainstream economics. Part of the problem is the book’s format: by following the lecture course each chapter is constrained to be roughly the same size. As a result, density is the endogenous variable. Having worked through these dense chapters, though, the reader will greatly enjoy the book’s final excellent chapter on growth and development—figure 11.1 on “filiations and oppositions in growth and development theory” could occupy an entire graduate seminar by itself. Briskly paced and highly entertaining, this chapter ends with Taylor taking a tilt at many big names and shibboleths in the neoclassical canon. It is easy to imagine the graduate students who have stuck with him through the rigours of earlier chapters applauding Professor Taylor on as he completes the course with this virtuoso flourish!

I close with two, minor, reservations about the book. The first is that, as with any partisan review, Taylor tends to build straw men. Mainstream theory is often caricatured, giving the reader the sense that deep issues have remained underexplored from within the mainstream. This tendency
is apparent to various degrees in all chapters, but perhaps the most notable instance is the almost complete failure to recognize the wealth of modern (mainstream) political economy.

A second, deeper, frustration is that despite its great length and broad sweep, and an acknowledgment in chapter 1 of the principle of falsificationism, the book never really engages with the empirical evidence that would support (or refute) Taylor’s thesis. True, some considerable space is devoted to a structuralist explanation of recent developments in the U.S. economy, but what is required is not that we can construct a coherent structuralist interpretation that is consistent with the data, but rather that mainstream economics is falsified at the same time. This evidence is never adduced. Perhaps more surprisingly, the book is remarkably silent on the evidence and experience of developing countries, especially those in Latin America where, arguably, structuralist ideas were most readily accepted in policy circles.

These are minor reservations, though. Few economists will fail to appreciate the scholarship and historical sweep of this book. It will annoy and amuse in equal measure, but ultimately it is unlikely to change any minds. It is also unlikely, sadly, to make it to the top of the “recommended reading list” for most graduate students in economics. This book introduces optimal control theory not in its most general (and rigorous) form, but rather in terms of a simple “prototype” economic control problem with wide applicability. In the one-dimensional version, the problem is to maximize the present discounted value of a “payoff” flow (e.g., cash flow, profit, utility), which is a function of a state variable (e.g., the capital stock, K) and a control variable (e.g., investment, I), subject to a differential equation linking the state variable to the control variable (e.g., dK/dt = I – δK) and one or more non-negativity constraints. Because this prototype problem has an infinite horizon, the application of the Maximum Principle is relatively simple, but it also has broad applicability. Weitzman shows how ten basic problems in capital theory fit this prototype framework—for example, the investment decision of a value-maximizing firm, the optimal extraction of an exhaustible resource, optimal tree harvesting, and the neoclassical optimal growth problem.

The most innovative aspect of the book is its treatment of national income accounting. Weitzman shows (in chapter 5) how a fairly general optimal multisector growth model can be structured and solved using the Maximum Principle. This is interesting in its own right because Weitzman introduces and explains the multidimensional version of the Maximum Principle of optimal control theory and its application to economics. It is an excellent textbook on optimal control theory—and its application to economics. It is a valuable resource for graduate students and practicing economists alike. The book provides a clear and comprehensive exposition of optimal control theory as a mathematical tool and, using a variety of examples, illustrates its application to problems in economics. Of course, there are other books that explain optimal control theory and show its use in economics; an example is the 1970 volume by Kenneth Arrow and Mordecai Kurz, Public Investment, the Rate of Return, and Optimal Fiscal Policy, Johns Hopkins Press, that Weitzman, I, and many others benefited from during and shortly after graduate school. However, there is no other book (at least that I am aware of) that provides such clarity, particularly with respect to the economic relevance of optimal control. Given the importance of the methodology and the breadth of economic applications, this is a textbook that should be assigned reading for most graduate students in economics.
Principle and shows how it can be applied to problems in growth theory. But the main payoff is the application to a comprehensive theory of national income accounting. Weitzman shows that the Hamiltonian of the optimal control problem can be interpreted as the income or “net national product” of the unit doing the optimizing. This is more than a theoretical curiosity. By incorporating, for example, resource depletion in a multisector growth model, one can derive a comprehensive (I will avoid the adjective “green”) measure of national income. Thus, this book addresses a fundamental problem in modern environmental economics.

Is anything missing? I would have liked to see a chapter devoted to explaining the relationship between optimal control theory, dynamic programming, and the calculus of variations. The typical approach in dynamic programming is to eliminate the control variable from the Bellman equation (by substituting in the first order condition) and then solving the Bellman equation for the value function. Depending on the problem, the Maximum Principle can be easier to apply because it is not necessary to actually solve for the value function. On the other hand, in some cases the value function itself can be very informative. (For a detailed discussion of dynamic programming, see Dixit and Pindyck, *Investment Under Uncertainty*, Princeton University Press, 1994). It is also worth noting that by differentiating the Bellman equation with respect to the state variable, one can then use the first order condition to substitute out the value function, which yields the Euler equation of the calculus of variations.

Weitzman introduces the Euler equation and discusses it briefly, but it actually deserves more attention. Why? Because that is the estimating equation used in most econometric models of dynamic optimization with rational expectations. In models of factor demands, dynamic consumption behavior, asset allocation, etc., it is usually not possible to solve the underlying stochastic dynamic optimization problem, but it is almost always possible to derive the Euler equation and estimate it to obtain the parameters of interest.

But these are quibbles. It is easy to ask for more when someone else has to do the writing. Weitzman deserves considerable applause for this clear and elegant book.

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B Schools of Economic Thought and Methodology

*Toward a Feminist Philosophy of Economics.*

Feminist economics emerged as a self-defined field in the mid-1990s, with the publication of the classic collection *Beyond Economic Man* (Marianne Ferber and Julie Nelson, eds., University of Chicago, 1993) and the inception of the journal, *Feminist Economics*, in 1995. Since then, feminists of many kinds have taken up the challenge of rethinking economics. They have added or emphasized the topics of women, gender difference and inequality, and unpaid work. They have made evident the existence of masculinist bias in mainstream or “neoclassical” economic theory. They have explored nonpositivist methodologies and alternative theories.

*Toward a Feminist Philosophy of Economics* serves as a kind of showcase of efforts of feminist economists to move beyond critiques of mainstream economics toward substantive positive contributions both in methodology and content. It focuses on core issues that have preoccupied feminist economists and includes essays by many leading feminist economists.

The book includes an excellent introduction by the editors which surveys feminist economics’ many aspects and contributions to feminist economics, and twenty essays, loosely grouped into five sections: “Rereading History,” “Science Stories and Feminist Economics,” “Constructing Masculine/Western Identity in Economics,” “Beyond Social Contract: Theorizing Agency and Relatedness,” and “Rethinking Categories.” Given limits of space, I will only be able to focus on a sampling of the book’s rich offerings.

In one of the book’s historical essays, Irene Van Staveren moves beyond the boundaries of mainstream economics by focusing on fiction written by the nineteenth-century feminist writer Charlotte Perkins Gilman. While Gilman is best known in economic circles for her nonfiction book, *Women and Economics*, Van Staveren shows that two of her fictional works, *The Yellow Wallpaper* and *Herland* are, like the
oft-used story of Robinson Crusoe, rich in insights about the core economic concept of efficiency. She shows how the first reveals the inefficiencies of the traditional sexual division of labor, through the wasting of the full-time mother's productive abilities, and through her mental deterioration as a result of her isolation and lack of creative connection to the world—issues not taken into account in the usual Beckerian analysis. From the second, about a women-only utopia in which women's care-taking work is highly valued and organized cooperatively alongside other forms of work, and in which all work out of intrinsic motivation, Staveren culls a new, holistic, and dynamic concept of efficiency. She notes that, because intrinsic motivation supplants the profit motive, “the high levels of trust and responsibility among producers and consumers reduce the occurrence of economic crises while limiting the incidence of negative externalities such as pollution” (p. 65).

Feminist economists have been searching for ways to make economic inquiry less male-biased and more feminist, realizing that the latter effort, by definition, takes a normative stance that disqualifies it from being science in the eyes of many or even most mainstream economists. Seven of the essays in the volume focus completely or partly on methodological issues, representing a wide range of positions. In one of these, leading feminist philosopher of science, Sandra Harding, discusses ways to avoid the pitfalls of both positivism and relativism, while another key feminist economist, Julie Nelson, makes a strong case for “process ontology” which views the universe as alive, interconnected, based in experience, and inherently value-laden, as opposed to mainstream economics’ dualistic view of “hard” positive theory and “soft, fuzzy, and unimportant” normative analysis. Finally, in a “post-ist” (e.g., postmodernist, poststructuralist, postcolonial) analysis of mainstream economics which is unusually comprehensible, Nitasha Kaul notes the ways in which economic theories are “productive enterprises” producing a version of reality, including a group of outsider topics and people.

The book’s third part, entitled “Constructing Masculine/Western Identity in Economics,” includes two fascinating articles that use psychoanalytic concepts to analyze mainstream economic concepts. Edith Kuiper looks at Adam Smith’s construction of the moral man in The Theory of Moral Sentiments, convincingly arguing that the common view that Smith’s man “feels for others” is incorrect. In fact, she argues, Smith views men as incapable of experiencing emotional connection or sharing feelings. This is part of Smith’s unconscious project of establishing a masculine identity that is “radically separate from women and all the emotionality and passion associated with them” (p. 146). The unconscious meanings connected with neoclassical economic concepts are the subject of Susan Feiner’s thought-provoking, psychoanalytically grounded piece. She argues that the “idealized market . . . mirrors the fantasy mother of the unconscious” (p. 185). Feiner attributes the resiliency of mainstream economics to its appeal to the unconscious, and notes that part of the feminist struggle has to take place on the symbolic level.

The book also contains a fine collection of essays on a key topic in feminist economics: the unpaid caring labor of women. Among these, Nancy Folbre’s classic article, “Holding Hands at Midnight,” examines different theoretical explanations for the undervaluation of caring labor and then underlines the problem of its declining supply as women increasingly focus on gaining economic equality with men as a result of the undervaluation of caring labor. A systematic analysis of “the caring situation,” centered in a set of useful concepts (relatedness, asymmetry, dependency, power structures, split functions, and one-way transfers), is contained in Maren Jochimsen’s contribution; Jochimsen claims that these theories can help economic theory understand the many non-exchange-based economic interactions. Susan Himmelweit presents a simple and quite useful evolutionary model of the shift from full-time mothering to wage-earning mothering.

The essays in the book are uniformly well written, and both rigorous and readable (even to the non-feminist-economist). I recommend it highly both to feminist economists who want to read cutting edge research in the field and to economists who are not yet familiar with feminist economics and want to learn about and from it.

JULIE MATTHAEI
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Modeling Rational Agents: From Interwar Economics to Early Modern Game Theory. By

JEL 2004–0032

While historians of twentieth-century economics have paid a great deal of attention to the work of Keynes and his followers, and to macroeconomics in general, the pendulum has begun to swing in recent years, with the appearance of a number of books focusing on the history of microeconomics, mathematical economics, and game theory. For example, some readers of this journal will have already encountered Ingrao and Israel's *The Invisible Hand* (MIT Press 1990) or Roy Weintraub's *Stabilizing Dynamics* (Cambridge University Press 1991), while many more will have read Sylvia Nasar's recent biography of John Nash, *A Beautiful Mind* (Simon & Schuster 1998). These books, and others like them, offer varying perspectives on the evolution of microeconomics in the century we have just left behind. Some focus narrowly on the theory; others emphasize the relevance of biographical details to scientific achievement; others attempt to relate the evolution of economic analysis to broader developments in mathematics and the sciences. New to this company of historical writers is Italian scholar Nicola Giocoli, winner of the History of Economics Society's 2001 Dorfman Prize for his doctoral dissertation and author of this new book.

*Modeling Rational Agents* comprehensively examines the evolution of microeconomic theory between the early neoclassicism of the turn of the twentieth century and the game theory of von Neumann, Morgenstern, and Nash. Giocoli's main thesis is that microeconomics, which is taken to include general equilibrium theory, has gone in that interval from being a theory focused on *Systems of Forces* (SOF) to one focused on *Systems of Relations* (SOR). In the former, economic analysis is as much concerned with the process leading to equilibrium as with the final state itself, and there is an explicit concern for the relationship between the equilibrium construct and the "real world." In SOR theory, on the other hand, little attention is paid to process or, for that matter, to the relevance of such systems for the actual economic world, and great emphasis is placed on the mathematical properties of equilibrium, such as existence, stability, and uniqueness. Taking the lead from historian of mathematics Leo Corry, Giocoli relates this shift in the *content* of economic theory to a change in the latter's *image*: if theory has moved in this particular direction, it is because theorists' expectations of what the theory is intended to accomplish have changed too. Empirical description has been relegated in importance, Giocoli suggests, with mathematical coherence tending to take precedence over everything else.

Giocoli steers the reader through a vast range of theoretical contributions, using as a guiding light the treatment of economic rationality and arranging his exploration according to several broad themes. An introductory chapter, "Two Images of Economics," sets out the plan of the book, relating the above-mentioned change in the image of economic theory to the mathematical–historical background of Hilbert's formalism and the later Bourbaki School. This is followed by four thematic chapters and a conclusion.

A chapter titled "The Escape from Psychology" treats the evacuation from economics of all traces of human psychology, with *homo economicus* ceding to a perfectly logical agent, and the exploration of the "how and why of equilibrium" giving way to a "consistency view" of economic rationality. "The Escape from Perfect Foresight" deals with the interwar attempts by Hayek, Morgenstern, Lindahl, and Hicks to create a theory that could both dispense with the assumption of the omniscience of agents and account for the emergence of equilibrium in time. The chapter on von Neumann and Morgenstern's game theory examines the former's fundamental minimax theorem paper of 1928 and four chapters of their 1944 book, *The Theory of Games and Economic Behavior*. The Nash Equilibrium and its various interpretations are treated in the following chapter, while the concluding one explores the fortunes of game theory in the postwar period, arguing, inter alia, that the reason why neoclassical economists initially ignored Nash's work was because of its inability to address their lingering *Systems of Forces*-style concerns for the relationship between rationality and equilibrium.

As Giocoli himself mentions at the beginning, his approach is to provide a *rational reconstruction* of the evolution of theory. The result is a history peopled by theorists who periodically arrive at decision nodes and make theoretical choices, which in turn constrain their subsequent choices. Thus, if the book purports to be a history, the
decision tree, nonetheless, lurks close beneath the text. In keeping with this, Giocoli’s exclusive focus on published theoretical contributions ensures that the economists of his history themselves remain rather one-dimensional creatures, lacking any of that psychic or human depth with which the book is otherwise so concerned: we never really get to know any of the characters involved. Also, the assiduous focus on economic theory, mathematics, and the philosophy of mathematics is achieved at the expense of acknowledging the tumultuous social context in which some of these theoretical developments took place. Finally, there is a suspicious tidiness to this history: theoretical commitments in economics are easily explained in terms of commitments elsewhere and theorists are conveniently grouped under rubrics such as the “escape from psychology” or the “escape from perfect foresight.” At times, given the sheer variety and interest of the theoretical contributions under discussion, one cannot help finding the SOF/SOR framework somewhat constraining.

These, however, are minor historiographical quibbles, for one does not have to adopt Giocoli’s interpretative framework in order to appreciate his thoughtful, measured, and detailed discussion of the contributions of a gamut of authors, from Max Weber to John Nash. This book is a rich and stimulating read, the product of careful labor, and it can be wholeheartedly recommended to readers of this journal, some of whom may wish to recommend it as background historical reading at the advanced undergraduate and graduate level.

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JEL 2004–0028

So what do economists retort when, after announcing their activity to a questioner, the reply comes back “Ah! The dismal science.” Do they mumble something about the stupidity of such a view? Or do they contend that Carlyle, the originator of the phrase, was not an economist? Why not approach the problem from an economist’s perspective and ask what was the context for Carlyle’s condemnation of our noble profession? Read this impressive compendium of papers, incorporating thirty-nine chapters written by forty-six historians of economic thought, and you will be ready to counter the man-in-the-street any time he mentions the “dismal science.”

The book is divided into two sections with part 1 incorporating historical surveys and part 2 dealing with historiography. The two sections are linked through Jeff Biddle’s introduction where he poses the question as to what historians of economic thought do? Are they primarily exegetical interpreters of the great person’s work? Or perhaps they are taxonomists sifting and classifying ideas under various schools and then drawing up the great battle lines that separate and have separated these schools? Or are they absolutists in pursuit of the holy grail of truth linking together the ideas of past economists to provide a continuous upward movement toward the ultimate truth of what economics is meant to mean? Or are they holistic interpreters of human activity believing that lives and ideas are interlinked while George Stigler snorts in his grave that science is separate from life?

The book starts with Todd Lowry’s beautifully condensed survey of the contributions of ancient and medieval economics. To this Hamid S. Hosseini provides a timely reminder, during a period when Islamophobia is gaining ground, of the contributions of medieval Muslim scholars to the history of economics. Hosseini attempts to debunk Schumpeter’s Great Gap view that nothing of any relevance was written about economics between the demise of Greek civilization and the writings of Thomas Aquinas. As the reader works up through Magnusson on Mercantilism, Brewer on pre-Classical economics, Steiner on Physiocracy, Skinner on Smith, O’Brien on Classical economics, he comes to Peart and Levy’s paper on “Post-Ricardian British Economics 1830–1870.”

These authors, developing their own work and that of Persky (Journal of Economic Literature 1990), show that Carlyle used the term “Dismal Science” when attacking economists for belonging to the antislave coalition. Carlyle attacked economists such as John Stuart Mill “because of a view of human nature that abstracted away from the possibility of racial difference” (p. 134). This difference of opinion between Mill and Carlyle on the issue of racial equality came to the
fore during the Governor Eyre Controversy in Jamaica. The Jamaica Committee, headed by Mill and including John Bright, Henry Fawcett, J. E. Cairnes, Thorold Rogers, and Herbert Spencer wanted an investigation of Eyre’s running of the Colony when over four hundred Jamaicans had been massacred and many others tortured and left homeless in 1865. The literati, led by Carlyle and Ruskin, and also including Dickens and Tennyson, sided with Eyre and opposed the Jamaica Committee. From Peart and Levy’s account the term “dismal” may be regarded as a badge of honor by economists as it was equated with the idea of racial equality. Martin Luther King or Nelson Mandela would have approved of being called “dismal” in this context.

The “dismal economics” story is just one small part of a very rich body of knowledge in this book. Mark Blaug, a man whom Lodewijks reminds us has undergone many “changes of mind” (p. 658), indicates the extent to which economics underwent a formalist revolution in the 1950s during which end-state equilibrium theorizing took over the high ground and left “process analysis relegated entirely to unorthodox Austrian economics or equally unorthodox evolutionary economics” (p. 396). He accuses many of today’s economists as being the children of these formalists producing a “community of mathematicians” where “cleverness, not wisdom or a concern with actual economic problems, now came to be increasingly rewarded in departments of economics around the world” (p. 408). Is economics being set up for a future accusation of “dismal”? Or is it the case that the history of economic thought is becoming a refuge for a combination of writers that are fleeing from (a) the abstractions and technical demands of modern economics and/or (b) the strong ideological bias of free market economics that dominates current thinking? Lodewijks in “Research in the History of Economic Thought as a Vehicle for the Defense and Criticism of Orthodox Economics” provides a helpful paper to assist historians of economic thought through these issues.

I have one regret with respect to this book. José Luis Cardoso dedicates his chapter “The International Diffusion of Economic Thought” to the late Ernest Lluch, a Spanish historian of economic thought, who was brutally murdered by ETA terrorists in November 2000. Lluch was both an inspiring academic and politician. Given the importance that the editors attach to the link between economic theory and policy it would have been most appropriate to dedicate this book, rather than a chapter, to a man who, as a minister in the Spanish government, spent part of his life linking economic theory and policy. Lluch will serve as a role model not only for future generations of historians of economic thought but also for young people who believe economic ideas may be harnessed for the benefit of society thereby dispelling the “dismal” association with the subject.

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D Microeconomics


Amadae has written a fascinating, detailed, and challenging book that goes well beyond what the title intimates and provides the reader with a history of rational choice theory in the context of the Cold War and the ideological struggle between philosophies rooted in individual freedom and those embedded in Big Brother and the Philosopher King. A central premise of this book, laid out in the Introduction and the Prologue, is that rational choice theory (RCT) and its close relation, rational choice liberalism, played a penultimate role in the defeat by free market democracy of Communism and its statist, authoritarian, organic, and group ideological analogues. Central to this war was the development in America of a mathematized “neoclassical” economics as reflected in public choice theory which provided necessary theoretical weapons to do battle in support of individual rights and market economies. The oeuvres of Hayek, Schumpeter, and Popper, for example, could not suffice in the ensuing struggle (little mention is made of Isaiah Berlin and other key European intellectual cold warriors).

One might quarrel with particular theses put forth by the author or even with some critical omissions. There is little discussion of Keynes
and the Keynesian Revolution as being possibly critical to the victory of free market democracies. Nor is there consideration of multiple types of successful free market democracies or the existence and stability of authoritarian organizational forms, or how the rise of "new" ideologies (postmodemism, relativism, behavioral irrationality) might give legitimacy to such authoritarian alternatives. And one can question the centrality of math as the necessary condition for the rise of scientific economics and rational choice theory. This book is nevertheless highly recommended to those interested in the development of contemporary economic theory and its relationship to socioeconomic and political change.

Amadae makes the case in chapter 1 that the Rand Corporation, which developed after World War II into a formidable interdisciplinary think tank, as well as the Ford Foundation, played pivotal roles in the development and spread of rational choice theory and rational choice liberalism. It was at Rand that Arrow's Social Choice and Individual Values, as well as William Riker's positive political theory and many of James Buchanan's formative ideas on public choice theory were developed. Arrow's contributions are viewed as foundational to the development of RCT and to the philosophical defense of free society. Key Rand and Ford intellectuals and movers and shakers (such as Robert McNamara) also played critical roles in reshaping American bureaucratic organization and practice (decision theory), and public policy in the 1960s. However, one can argue that their view was that the technocracy should rule scientifically, independently of politics, stands in stark contradiction to the basic premise of freedom and democracy and is quite consistent with authoritarian worldviews.

In chapter 2, Amadae presents a fascinating discussion of the details and evolution of Arrow's 'impossibility theorem,' the sovereignty and sanctity of individual preferences, rationality as consistency, the impossibility of collective choice absent coercion, and the implications of this for liberal democratic free market praxis. Amadae argues that Arrow's worldview undermines the theoretical underpinning for authoritarian ideology, but also serves to weaken the ideological fabric for any form of collective action.

In chapter 3, James Buchanan and Gordon Tullock's critique of benevolent governance by rational agents, which builds on Arrow's work, is well articulated. Although they constructed a theoretical foundation for minimalist government and status quo politics wherein the constitution must be derived from the preferences of rational self-interested individuals, Buchanan and Tullock's contributions and their creation of the interdisciplinary Public Choice Society opened the door to a multilayered discourse wherein the work of John Rawls was developed and discussed. Rawls' rational individual based constitution was open to more government within the framework of free market democracy as was indeed Arrow's theoretical frame. William Riker's view of a political science based on rational choice, building on Arrow, is discussed in chapter 4. Democracy serves here only to depose unwanted leaders. Mancur Olsen's modeling of problems surrounding collective action, absent coercion, derived from self-interested individuals, is presented in chapter 5.

RTC (especially Buchanan's perspective) and Adam Smith's views on a just society are contrasted in chapter 6. Amadae argues that critical differences exists between the two world views, given that Smith relies on both the self-interested individual and the same individual imbued with the moral sentiments of the "impartial spectator" whereas in RCT we are left with the unrestrained self-interested individual yielding maximum social welfare.

In chapter 7, marginalist economics with RCT are contrasted with regards to definitions of rationality and maximization. Amadae argues that the latter focuses largely on consistency with no attention to notions of scarcity, efficiency, or instrumentality. In chapter 8, RCT is discussed in the context of the contributions of John Rawls' rights based theory of governance, wherein RCT-based arguments for government intervention are raised. In this vein, the critical contributions of John Harsanyi, Amartya Sen, Russell Hardin, David Gauthier, and Ken Binmore are brought to the fore. In the Prologue, Amadae critically summarizes his views on RCT in relation to the marginalists and to the liberal democracy project.

This well-researched and thought provoking text, in declaring the victory of liberty and freedom in politics and the market, pays little heed to the variety of policy inferences drawn by different proponents of RCT as well as to the existence, persistence, and rise of authoritarian ideology and practice as we begin the new millennium. It
remains to be seen how the various faces of RCT and the interplay between freedom and markets will respond to this challenge.

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For many economists, hyperbolic discounting is the most familiar example of time-inconsistent preferences. This book seeks to expand our knowledge of the topic by examining other forms of time-inconsistent discounting, by discussing how individuals use self-control to overcome these inconsistencies, and by applying these theories to health behavior and economics.

This book contains a collection of papers written by psychologists, behavioral economists, and other researchers in decision science. Originally prepared for a conference on intertemporal choice, most of these papers discuss recent developments in the study of time-inconsistent preferences.

Four sections follow the preliminary chapter of the book, which presents the standard economic model of intertemporal preferences (additive utility, exponentially discounted) and describes its logical shortcomings and empirical rejections. The first section addresses the underpinnings of intertemporal choice. One essay examines the philosophical aspects of discounting (how can we justify attaching differential weights to rewards at different times?); another analyzes the evolutionary advantages of different patterns of behavior; a third describes the neurobiology affecting intertemporal choice. For an economist, these essays primarily motivate the remainder of the book: they illustrate that the standard economic models, based on time-consistent preferences, may be difficult to justify philosophically or biologically.

The second section presents theories underlying specific self-control behavior in situations where individuals have conflicted preferences over a choice. One study discusses how the enjoyment received from delayed gratification may change over time (as one becomes weary of a diet, for instance); another presents "self-control" as a limited resource that individuals apply to the most critical situations; a third examines how individuals shy away from circumstances that would create conflicted preferences. The final chapter in this section examines how individuals force themselves to engage in activities (such as avoiding alcohol before noon) that signal "desirable" preferences. Section 3 presents more general models of behavior. The first paper argues that most empirical tests of hyperbolic discounting indicate "subadditive" preferences instead: that discounting over a long span may be less than the sum of discounting over its subintervals. Another paper in this section analyzes how actions shape individuals' future preferences, and how this in turn affects their current behavior.

The final section contains applications. Most of these relate to health: a call for action on studying how time-inconsistent preferences affect preventative health behavior, a study of the relationship between discounting and drug dependency, a paper displaying how "fear" can be an effective policy instrument to offset time-inconsistencies, and a discussion of behavioral anomalies of dieters.

The final two papers are the most relevant for economists. A chapter on inventories and self-rationing is particularly noteworthy. Its author argues that, because of impulsive behavior and self-control problems, individuals' current stocks affect their current consumption (in contrast to the predictions of the standard life-cycle model); furthermore, marketing seeks to exacerbate impulsive behavior. This model suggests that consumption should track income somewhat; furthermore, it suggests that the availability of credit boosts spending even when liquidity constraints are not binding (the author argues that this is why furniture stores advertise easy financing). The author discusses how consumers respond by self-rationing (e.g., destroying extra credit cards to eliminate the temptation to spend).

The last paper simulates the life-cycle consumption and savings patterns of hyperbolic and exponential discounters. The authors compare these to observed profiles; they argue that hyperbolic discounting can explain why households hold relatively little liquid wealth and relatively high debts. As a consequence of these obligations, consumers are unable to smooth consumption fully. Hyperbolic discounting can also be
used to explain phenomena such as the drop in consumption around retirement.

Overall, these essays present compelling arguments that individuals may have conflicted preferences. They present supporting evidence, mainly from experiments designed to identify the hypothesized phenomena. However, aside from the last two chapters, little of the book attempts to relate the behavior to economics. One ends up wondering whether these theories predict substantively different behavior from standard economic models when used to examine retirement behavior or labor supply or education, for instance. There is little discussion of such topics; there is less evidence to show that standard models are inadequate. At the same time, these potential shortcomings make the book an ideal starting point for economists who wish to apply the theories to consumer behavior, health economics, and intertemporal choice. Questions that remain to be answered include: How does one make welfare inferences when individuals' actions conflict with their preferences? Do these theories better describe observed nonexperimental behavior, and how poor an approximation are the standard models? How can we use these theories to boost our understanding of economic behavior?

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E Macroeconomics and Monetary Economics

Evolution and Procedures in Central Banking.

JEL 2004–0091

This volume collects seven papers and fourteen comments delivered at a May 2001 conference on “The Origins and Evolution of Central Banking,” the first-ever conference to be sponsored by the Central Bank Institute of the Federal Reserve Bank of Cleveland. The work stands as a fitting memorial to its coeditor, Bruce D. Smith, the prolific monetary theorist who passed away in 2002.

Papers are grouped into three sections: “Operational Issues in Modern Central Banking” (authors Jasmina Arifovic and Thomas J. Sargent; Charles Goodhart); “Monetary Union” (Jürgen von Hagen and Matthias Brückner; Alberto Trejos); and—mirabile dictu for a conference sponsored by a central bank—“Private Alternatives to Central Banks” (Gary Gorton and Lixin Huang; Arthur J. Rolnick, Bruce D. Smith, and Warren E. Weber; Randall S. Kroszner).

In choosing discussants, the editors might have done more to avoid cliquishness. Several discussants are former coauthors or long-time former colleagues of their assigned authors. A very heavy share of the authors and discussants are presently or formerly associated with a single Federal Reserve Bank. With respect to monetary theory, Cleveland is apparently a suburb of Minneapolis.

But, in its concern for understanding concrete historical episodes and institutions, this volume contrasts markedly with the Minneapolis Fed’s 1980 volume Models of Monetary Economies.

The four most interesting papers question in some way the rationale for central banking. Gorton and Huang observe that, while the Federal Reserve Act was motivated by runs and panics in the nineteenth century U.S. banking system, the panic-free experience of other countries like Canada shows that “these problems do not arise in banking systems with a small number of well-diversified banks.” They survey the historical experience and then present a model in which “banks are not inherently unstable institutions prone to panics.” (As discussant Edward J. Green points out, their model contrasts with “bubble” or “sunspot” models that picture banks as inherently unstable. Such models cannot explain the historical cross-country differences in panic-proneness.) They extend the model to show that, given a panic-prone industry of under-diversified unit banks (as created by branching restrictions in the nineteenth-century United States), a bank panic can create negative externalities by disrupting the payments system. These externalities rationalize in principle a (second-best) role for government. Interpreting Gorton–Huang as making a second-best argument resolves the puzzle, noted by discussant John H. Boyd, that the extension’s rationale for intervention reads on its face “like a critique of the preceding sections” that supported the optimality of laissez-faire.

Rolnick, Smith, and Weber contrast the practices of the Suffolk System (1826–58), a private note-clearing arrangement, with those of the
federally chartered Second Bank of the United States (1816–36) with respect to their success at achieving a uniform currency (par circulation for banknotes from all issuers). They conclude, as persuasively as the incomplete evidence allows, that the Suffolk System “came much closer.” It would be interesting to investigate whether other countries likewise achieved par circulation through private clearing arrangements. A bonus of the paper is a fiscal explanation for why state governments took a notoriously lax attitude toward enforcing the banks’ contractual obligations to redeem notes at par on demand: many state governments either owned banks themselves or heavily taxed their profits. In a couple of places, the paper says that some banknotes “circulated at a discount,” but this phrase should probably not be taken too literally. Given the inconvenience, it seems implausible that notes continually circulated (passed repeatedly from hand to hand) below par, in areas where speculative or local at-par notes were available. Rather, out-of-town notes traded at a discount when swapped for at-par notes or specie at specialist note-dealers (who thereby removed them from circulation). This interpretation is consistent with the evidence we do have, series of quoted prices from such dealers.

Trejos argues that “given current trends toward increased openness, small economies will find it undesirable and even unfeasible in the long run to maintain a central bank and an independent currency.” The paper surprisingly neglects the existing literature on dollarization, but discussant Klaus Schmidt-Hebbel partially fills the gap. Schmidt-Hebbel offers an interesting diagram capsulizing different views on welfare properties of various exchange rate systems, but unfortunately the labels on three of his diagram’s five curves do not match his text.

Kroszner provocatively proposes that central banks have inflated less since 1980 because improvements in transaction technology have subjected them to stronger competitive discipline. Discussant Jeremy C. Stein correctly cautions, however, that erosion of a central bank’s market share can lead it to inflate more if it tries to make up for a smaller seigniorage tax base with a higher tax rate. More evidence is needed to confirm Kroszner’s explanation. Kroszner considers future scenarios for currency competition suggested by the “free banking” literature.

Three other papers take central banking for granted and consider issues related to monetary policy regimes. Arifovic and Sargent report on interesting laboratory experiments in which participants were placed in a repeated Kydland–Prescott-type monetary policy game. The time-inconsistency problem reared its head in the lab, but not as severely as in the one-shot KP model. Goodhart muses on the future of monetary policy. Von Hagen and Brückner describe the first two years of the European Central Bank’s making of monetary policy.

One hopes that any future conference volumes from the Central Bank Institute meet the high standard for relevance established here.

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The subtitle to this book is “Coping with Aging Societies, Climate Change, and Other Long-Term Fiscal Challenges.” In fact, the book isn’t about specific policies to cope with climate change or demographic shifts. Rather it is, first, an analysis of why governments should look far ahead in planning spending and tax policies and, second, how the framework within which fiscal policy is set should be adapted so as to avoid unsustainable paths.

The first half of the book is in many ways the more interesting and controversial. Heller begins by spelling out some of the major factors that will shape the environment in which fiscal policy is set in the future. Early on a rather daunting list of factors is given:

“... the ageing of populations, global climate change and other environmental pressures, globalization and the greater interconnectness it brings, the growing disparities between rich and poor nations, the explosive growth of massive urban centers, continued rapid technological change, and potential resource scarcities. The prospect of disruptive events that are even more difficult to foresee—like those of September 11, 2001—adds a further layer of uncertainty.”
Heller's position is that it is crucial for policymakers to begin to grapple with the challenge of insuring long-term fiscal sustainability in the face of these highly uncertain developments. But he notes that there are at least two other possible responses, quite contrary in their implications. One argument would be that living standards one hundred years from now will be so much higher than they are today that the fiscal consequences of the range of factors listed above can be reasonably handled. A quite different line of reasoning is that the degree of uncertainty about these factors is so enormous that planning fiscal policy today, in the light of where debt levels might be 50, 60, or 100 years from now, simply doesn't make much sense. Heller takes issue with both of these arguments, and along the way makes some good points. He argues, surely correctly, that amid the very many uncertainties about the future, some things are known with sufficient certainty to raise very clear doubts right now about the capacity of governments to pay for explicit and implied commitments they have already made to their citizens. And numerous calculations on calibrated, overlapping generations models show that these are not problems that can simply be passed on to some unknown wealthier future generation; doing so would almost certainly impose dramatically larger burdens on generations of future tax payers and the damage this might do is potentially large and the ability of future generations to tolerate such a burden uncertain.

As regards ageing, the argument that governments should pay attention to the likely long-run implications of current commitments on pensions and health is compelling. The argument as applied to the impact of climate change is, however, less so because the uncertainties are so much greater. Recent work by Nordhaus (1998, "New Estimates of the Economic Impacts of Climate Change," mimeo, Yale University) suggests that the costs of putting in place new policies to reduce emissions of greenhouse gases by enough to have any substantial effect in the future are very large and the value of any effect in the future is so uncertain that a policy of "wait and see" (while trying to reduce uncertainty by devising means of seeing better) is optimal.

On balance, Heller makes a pretty convincing case in the first part of the book that in setting fiscal policy governments should look far further ahead than is typical, where projections three to five years out are often thought adequate. Heller then moves on to consider how the way in which fiscal policy is formulated can better reflect those long-term factors. There is a pretty long and diverse list of recommendations that emerges in the second part of the book. Some of these are hard to disagree with—more information on the long-term implications of policy, greater transparency, and greater disclosure of accrual of liabilities all seem unambiguous goods. It is also hard to disagree that governments should use stochastic simulations to assess how policy might need to evolve under different scenarios.

It is rather less clear what to make of the recommendation that peer pressure from multinational regional institutions is helpful (as is implied on page 225). Indeed it is very doubtful whether "the surveillance mechanisms within the European Union provide a useful model." First, under the stability and growth pact the governments of the largest economies in Europe have, effectively, ignored its rules on deficits when it favored their doing so. More fundamentally, because the way in which deficits are measured does not take into account longer term factors (for example making no distinction between deficits to finance current as opposed to capital expenditures) it really is a rather poor tool for these purposes.

In many ways, Heller reaches somewhat gloomy conclusions about the ability of governments in developed economies to handle some of the more predictable problems emerging from demographic change. He is skeptical, for example, about whether there is much scope for significant cutbacks in other areas of spending to make room for the anticipated growth of pre-committed expenditures on pensions and healthcare. His conclusion is that reform needs to be made at the level of individual programs, rather than in the aggregate budget.

In his conclusions, Heller once again stresses that certain events are highly likely and, while there is great uncertainty about many factors, the fact that some things are highly predictable means that taking account of them now is sensible. This is a reasonable argument. But when he comes to list those things that "will occur with a high degree of probability" (p. 221) one is not entirely convinced. The list is: shifts in the age structure of populations, changes in climatic conditions, continued
globalization, and ongoing dramatic technological progress. Added to the list we have: "The prospect of increased geopolitical tensions is also very real" (p. 221). I left this book doubtful that even a majority of this list of projections would occur with high probability.

David Miles
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F International Economics


The costs and benefits of free trade took on great political significance in the recent election year environment. Politicians who deal with trade issues have much to learn from economists, and indeed from this book, which provides solid empirical evidence in support of two key principles that should inform policy debate. Principle #1 is that trade is good absolutely. By that economists mean that trading between countries in exploitation of comparative advantage increases efficiency, defined as resulting globally in the same goods and services being produced for less cost, or more goods and services being produced for the same cost. Principle #2, and the real focus of this book, is that the consequences of international trade are uneven and because of this trade produces "winners" and "losers" even where the economy as a whole may benefit. Pro-free trade groups tend to highlight the benefits of trade owing to Principle #1, whereas anti-free trade advocates focus on broad based differences in the consequences of trade for particular sectors or groups of workers. This book is clear that economywide positive net benefits do not preclude localized negative net benefits. Its emphasis is on the distributional costs of free trade.

This book has three main parts—a review of the theory, a review of the empirical literature, and the author's empirical analysis for manufacturing industries. The theory review is a necessary synopsis of the international trade theory literature from "Comparative Advantage" to the "New Trade Theory." It is a well integrated review for the informed economist and an easy to follow primer for the less well informed. The empirical review offers evidence from earlier studies on globalization and its connection with recent U.S. manufacturing woes. It makes clear that the literature (exclusive of this study) has focused primarily on the impact of trade on wages and only secondarily on the impact of trade on employment. Job loss (as distinct from employment) has been virtually overlooked. A consensus emerges that international trade has had minimal effects on the level of manufacturing employment in the United States over the 1980s and early 1990s, but that it has profoundly impacted the distribution of employment across sectors. The third part of the book—and the book's core—is the author's empirical evaluation of the distributional impact of international trade on employment and job loss in manufacturing from 1979–94. The relative costs and benefits of free trade for particular manufacturing industries are evaluated here.

Using descriptive data and empirical analysis, the book offers the reader several important facts. The descriptive data reveal that sharply declining exports were strongly associated with employment decline, particularly in industries that accounted for the bulk of manufacturing sector employment loss over this period. Although rising imports are also strongly associated with employment decline, this is typically the case in smaller industries that are heavily import-competing. The impact of exports and imports for employment and job loss is not exactly balanced; export-declines exert a more potent effect. Finally, "unbalanced" manufacturing industries, characterized as heavily importing or heavily exporting with little intraindustry trade, were significantly more vulnerable to employment losses over this period.

The empirical evidence offered adds to our understanding of the distributional impact of trade on workers by examining employment and sectoral job loss. The book establishes that increasing exports (and domestic demand) enhanced employment to a considerably stronger degree than increased imports reduced employment. Within an industry on a year-to-year basis, rising exports were more strongly associated with employment growth than were increases in domestic demand (although the parameter estimates on exports are overstated given the partial
equilibrium structure of the model). Within-sector estimates reveal that job loss is more closely linked to the loss of exports than either the loss of domestic demand or rising imports (which is statistically insignificant in any case).

The United States lost 13.4 percent of its manufacturing jobs over the 1979–94 period studied in this book. What would employment trends have been if trade flows had remained constant over the 1979–94 period? According to the estimates presented in this book, manufacturing employment would have declined by even more—16.2 percent—in the absence of changes in the terms of trade. This is because the positive effects of growing exports after 1985 was a stronger net job creator than the negative effects of rising imports. Among the book’s key conclusions is that trade has had a small albeit net positive job creation influence over this period in manufacturing, to the tune of an estimated 7,626 new jobs.

But of course domestic employment and job loss outcomes have not been uniform across industries. For some industries, especially those with high import-share content, the link between rising imports and employment and job loss has been profound. Since these industries may be relatively high paying and heavily unionized (steel and other metal-related industries for example), the consequences of displacement for workers are more severe and long term. Although the book advocates in favor of domestic adjustment assistance programs such as retraining and partial compensation to protect displaced workers, it does not spell out a clear strategy or a set of programs that might actually work. It does not clear on the costs of such a program. Since the outsourcing of jobs is a critical policy issue today, this is a miss. The book makes a convincing argument in favor of domestic adjustment assistance programs such as retraining and partial compensation to protect displaced workers, it does not spell out a clear strategy or a set of programs that might actually work. It is also not clear on the costs of such a program. Since the outsourcing of jobs is a critical policy issue today, this is a miss. The book makes a convincing argument in favor of multilateral reductions in trade barriers and export promotion and backs it up with solid analysis. Few economists would argue that this is sound policy because the benefits outweigh the costs. But what’s left for good governance is to figure out a better way to compensate the losers and move the policy forward.

Linda A. Bell
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The main purpose of this book is to examine the many issues that countries need to consider as they contemplate a move to (de jure) dollarization. No definite conclusions are drawn with regard to the long standing debate on fixed versus flexible exchange rates. The latter, as the editors correctly observe, can never be completely resolved, owing to the inherent complexity of the issues involved as well as the many country-specific factors that have to be considered.

Although the objective of the book may be limited, the papers that comprise this volume nevertheless cover a broad range of issues. They were written as part of a study commissioned by the Central Bank of Argentina at a time when Argentina and several other Latin American countries were actively considering dollarization. While some of the momentum and early enthusiasm for this exchange rate alternative has dissipated, the analyses that are presented provide many useful insights and have more general applicability.

The main message that emerges is that dollarization, like marriage, should not be regarded as a panacea nor entered into lightly. The practical problems that must be confronted, coupled with the uncertain consequences of such a decision, recommend against any hasty commitments. Although some proponents of dollarization still favor a “just do it” approach, in which countries dollarize in the hopes that optimum currency area considerations and institutional details will take care of themselves, this seems to be a minority opinion.

The book begins with a “Primer on Dollarization,” by Levy Yeyati and Sturzenegger, which provides a summary of the seven other papers in the volume and a review of the major theoretical arguments for and against dollarization. Traditional Mundellian approaches to the choice of exchange rate regime are contrasted with the more recent literature, which places greater emphasis on credibility concerns and financial vulnerability as important drivers of dollarization. This capstone paper is followed by a similar piece, by Chang and Velasco, who use a simple stylized model to highlight some of the ambiguous welfare implications of dollarization. Like many of the papers that follow, it is, on balance, not very supportive of the dollarization option.
Panizza, Stein, and Talvi take a more grounded approach in their paper and attempt to measure the net benefits of dollarization from the perspective of Central American and Caribbean countries. While the results suggest that many of the countries in their sample would be better off under de jure dollarization, these should be interpreted with considerable caution owing to the strong assumptions that the authors are forced to make.

Neumeyer and Nicolini focus on a much narrower question (but the one that was probably of greatest interest to those who sponsored this study). They look at balance sheet data for various sectors of the Argentine economy over the 1994–2000 period, and try to determine if it would be preferable (from a fiscal standpoint) for Argentina to officially dollarize and then “explicitly default” on its domestic debt or, alternatively, to float and “implicitly default” on its domestic debt through devaluation and inflation. In the end, the authors conclude that it is impossible to say anything definite, given the difficulty associated with interpreting the balance sheets of Argentine banks.

Devaluation and default risk are also the focus of Powell and Sturzenegger, who explain why interest rate spreads and default risk will not necessarily decline following dollarization. While de jure dollarization would eliminate the risk of devaluation (at least against the chosen currency), it might also result in more rigid prices and wages, reduce government revenues through lost seignorage, and restrict the government’s room for manoeuvre in the event of a financial crisis, owing to the absence of a lender-of-last-resort (LLR) facility.

LLR considerations are highlighted in many of the papers included in this volume. However, Broda and Levy Yeyati give them special attention and highlight some of the perverse effects that LLR might have on risk-taking behavior and the vulnerability of the banking sector under partial dollarization. Full dollarization, they note, reduces the probability of exchange rate induced banking crises, but leaves the country exposed to other, more traditional, financial sector shocks.

The last two papers in the volume concentrate on a separate set of issues, concerning the transition to dollarization. Gruben, Wynne, and Zarazaga provide a practical step-by-step guide for would-be dollarizers, describing how countries should dollarize as opposed to if they should dollarize. While many of their recommendations make sense, the most intriguing aspects of this piece are the well reasoned, yet controversial, views that they offer on seignorage, LLR, and adjusting the terms of financial contracts. Although their heretical ideas are unlikely to be shared by many other authors, they nevertheless provide interesting food for thought (and are consistent with my own free market biases).

The final paper, by Frieden, serves as a useful, if somewhat ironic, conclusion to everything that has preceded it. He examines the political economy of dollarization and observes that much of the previous material in the volume is irrelevant. Drawing on his earlier research as a political scientist, he notes that the decision to move to a new currency regime is typically determined by two or three common factors. When he applies his predictive model to European and Latin American data, he finds general support for his theories—with Argentina as a notable exception.

Readers looking for definitive answers to the dollarization question or strong support for such a move will be disappointed with this volume. For those interested in thoughtful analysis and insightful investigation of a range of currency issues, the experience will be more rewarding. Every paper in the volume is worth reading and provides a useful perspective. The main message that emerges is one of caution. Do not race into dollarization; the results are uncertain and the transition is likely to be painful. It is impossible to know what influence this study might have had on some of the policy decisions which have been made over the last few years. In the event, however, Argentina, the country which originally sponsored the study, has recently opted for a flexible exchange rate and a monetary policy framework based on inflation targeting—a course correction for which I have considerable sympathy.

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G Financial Economics


*JEL 2003–0130*
This short book is a research allegory. Two researchers pursue a grand idea. Alas, it proves to be intractable, or at least inscrutable. Properly chastened, the researchers refocus their efforts on a less ambitious objective and emerge with some interesting and provocative results.

Sanjai Bhagat and Richard Jefferis investigate whether a firm’s takeover defenses affect its independence and managerial turnover. Conventional wisdom holds that takeover defenses tend to entrench managers. If so, then firms with defenses will tend to remain independent and their managers will keep their jobs even in the face of poor performance.

The authors’ grand idea is that takeover defenses, takeover activity, and managerial turnover all are determined endogenously with the firm’s governance, performance, and ownership. Previous research rarely acknowledges the simultaneous nature of these phenomena. As a result, empirical estimates of say, the effect of takeover defenses on takeover activity, are biased. A solution to this problem would be to estimate a structural model that treats takeover defenses, takeover activity, and managerial turnover all as endogenous. The authors even write down such a model on page 35. But identifying the model is problematic. For example, what exclusion restrictions make sense? Are there any exogenous factors that affect takeover activity but not firm performance? “...[W]e estimated different specifications of the system... based on exclusion restrictions and distributional assumptions,” the authors report. But, “We found the results of this exercise to be uninformative” (p. 38). I think this means that the results are insignificant, counterintuitive, or highly sensitive to model specification.

So the authors redirect their efforts. Rather than tackling the endogeneity issue head-on, they conduct a series of tests in which controls for firm performance play a central role. The first tests examine firm performance using analysis of variance models. Two measures of firm performance—market-adjusted stock returns and operating cash flow—are higher for firms that remain independent during a subsequent two-year period than for firms that subsequently are acquired. Performance also is relatively high for firms with no change in management during the subsequent two years. This implies that poorly performing firms are strong candidates to be acquired or change their managers.

No surprise there, given prior research. But the data also indicate that prior performance is particularly strong among firms that remain independent (or do not change managers) and have poison pills. This is a head-scratcher, because if strong performance is a defense against takeover, why do firms with very strong performance require their poison pills? Do the pills contribute to the better performance? Did firms that were acquired have pills? If so, what was their performance? Bhagat and Jefferis do not discuss these questions. Instead, they conclude simply that the data support their initial claim, namely, that performance, defenses, takeover activity, and managerial turnover all appear to be related. An acerbic reader would note that we already knew that.

The authors then estimate probit models in which firm acquisition and managerial turnover are the (transformed binary) dependent variables. In an unconditional test, firms without takeover defenses are more likely to be acquired. Again this is consistent with prior research. But Bhagat and Jefferis’s most striking result is that acquisition likelihood is negatively related to prior performance only among firms that have takeover defenses, particularly poison pills. The authors infer from this that performance swamps any effect of takeover defenses on the probability of being acquired. The correct interpretation, however, is that poorly performing firms are more likely to be acquired if they have takeover defenses than if they do not.

This is a surprising result because it runs counter to conventional wisdom. The conventional view is that takeover defenses attenuate the link between firm performance and the disciplining effects of the market for corporate control. But Bhagat and Jefferis’ data imply that managers increase their exposure to market discipline when they deploy takeover defenses!

The authors do not make this inference, perhaps because they are aware of the endogeneity problem highlighted earlier in the book. For example, managers might adopt takeover defenses precisely because they anticipate poor firm performance and future takeover bids. This would explain the result that the performance–takeover linkage is particularly strong among firms with takeover defenses.
Still, having opened the book with a promise to investigate the effect of defenses on acquisition probability, the absence of any investigation of this issue is a disappointing omission.

The authors may avoid tackling some of these more challenging questions because of data limitations. The sample is creaky with age, consisting of 196 firms that had takeover defenses in 1984 and 1985, plus 148 control firms without such defenses. Acquisitions and management changes among these firms are tracked for only a subsequent two-year period. Given the much larger samples and longer sample periods typical of current research in the area, few researchers will find inferences from this sample to be definitive.

There also are problems in the definition of takeover defenses. State takeover laws are ignored, even though other researchers have found them to substitute for firm-level defenses. The authors argue that they can ignore state antitakeover laws because the 1984–87 sample period precedes their advent. This is simply untrue. From 1982 through 1987, twenty-eight different states adopted approximately forty different antitakeover laws.

Also important is how firm-level defenses are defined. The authors include fair price provisions and classified boards in their analysis. Poison pills are treated separately. These are important provisions, but the analysis ignores such other defenses as supermajority vote requirements, restrictions on shareholders’ right to written consent, restrictions on shareholders’ abilities to call special meetings, and charter provisions that direct managers to consider the interest of nonshareholder stakeholders in directing corporate resources.

Despite these empirical criticisms, Bhagat and Jefferis’s book provides an excellent summary of the deliberations of two accomplished researchers who have thought long and hard about corporate governance issues. Not only are corporate governance-related issues endogenous, but they also are difficult to unravel. The tests reported in this book underscore just how complex some of these relationships are.

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JEL 2003–1397

Guy Stuart analyzes the mortgage industry with an eye toward developing a new institutional structure that “allows public and informed debated about ‘who gets what’ in terms of access to capital.” The value he most cherishes in developing this structure is the “ability of people of all races, ethnicities, and incomes to live in any neighborhood, subdivision, or suburb they choose” (p. 206).

To that end, he describes the lending process in the modern era with discussion of its historical antecedents. He shows that mortgage lending is a complex process, but the central issue is the predicted ability of the borrower to repay the loan. The industry believes that the ability to repay is a function of the borrowers’ income, the price of the house, measures of the person’s creditworthiness, and the relative quality of housing in the neighborhood where the house is located. Mortgage lending in the early 1900s relied more heavily on personal knowledge by lenders who could assess the “character” of the borrower. The development of FHA and VA mortgage insurance and secondary loan markets through Fannie Mae led to standardization of loans and the development of rule-based decision criteria, such as credit scores, that allowed such loans to become commodities that could be bought in secondary markets. The relative importance of these factors in determining who obtains a loan has changed over time. Even so, decisions made by real estate agents and loan officers still influence the process.

Since independent appraisals of house value are central to the process, he provides a chapter on the development of the appraisal profession, including how different approaches to value were synthesized by the leading professional group and the long-time emphasis on homogeneity of neighborhoods. He discusses the role of government policy in initially reinforcing segregation and homogeneity of neighborhoods through the 1960s and the attempts to reduce discrimination in lending since. Using data from Chicago, he provides evidence on how neighborhoods are “constructed” by the real estate industry and then provides tables of evidence on differential denial rates for ethnic groups for various types of loans, institutions, and housing markets.
Throughout the book, he is particularly interested in the feedbacks between rules, lending networks, and the spatial distribution of people. In many cases, he suggests that lending network decisions are based on rules that are influenced by the neighborhoods where people are seeking to buy. Borrowers seeking homes in neighborhoods with substantial heterogeneity in value and/or dominated by low income people and minorities tend to face greater problems in obtaining prime mortgage loans. Since absence of lending can lead to deterioration of neighborhoods, the failure to lend can lead to a self-fulfilling prophecy of foreclosures and neighborhood failure. This in turn creates problems for minorities and low-income people to show that they are good credit risks. The final chapter of the book is devoted to presenting policy prescriptions designed to breakdown these feedback effects. The big question is the severity of the problem. Stuart gives evidence that the problems might still persist, but before I put the industry through Stuart’s policy overhaul, I would like to know much more about the magnitude of the problem.

Stuart’s historical discussions and institutional case studies are quite useful. Yet, economists will be disappointed in the analysis of discrimination in chapter 5 and assessments of whether the lending criteria met standards of “business necessity” in government Fair Lending criteria (pp. 193–95). The tables designed to assess discrimination in denial of loans are simple cross-tabulations and the author does not attempt his own multivariate analysis. The author’s analysis is, therefore, much simpler than the complex analysis done by a number of institutions in the real estate industry. A central problem in Stuart’s view is that existing parameters that determine loan quality might be focusing too much on the quality of neighborhoods. He cites one study that shows that credit scores are influenced by economic features of the zip codes in which the person resides. But then we find that the cited study includes no information on the individuals themselves (p. 173), raising real doubts about the conclusions we would draw had controls for individual characteristics been included.

There is no careful attempt to analyze whether the loan procedures actually meet the “business necessity” criteria for fair lending. The author never really defines what he means by “business necessity.” Too often he relies on statements from other studies without giving us a sense of the methods used in those studies.

In the final chapter, he suggests a series of policy changes. As one example, he calls for an expansion in the roles played by Fannie Mae, Freddie Mac, and the FHA in sub-prime lending markets. But there is very little discussion of how the three institutions already play roles in parts of the sub-prime market and of their antidiscrimination policy changes circa 2000. His prescription that Freddie and Fannie insure sub-prime loans raises a whole host of issues about subsidized insurance that are addressed inadequately. I share his goals that people of different ethnic groups and minorities should not be denied loans unfairly. Although his intent “is not simply to act as a bomb-thrower” (p. 206), I was left hungry for more information that would allow me to assess whether his policy bombs will be improvements that solve more problems than they create.

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A recent wave of scholarship has focused on the nexus between financial development and economic growth. Advocates of the “finance-led” view of growth have suggested that institutional differences in banks, securities markets, and monetary arrangements may provide additional insight into why we observe variation in rates of capital accumulation and technological growth across countries. The roots of empirical inquiry into this subject lie in the observed differences in the structure of financial systems across countries, even those with comparable levels of economic development. Why do such differences exist? In Moving Money, Daniel Verdier tackles this question for a set of industrialized countries over the past one and a half centuries. Verdier focuses on how state structure has influenced the evolution of financial systems in roughly a dozen countries—a set that consists of Western Europe and a group of British offshoots. The small sample permits the author to develop a fairly detailed historical account of how their financial systems
evolved and to do so in a comparative fashion. Scholars interested in understanding how national banking systems have changed over time and the forces that induced these changes will find the rich historical detail particularly useful.

In the first part of the book, the author explains how centralization might affect the structure of banking systems and the mobility of capital. Verdier’s main point is that the extent to which a state is centralized determines how financial markets are organized. He argues that, due to agglomeration effects, financial capital, if left unimpeded, has a tendency to flow toward centers of economic activity. (A mathematical model is provided in an appendix.) If governments are decentralized in orientation, then local governments, businesses, and banks will have proportionately more power, and will use this to block or impede the flow of finance from the periphery to financial centers by encouraging regulatory intervention. As a result, he argues that banking structures in decentralized countries will have less market liberalization in their financial sectors, less developed stock markets, and less internationalized banking systems. In contrast, more centralized countries are not as likely to impede the flow of finance to financial centers; this, in turn, will lead to financial systems with deeper financial markets and an international orientation as well as more concentrated and specialized banking systems. He suggests that bank regulation over the last 150 years has been directed at altering the competition for market share between centrally located banks and those on the periphery.

The next two sections use cross-country data from two periods, 1850–1913 and 1960–2000, to test his thesis. For each period, Verdier devotes one chapter to four organizational characteristics of financial systems: geographical concentration, internationalization, intermediation, and product specialization. Each chapter provides a general description of historical factors influencing the characteristics; this is followed by a description of the predictions from his model, scatter plots based on a subsample of present-day OECD countries, and simple OLS regressions. The organization of these chapters makes for relatively easy comparison between the two periods; however, treating each characteristic in relative isolation from the others leads to parsimonious econometric specifications, which fail to consider the possibility of feedback effects.

Although Verdier acknowledges that other factors may determine where capital locates and how financial systems are structured, the book’s single-minded focus on the role of state centralization as an explanatory variable gives other potential influences short shrift. This is not meant to imply, for example, that changes in financial products are not discussed, but rather that technology and economic growth are also important determinants of financial structure (and for altering regulation), and therefore could have been given more prominence, especially in the empirical section. Similarly, the thesis pays too little attention to differential rates of return as an explanation for the location of capital. The author’s conclusions seem most relevant for the European countries in the sample; the British offshoots are often treated as outliers with unique historical circumstances, and sometimes, peculiar definitions of these countries are employed so that statistical results including them will conform to the thesis. (For example, all state-chartered banks in the United States are categorized as non-core (or periphery) banks even though many state banks were located in large cities.)

The statistical analysis often suffers from lack of identifying variation, problems of small sample size, and some curious omissions from the sample (Japan and Argentina). Ad hoc specifications, endogeneity, and omitted variables bias also undermine the empirical findings. And in some chapters, variable definitions seem peculiarly chosen. For example, why should market internationalization be measured using gross foreign investment in 1914—a year when, because of the war, investment flows are distorted and financial markets are operating abnormally? (The U.S. stock market actually shut down for nearly half the year out of fear that Europeans would liquidate their holdings.) And why is it appropriate to compare U.S. net foreign investment with gross flows in other countries in 1914 to measure internationalization, especially since U.S. gross flows are large? (At this time, the United States was moving from a net debtor to a net creditor position.)

The book draws few lessons as to how an understanding of the factors that have shaped these banking systems over the long run should be used by policymakers. If state centralization has significantly altered banking structures, what is the appropriate response of policymakers? Does it matter for economic growth or for
financial stability? (Verdier's view seems to be that the current pace of disintermediation makes his story about nineteenth century competition over resources by core and periphery banks less relevant for understanding the future.) Since it examines the present as well as the past, it would have been nice if the book could have considered in more detail how the movement to harmonize banking regulations across countries (Basle I and II accords) is altering the shape and structure of national banking systems.

**Kris James Mitchener**

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**H Public Economics**


This book is a highly creative and lucid analysis of the determinants of the number and size of nations by the leading economists in the field. It encompasses both theoretical modeling and empirical evidence; and the discussion ranges from the European city-states of the thirteenth century, through the colonial empires of the twentieth century, to the modern-day European Union. The book is a tour de force. It stands as an outstanding example of how the rigors of economic analysis can be applied to new fields and of the insights to be gained from elegant and tractable formal models.

The nation-state is defined in terms of a monopoly of coercion and the legal use of force within its boundaries. The central tenet of the book is that the size of nations is determined by a trade-off between the benefits of economies of scale in providing public goods (e.g., defense) and the costs of heterogeneity in preferences over the provision of these public goods.

This raises the question why, instead of a unified nation-state, we do not observe a series of overlapping jurisdictions that best resolve this trade-off for individual public goods. The authors argue convincingly that such a configuration would face prohibitive transactions costs and fail to internalize economies of scope. The nation-state monopolizes the provision of essential public goods (law and defense), and adopts a host of other functions because of economies of scope and transaction costs. Some functions are delegated to subnational levels of government, but subnational jurisdictions do not cross national borders.

The book highlights a number of considerations that shape the way in which the trade-off between economies of scale and heterogeneity in preferences is resolved. One is the political regime. Dictators will choose larger states than democracies because they can extract larger total rents from larger populations. One of the important predictions of the analysis is, therefore, that democratization will be associated with political separatism and the breakup of large countries. This resonates with the experience of the former Soviet Union and the post–World War II period as a whole. A further finding is that democracy may not yield the socially optimal number and size of countries because people who are ideologically or geographically far from the center may have an individual incentive to secede. Whether the social optimum is attained under democracy depends on the feasibility of transfer payments.

Another consideration is the trade regime. Economies of scale imply that, under autarky, there is an economic advantage to being a large country. In contrast, under free trade, a country's market size is independent of its political size, and small countries may prosper. Thus, another noteworthy prediction is that economic integration should go hand in hand with political disintegration. In their historical overview, the authors argue that this is consistent with the experience of the protectionist interwar period, when few nations were created, and the trade liberalization of the post–World War II period, when many new countries came into being.

The international military climate also plays a role. If there are economies of scale in the provision of defense, a more bellicose world tilts the trade-off between economies of scale and heterogeneity costs toward the former, so that countries will tend to be larger and fewer in number. A further prediction of the analysis is, therefore, that a more peaceful world should lead to political separatism. Again this is consistent with events since the end of the Cold War. For a given probability of conflict between any two nations, an increase in the number of countries raises the expected number of conflicts, perhaps explaining why the post–Cold War “peace dividend” has been smaller than anticipated.
The authors examine issues of federalism and decentralization, as well as the role of supranational organizations such as the European Union. The trade-off between economies of scale and heterogeneity costs provides a framework for examining which public goods should be provided at various levels of government. Decentralization may be a response to political separatism in order to keep the nation-state intact.

As the authors themselves state, the book is not meant to be the definitive word on the subject, but is intended to open up novel areas for research and ask more questions than it answers. More empirical evidence on the economies of scale in the provision of various public goods would be welcome, as would more evidence on the role of size in determining prosperity and on how this depends on the degree of economic integration. As discussed in the book, variation across subnational units within countries may provide an important source of identification.

There are links with a variety of other economic literatures that seem open to fruitful further research. These include work on the microeconomics of sorting and segregation. Also relevant is the economic geography literature, which endogenizes the distribution of income and people across space, but typically takes country size as given. There are a number of issues of political economy that could be explored further. How is the quality of domestic institutions and external trade policy determined jointly with country size? What is the role played by vested interests? How does this interact with the process of economic development?

What is clear is that this outstanding volume makes path-breaking contributions and lights the way toward a rich seam for further research.

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JEL 2004–0553

Peter Diamond and Peter Orszag have produced an important book offering an objective assessment of Social Security's financial outlook and a plan to restore its long-term solvency. Their proposal has the important advantage of honesty—it is a comprehensive package of benefit cuts and tax increases that actually would fix the problem. There are no tricks and no free lunches. Anyone who reads the book carefully will come to understand the rather complicated U.S. Social Security system, how we got into the current situation of projected shortfalls, and the details of a particular plan to fix it. While I don't agree with all aspects of the authors' prescription, their plan merits serious economic and political consideration.

The authors proclaim in their title and repeatedly in the text that theirs is a balanced approach. It is in one sense. The Diamond–Orszag plan relies roughly equally on cutting benefits and raising taxes to achieve long-run financial stability. In another way, however, the plan is not balanced. The big debate over the last twenty years regarding Social Security is whether the system should remain a defined benefit system (with benefits determined by formulas) or whether we should switch to a defined contribution system (usually termed “individual accounts”). In this debate, Diamond and Orszag are firmly in the defined benefit camp. They want to keep the basic structure of the system intact. I favor a two-tier or “some of each” approach and would characterize such two-part proposals as more balanced.

Diamond and Orszag concentrate on three reasons why Social Security is in need of significant reform. First, there is the good news of dramatically increasing life expectancies. That may be good news, but it imposes financial problems on a system providing inflation-indexed life annuities. Second, they list increasing earnings inequality. And, third, they consider the legacy costs of the extraordinary deal that Social Security offered the first couple of generations of participants. Each of these factors causes the authors to propose adjustments in legislated benefits (mostly cuts) and payroll tax rates (all increases). The whole menu of adjustments is pretty sobering—but any plan that restores long-term solvency must have a comparable list of tough provisions.

The Diamond–Orszag plan can be characterized as a “soak the rich” proposal. This isn't particularly surprising given their concern with increasing inequality. For instance, they would introduce a new 3 percent payroll tax applying only to earnings above the Social Security cutoff, currently $87,900. This tax would be over and
above the 2.9 percent Medicare tax that already applies above the same cutoff. This new 3 percent tax would be gradually increased as life expectancy improved and these contributions would not enter into the calculation of Social Security benefits. This feature of completely separating these contributions from future benefits is a major departure from current practice and would certainly be controversial. The Diamond and Orszag proposal would also raise the limit on earnings subject to full OASDI taxes faster than the growth rate of wages in the economy for the next several decades. Again, it is the relatively high income workers who will be affected by this feature.

Diamond and Orszag would also concentrate the benefit reductions on people in the upper middle class and above. For instance, someone whose average indexed lifetime monthly earnings goes up by $1 and who earns between, say, $50,000 and $60,000 currently gets an extra 15 cents per month in Social Security benefits. Gradually, Diamond and Orszag would lower this marginal connection between average monthly earnings and benefits to 10 cents. Meanwhile, their plan would increase the benefits for those with the lowest lifetime earnings histories. There is nothing wrong with the choices they make—I am simply pointing out that they propose to substantially increase the system’s progressivity—worsening the returns for the well off and improving them for the poor.

Even though I have characterized the book’s plan as a soak the rich one, the middle class hardly escapes pain. They can’t—in Willie Sutton’s famous phrase—“that’s where the money is.” The current combined employee and employer payroll tax rate to finance OASDI is 12.4 percent. The Diamond–Orszag proposal would gradually raise that rate to 13.43 percent in 2040, 14.43 percent in 2060, and 15.36 percent in 2078. Average earners would bear these higher rates and still see their benefits reduced from current legislated levels. Someone who is twenty-five years old today would have their benefits reduced by about 8.6 percent, for instance.

There are aspects of the book with which I disagree. I think that both defined benefit plans and defined contribution plans are risky and that elementary portfolio theory would argue that Social Security should have some of each. The authors think that the accumulation of the Social Security trust fund over the last twenty years has added to national saving. I think that the evidence indicates that the extra Social Security receipts have financed income tax cuts and government spending and not resulted in any extra national wealth. I think that they may be overdoing it in the name of progressivity in removing the link between taxes and benefits for high-income participants.

However, despite these disagreements, this is an important book that must be considered by anyone interested in reforming Social Security. It is the most complete, thoughtful, and honest proposal yet of the “save the current system” type. I favor a more radical overhaul of Social Security, but it is great to have such worthy opponents.

JOHN B. SHOVEN
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A furious debate surrounds the future of the tax levied on the estates of wealthy individuals after they die, a tax scheduled to be phased out by legislation enacted in 2001. For arcane reasons, the estate tax will only be eliminated for one year—2010—after which it will rise from the grave when the 2001 act expires.

This book’s authors are two of the nation’s most prominent supporters of the estate tax. Bill Gates Sr., father to the Microsoft billionaire and a wealthy man in his own right, organized the famous millionaires’ petition to preserve the tax. Chuck Collins is a liberal activist who founded several organizations aimed at advocating in favor of progressive taxation and preservation of the estate tax. Their book is a panegyric to the estate tax.

The short book is a remarkably easy read for a tax treatise because it is well written and the authors’ passion for the subject comes through. The book is not a balanced assessment of the estate tax, and not intended to be. The authors are convinced that “the estate tax helps make America great” (p. 2). Although possibly in need of reform, they argue the tax is absolutely essential to support the American way of life.

The book’s treatment of economic issues involved in the estate tax debate is selective and will not be of much interest to economists. A

Instead, *Wealth and Our Commonwealth* is primarily a philosophical treatise, arguing in almost Rawlsian terms that taxing estates is a philosophical imperative justified by first principles. Indeed, the book invents a new version of the Rawlsian veil of ignorance: how much of one’s estate would someone precommit for the privilege of being born in the United States rather than in an impoverished third world country?

The philosophical arguments are backed by historical research and even biblical citations. Some of the historical tidbits are fascinating. Estate tax advocates during the Civil War and Spanish–American Wars promoted it as a “conscription of wealth” (p. 36), a partial offset to the conscription of life and limb exacted from those of lesser means. The modern estate tax came about as a result of the efforts of progressive Republicans, led by Theodore Roosevelt, who saw it as of a piece of their efforts to rein in the robber barons.

The book’s central thesis is that the estate tax is an appropriate levy on the wealthy, those who benefit most from government, a persuasive argument in 1916 when the estate and income taxes were created. Even tax cutter Andrew Mellon apparently concluded that “unearned income,” that is, income from capital, should be more heavily taxed than earned income, or labor income.

The book also provides a nice description of the movement that led to the successful (even if only temporary) repeal effort, and devotes a chapter to dispatching the weakest arguments made by estate tax critics. But the book largely ignores the critics’ strongest arguments. For one thing, economic efficiency is barely admitted as a criterion for tax policy. Thus, the book does little to address the concern that the estate tax penalizes both work and saving. The authors do not address the question of why those who spend their fortunes should get more generous tax treatment than those who would prefer to save the wealth to pass on to their children. Indeed, they quote Andrew Carnegie: “By taxing estates heavily at death the State marks its condemnation of the selfish millionaire’s unworthy life” (p. 38).

Oddly, given that the purpose of the book is to argue for the estate tax’s retention, their case seems to run out of steam when it gets to reform options. A very short last chapter suggests that voters may better connect the estate tax to government services if the revenues were earmarked for particular programs. They argue “... revenue from the estate tax should go directly to initiatives that maintain retirement security or assist in building wealth and opportunity for everyone” (p. 137). For example, the revenue could be earmarked for Social Security solvency or to provide financial aid for higher education. They also support raising the exemption to spare more small estates, but argue that top rates of as high as 55 percent should be maintained. More radical reform ideas, such as replacing the estate tax with an inheritance tax, don’t get raised even though an inheritance tax would seem to be a better instrument to limit large concentrations of wealth—a major concern of the authors. And they never discuss eliminating the many loopholes that make estate tax planning so complex and inequitable. Indeed, closing loopholes could raise revenues used to cut marginal tax rates without reducing the revenue yield of the tax.

Bottom line: the book is fascinating and well written, but it is not an academic treatise. It will amuse and inform estate tax supporters and infuriate critics. It is written at a level accessible to undergraduates, but probably should not be offered without also presenting the opposing point of view.

LEONARD BURMAN

*The Urban Institute, The Tax Policy Center,* and *Georgetown University*


Some years ago Richard Titmuss (for whom Le Grand’s chair is named) produced a very strong argument that the United States was wrong in purchasing blood for blood transfusions while England did not do so but relied on voluntary provision. His argument was essentially moral but he also thought that paying for blood would actually reduce the supply. He pointed out that England where blood was not paid for had a surplus and United States where
it was paid for had continuous difficulties. About ten years later England had to buy blood from the United States.

As far as I know Titmuss made no comment on the matter, but he had a sizeable impact and set off a lot of research. Le Grand does a very good job of discussing this research and its results which are rather surprising to most economists including, originally, myself. It appears that you may not get much return on your payment. The payments may actually reduce the amount of blood or other charitable activity available unless they are quite large.

The explanation given by a variety of people and on the whole accepted by Le Grand is that the payment weakens the charitable motive for gifts. To take an example, the small town in Iowa in which my sister lives has an organization called “meals on wheels.” The housewives participate in it voluntarily, one day a week in rotation, taking meals to housebound inhabitants. They not only are not paid, they contribute gasoline and time, and some cases add a little bit to the meal.

Assuming that the thesis of the book and the quite considerable amount of research by people other than Titmuss which is reported in it is correct, offering a payment might reduce the amount of time people were willing to put into this food delivery. The general effect, if I may quote some work done by one of my former graduate students, is that the amount of volunteer work falls off as the payment increases, but, of course, if you increase it enough you get non-volunteer work. The motives for volunteer work are weakened by actual payment rather than supplemented.

It should be pointed out that people who are not volunteers in the strict sense, ordinary employees, may get some satisfaction out of their work. The man who, like Charlie Chaplin, tightens bolt 17 on the assembly line may feel, quite rightly, that he is contributing to society. After all, if the bolt is not tightened it may lead to accidental death.

To repeat, the research is not merely that of Titmuss, but also that of a number of other quite respectable experimenters. It’s surprising at first glance. We don’t depend on volunteer labor for most of the things we make. We do however use a good deal of volunteer labor here and there. The candy striper seen in most hospitals make life of the injured and sick much better than they would be without them. It would not be surprising if a small payment reduced their numbers. The weakening of the charitable motive by a real payment may make the society as a whole worse off than it was when there was no such payment.

I originally found this surprising, and I think Le Grand was also surprised. Mostly we depend on paying for work, but the fact that you can get some for essentially charitable motives means that society is better off. Zero or sizeable payments produce more output than small payments. At the bottom, the return on payments is negative.

GORDON TULLLOCK
George Mason University


This well-written nontechnical book by three past and present staff members of the OECD deserves to be widely read. It provides a useful corrective to the tendency of most people, whether tax experts, general economists, or ordinary citizens to consider particular features of their own tax systems in isolation both from each other and from the world of taxation more generally. It contains a tremendous amount of information on a wide variety of tax issues and the many different ways in which they are resolved in different countries. Its discussion of a wide variety of controversial fiscal issues—family taxation, capital gains taxes, corporate tax rates, etc.—is almost always reasonable, fair, and balanced. Though fiscal experts will no doubt find something to argue about, most will also find some new ideas in this book. More general readers will find a very useful overview of the messy set of complex political, economic, and practical issues that constitute “tax policy.”

The book should be of interest even beyond the OECD countries. While the authors exaggerate when they say in the preface that the tax policy problems of developing and emerging countries likely differ from those in more developed countries mainly in terms of “the relative efficiency and integrity of the tax administration,” (p. v), they are nonetheless right that the issues and problems facing those countries are not all that different from those faced in the more developed countries of the OECD. In rich countries or poor,
whether one is immersed in tax matters or not, it is often a good idea to stand back and look at the system as a whole, from above, as it were. In short, for those in any country or line of work who want to know what tax policy is all about this book is an excellent starting point. The authors do a masterful job in short compass of both describing the major developments in tax policy in over twenty countries over the last half-century and placing them in appropriate political, economic, and intellectual context.

The book is divided into an introductory chapter and three main sections. First, twentieth century trends in revenue, in policy, in administration, and in fiscal federalism are discussed. The major part of the book then takes up the design of taxes, treating in turn the personal income tax, social security contributions, corporate income tax and other business taxes, consumption taxation, and taxes on wealth, capital gains, and property. Finally, a short chapter provides a capsule comparative overview of tax policy choices within different subgroups of OECD countries (Europe, other industrialized OECD countries, and OECD's developing countries—Mexico, Turkey). The book concludes with a brief discussion on what the future may hold for taxation.

Throughout, the discussion is enriched by the surprisingly extensive information base on tax matters that has, over the years, been built up in the OECD. The book also contains an excellent set of annexes, which provide valuable commentaries on such matters as the “irrationalities and confusions” (p. 225) that often bedevil discussions about taxation and the limitations of international tax comparisons. If these annexes could somehow be distilled into an elixir that had to be consumed by all journalists and politicians before writing or speaking about tax matters, the level of public discussion on tax policy would be vastly improved.

Another point worth noting is that this book treats social security contributions as taxes. Too often discussions of tax policy simply ignore these levies, although in many OECD countries not only has this source of revenue grown more quickly than any other over the last half-century but they now, in most countries, constitute the most important taxes imposed directly on most people’s income. The rationale for including social security taxes is set out clearly and convincingly (pp. 21–22)—although, as with most tax issues, those who do not want to be convinced by reasonable argument will not be persuaded.

Finally, to illustrate both the measured reasonableness of the book’s presentation of such (sometimes) controversial issues and to give a flavor of what those who dip into it will find, here are two of its more striking conclusions: First, while there have clearly been some important recent trends in the rates of major taxes around the world (with income tax rates coming down and VAT and social security rates generally going up), there is neither much evidence of convergence of tax levels or tax structures (mixes) across countries nor any clear evidence that “globalization” has restricted the freedom of even neighboring countries to do pretty well what they want in terms of tax policy. There seems no pressing reason to expect this situation to change drastically in the near future. Second, with the major exception of the VAT, which has—outside of the United States—swept the world, most tax policy innovations over the last fifty years (e.g., integration of personal and corporate income taxes, and dual income taxes) seem—like academic ideas such as optimal taxation that have had no visible policy impact—to have been little more than “transient fashions” (p. 3).

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JEL 2003–0974

This book deals with a timely and important topic, given the increasing trend toward decentralization within the government sector in developing countries over the past two decades. This represents a shift parallel to globalization, which also reduces the authority of national governments over economic matters.

A general problem with such decentralization is the propensity for subnational governments or provinces to engage in noncooperative behavior resulting in collectively suboptimal outcomes, a particular example of which is the problem of the soft budget constraint (SBC). Fiscal decentralization typically devolves greater expenditure than revenue responsibility to provinces, owing to problems of tax coordination and the need to
redistribute across provinces. The vertical imbalance necessitates fiscal transfers from central to provincial governments. Such transfers should optimally be based on measures of relative need, but provinces are better informed than the center about their own needs, giving rise to a classic agency problem. The problem is substantially exacerbated by the inability of the center to commit to not bail out provinces that overspend, nicely illustrated by the editors in their introduction with their firetruck analogy.

Anticipating such bailouts, provinces have incentives to overspend, resulting in fiscal indiscipline and macroeconomic instability, a story having some relation to recent macroeconomic problems of Brazil and Argentina. This book addresses the question of institutional mechanisms that rein in the SBC problem, and the experience of diverse countries across the globe with such mechanisms. Why do they seem to work in some countries and not others? The outcome of a World Bank research project, this volume pulls together an impressive range of country case studies addressing the nature of their respective SBC problems.

The volume starts by reviewing a set of OECD countries: the United States, Canada, Norway, and Germany, which are by and large more successful than middle income and developing countries. A masterly essay by Inman covers the U.S. experience, spanning theoretical considerations, historical evolution, and a survey of recent econometric evidence. The relative success of the United States in reining in the SBC appears to be the result of market-based disciplinary mechanisms, a historical combination of a weak redistributive mandate for the national government, and a reputation created by the latter in the nineteenth century for refusing to bail out profligate states. The Canadian experience has been equally successful, despite a stronger interprovincial redistributive mandate for the national government. Bird and Tassonyi explain that control of local governments by provincial governments was made possible by hierarchical controls, while the solution of the problem at the provincial level is explained in cultural terms, so remains a bit of a mystery—at least for economists. Norway is a more interesting case for developing countries, characterized by greater vertical imbalances than Canada and the United States, and the absence of market-based control mechanisms. Yet Norway also managed to avoid SBC problems, largely due to the application of strict hierarchical control of local governments by higher tiers. Germany appears to be the only OECD country that experienced the SBC disease to some extent, owing partly to a strong redistributive mandate and greater commitment problems for the central government.

The second section of the book deals with three countries with a more pronounced affliction: Argentina, Brazil, and India, all large countries with high inequality, fragmented central governments, strong regional powers, and pervasive discretion in fiscal transfers. The worst problems appear to be in Brazil where states have substantial revenue raising authority. The Argentina chapter is a bit disappointing, especially given the interest in understanding the sources of its macroeconomic instability. A single sentence of the chapter refers to the independent monetary authority of provincial governments: one would have hoped that the chapter would have explained the origin and consequences of this unique phenomenon. In contrast to many other chapters, this one tends to be more journalistic rather than an effort to explain the evolution and functioning of the system. The India chapter on the other hand is detailed and perceptive, exposing clearly various distortions inherent in the intergovernmental fiscal framework.

Indeed the Indian experience drives home a point that needs more emphasis than is awarded to it by the editors—that the agency problem has many other dimensions apart from the SBC problem. McCarten argues that the key problem in India has been underinvestment in public infrastructure, rather than fiscal indiscipline. It underlies a disquiet about the tendency—typical of the IMF—to evaluate institutions in the developing world in terms of the potential for fiscal indiscipline alone. This is a bit surprising in a World Bank research project, given that institution's own interest in decentralization as a mechanism to promote accountability of government and improve delivery of public services to the poor. Moreover, there is evidence that the result of fiscal decentralization in much of the developing world has been the precise opposite of the SBC—local governments strapped for funds owing to a hard budget constraint, relative to spending mandates devolved on them, resulting
in inadequate quantity and quality of public services delivered. Nevertheless, with regard to the SBC problem the volume is an excellent and comprehensive reference, devoting equal emphasis to politics and economics. If anything, the wealth of material is a bit too rich to digest at one sitting; one wishes there were an executive summary of the country case studies available for the impatient reader.

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I Health, Education, and Welfare


JEL 2004–0576

August 2004 represents the fortieth anniversary of Lyndon Johnson’s War on Poverty. Looking today at the set of social safety net programs in the United States, one sees an incredible patchwork of programs serving different groups (elderly, disabled, single parent families) and covering different needs (health insurance, housing, food and nutrition, job training). Some programs are federal and have little cross-state variation (Food Stamp Program) while others are mandated federally but vary locally (Temporary Assistance to Needy Families, Medicaid) and still others are purely local (state Earned Income Tax Credits). In fiscal year 2002, the total cost of U.S. means tested programs amounts to $1,809 per capita and represents about 5 percent of GDP (Vee Burke, Cash and Noncash Benefits for Persons with Limited Income: Eligibility Rules, Recipient and Expenditure Data, FY2000–FY2002, Congressional Research Service). This compares to $418 per capita in 1968 (in 2002 dollars) and about 2 percent of GDP. Research on these programs has grown substantially in the past ten years, driven in part by major reforms and policy expansions.

Means Tested Transfer Programs in the United States, edited by Robert Moffitt, is an excellent volume which promises to become the definitive resource on means tested transfer programs. The book consists of an introductory chapter written by Moffitt, followed by nine stand alone chapters written by leading researchers in the field and each covering a separate program or cluster of programs. The chapters include: Medicaid, SSI, Earned Income Tax Credits, Food and Nutrition Programs, Temporary Assistance to Needy Families, Housing Programs, Child Care Subsidy Programs, Employment and Training Programs, and Child Support Programs. The scope and structure of each chapter is similar and begins with current and historical institutional details (eligibility determination, benefit assignment), program statistics (caseload, expenditures), and law changes. This is followed by a summary of the economic issues in the literature and a review of the evidence. The chapter typically ends with a discussion of current policy (where applicable) and unanswered research questions.

The volume fills a significant void largely because of the scope of each of the chapters. People interested in institutional details, program statistics, or rule changes usually start with the Green Book (Background Materials and Data on Programs Within the Jurisdiction of the Committee on Ways and Means, U.S. House of Representatives). The Green Book, however, does not provide much coverage or discussion of the economic issues and empirical literature. Current research papers can provide the conceptual issues and literature review but rarely provide the rich institutional details and the long historical view of the programs. Many excellent full length books exist that analyze one program or sometimes a cluster of programs. This volume provides all of this—a comprehensive set of programs, with the background, statistics, economic issues, and empirical findings.

The book will interest a wide range of readers. It is surely of interest to scholars interested in public programs and poverty. The economic arguments are nontechnical and quite accessible using intermediate microeconomic level arguments. Some of the econometric issues require more background and training. Overall, it should appeal to economists, noneconomists, policymakers, and advanced undergraduate students.

In sum, Means Tested Transfer Programs in the United States is a useful, important volume that is likely to become the definitive reference of its time on means tested transfer programs.

HILARY W. HOYNES
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J Labor and Demographic Economics


This book builds on the strengths of neoclassical economic theory, showing how models of individual utility maximization can help explain family behavior. It departs from more traditional approaches by acknowledging conflicts of interest among family members and warning of inefficient outcomes that can result from coordination problems. It reaches beyond traditional disciplinary boundaries to describe social interaction effects that challenge a strictly individualist approach. Yet its overall approach makes it more useful as an exercise in the application of optimization theory than a guide to empirical research or public policy.

Any analytical modeling effort must be willing to invoke the protective “it is assumed that . . . .” Ermisch states his assumptions clearly and often illustrates his points by working through the comparative statics associated with an explicit utility function, which is pedagogically useful. He models parental expenditures on children as a specific example of the larger problem of private contributions to public goods, contrasting the noncooperative equilibrium that would emerge if family members could not communicate with the Pareto-efficient allocation that can emerge from cooperation. He begins with the presumption that one parent maximizes utility subject to the other parent achieving at least a given level of utility (as well as a resource constraint). The result is an extremely clear motivation for the so-called “collective” model of family decision making developed by Francois Bourguignon and Pierre-Andre Chiappori that has largely displaced the joint utility function approach.

The “collective” model generates important predictions regarding the impact that different sources of income might exert on expenditures on children. However, it takes the preferences of mothers and fathers as exogenously given and offers little insight into patterns of greater investment in children by mothers, a major determinant of gender inequality. Because it focuses on individual optimization, the model has little to say about public spending on children. Yet children are public goods not merely to parents but also to taxpayers, who devote considerable resources to them and expect to receive considerable fiscal benefits from them as they grow up to shoulder a large burden of public debt. For the last several years, the British government has developed a major policy initiative aimed at eradicating child poverty, yet Ermisch, one of Britain’s most highly respected family economists, apparently considers this topic beyond the scope of his book.

The assumption that individuals try to make rational choices is a strong but credible one. The additional assumption made in all but one chapter of this book—that the preferences that individuals act on are exogenously given and resistant to change—is difficult to defend in the realm of family life. Both parents and society devote considerable resources to shaping the preferences of young children. Most people who have experienced a divorce would describe it as the result of a change in their preferences rather than merely, as Ermisch suggests, the “revelation of new information” (p. 169). They might even, using a noun that seldom appears in this book, describe it as a change in their emotions.

As a growing body of research in behavioral economics demonstrates, people do not always conform to postulates of strictly rational behavior. As Robert Pollak and others have long argued, information and transactions costs often complicate family decision-making. Many of the lessons of evolutionary biology are also relevant (see, for instance, Bobbi S. Low, Why Sex Matters (Princeton University Press, 2000). The preferences that human beings act on are not randomly given; they have been shaped by the pressures of natural selection in a rapidly changing environment.

Men and women, like males and females in other sexual species, may be predisposed to different behaviors. That females invest more physiologically in offspring than males suggests that they prefer a higher “quality to quantity” ratio than males do. In game-theoretic models of resource allocation, males can exploit their bargaining power by making credible threats to abandon their offspring. This asymmetry strengthens female motivation to bargain both individually and collectively for enforceable contracts governing paternal behavior.

In chapter 11, Ermisch departs from stylized models of optimization to consider how
preferences may be shaped by what others in society are perceived to be doing. Although brief, this chapter provides an elegant mathematical and graphical exposition, as well as a useful summary of a growing literature building on the insights of Thomas Schelling. So called “social multiplier” and “tipping” models help explain why societies seem to experience sudden shifts in behavior, such as nonmarital childrearing and cohabitation, that cannot be explained simply by changing prices and incomes. One can hope that the example Ermisch sets here will have a multiplier effect on other economists, tipping them toward a broader perspective.

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JEL 2003–1010

For well over a decade now, economists at the Economic Policy Institute have been serving up a periodic assessment of the state of working America. Part Handbook of Labor Economics review article and part American Prospect policy piece, the chapters of each volume contain information on income, wealth, fringe benefits, and work hours, with a special focus on the less fortunate members of the labor force. The volume under review gives an analysis of these various barometers of worker well-being during the late 1990s boom, along with their trajectory over the last third of the twentieth century for historical context.

The current edition covers family income, wages, jobs, wealth, poverty, regional analysis, and international comparisons in as many chapters. Special care is taken with the first two subjects, the analysis of which composes roughly half the book’s length. Within the chapters, one finds analyses of growing income and wage inequality, nonstandard work arrangements, the inadequacy of retirement wealth, the burden of student loans, the impact of welfare reform, how the U.S. model stacks up against international competitors on the basics of worker well-being, and much more. The basic strategy is to slice and dice the data in question—by education or income quintile, race or gender, state or country—in order to lay bare the reality of workers’ experiences.

Simple descriptive statistics are the norm (properly sliced and diced of course) and, in a few instances, multiple regression analysis is employed to parse out causal determinants. The use of descriptive statistics makes the presentation accessible to policymakers and lay people, which, judging from the book jacket blurbs, is the intended audience, but will likely prove to be only mildly frustrating to economists and other social scientists. There is so much information here, and it is so carefully and thoughtfully presented, that even top-notch empirical labor economists are likely to learn things they did not know. In fact, it is even conceivable that some will be spurred by the presentation of simple relationships to investigate matters further, since rarely is the analysis conducted herein the last word on the subject.

This is a “big picture” approach, one that forces us to step back and focus on the forest instead of the trees. I found the analysis of family income, for example, to be truly revealing after so many years of thinking about and working on the subject of individual worker wages. The rising share of income going to capital in recent years, the average net worth of black versus white households, and the changing nature of rising wage inequality in the 1990s are all topics that were both new to me and interestingly explored in this edition.

The volume is best treated as a reference book rather than as a sustained and engaging read. In contrast to years past, I read the current edition cover-to-cover in the course of three or four days and wished I hadn’t. Read in this way, it becomes dry and repetitive. Buy a copy, keep it on your bookshelf, and use it periodically as an excellent source of information, both current and historical, for research and teaching. One finds citations to this work in economics journal articles and books. I have used it to great effect in undergraduate labor economics courses. It would be an excellent tool for empirical methods courses as well; there is a careful discussion of measurement issues, and the analysis typically leaves off just at the point where a careful treatment of causal determinants would begin, leaving to the students’ imagination the hypothetical relationships to be tested.
Graduate students and faculty may benefit similarly, but they will be frustrated by the lack of citations to the literature. While the authors periodically refer to the work of economists and other social scientists, they typically do so with phrases such as “economists contend” or “critics maintain,” offering citations only in those instances where the research of others is presented. Moreover, some chapters are more novel and informative than others. The “jobs” and “wealth” chapters rely a great deal on previously published findings of other economists. I kept wondering in the “regional analysis” chapter whether the differences discussed were truly different in a statistical sense.

Still, there is real strength in this volume; it forces us to focus on the big picture and it offers coverage of recent events that would be hard to get, given publication lags, in published journal articles. The late 1990s boom brought a reversal of growing wage inequality, and was especially rewarding for workers at the bottom of the wage distribution, despite continued low-skill immigration, welfare reform, and growing low-wage employment. Low unemployment rates were a key ingredient; they are particularly beneficial to the dispossessed. How has working America fared during the bust? Those of you with the Current Population Survey on your hard drive already know the answer to this question; the rest are eagerly awaiting the next volume of The State of Working America.

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Like researchers in many other fields of science, some labor economists have been looking for a “universal theory of everything” or at least one that encompasses labor market outcomes like employment and unemployment, wage distributions, firm size, seniority returns, and so on. The enthusiasm with which the literature has adopted the Burdett–Mortensen (1998) and Pissarides (2000) frameworks suggests that some macro-based labor economists, especially in Europe, believe they have found their holy grail. At the most recent European Economic Association–Econometric Society European Meetings joint conference in Madrid, I counted over 40 percent of the papers with theoretical contributions in labor economics sessions being based on one or the other of these models.

The (short) new book by Dale Mortensen, a founding and frequent contributor to both strains of this literature, could easily serve as a primer for this burgeoning field. Its early chapters are well written from a pedagogical perspective and its later chapters introduce extensions of the base model that eliminate inconvenient empirical inconsistencies and accommodate ever-more complicated types of behavior. Although the basic framework for the book is the Burdett–Mortensen model, its links to the Pissarides model are highlighted and the implications of replacing the Burdett–Mortensen wage posting hypothesis with the Pissarides bilateral bargaining and endogenous job vacancies hypotheses are discussed in detail. A good graduate labor economics class could use this book as support for teaching the Burdett–Mortensen model, which should over time become a standard part of any labor economist’s theoretical baggage.

Chapter 1 lays out the motivation for the rest of the book by citing numerous empirical studies, especially those based on linked employer–employee data, that demonstrate significant wage dispersion across firms for observably (and unobservably) equivalent workers. It then proposes a static job search model with homogeneous workers who can receive multiple offers and Bertrand competition among homogeneous employers. It describes several extensions of this model, such as introducing heterogeneity in firm productivity, multiple contacts per firm and bilateral bargaining à la Pissarides. Compensating differentials, efficiency wages and sorting can all be accommodated by this framework, although Mortensen argues that they are unlikely to be the main source of inter-firm wage dispersion.

Chapter 2 then presents the Burdett–Mortensen model, which is a dynamic extension of the static model in chapter 1 and on which the rest of the book is based. This model includes on the job search by workers and undirected search by employers, has an identical job offer arrival rate for unemployed and employed workers and allows job-to-job mobility of employed workers
who receive better offers. From the inflow and outflow equations at each wage, Mortensen derives the steady-state equilibrium offered wage distribution in closed form, the employed wage distribution and the analytical link between the two. He also discusses the role of the “market friction parameter” (the ratio of the job destruction rate to the aggregate job contact rate), a version of which also appears in the Pissarides literature. Although certain simplifications make the exposition of the base model more straightforward, they unfortunately push several interesting implications and complications, such as those highlighted in Ridder and van den Berg’s (1998) work, into the background.

Chapter 3 considers the empirical implications, in particular the convex, increasing offered and employed wage densities, of the Burdett–Mortensen model and compares them with data from a Danish linked employer–employee data set. The chapter first documents the densities of cross firm average and initial wages in the Danish data (which seem consistent with a different parametric distribution than that implied by comparable U.S. data) and calculates some summary statistics, broken down by occupation, for calibrating and comparing the extended versions of the model that appear later in the chapter. Although some choices seem ad hoc and some numerical justifications are complicated calculations based on shaky foundations, they do not invalidate the fundamental motivation of seeing whether the empirical implications of the model can be squared with data for at least one country. The chapter then extends the baseline model by allowing exogenous productive heterogeneity and endogenous recruiting effort. A model that endogenizes the productivity distribution by allowing firms to invest in general or match specific capital similar to Robin and Roux (2002) is also introduced, although it does not fit the Danish data as well as the model with endogenous recruiting effort.

Chapter 4 further extends the baseline model by allowing workers to vary their search effort. With (exogenously) heterogeneous firms, endogenous vacancies and endogenous search effort, the model becomes significantly more complicated than the base case, although the mechanisms are more interesting. Under fairly standard assumptions, Mortensen shows that a unique steady state equilibrium exists in both the wage posting and bilateral bargaining cases. The confrontation of theory and data in this chapter suggests that a Pissarides-type bilateral bargaining approach to wage determination is more appropriate for the Danish data than the wage posting model in the base Burdett–Mortensen framework, and the shapes of the wage densities and the marginal cost of hiring function are derived explicitly. Although he again makes some ad hoc assumptions, the model that comes out of this chapter and the empirical implications that are drawn and tested are the high point of the book.

The final chapter, on the other hand, is a step back. Chapter 5 addresses the wage–tenure relation and is based on three other papers: Postel-Vinay and Robin (2001), Stevens (1999) and Burdett and Coles (2001). Postel-Vinay and Robin provide the most direct extension of the model that generates a wage-seniority profile, but Mortensen is unconvinced by the counter-offer approach of their model. As a result, the presentation is confused, important functions are left undefined, and the reader comes out bewildered. The Stevens contract theory-based model is presented as an alternative, but Mortensen admits that it is not based on the Burdett-Mortensen framework and introduces another contract-based model, that of Burdett and Coles. This model is also interesting (and relevant), although not necessarily more realistic than Postel-Vinay and Robin, while its presentation is only slightly less confusing. When teaching from this book, one could reasonably skip this chapter or go straight to the papers as there is little value added in Mortensen’s presentation.

In conclusion, this is a book that all labor economists who are not already deep into this literature should have. The pedagogical presentation of a model that is so intuitively appealing and can be generalized so straightforwardly to accommodate such a large number of labor market behaviors makes it a good way to become familiar with a framework that is taking on impressive proportions. The confrontation of theory with data, although not always entirely convincing, is a noble effort, deserves to be applauded, and leaves even the microeconometrician interested in reading more.

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JEL 2004–0196

This book explores the causes and consequences of social and institutional conformity. Professor Sunstein claims that conformity prevents individuals from disclosing their private information and, therefore, tends to cause individuals and groups to make poorer decisions than they otherwise would. Though conformity is not always bad and dissent not invariably good, Sunstein claims that the dangers of conformity justify policies that tolerate, subsidize, and otherwise promote dissent.

Several chapters address the causes of conformity. Here, Sunstein nicely summarizes literatures from psychology and economics, which together identify two mechanisms: information and reputation. Economists tend to emphasize information-based conformity. Rational individuals update their beliefs in light of the beliefs of others, which they infer from the others' behavior and statements. Economic theory describes and an experimental literature demonstrates the "herd behavior" that results. At the extreme, the first decision controls all that follow. This process explains sudden changes in behavior or "cascades." An example is a "bandwagon disease" produced when doctors, relying on their colleagues' judgment rather than their own, all diagnose the same new disease among their different patients.

By contrast, psychologists tend to emphasize reputation-based conformity, which occurs because individuals value the good opinion of others. Although Sunstein does not use the term, he refers here to the pursuit of "esteem," as in Geoffrey Brennan and Philip Pettit, *The Economy of Esteem* (Oxford University Press 2004). The desire for esteem can cause individuals to conform to behavior they expect will attract esteem. Reputational cascades cause sudden conformity. Examples may include the quick emergence of "political correctness" and large scale self-censorship among skeptics of an impending war. In either case, a community may appear to favor a certain norm or policy when most of its members are actually concealing their dissent out of fear of disapproval.

Sunstein also describes the related phenomenon of "group polarization," which is the tendency of deliberation to push group members toward a "more extreme position" (p. 11). Sunstein's own experimental work, for example, demonstrates that deliberation among mock jurors causes them to reach a more extreme assessment about the punishment the defendant deserves. If the pre-deliberation median of the mock jurors' punishment assessments is low (below 4 on a scale of 0–8), deliberation tends to drive down the median; if the pre-deliberation median is high (above 4), deliberation tends to drive it up. This study confirms the group polarization found in hundreds of studies around the world. Sunstein offers interesting analysis of how this literature relates to the older "groupthink" theory (pp. 140–44), the more optimistic prediction of the Condorcet Jury Theorem (pp. 119–20), and new work on "deliberative polling" (pp. 163–65).

Sunstein's normative point is the need for policies protecting dissent. Inducing individuals to reveal their actual beliefs helps to prevent pathological conformity, cascades, and polarization. The book derives a wide range of policy implications. A striking example is his claim that group polarization explains terrorist recruiting, which implies that an antiterrorist foreign policy should focus on promoting dissent abroad (pp. 115–18). Not surprisingly, Sunstein views the first amendment right to free speech as a key tool to promote dissent by limiting governmental censorship and protecting access to "public fora" (streets, parks, etc.) where dissenters can reach a diverse audience. Elsewhere, a chapter discusses how certain governmental structures—e.g., bicameralism, separation of powers, and federalism—impede the influence of cascades on decision-making. Another claim is that law in a democratic society tends to induce obedience—conformity—by the signals it sends: signals of what people in the community disapprove produce reputation-based conformity, while signals of private beliefs of lawmakers about the harms of prohibited behavior produce information-based conformity. In either case, one enhances legal compliance by publicizing the law and (any) high compliance rates it produces.

In addition to large-scale social phenomena, Sunstein addresses conformity in groups and institutions: how policies protecting dissent may improve the function of investment clubs,
corporate boards, and presidential cabinet war planning. Other chapters discuss the importance of maintaining political diversity on three-judge courts—given findings that federal judges decide cases differently depending on whether the other two judges were appointed by a president of the same or different political party—and the possibility that affirmative action in higher education works to undermine conformity by promoting a diversity of views. Given the attention to juries and law, however, there is surprisingly little discussion of how the jury system does or could protect dissent or avoid polarization.

The book identifies but does not resolve a number of important trade-offs. First is the idea of the “optimal level of dissent.” Dissent from true or morally correct beliefs is undesirable. Some minimal costs to dissent may be desirable so that what that remains is more likely to be useful. Sunstein notes the particular waste of merely “contrarian” dissent, but rejects the possibility of saying more in the abstract about optimality. Second, there is a trade-off in the social connectedness of groups. Other research suggests that close-knit groups have certain advantages in solving collective action problems. But the same bonds that deter free-riding also increase the reputation costs of dissent, causing more self-censorship and poorer decisions. For some group decisions—e.g., investments—the optimal level of social connectedness may be zero. Third, Sunstein identifies a trade-off in the optimal composition of deliberative groups or “enclaves” in society. Individuals seek groups with like-minded people. On the one hand, self-selection can cause intense intra-group conformity and inter-group polarization. On the other hand, there is a potential gain if individuals exit groups in which they will not express their beliefs and enter groups in which they will, at least if there is some communication between groups. The latter proviso is crucial; Sunstein claims that truly isolated deliberative enclaves are essential for producing terrorists.

In sum, Sunstein offers a rich account of conformity, novel justifications for protecting dissent, and an array of policy recommendations. Though many issues are unresolved, the book is a valuable starting point for the subject it addresses.

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L. Industrial Organization


JEL 2004–0203

During the late 1980s and early 1990s, reports of firms downsizing their operations in America rippled through the business pages of the nation’s major newspapers. Were these reports a fad having little to do with the shifts in the industrial landscape? Or did they actually signal a trend toward smaller sized firms? This is a carefully crafted retrospective by three leading economists on what was reported at the time, what happened in the various sectors, and the consequences of this episode. The authors assemble an impressive array of data to investigate six hypotheses about downsizing, which are laid out in their first chapter. First, technological change favored smaller enterprises during that era. Second, faster innovation led to more labor churning. Third, foreign competition compelled domestic firms to trim their fat. Fourth, labor saving devices led to smaller firms. Fifth, the social contract between labor and the owners of capital was ruptured. Sixth, blue-collar jobs replaced white-collar jobs.

The data is mainly drawn from the Census Bureau, Compustat, and the Panel Study of Income Dynamics. One challenge the authors confronted at the outset was how to define the entity being restructured, what “downsizing” means, and how the definitions of variables in the data relate predictions from economic theory. But the most original part of the data assembly was stimulated by their interest in documenting how much downsizing was a phenomenon in the public consciousness during that time period. The authors conducted an electronic search for the word “downsizing” in the archives of the New York Times and the Wall Street Journal for the period 1993 through 1997. Their search formed the basis for a qualitative and quantitative exegesis on what people wrote at the time. One important finding is that the reporters for these newspapers discovered “much of what was really going on” (p. 61). In this case, statistical data largely corroborated what newspapers first reported. Perhaps a more
comprehensive analysis would include other newspapers and weekly bulletins, grade the importance of the articles, and compare the importance of "downsizing" with business news on other topics of that era. Even so, their approach is a very ingenious use of the Internet, which may well become an empirical basis for economists analyzing the measurement of public perceptions and expectations formation.

Many of the findings about what actually happened are reported and discussed in chapters 4 through 7. Only the manufacturing sector experienced significant downsizing in the 1980s and 1990s, and this trend began in the 1970s. In the services and retail trade sectors (both of which account for more employment), average employment, per firm and per establishment, has increased. Within the manufacturing sector, downsizing was not uniform; larger firms tended to downsize, but smaller firms tended to expand. Since many industries within the manufacturing sector contracted over this period, and the opposite was true of many industries within the retail and service sectors, the facts seem broadly consistent with the theoretical model outlined in chapter 3. Having sketched the broad picture in chapter 4, the authors focus more closely on the manufacturing sector in chapters 5 and 7 and, less comprehensively, review changes in the retail and service sectors in chapter 6.

The positive correlation the authors found between changes in firm size and industry growth implies that the six hypotheses the authors wish to investigate could also be expressed in terms industry growth rates, rather than the size distribution of firms. In their findings of what caused downsizing to occur within the manufacturing sector, reported in chapter 5, several of the hypotheses receive support. For example, the ratio of industry R&D spending to sales is negatively related to firm and establishment growth, and this is consistent with the first two hypotheses. Consistent with hypothesis 5, unionized industries are more likely to downsize than their nonunionized counterparts. Curiously, hypothesis 4 (that labor saving devices led to smaller firms) does not receive much support from the data.

In chapter 7, the authors focus on the consequences of downsizing in the manufacturing sector. They found that profits increased in the sector, labor costs declined, productivity may have fallen, and value of the firm declined too. This is consistent with hypothesis 5, that the social contract between labor and capital may have also frayed, and also with hypothesis 2, that foreign competition compelled domestic firms to trim their fat. The authors assert in chapter 1 that trimming fat “should raise the average productivity of labor substantially” (p. 19), and the data contradict this implication. But today's overweight population gives fat a bad wrap; it was once regarded as capital, literally stored energy, or interpreted in this context, reputation that owners and managers cultivate with labor to provide stable employment relationships. The facts that the value of the firm falls when profits rise and, as the authors report in chapter 7, that job turnover increased throughout this period, suggests that domestic firms anticipated lower profits in the future, and were writing off their physical capital at a faster rate, and also loosening the ties with their own employees. Whether productivity would rise or fall in such circumstances seems less clear to me.

Rounding off their study, the authors weave an analysis of job turnover into an extensive existing literature on this subject. Some of their conclusions in chapter 8, such as the finding that the young change their jobs more frequently than older workers, are well established facts. A contribution of this chapter is to integrate discussions of labor markets and changes in firm size. For example, the fact that men who changed jobs fared worse than non-job changers with similar socioeconomic characteristics suggests that adjustment costs from foreign competition are borne by the owners of the enterprises and also by their employees.

This is a delightful book about an important episode in American industry. Moreover the methods used by the authors can be applied to today's hot topics, such as outsourcing technology and the continued upsizing within the retail sector.

ROBERT A. MILLER
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The tight relationship between American universities and spectator sports is unique in the
world. Why do American universities combine teaching and research efforts with commercial sports operations rather than, say, producing movies? Do college sports affect the financial condition of universities? How does the National Collegiate Athletic Association (NCAA) fit in? Is it an association aimed at helping universities achieve their chartered goals? Do intercollegiate sports enhance teaching and research at universities or detract from them? These are among the questions addressed in this collection of essays. A reader can learn some interesting things by reading this volume. Here are some examples.

In opening the volume, Joe Maxey argues that recent NCAA reorganization, in which those universities most committed to big-time sports secured greater autonomy, pretty much insures the preservation of the college football bowl system, because 90 percent of bowl revenues flow to teams in “power conferences.” A football playoff modeled on the NCAA men’s basketball tournament would likely return only 60 percent of the loot to those teams.

Evan Osborne expects that at least some of those colleges claiming to lose money on their Division I athletics programs would close their sports operation. Because few do, he concludes that athletic programs must serve some other university objectives. Osborne believes that students demand college athletics. They are willing to pay for sports, perhaps even enough to allow universities to charge a premium for undergraduate education in order to cross-subsidize research and graduate education.

Brian Goff points out that, typically, university athletic financial statements include scholarship costs based on list price tuition. The correct way to account for opportunity costs, however, would be to measure the marginal cost of an additional student-athlete at open enrollment institutions or to compute lost tuition net of financial aid at institutions where admissions is selective. In either case, reported losses would be trimmed.

Robert Sandy and Peter Sloane exploit the change in NCAA level of sports affiliation (109 up; 15 down) of 124 colleges from 1991 to 1999. They find evidence that, other things equal, institutions operating at a higher level of sports affiliation enjoy more applications from prospective students and higher SAT scores of their enrolled class.

Promoters of the men’s NCAA “Final Four” basketball tournament commonly release “studies” predicting a huge economic boost to the local economy. Economists, in general, scoff at such conclusions because they know that many locals flee the area during a “mega-event” like the “Final Four,” taking some local spending along, and much of the revenue generated by such events immediately leaks from the area as pure economic rent. Robert Baade and Victor Matheson use a model of urban growth to compare these conflicting views. Remarkably, they find only one out of thirty “Final Four” tournaments provided any economic return for the host community at all.

Using a cross-section of 201 colleges, Michael Leeds, Yelena Suris, and Jennifer Durkin show how profitable football programs subsidize women’s athletics modestly ($34 of each additional $1,000 of net football revenues), but football programs in general drain $112 from women’s athletic programs for every $1,000 spent. On balance, the average football program reduces expenditures on women’s athletics by more than $250 thousand annually.

Robert Brown and Todd Jewell estimate the marginal revenue product of a premium college football player at around $400 thousand, and a basketball player at around $1.2 million. These estimates leave a wide margin between the value players create and the cost of their tuition, room, and board, even after accounting for the admissions advantage accorded elite athletes.

Using data from Penn State, John Fizel and Timothy Smaby find that participants in club sports and intercollegiate sports other than football earn grades comparable to the overall student body. In contrast, football players, who are admitted with substantially lower average SAT scores than the typical student, fail even to perform at the modest academic expectations such low scores imply.

Fizel and Michael D’Ittri measure the efficiency of NCAA Division I college basketball coaches from 1984 to 1991. They then review which coaches were fired during the period and learn that winning is more important for job retention than coaching efficiency. I suspect the coaches already knew this!

Analyzing major changes in NCAA regulations from 1888 through 2001—for example, the formation of the NCAA in 1906, the 1949 agreement not to pay players, and institution of
a minimum 1.6 GPA for eligibility in 1965, Craig Depken and Dennis Wilson find that NCAA “reforms” generally are followed by less competitive balance in college football. They conclude that this validates “the public choice hypothesis of special interest voting behavior for determining the direction of the NCAA.”

The book would be more persuasive if it contained fewer errors. As just a few examples, the variable INSTRPC turns into OWNRESPC on page 59, UGTEACH appears on page 60 with no definition, Georgia State morphs into Georgia Southern on page 78, a quarter of page 172 is repeated verbatim on page 173, and on page 206 the sign of the estimated coefficient on the variable indicating the formation of the NCAA in the table is at odds with its interpretation on page 207. On page 105, we are told that MIT lost its case defending collusion among the overlap group on financial aid, when, in fact, MIT is the only institution that went to trial, and it eventually settled an appeal out of court.

The book is easy to read. Economists interested in the microeconomics of college sports will find the time required to read this book worthwhile. The overall impression it conveys is that big-time college athletics is beyond academic control. If that is true, we need to know if the behavior of those who do, in fact, control big-time college athletics is congruent with the missions articulated in the charters of our institutions of higher education. This reviewer, for one, is not optimistic.

JOHN SIEGFRIED
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The efficacy of institutions often varies from place to place. For example, there is evidence showing that secure property rights helped promote growth in early Western Europe, but undermined growth in twentieth-century Africa. Similarly, there is evidence showing that publicly owned water systems were associated with higher waterborne disease rates than privately-owned systems in late-twentieth century Argentina, but that publicly-owned water systems had relatively low disease rates in the early twentieth century United States.

These examples suggest that economists and policymakers need to recognize the potential contingency of any policy regime. What works well in one context might not work well in another context. These examples also highlight the importance of perspective, whether historical or international, in analyzing the effects of regulation. Put another way, to understand the effectiveness of any institutional regime, one needs to understand not just how that regime works in general, but how it has functioned in different settings.

It is this sort of perspective that makes José A. Gómez-Ibáñez’s new book a welcome addition to the literature on public utility regulation. The book analyzes the effects of governance regimes in a wide range of settings: water in Britain, the United States, France, and Latin America; buses and telephones in Sri Lanka and the United States; the railroads and airlines in the Americas and Great Britain; and electricity in the Americas. In all of these settings, the industries employ network technologies with high fixed costs, which in turn leave all concerned parties vulnerable to opportunistic behavior once the relevant investments have been made.

For Gómez-Ibáñez, the governance of public utilities is a long-term contracting problem with no perfect solution. Awarding franchises to private companies is attractive because it encourages ex ante competition, but as a contracting device, is necessarily incomplete as all future contingencies can not be anticipated. Regulatory commissions address this concern but are subject to capture by concerned interest groups. Public ownership is a form of vertical integration—the government that acquires an electricity provider because it cannot commit to treating that provider fairly is akin to the manufacturer that acquires the supplier of a key input because it cannot commit to treating that supplier fairly. Problems with public ownership include patronage and failures to maintain the capital stock. In developing this view, the book treads territory well worn by previous authors such as Victor Goldberg, Oliver Williamson, and Pablo Spiller.

The novel aspect of this book is to be found in its presentation of numerous case studies of regulation. Taken as a whole, these studies provide important insights into how governance regimes evolve and how effective these regimes are across time and space. Consider the description of the
Sri Lankan bus system. Initially, the system was one of private provision dominated by a few monopolies. But consumers soon grew weary of the high rates and poor service. Eventually, the system was taken over by the government but this regime was plagued by patronage employment and there were strong electoral incentives for officials to keep rates too low. Because of high wages and unremunerative rate structures, the associated infrastructure deteriorated. Later in the twentieth century, the bus system was privatized and there were numerous competing bus lines. The problem with competition, however, was that there was too much of it. As bus drivers raced one another to stops, safety and service quality fell. This example illustrates the difficulties inherent in the governance of any networked industry.

Also instructive are two chapters exploring the vertical unbundling of services associated with the privatization of railways in Great Britain and the move to market-based governance of Argentina’s electricity system. These chapters highlight a tradeoff that regulators must confront when they break up vertically integrated systems. Such break ups promote greater competition and increased efficiency on many margins, but they also increase coordination problems. For example, when Britain privatized its rail system it also separated control over the tracks from control over the trains that used the tracks. The subsequent confusion over who was responsible for track safety probably resulted in three fatal train accidents.

In any book that covers so much territory there are bound to be at least a few lapses. Among other things, I found the discussion of substantive due process wanting. The author argues that in the United States substantive due process prevented the government from expropriating the investments of private electric companies, while in places that had fewer constitutional constraints, such as Canada, expropriation was much more common. The problem here is that these protections are overstated and not adequately qualified. When private companies in the United States appealed for the protections of substantive due process, that protection was neither cheap nor timely (see, for example, William R. Wilcox v. Consolidated Gas Company of New York, 29 S.Crt. 192 1908; and Des Moines Gas Company v. City of Des Moines, 35 S. Crt. 811 1914). Also problematic was the description of gas-rate regulation in Chicago during the early 1900s, which ignores the political opportunism associated with that regulation.

But these are relatively minor criticisms of an otherwise thorough and thought-provoking book. Because the book is clear, well written, and covers much territory, it would be a logical choice for a graduate course on regulation. Authors wanting a firm empirical grounding in what the new institutional economics has to say about regulation will also find it useful.

WERNER TROESKEN
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“‘You are what you eat,’” or so your high school health teacher told you, or possibly your mother, though she might have put it differently. The papers in this volume, edited by Dora Costa, suggests that you are also what “exposure to infectious disease, occupational hazards, malnutrition, and other types of biomedical and socioeconomic stress” (p. x) make you. More importantly, from a historical perspective, these biological and social factors change over time; thus, we—that is, homo sapiens—change over time. The homo economicus of today’s principles texts might be solving the same Lagrangian problems that she solved in Alfred Marshall’s day, but she’s not the same woman. These biological changes manifest themselves in numerous economic phenomena, including labor force participation, effort, retirement, death, and so forth. If you are intrigued by the relationship between health and economic activity and changes in both over time, then you will find the papers in this volume worth reviewing.

The volume opens with a chapter in which Larry Wimmer summarizes the history of the research project entitled “Early Indicators of Later Work Levels, Disease and Death.” The data generated by the project, and related projects, are employed in one form or another by
most of the authors of the volume. Union Army records on Civil War recruits serve as the primary source of data, which includes thousands of variables and more than 35,000 individuals. (The details are in an appendix authored by Peter Viechnicki.) It is a big job. All who participated are to be congratulated. In her preface, Costa explains the importance of this data set, noting that, unlike more recent studies, it covers the life cycle from adolescence, or early adulthood, through old age and death. Thus it allows scholars to analyze long-run relationships between health and economic activity.

A theme that runs through several chapters in the volume is the role of wealth in health and mortality. Analyzing the link between wealth and mortality is particularly important, because an oft-cited body of scholarship suggests that historically a weak relationship exists between mortality and standard economic indicators, such as income and wealth. To address this issue, among others, Joseph Ferrie linked individuals from the antebellum population and mortality censuses, thus allowing him to test for a correlation between social data and death rates. His finding that, even prior to the mastery of the germ theory of disease, wealth had a substantial negative impact on mortality rates after controlling for other factors, contrasts with the stylized view that there was little or no connection.

Using the data on Union Army recruits, two additional chapters analyze other aspects of the apparently subtle relationship between health, wealth, and mortality. One, by Chulhee Lee, contains an empirical investigation of the impact on mortality from exposure to disease. Recruits from urban areas and areas with high child mortality rates tended to have lower mortality rates than recruits from rural areas, suggesting those who survived prior exposure fared better once they were thrust into the high-disease, military environment. This finding differs from the so-called insult accumulation model, which suggests that prior illness weakens an individual and would therefore result in higher subsequent mortality. Lee also finds that, although wealth reduced the chances of contracting a disease, wealth did not lead to lower mortality rates. Daniel Scott Smith also looks at the high mortality rates resulting from disease among recruits, and finds no single smoking gun as the source, but rather argues that the high rates were caused by the sheer diversity of sources. Among the more interesting of these is the discovery that environmental factors, such as better food and toilet facilities, which separated officers from enlisted personnel, contributed to mortality rate differentials within the ranks. Although strictly speaking, this difference is not a wealth effect, and the Union Army was arguably more egalitarian than most nineteenth-century armies, the difference between officers and other ranks was still a socioeconomic one.

Much work on the biological standard of living has revolved around human stature. Sven Wilson and Clayne Pope analyze the impact of local environmental factors on the height of Union Army recruits. Confirming the findings of earlier studies, they find that urbanization had a negative impact on stature; while recruits from farms were taller on average. Thus the authors support the expectation that exposure to disease during childhood reduces stature; while access to nutrients increased it. Environment also mattered when it came to other indicators of health later in the life cycle. Werner Troesken and Patricia Beeson find that veterans residing in cities in which the municipal water systems employed lead mains experienced health problems at greater rates than other veterans.

Because of the longitudinal nature of the Union Army data, they are well suited for the study of economic behavior over the life cycle. Chen Song and Louis Nguyen focus on the impact of a common and particularly troublesome ailment among the veterans—hernias. Though viewed as a relatively minor ailment today, hernias were potentially debilitating in the past. Song and Nguyen find that, after controlling for other health and economic factors, the presence of a hernia had virtually no impact on a veteran’s decision to retire. This finding contrasts with studies on more recent workers that suggest a strong positive connection between poor health and retirement. Also, Tayatat Kanjanapipatkul confirms an earlier finding of Costa’s that the income elasticity of retirement was larger in the past than it is today. Kanjanapipatkul also finds that, among Union Army veterans, workers who could be classified as “professionals” had the largest elasticities, a result that is just the opposite of that of today’s workers.

In other papers, Mario Sanchez shows that migrants were more susceptible to disease than nonmigrants; thus, the relatively high rates of
internal migration in the United States contributed, ceteris paribus of course, to a reduction in the biological standard of living in the nineteenth century. This was probably an important, and underappreciated, piece of the so-called “antebellum puzzle,” the term employed to describe the divergence of economic and biological indicators of the standard of living in the United States. Sven Wilson finds that, among Union Army veterans, respiratory disease increased toward the end of the nineteenth century, and that veterans who experienced respiratory infections during the war were more likely to have respiratory diseases later in life, providing some support for the insult accumulation model.

Although the papers in this volume cover a large range of topics, in Costa’s concluding chapter she offers a number of suggestions for future research. Space prohibits a detailed accounting of that agenda, but one can only hope that she has another volume in the pipeline.

LEE A. CRAIG

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Paul R. Gregory’s book is a welcome attempt to relate the new data from the Soviet archives on the Stalinist economy to the concepts and analytical tools of economics, to see how far this system can be understood in terms of principal/agent relations, and institutional economics. Gregory argues that the Stalinist state corresponded in large measure to Mancur Olson’s “stationary bandit,” based on a rational calculation of how the ruler could maximize the resources at his disposal, whilst recognizing that those subject to his control would use their area of autonomy to maximize their advantages, through rent seeking activity and other strategies.

Thus the adoption of the command administrative economy followed a clear logic, aimed at maximizing the ruler’s control over the economy, and instituting forced primitive capital accumulation. Following F. A. Hayek and Ludwig von Mises, Gregory sees the centrally planned, state owned economy as crucial in establishing a totalitarian regime. He presents the relationship between economic dictatorship and political dictatorship in terms of an analogy: the jockey chose the horse, but the horse required a particular kind of jockey. Gregory echoes Hayek’s assertion that, under this system, the political leaders who came to the top were those with “a comparative advantage in brutality” (p. 50).

While there were no ineluctable laws that meant the destruction of capitalism in Russia in 1917, so there was no inevitability about the abandonment of the New Economic Policy and the adoption of the command economy after 1928. Gregory acknowledges Stalin’s central role as architect of this system. While the investment rate doubled from 1928–37, there was large scale destruction of the capital stock in agriculture. Moreover, Gregory endorses the view of James Millar that there was no net transfer of resources from agriculture to industry. Surprisingly, no new calculations have been done on this matter since the opening of the archives. Whether the growth rates actually attained by heavy industry and the defence industries in the 1930s could have been attained under NEP remains a matter of contention.

Gregory takes up R. W. Davies’s finding that there were distinct investment cycles in the Soviet economy, with sharp cuts in investment in 1932 and 1937. The regime’s objective, he argues, was always investment maximization to ensure the highest rate of growth, but it had to take account of the need to motivate the labor force, to deal with labor unrest and high labor turnover, and low labor productivity. This involved favoring consumption, and was dictated by the need to accommodate some conception of what constituted a fair wage at least among key workers. He acknowledges that the Soviet notion of a fair wage was extremely elastic; while high bonuses were paid to shock workers and Stakhanovites, others suffered sharply depressed incomes, income inequalities became acute and a substantial part of the economy was placed under forced labor. Paradoxically, as Gregory notes, the impact of Stakhanovism on productivity is very uncertain. The investment cycles can also be explained more conventionally as a response to investment surges that created imbalances and overheating in the economy, threatening supplies and credit and financial stability.
Gregory endorses the view of Eugene Zaleski that the real planners’ preferences were reflected in the investment plans, not the output plans. The five-year plans were not converted into operational plans: the plans were subject to multiple revisions, were adopted after the plan period had begun, and the output plans remained highly aggregated allowing for decision making to be delegated to lower levels. The Politburo’s main concern, Gregory argues, were the investment figures. Nevertheless, output figures remained important, as they provided a check on performance, particularly in failing sectors, with even daily output figures being published in the press. Gregory questions how far this was a planned economy, arguing that it was based largely on a system of resource management and ruble control.

Gregory insists that Stalin and the Politburo could make pitifully few decisions in relation to the number of decisions taken within the economy. Undoubtedly there was a growing trend, already evident in the early 1930s, for the details of economic management to be taken over by the government (Sovnarkom), the state planning commission (Gosplan), and the chief economic ministries. But there is a danger of exaggerating Stalin’s lack of influence. Very little happened without his say so. Moreover, in three crucial areas—quarterly industrial investment targets, procurement targets for the agricultural regions, and targets for foreign trade—Stalin’s word was law.

While the Second Five-Year Plan saw greater realism in terms of industrial and agricultural output figures, the figures for consumption were greatly exaggerated. Gregory relates this to the intrinsic failures of the planning system, in terms of inadequacies of information flows, institutional obstruction, interdepartmental and interregional conflicts, the tendency toward over-taut planning, the unreliability of supply chains, and the trend toward institutional autarchy. All this led to an overburdening of the centre, recourse to administrative controls, interspersed with waves of repression. Much of this will be familiar to scholars, and has been well explored by Janos Kornai, and by Alec Nove, especially in the latter’s *The Economics of Feasible Socialism*, which, curiously, receives no mention.

In considering the destruction of the administrative-command economy under Gorbachev in the late 1980s Gregory places emphasis on the dismantling of the apparatus of central planning and control. Attention might also have been given to the destruction of the state’s revenue base through the anti-alcohol campaign and the law on state enterprises.

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**Book Reviews**

**O Economic Development, Technological Change, and Growth**


Taxation and development may not seem a very natural pairing. As this study shows, however, a country’s tax system and its pattern of development are closely interdependent. Three groups of readers should find this book particularly interesting. Development economists will find here excellent case material for classroom use, for example, with respect to real exchange rates and trade protection. They will also learn much about the important role taxes play in determining economic outcomes. Public economists will find useful insights on the design and efficacy of tax incentives and the interplay of tax administration and tax policy. In addition, however, they will also learn that if they do not consider tariff and trade policy carefully, they may seriously misunderstand the effects of tax policy in developing countries. Finally, anyone interested in understanding the “Asian miracle” will learn much about the roles played by macro, trade, and tax policy in bringing about persistent high rates of growth in one of the leading “tigers,” Taiwan.

The book begins with a description of the evolution of Taiwan’s tax system over the last fifty years and a condensed history of the various stages of economic policy and economic development—import substitution, export expansion, infrastructure development, and liberalization. The authors both set out clearly the complicated and changing set of policies that were utilized to
promote economic growth and describe some important institutional peculiarities that shaped Taiwan’s polices (such as the overlay of “national” and “provincial” administrations).

The core of the book analyzes how tax, trade, and macro policies interacted to increase per capita GDP from about US$1,400 in 1955 to over US$13,000 in 1999. Three main conclusions emerge from this analysis. First, the many specific and focused incentives put in place over the decades had at most a minor impact compared to more fundamental changes in real wages and real exchange rates. Second, by far the most important incentive policies were those related to exports, particularly the combination in the earlier period of relatively high tariffs on manufactured goods and the duty rebates on inputs used in making export goods. An important reason why this policy worked so well, however, was because exporting firms were allowed to sell in the domestic market provided they paid tariffs on imported inputs used for such production so that the “effective protection” provided by high tariffs on final goods was largely competed away. Third, although the authors make a valiant effort to analyze the impact of the incredible variety of tax incentives used to promote investment and savings, on the whole their conclusion is that, as usual in such analyses, the effectiveness of the incentives is largely unclear.

The last two substantive chapters discuss tax and trade administration. While of some general interest in showing the critical importance of administration in determining both the nature and the effects of policy, this material is perhaps of most interest to tax specialists. As an example, Taiwan’s VAT is unusual in several important ways. For many years (until 1999) it was a “subnational” tax, imposed by the local rather than central government. This system was not very satisfactory both because taxes accrued to where companies filed taxes rather than where consumption (or production) took place and because rebates to exports often had to be paid by jurisdictions other than those that had collected the taxes on inputs. Nonetheless, it worked surprisingly well because from the beginning, in part precisely because of the constitutional need for the VAT to be subnational, Taiwan employed a heavily computerized administrative system which has enabled it to be the only place that has successfully managed to “match” VAT invoices, thus providing a powerful (if costly) check on evasion. The same feature has enabled Taiwan to be one of the very few jurisdictions that does not collect VAT at import from registered taxpayers: rather, tax is deferred until the goods are sold. Taiwan’s unusual reliance on, and experience with, land-based taxes is also of considerable interest.

To sum up, this is one of the best case studies of the role of taxation in economic development that I have read. It is not, of course, perfect. For example, the authors’ harsh condemnation of Taiwan’s “mistake” in initially paying too much attention to the income tax may be right in efficiency (avoiding “double taxation” of dividends) and especially administrative terms, given the extent to which the proliferation of income tax incentives seems to have been generated by the then relatively high tax rates. But the absence of any serious discussion of either distributional issues or the political economy of Taiwan weaknesses the case. Similarly, despite the repeated stress on the importance attached to reducing compliance costs, these costs are never really discussed with respect to inland taxes. Finally, nothing substantive is said about the effect, if any, of tax incentives on capital flows and in particular on direct foreign investment.

Richard M. Bird
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Social policy and economic reform appear to be antagonistic: the current economic reforms especially in Europe cut deeply into the omnipresent cushion of social policies. This book argues that the situation in emerging market countries is quite different: economic reforms require the cushion of social policy. Social policies, so the authors argue, provide the insurance which make liberated markets with their increased risks economically and politically viable.

The book draws lessons from a workshop sponsored by the Russell Sage Foundation. It has ambitious aims: to collect experiences from social policy experiments around the entire emerging world; to do this in a strictly comparative fashion and with an explicit theoretical
focus; to see where “best practices” have emerged and whether these lead to a converging policy approach. Most of the authors are closely linked to the World Bank and the Inter-American Development Bank, and the book represents the ongoing search for a more balanced view on social policies away from what has been construed as the neoliberal “Washington Consensus” among the large non-government institutions situated there.

The core of the book consists of four chapters on broad geographic regions: Nicholas Barr begins with a sober assessment of the welfare state reforms in Central and Eastern Europe. His main (and very political) point is that the crisis in the welfare state should not be overstated: while reforms are needed particularly in their pension systems, there is no reason for panic or despair. Barr stresses an aspect underemphasized in the rest of the book: social insurance needs to be as close to actuarial as possible in order to minimize labor market distortions. Nancy Birdsall and Stephan Haggard review social policies in East Asia and point out a crucial difference in the social contract: while in Western countries, a sophisticated social contract has emerged over the last centuries, such a contract is simply missing—they argue—in East Asia.

I would not put it so dramatically. Rather, the division of obligations between state and family is a fundamentally different one in Asia, making the state the residual safety net and the family the primary one, not the other way around as is so prevalent in Europe. Miguel Székely and Ricardo Fuentes try to explain the paradox that Latin America has a tradition of labor protection explicitly aimed at reducing poverty, yet it has the highest income inequality in the world. Their (controversial) answer is that poverty alleviation failed because most Latin American countries failed to protect the poor from the implications of vast macroeconomic volatility. Zafiris Tzannatos and Iqbal Kaur review public social spending and programs in the Middle East and North Africa with their huge public sectors. This well-written, sometimes gloomy article stresses the importance of fixing the large public program, in particular their ill-designed incentive effects and governance structures, because privatization can only proceed slowly for economic and political reasons in the wake of the slowdown after September 11.

These four regional chapters are framed by an extensive introduction, a historical account of the emergence of social policies around the world, and a brief section on policy recommendations. The introduction by the editors attempts to establish a common point of view, in spite of the striking diversity of policies, histories, and cultural norms that make the analyses and answers in the four regions so different. It is a very useful and thoughtful overview of development economics. Peter Lindert searches for the common drivers of social spending from 1780 to 2020. His cliometric exercise puts most weight on demography, then ethnic fractionalization, then secular income growth. Lindert explains which political economy mechanisms transform these causes into social policies and bravely projects them into the future—a somewhat questionable exercise given the huge regression errors.

The book as a whole compels through its broad range of issues and views, encompassing historical, sociological, economic, and political science mechanisms. It also makes crystal clear—the title is program—that the primary task for development is to tackle badly failing markets. The book does not really fulfill its aim to provide a unifying, explicitly theoretical framework. It would help to carefully distinguish social policies between pure redistribution, public insurance where private markets fail, and regulation where private markets exist but would generate socially undesirable outcomes. The global economics of actuarial insurance are very different from the effect on redistribution. In this respect, a more careful distinction between emerging markets and developing countries would have helped. Emerging markets, the countries addressed in this book, have in general sufficiently advanced institutions that can carry actuarial insurance and sufficient income to support redistributive taxes. More often than not, both fail in the truly developing countries—misleadingly depicted on the book’s cover. The book also provides little analysis why and which markets fail, and how these failures can be addressed directly. This lack of analysis may be the main reason why the final chapter of this volume, dedicated to policy recommendations, comes out weakest.

The book provides the reader with a convincing line of arguments that social policy is badly needed to provide economic and political support to economic reform in the emerging market
countries because markets fail. It shows that a “one size fits all” approach is inappropriate because different markets fail in different emerging countries. We still have a fascinating research agenda ahead of us to understand how we deal with these market failures.

AXEL BOERSCH-SUPAN
University of Mannheim


The set of reforms pursued by many Latin American countries at the beginning of the 1990s aimed at achieving macroeconomic stabilization and a market economy, generally known as “The Washington Consensus,” have lost its luster in recent years. The public opinion in the region, as shown in recent polls, has turned against pro-market reforms. The rhetoric of policymakers has increasingly blamed free market policies for many of the economic ills in the region. International organizations, the linchpins of structural reform around the world, are beginning to question the idea that pro-market reform is a necessary condition to achieve economic development. Even some academics are having second thoughts about the role of the market. The reasons are simple. Economic growth in Latin America, while positive, has been disappointing. Poverty remains rampant. Income inequality is as dreadful as ever.

Kuczynski and Williamson have brought together an impressive cast of practitioners and academics who have been able to produce an appealing book that makes an attempt at understanding the reasons for the unsatisfactory performance with the aim at developing a new agenda for reviving economic momentum in Latin America. The editors organize the topics in eleven chapters, one overview, and one appendix in four broad topics, namely: crisis proofing (chapters 4, 5, and 6), completion of first-generation reforms (chapters 2, 7, and 9), second-generation reforms (chapters 8 and 10), and social issues (chapter 3). Essentially, the underlying premise of the book is that the initial reforms carried out during the 1990s were not pushed far enough. Neglected or incomplete reforms, compounded with a decade of macroeconomic crises in the region, resulted in a stumbling process that ended up bearing little tangible fruit. Thus, the book makes a case for the completion of first-generation reforms, such as the privatization of public banks, the improvement of market access to industrial countries, and labor market liberalization issues, as well as on crisis proofing issues, such as budget surpluses, hard budget constraints, reserves and stabilization funds, exchange rates flexibility, de-dollarization of the economy, prudential supervision of banks, and a strengthened fiscal position.

Furthermore, a perusal of the book corroborates that the policy recommendations discussed are not simply just “more of the same with a twist.” The insights on many issues are, in fact, thought provoking and some of the ideas are innovative, for instance, the proposal to establish a regional peer monitoring of Maastricht-like commitments to fiscal responsibility which, as feasible as it may or may not be, has the merit of being the result of a broader, out of the box perspective. In addition, there is some focus on institutional as well as on social issues. In fact, unlike in the original agenda, there is an explicit concern on social welfare in general, and income inequality, in particular beyond the usual “Field of Dreams” view—“If the country grows, redistribution will come.” In fact, the book argues that countries should aim at making fiscal systems more progressive, for instance, by focusing expenditures on the universal provision of high-quality basic education and health care. The idea is to give the poor access to assets that will enable them to make and sell things that others will pay to buy. In this context, the poor should be empowered with education, land, credit, and titling to provide them access that will enable them to earn a living in a market economy. In this context, a “cannot miss” is the excellent work done by Williamson in the last chapter drawing together the ideas expressed throughout the volume.

The book correctly points out that careful reform of the political system, the civil service, and the judiciary are imperative and that the political economy of reform will be as important, if not more important, as compared with the previous round of reforms. It discusses potential rules change in the political game, in particular, in the electoral systems. Examples are the proposal to set up legislatures composed of career
professionals dependent on constituents, proportional representation in districts of modest size, and consolidation of election dates. However, for all the emphasis placed on political economy, the book is somewhat unbalanced in that it lacks sufficient policy detail on microeconomic aspects despite the fact that second-generation reforms are essentially microeconomic in nature. As a result, the overall emphasis of the book tends to be more on the what rather than on the how, whereas policymaking is about the latter.

The discussion of microeconomic interventions is, perhaps, a good example of this. Several countries in the region are currently engaged in interventions to attract foreign direct investment, support small firms, promote innovation, and promote exports that try to mirror the experience of such countries. While the book is skeptical about “strategic” interventions similar to those pursued in several East Asian economies, it would have been even more useful had it discussed the theoretical and empirical foundation for such kind of interventions which are not as solid as is commonly believed. This is understandable in that it is not possible to discuss all the policy-relevant issues in detail in such a broad book.

The book leaves the reader with a profound sense of the crucial challenges in Latin America in years to come and, in particular, the tough road ahead in the process of transforming institutions from dysfunctional to functional. At the same time, it also gives the impression that, despite the reform fatigue, the medium-run horizon of the proposed new reforms, and the limited patience of the population, the region has embarked, for better or worse, on an irreversible path of transformation.

ALBERTO CHONG

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P Economic Systems


In the early 1990s, Russia began to embark on its transition from operating as a socialist economy to functioning as a market economy. Under the socialist system, prices were largely set according to administrative criteria; the state owned most of the enterprises, land, and housing; the state determined the overwhelming share of domestic investment; and the state controlled international and financial trade flows. The initial round of “big bang” reforms represented a radical break from this socialist legacy: the Russian leadership mandated an immediate liberalization of most prices in January of 1992; a massive privatization of large, medium, and small enterprises was rapidly implemented during 1992–95; major efforts to set up financial markets and to establish appropriate corporate governance procedures were implemented and Russia became much more integrated into international commodity and financial markets. This book draws upon a large body of work employing state of the art theory, survey, and econometric methods. The research was conducted primarily by a young group of economists working at the Center for Economic and Financial Research in Moscow in order to analyze Russia’s economic prospects after more than ten years of reform.

The chapter entitled “Accounting for Growth” contains an overview and analysis of the Russian growth record and the determinants of growth. Before President Putin came to power, Russia had one of the sharpest and most sustained declines in aggregate output within the group of post-socialist transition economies in Central and Eastern Europe and the former Soviet Union. However, the Russian aggregate growth record has been impressive since 1999. The authors persuasively argue that the current growth record cannot be sustained without a major improvement in industrial productivity and a sharp and a sustained increase in investment. The analysis of just how the highly controversial financial groups (FIGS) that spontaneously emerged during the mid-1990s have contributed to growth is superb. FIGS have enhanced industrial productivity because they, in general, use their substantial financial power to buy out inefficient owners and managers of enterprises. This threat of buyout also encourages existing enterprises to operate more efficiently. Thus, in an environment in which corporate governance procedures and bank lending for enterprises are relatively primitive, FIGS have emerged as a growth enhancing informal institution via their impact on productivity. However, FIGS may also impede growth via their
impact on foreign direct investment. Careful research shows that foreign direct investment is important for both productivity gains and overall investment. After the devaluation of 1998, FIGS became even more entrenched as owners and lenders in a larger number of sectors in Russian industry. Foreign investors, however, are now hesitant to work with FIGS because they employ informal and nontransparent lending and takeover procedures. Thus, more generally, the challenge to Russian reform is to somehow improve property rights, small shareholder and creditor rights, and promote the development of banking in order to attract more foreign direct investment. One would like to know, however, just how FIGS should be dealt with so that they will support or, at least, not block these reforms.

The third, fourth, and fifth chapters provide a fascinating overview of the ongoing drama associated with creating and sustaining market-enhancing political institutions such as an independent judiciary, efficient and honest regulators, federal and regional executives and legislatures that are responsive to their constituents, etc. One of the notable successes of the Russian reform is the widespread introduction and use of competitive federal, regional, and local elections. Nevertheless, drawing on the modern theory of political institutions, the authors caution that the emergence of direct democracy is necessary but not sufficient to provide incentives to employees in the public sector to push ahead with reform and to be accountable to their constituents. In particular, it is also necessary to establish a system of checks and balances between agencies such as the judiciary, legislature, and executive branch; and, it is also necessary that foreign institutions, such as the WTO, have the ability to monitor the public sector. A very useful comparison of the accountability under the Yeltsin and Putin administrations is presented and the authors conclude that the Putin government has the potential to move ahead with reform because of its strong cooperation with federal legislature.

There are, however, several caveats about the power that the Putin administration currently enjoys. First, the potential for the Putin administration to abuse its power is worrisome. Secondly, and this is an open question, it is unclear whether an independent judiciary that has the power to enforce its rulings can emerge under the current political conditions. There are powerful theoretical arguments backed with strong evidence that legislatures and executive branches in direct democracies are more likely to empower the judiciary when they face political competition and when there are substantial policy differences across parties (see William M. Landes and Richard A. Posner 1975 and F. Andrew Hanssen 2004). It is clear from reading the book that, even though reforms to strengthen the judiciary have been implemented, much more needs to be done "to achieve a judiciary that functions at a level that inspires confidence in its impartiality and specifically in its ability to enforce property rights." (p. 96) It is important, however, to consider just how to provide incentives to the Putin administration to empower the judiciary or to consider just how to create the kind of political competition that is conducive to the emergence of an independent judiciary.

This book should be of interest to economists working on post-socialist transition as it provides a very sophisticated overview and analysis of the Russian reform. It should also be of interest to development and comparative economists because it contains an excellent analysis of the contribution of institutions such as informal lending groups and the judiciary for economic performance.

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JEL 2004–1133

Every economist could benefit from reading this book. It explores fundamental questions about the role and moral implications of the market in society. The questions are not new but are addressed here with honesty and a fresh relevance.
Blank is an economist with a Ph.D. from MIT and is currently dean of the School of Public Policy at the University of Michigan. McGurn is the chief editorial writer for the Wall Street Journal. The book consists of separately written sections in which each responds to the other. They were selected by the Pew Forum on Religion and Public Life and the Brookings Institution not just because of their professional qualifications but also because they are deeply committed Christians—Blank an active member of a Protestant denomination and McGurn a devout Catholic.

It is not that the moral criteria they apply to the market are much different from those applied by other Americans, religious or nonreligious. They are concerned with matters of social equity, individual freedom, impact on family and community, and in general the effects of market self-interest on other social values. Their Christian commitment comes into play more in terms of their willingness to examine the morality of the market explicitly, dropping the standard professional stance of a strictly scientific inquiry and value-neutrality. The result is an unusually open and candid dialogue among two well informed and sophisticated observers of American economic life.

Blank and McGurn both dismiss out of hand the idea that a market can be simply an impersonal device—a “market mechanism”—to be judged by its ability to promote economic efficiency alone. As Blank writes, many of the losers in a market competition will feel real “pain” and indeed the market “can be extremely cruel.” Whatever economists may say about Pareto optimality, there is seldom any actual compensation for market losers. To endorse a market system is, therefore, to make a value judgment that other things advanced by the market are equally or more important than the stresses and strains experienced in the market process. The many losers in that process may be part of the necessary “price of progress,” a price that most reasonable people perhaps will judge that society should be willing to pay. However, this is not a “scientific” determination.

Besides the economic advances that can lift whole nations out of poverty, another desirable market feature for McGurn is the greater individual freedom it offers. In his view, human beings are born with a natural curiosity and a desire to express their creativity—related to the biblical message that men and women are created in the image of God. The market provides an opportunity for millions of people to apply their individual intelligence while serving the needs of others. Indeed, the act of production may be as worthy as the act of consumption, a truth preached by many religions of history, if much neglected in the current analytical framework of economics.

Both Blank and McGurn are concerned that a market ethic of self-interest should be limited to commercial domains within society. Whatever many University of Chicago economists may have argued, a family that is a merely a contract among self-interested individuals is not a real family. Indeed, even within the domain of the market, there is a danger that attitudes of greed and self-interest can extend too far. A market system, as Blank and McGurn agree, must be grounded in shared values of trust, honesty, and fair dealing. For them a world of total individual pursuit of self-interest would function poorly in almost every area of society, including the market itself.

It is thus an open question whether the market may contain the seeds of its own destruction, if it encourages an excessive individualism that might eventually be severely harmful to its workings. Yet, both Blank and McGurn are optimistic; for them, the Christian religion provides a strong enough moral foundation outside the market. Because higher values are so well defended there, an ethic of self-interest inside the market is acceptable (and for McGurn even admirable). They do not address a question that will occur to many others—absent such a powerful religious faith in society, or some other firm grounding for ethical behavior, how well will the market work?

Reflecting in part her Christian commitment, Blank argues that a “collection of individuals each pursuing his or her self-interest” can not be a world of “fully functioning human beings.” McGurn argues that “it is culture that determines the boundaries within which the market operates and beyond which the market dare not go.” An appropriate set of outside cultural rules is needed in society, he says, to sustain “the institutions and values that no market can survive without.” Does this mean that a favorable judgment on the market system requires the presence of the Christian religion or some other satisfactory faith complementary to the market? Must a
provider of economic policy advice first determine the religious character of a society, and then tailor the economic recommendations to the actual religious circumstances—as Max Weber early in the twentieth century might well have argued? If a society has a religion that is less congenial with the market (some versions of current Islam, for example), might an economist suggest the need for new religious thinking, or would it be necessary to suggest an alternative set of economic institutions more compatible with existing religious convictions?

In a book that raises issues of such broad scope, it is not surprising that Blank and McGurn do not have—or claim to have—the full answers. One could well argue that Christianity has been marginalized in the modern era by more powerful secular religions. If they had written more, the authors might have explored whether a secular religious commitment to the market may even be a necessity for fending off rent seeking, opportunism, and other powerful forces in society that might otherwise undermine the market’s effectiveness. Yet, if that is the case, it may be necessary almost to worship the market to achieve its maximum benefits—perhaps seeing the market as the best possible instrument in a secular religion of economic progress that will lead to a new heaven on earth. In that case, the tensions between “market values” and “Christian values” in society may be considerably greater than either Blank or McGurn acknowledge.

It also follows from their arguments that the subject matter of economics as presently conceived is incomplete. Current economists have little to say about the matters of the formation and defense of “social capital” that Blank and McGurn see as critical to the functioning of the economic system. Most current economic inquiries thus address only a limited part of the full terrain of major influences on economic outcomes. The economics profession at present therefore has a choice: it can either concede that many important “cultural” influences on the workings of the economy fall outside the scope of accepted professional inquiry or economists can significantly expand the scope of legitimate areas for their own investigations.

Until recently, any suggestion of the latter would have seemed impossible. However, major American foundations, such as Pew, are today committing significant funding to the study of the connections between economics and religion. It is no longer especially controversial to suggest that a value-free economics is an impossibility. When the Brookings Institution asks two devout Christians to explore together the moral merits of the market, and publishes the resulting book under its auspices, it is surely a sign that times are changing.

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JEL 2003–0369

Financial policies have assumed a crucial role in the management of economies throughout the world over the last thirty years or so. Financial crises have increased, at the same time, in terms of both their frequency and severity. The “emerging market economies” have not been unaffected. On the contrary, the majority of financial crises over the period affected them, and in some cases severely, especially in the second half of the 1990s. The aim of this book is to deal with financial policies in Emerging Markets (EMs), as the title suggests, by concentrating on two interrelated issues: the degree of financial vulnerability of EMs, and the possible connection between the exchange rate system and financial vulnerability. The book is divided into two sections, with the first entitled New Evidence on Financial Policies and the Impact on Emerging Markets, comprising three chapters, and the second The Euro and Financial Policies in Central and Eastern Europe, also comprising three chapters.

The editors in chapter 1, “Financial Vulnerability and Exchange Rate in Emerging Markets: An Overview,” provide a helpful commentary and raise a number of issues that are addressed more extensively in the rest of the book. In chapter 2, entitled “Original Sin, Passthrough, and Fear of Floating,” Ricardo Hausmann, Ugo Panizza, and Ernesto Stein examine the extent to which EMs float their currencies. The authors conclude that EMs do not have the “luxury” of exchange rate flexibility and independent monetary policy; most EM countries intervene heavily in the foreign exchange market. It is demonstrated, both theoretically and empirically, that a strong negative
correlation exists between a country’s inability to borrow in own currency and degree of exchange rate flexibility.

In chapter 3, Barry Eichengreen and Carlos Arteta in “Banking Crisis in Emerging Markets: Presumptions and Evidence,” identify three contributory factors to the instability of the banking system as a major disruption in the EMs: the rapid growth of domestic credit; the large size of bank liabilities; and financial liberalization. The authors find no stable relationship between the exchange rate regime and banking crises (if anything it is banking crises that cause currency crises), nor do they find evidence that the quality of institutions, or deposit insurance arrangements, matter. Ultimately, though, the authors conclude that, as far as the causes of banking crises in EMs are concerned, “it is fair to say that the jury remains out” (p. 55).

J. Onno de Beaufort Awijnholds and Arend Kapteyn in chapter 4, entitled “International Financial Crisis: The Role of Reserves and SDR Allocations,” argue that while a stable macroeconomic environment and a sound financial system might be important prerequisites to avoid a crisis, EM countries would be strongly advised to hold an adequate level of international reserves. It is, of course, recognized that holding reserves entails costs and benefits. EM countries with relatively high reserves are able to manage to withstand financial crises better than those with relatively low reserves. A considerable cost is identified in the case of countries that hold excessive reserves. It could lead to macroeconomic lassitude since the external constraint is thereby removed. An adequacy benchmark is, therefore, proposed.

Jacek Rostowski in chapter 5, “The Eastern Enlargement of the EU and the Case for Unilateral Euroization,” examines the issue of the choice of an appropriate exchange rate regime by the “eastern applicant” EM countries that aspire to EU membership. For these countries, the question is the exchange rate regime that optimizes their path to the EMU. EM countries should adopt the euro unilaterally as the best way to achieving convergence. Chapter 6 by D. Mario Nuti in “The Costs and Benefits of Euroization in Central and Eastern Europe Before or Instead of EMU Membership” deals with a similar theme. The costs and benefits of euroization are extensively discussed to conclude that ultimately the net balance is an empirical question. Early euroization may have clear advantages but the costs cannot be ignored.

In chapter 7, entitled “Currency Substitution, Unofficial Dollarization, and Estimates of Foreign Currency Held Abroad: The Case of Croatia,” Edgar L. Feige, Michael Faulend, Velimir Šonje, and Vedran Šošić attempt to measure the amount of foreign currency in circulation in a country, knowledge of which is important to economic policymakers in their choice of exchange rate regime. Despite this there is no reliable evidence on the extent of unofficial dollarization. This measurement is attempted in the case of a number of countries, and Croatia in particular, in the case of dollar and DM holdings. It is argued that, when the euro replaces national currencies, the results of exercises of the type proposed in this contribution should be very helpful.

Although one may very well quibble about a number of aspects of the theoretical and empirical parts of the book, especially the variables utilized in the estimations and techniques utilized, this is no doubt a topical book, dealing with relevant issues concerning EMs and their place in the world economy. There are, however, three aspects that are sadly downplayed in the book: the role of institutions (legal and political economy factors in particular); the importance of governance; and more quantitative analysis of a number of issues raised in the book should have been forthcoming (e.g., cost and benefits referred to in a number of instances in the book). These are key aspects to the problems faced by EMs, especially so in view of the conclusion reached by the editors that EMs “must look within their own respective countries and find specific answers to specific problems for a better future. There is no alternative to a home-grown development” (p. 14).

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The book analyzes Russia’s transition from a command economy to the market, with particular
emphasis on the role of Russia’s industrial and behavioral heritage in slowing down the pace of transition. The “virtual economy” is an environment in which large loss-making enterprises were able to survive without restructuring by using informal and highly personalized networks that allowed value-subtracting goods to be produced and exchanged; the oil and gas industry was the ultimate source of value infusion.

Russia’s transition to a market democracy is far from being complete, and any analysis is bound to be interim. Reality played a cruel joke on the authors: the book, which was begun in 1997 with the aim of explaining the disappointing results of economic reforms, was out of print by 2003, the fifth consecutive year of high growth rates. Yet what might be concealed by these rates fueled primarily by high oil prices and dramatic ruble devaluation in 1998 is that the overall economic structure remains largely intact. Recent estimates by the World Bank show that transfer of value from extractive industries to the rest of the economy remains a pervasive feature of the Russian economy.

For a book aimed at a general economics readership, Russia’s Virtual Economy is a little bit too systematic. To provide a comprehensive picture, the authors invoke a lot of peculiar features of the Russian economy and attempt to put them into a general perspective by contrasting them with the Economics 101 wisdom. While the authors’ command of Russian details is indeed impressive, the number of issues tackled and the number of explanations suggested seem overwhelming. Still, despite doing a poor job of drawing interest to Russian transition, the book provides a superb analysis to those who are really interested. Starting from a case study of a medium-size provincial firm, the book explores the basic mechanism that allows “industrial dinosaurs” to survive in the era of rapidly changing market conditions. Instead of investing money and efforts into transition toward the efficiency frontier, firms maintain nonmarket relations with one another and accumulate relationship capital which is crucially important in dealing with state authorities. The whole economy is then stuck in a bad equilibrium, where those firms that try to do business in a standard market mode are relatively penalized, while those who play by the “virtual economy” rules are rewarded. The symptoms of the economy’s disease include widespread barter, in-kind taxes, and other nonmonetary transactions.

In part, the Russian virtual economy was a reaction to a particular strategy of exchange-rate based macroeconomic stabilization pursued by the Russian government in 1995–96 rather than the legacy of the Soviet past. The same “virtual economy” symptoms were observed lately in Argentina between the Brazilian devaluation and the Argentine default. Still, one of the truly valuable points raised by the book is the extent to which enterprises inherited from the Soviet era have been unable rather than unwilling to restructure. Early students of Russian reforms put a lot of emphasis on incentives. Though there is no doubt that proper incentive design is of crucial importance, Gaddy and Ickes are right to point out that the constraints due to initial conditions have been binding for large industrial enterprises. This is in contrast with the empirical evidence on newly created firms, which are shown to respond to changes in the rules of the game, e.g., an improvement of property rights protection, in a market way. The authors are right to argue that Russia’s unique features by no means imply that a market system cannot ever work properly in Russia. All the available evidence demonstrates that transition economies are inhabited by agents who are genuinely interested in enjoying fruits of their own efforts.

One aspect, mysteriously missed in the book, is politics. Russia is a federal country, and governors who are elected in contested elections have played a crucially important role in regional economies. Without taking governors’ incentives into account, it is hard to understand why it is so important for the directors of large enterprises to maintain employment far above the efficiency level, and why close relations with large regional business lead to governor’s unfriendliness toward small and new business development. In the book, the “missing politics” phenomenon is more of a linguistic nature. With an apparent aim to widen the readership, the authors sometimes choose to introduce new concepts rather than to stick to more traditional academic language. For example, once investment in relational capital—be it a bribe to a police officer or putting a senator’s wife on the payroll—is understood as a private protection of property rights, the picture starts to look more familiar to students of capitalist economy.
The whole phenomenon of the “virtual economy” was probably no less characteristic of the late decades of the Soviet Union than of post-Soviet Russia. In the 1990s, while official Russian GDP may have been overestimated as a result of “virtual economy” accounting, as the authors claim, the same estimates might also have failed to account for the informal economy, and thus the net effect may be that GDP was understated. In the Soviet Union, where prices had been set by a central planning body, value was transferred from extracting to manufacturing industries through artificially low energy prices.

To those readers who reach chapter 9, there is an analysis that is a model of clarity and precision, which are sometimes lacking in earlier chapters. The authors outline the “impossible trinity” of the Russian state: to simultaneously achieve economic growth, democracy, and security. The current trend of cutting down still-young democratic institutions in the name of sustaining high growth rates and maintaining security fits very well into this paradigm. In the final analysis, it might be a bit of overstatement to say that the book is a must-read for anyone who is interested in the economics of transition. But the “virtual economy,” the main mechanism that Gaddy and Ickes were first to identify, describe, and analyze, is surely something that must be understood.

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JEL 2004–0339

Junko Kato, a political scientist, tells a historical political story of the relationship between the adoption of regressive taxation—value-added taxes (VAT)—and the growth of the welfare state—social security expenditures—across various industrialized and newly developed or developing nations during the twentieth century. Kato’s interest is in examining the funding base of the welfare state. She begins with a look at evidence on the cross-national patterns in welfare expenditures for 1960–1996 in eight industrialized nations, concluding that there was little convergence among high-spending and low-spending welfare states. The explanation: there was in fact little retrenchment among the high-spending states during the 1980s welfare retrenchment era. Kato argues that a greater funding capacity within a nation in the 1980s blunted any movement toward welfare retrenchment, while in nations with financial problems retrenchment was inevitable. She further contends that “The divergent funding capacity of the welfare state is path-dependent upon the institutionalization of regressive taxes” (p. 1). If the regressive (VAT) taxes were institutionalized during periods of high economic growth, they created greater funding capacity and welfare spending; if during periods of low growth, they caused less funding capacity and spending.

The book does not have an introductory chapter; instead, chapter 1 is organized similarly to a journal article. It serves as an introduction to the topic, places it in the context of the (principally) political science literature, discusses various hypotheses and propositions, analyzes data, discusses findings, and draws conclusions. The chapter examines the cross-national patterns in tax revenues, tax composition, and welfare expenditures for 1965–2000 for eighteen OECD countries, which are labeled according to various political classifications that may seem odd to economists. Thus, the high-spending, high-tax Denmark, Finland, Norway, Sweden, Austria, and Belgium are labeled “nonright hegemony” welfare states while the low-tax, low-spending Canada, France, Ireland, Japan, Switzerland, and the United States are labeled “liberal” welfare states. The high-tax, low-spending Australia, New Zealand, and the United Kingdom are labeled “radical” while the low-tax, high-spending Germany (West), Italy, and Netherlands are labeled “conservative” (p. 7). Detailed data on tax composition and tax revenues for the eighteen countries is presented next, indicating a large cross-national variation that increased between 1965 and 1980. But while total taxes generally increased, the relative positions of countries remained the same in 1995 and 2000. The later years still indicate a large, but unexpected, cross-national variation in both the composition and level of tax revenues. The continuing variation in the postwar period is unexpected, according to Kato, because of the widespread diffusion of the progressive income tax during the 1940s and 1950s and the “worldwide tax reform in the
1980s” (p. 13), both of which should have decreased cross-national variation in tax composition. Yet despite the worldwide movement away from reliance on progressive income taxes toward reliance on regressive VAT taxes, there was relatively heavier reliance among the high-tax countries, which was not diffused to the low-tax countries. Finally, the chapter finishes with estimation of a two-stage regression model of welfare spending in the eighteen OECD countries for 1965–1992. The econometric findings (pp. 42–51) generally support the hypothesis that higher welfare expenditures are consistent with heavier reliance upon VAT. While globalization and other political and economic factors also influence welfare spending, they do not blunt the effect of reliance on VAT.

The next three chapters present detailed descriptive case studies of the history of selected countries’ tax and welfare policies during the last half of the twentieth century. The case studies are based on existing political science literature interspersed with references to statements of various political actors involved in formulating different countries’ tax and welfare policies, made in interviews with the author. None of the chapters contains formal quantitative analysis; they rely on qualitative descriptions of detailed minutiae of the policy-making process, with emphasis on the history of tax legislation. Chapter 2 presents case studies of Sweden, the United Kingdom, and France. Chapter 3 presents case studies of Australia, New Zealand, Canada, and the United States. Chapter 4 presents a detailed case study of Japan with a brief overview of South Korean policy (and mention of Taiwan) for comparative purposes. But prior to the case study, the chapter looks at data on the cross-national patterns in tax revenues and welfare expenditures for nine newly developed or developing nations (Czech Republic, Greece, Hungary, Mexico, Poland, Portugal, South Korea, Spain, and Turkey) during the last third of the twentieth century, indicating that the relative positions of most remained the same between 1965 and 2000. Chapter 4 also looks at when VAT taxes were introduced and its rate in nearly three dozen Latin American, African, and former Eastern Block countries. The detailed case study of Japan and overview of South Korea are then presented.

The case studies overall are well done for what they are: detailed political histories of the minutiae of tax and welfare policies relying on secondary literature supported with a few interviews of political actors involved. The cases contain little or no economic analysis, though. And readers will have to determine how much credence they wish to give to the recall and rationale given by policy makers for particular tax or welfare legislation.

The concluding chapter 5 presents (1) an overview of financing the welfare state, including why some countries were outliers, (2) income inequality data indicating heavy reliance on regressive VAT revenues does not work against equality (because the revenues are used for redistribution), and (3) public opinion data on tax and public spending policies in the eight case-study-countries that in the author’s opinion offer some support for her hypotheses.

Overall, depending upon their interest, readers may find this book worth a look.

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Q Agricultural and Natural Resource Economics – Environmental and Ecological Economics


Is it progress if a cannibal uses knife and fork?
—Stanislaw Lec, Unkempt Thoughts

The preceding quote captures the essence of Priceless, a remarkably well-written book by an economist (Ackerman) and an environmental law professor (Heinzerling), hereafter AH. Through many anecdotes, AH argue that benefit–cost analysis is fatally flawed for public sector decisions, frequently resulting in outcomes that sensible people find both bizarre and inappropriate. The typical activist will find the book very appealing and persuasive, while the typical economist will likely be uncomfortable with both the arguments and the emotional tone. There are good reasons for both reactions. However, a careful reading of AH should reduce smugness on the part of economists who see nothing wrong with applied benefit–cost analysis.

In part, environmental and health activists will like Priceless because it places great emphasis on

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the very real problems of valuing outcomes in the public sector. What, for example, is it “worth” when a policy changes the probability of death? AH conclude, from a nice discussion of the issues involved, that many such outcomes are fundamentally impossible to price in the way that is required for benefit–cost analysis. Activists will also likely enjoy AH’s barbs aimed at those attempting such valuation (e.g., Kip Viscusi’s VSL measures). AH argue (p. 234) instead for “an attitude rather than an algorithm,” claiming marginal willingness-to-pay to be impossibly difficult to quantify in practice.

Activists will also share AH’s equity views which further undermine the policy relevance of aggregated marginal willingness-to-pay in public sector decision making. The impoverished of the world have a smaller marginal willingness-to-pay for anything, including policies protecting their health and environments, than do the rich (Larry Summers being on AH’s hot seat here). To attempt to provide public good levels and locations (for location-specific public goods) as they would be provided by a perfectly functioning private market, were it able to exist, is taken by AH to be unfair.

I approached the reading of Priceless with hope since I also have very strong misgivings about benefit–cost analysis as it is currently practiced. For example, benefits might be dramatically understated because there is no incentive to generate income if you cannot get more of what you want by doing so (see Graves 2002 at http://spot.colorado.edu/~gravesp/GravesRevtext.htm for details). That is, if what you care about are ordinary private goods, you will know that you can acquire them if you generate the income to do so. However, to the extent that you care about goods such as clean air, species preservation, CO₂ reduction, expanded wilderness areas and the like, generating income does not enable you to get what you want; since leisure is valuable, you will therefore undergenerate income. You will look, to economists, like you have little marginal willingness-to-pay, despite very large public good valuations. Hence, the income levels at which benefit–cost analysis is being conducted are too low—and all of the ungenerated income would have been spent on public goods, apart from general equilibrium effects.

It was arguments akin to the preceding I was hoping to find more of in Priceless. Illustrating further, what is the appropriate jurisdiction over which the benefits and costs should be calculated? Americans care about whether the giant panda is preserved, but China is unlikely to consider our preferences (CO₂ abatement provides a particularly thorny example of jurisdictional problems). The distinction, in the case of air quality, between primary and secondary standards is similarly irrational (one should add up all the benefits a given policy generates to compare them to the costs, not just a portion of them). Damages to other species that humans do care about are seldom included in the benefit calculations of an environmental policy. That benefits for normal public goods will grow over time due to both rising income and rising population is typically ignored in practice, yet the combined effects could easily offset discounting impacts (AH discuss discounting in a chapter called “Honey, I Shrank the Future”). And, of course, the magnitude of the physical effects are in many cases as uncertain as the values to be attached to them, the latter being the specific concern of AH. Yes, there are certainly a great many problems in the conduct of benefit–cost analysis.

Ken Boulding used to speak of the “tragedy of the radical,” arguing that even valid criticism of the established way of doing things is of little value in the absence of a preferred alternative. This is the problem that many mainstream economists will have with AH’s Priceless. We have to make decisions. That some of these decisions are difficult does not alter the fact that they are necessary, as a matter of scarcity. The essence of rationality is to compare the advantages with the disadvantages of alternative courses of action, pursuing those with highest net advantage. The use of dollars, per se, is of no consequence—real, physical effects either are or are not going to occur as a result of a public policy decision. Some of the benefits and most of the costs automatically come in dollar terms, making dollars the most convenient unit of account to gauge the (inevitably occurring) advantages and disadvantages. Attempting this calculation, particularly with sensitivity analysis to alternative values when they are highly uncertain, allows at least rough comparison/ranking of projects. This is necessary to prioritize the virtually limitless list of competing health and environmental projects, something that having the right “attitude” does not allow.
AH would retort, with some empirical justification, that tax cuts and military spending decisions are not made on benefit–cost grounds, so decisions about health and the environment should not be made that way either. This is particularly so if political tampering renders objective benefit–cost analysis unlikely. Moreover, AH would assert that many very good decisions were made without reliance on benefit–cost analysis in the 1970s and 1980s.

Many traditional economists will still feel that AH “throw the baby out with the bath water” in arguing that benefit–cost analysis should be scrapped, rather than greatly improved. But Priceless makes some important arguments that deserve greater discussion within the economics profession.

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Economists with research interests in the environment are used to evaluating and defending the relevance of economic theory as it applies to environmental problem-solving. In The Wealth of Nature: How Mainstream Economics Has Failed the Environment, Robert Nadeau assembles a pointed attack on the ability of neoclassical economics to ever contribute to the resolution of environmental problems. The crux of Nadeau’s argument is that there is a fundamental disconnect between the assumptions embedded in neoclassical theory (mostly relating to the presumption that individual decisions lead to socially optimal outcomes) and the functioning of natural systems.

While the carefully researched historical account of the foundations of neoclassical theory is effectively and convincingly articulated in this book, I found the author’s primary thesis to be deficient in two ways. First, and most importantly, the book pays scant attention to important innovations by environmental economists over the past several decades that address the economic and environmental problems associated with public goods provision and externalities. For example, scholars spend careers developing economic approaches for balancing the costs and benefits of environmental protection, a need that arises precisely because many environmental goods are not traded in traditional markets. Contrary to claims in the text, while imputing a monetary value to those goods poses a challenge, most economists do not dispute the existence of their value. Second, the book provides a paucity of evidence that economics has, in fact, “failed the environment.” For these reasons, the book reads more like a criticism of the ability of the free market to solve environmental problems than either (1) a detailed account of how economic reasoning has lead to deleterious environmental outcomes or (2) a careful analysis of why economic theory is inherently inapplicable to the environment.

Nadeau does strike an appropriate and persuasive balance between the details of the origins of neoclassical economic theory and the relevance of that theory to environmental problem-solving. The author’s primary focus on the inconsistency between parts (individuals acting in a market system) and wholes (market outcomes) of free-market operations vs. nature is well articulated in the introduction and well supported throughout the text.

The book begins with an overview of the origins of neoclassical economic theory that highlights several thought-provoking but rhetorical assumptions about mainstream economics, such as “the external environment is a bottomless sink for waste materials and pollutants” (p. 9). Underscoring the physical science foundations of economics, the book turns to a discussion of free-market operations and makes some tenuous references to the “environmental crisis” (e.g., p. 100) and the fundamental inability of neoclassical theory to contribute to its solution. To his credit, Nadeau acknowledges the (mainstream) field of environmental economics, although I found his description to be somewhat thin given its relevance to his main thesis. For example, Nadeau writes: “The power of a perfectly functioning market rests in its decentralized process of decision-making and exchange; no omnipotent planner is needed to allocate resources” (p. 116). While certainly true, this sentence reflects Nadeau’s tone throughout that under no circumstances would economists view market intervention as sound policy. This is simply not the case, as nearly all economists would
agree that intervention may be justified when markets are incomplete.

Indeed, most university courses on environmental economics begin with a derivation of the First Fundamental Welfare Theorem. The second day of the same course typically illustrates that public goods and externalities (with associated environmental problems such as biodiversity loss, fishery collapse, or pollution) are inconsistent with the assumptions of this theorem. The remainder of this canonical course is then devoted to deriving efficient economic instruments to internalize these external costs. Focusing largely on the inability of free markets to efficiently allocate environmental resources, Nadeau provides insufficient discussion of the opportunities for market-based solutions to environmental problems, though the text is clear that “neoclassical economic theory cannot in principle realistically account for the costs of doing business in the global environment” (p. 17). The fact is that a large fraction of the research in environmental economics is now devoted to deriving market-based solutions to environmental problems. And while work is yet to be done, the past successes of economists should not be trivialized.

Nadeau concludes the book by proposing a “new theory of economics” (p. 185) that presumably could account for the environmental costs of economic activity. In addition to other features, his theory contains (1) a role for government in environmental decision-making, (2) scientifically valid measures of the relationship between economic activity and ecosystem function, (3) an ability to inform decision-making under uncertainty about environmental outcomes, and (4) fees that would reflect the environmental damage associated with production or consumption. Nadeau never acknowledges that these are already central features of contemporary environmental economics.

Despite my critique, Nadeau's account of neoclassical economic history and theory, in particular as it applies to part/whole relationships and their links to environmental problem-solving, make reading this book worthwhile for economists who have an interest in the environment. I suspect that the book will be welcomed by a broader environmental community already skeptical of the ability of economics to improve efficiency, much less environmental outcomes.

I enjoyed the rhetorical criticism delivered by The Wealth of Nature, whose subtitle might be more accurately rewritten as How the Free-Market Might Fail the Environment. But, to the extent that environmental problems merely reflect public goods and/or externalities, this thesis would be wholly consistent with contemporary thought in mainstream economics.

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JEL 2004–0400

In the often fractious debate surrounding climate policy, it is easy to lose sight of the broad agreement among economists of all methodological persuasions that governmental action to mitigate global warming is warranted. Most economists recognize the need for action to reduce greenhouse gas emissions, because responsible earth scientists are concerned that global climate change is a very serious problem indeed. Hirofumi Uzawa's Economic Theory and Global Warming is a significant contribution to the economic consensus.

Uzawa's approach is thoroughly neoclassical. His analysis falls strictly in the tradition of optimizing agents operating with full information in perfectly competitive markets. Greenhouse gas emissions are a global public bad that have an increasingly negative impact (as atmospheric concentrations approach the point of “irrevocable damage on the global environment” (p. 25)) on the utilities of the representative consumers who make up each of the nations of the world. Uzawa shows how a system of emissions taxes or tradable permits would increase world welfare, given minimal assumptions about the size and shape of the global warming impact factor.

The book's introduction lays out the main scientific facts about climate change, and reviews some of the political and institutional responses to date. Here and elsewhere, Uzawa is keenly aware of the equity issues that are central to the climate problem. He notes the discrepancy between those of the current generation who benefit from costless emissions of greenhouse gases and the future generations who will suffer the worst consequences of those emissions, as well as the disparity between...
the wealthy countries whose economic growth has been built to some degree on fossil energy and the poor countries that have not made as great a contribution to the buildup of greenhouse gases in the atmosphere.

Most of the book is devoted to extensions of the basic model that is set forth in chapter 1. In the first part of this chapter, Uzawa shows that the Nash equilibrium with differential carbon taxes is characterized by taxes proportional to national incomes. The remainder of chapter 1, along with chapters 2 and 3, is devoted to exploring the consequences of a globally uniform carbon tax (or emissions permit system), and the circumstances under which such a framework produces a Lindahl equilibrium.

Uzawa finds the Lindahl equilibrium appealing on welfare grounds because it "corresponds to the situation in which the present level . . . of total CO₂ emissions is exactly equal to the level that would be chosen by each country . . . when it would be free to choose the most desirable level on the assumption that the price to be paid would be equal to that country's marginal disutility" (pp. 45–46). He shows that a Lindahl equilibrium exists, and that such an equilibrium can be reached by a suitable allocation of tradable emissions permits. The rub is that the allocation that achieves the Lindahl equilibrium is one in which the permits are given out in proportion to the countries’ national incomes, an outcome that "has a tendency to reinforce, rather than mitigate, the inequality that exists for the initial distribution of welfare among individual members of the society" (p. 62). Of course, the Lindahl criterion is not the only possible equity standard, and Uzawa later (in the summary and concluding notes) advocates creation of an International Fund for Atmospheric Stabilization under which a fraction of the emissions tax or permit revenues would be transferred from the richer to the poorer countries to promote development and assist in environmental protection.

In chapters 4 and 5, Uzawa addresses the question of sustainability under dynamic conditions.

He notes that it is unsatisfactory to regard the welfare of future generations purely from the perspective of the present, then solves this problem by "defin[ing] the concept of sustainability in terms of imputed price. That is, dynamic processes involving social overhead capital such as forests, the oceans, and the atmosphere are sustainable when each member of the society tries to ensure that the imputed prices of the various kinds of social overhead capital remain constant over time" (p. 155). This neatly enables Uzawa to add the condition of sustainability to dynamic growth models.

The other equity problem, between countries of different income levels, is touched on in chapter 6 (Global Warming and Forests), the only part of the book containing numerical calculations. Using a parameterized version of the climate impact function, Uzawa estimates the imputed carbon price and corresponding per capita carbon tax assessment for a number of countries. For example, the imputed carbon price for the United States is $319/ton with a per capita carbon tax assessment of $1,700 per annum; for a low-income country like Indonesia the imputed carbon price is $6/ton with a per capita assessment of $2 per annum. The calculations are based on the theoretical model, so the imputed carbon prices are proportional to per capita national incomes.

It would be interesting to see how the results might change under assumptions less strong than those made by Uzawa—that the utility functions are strongly separable in consumption and greenhouse gas emissions or atmospheric concentrations; that the climate change impact index is the same across countries; and that capital accumulation exhibits the Penrose effect. Questions like these should not obscure Uzawa’s contribution, however; this book demonstrates that, even in a purely neoclassical world, careful economic analysis affirms the need for action to protect our planet’s climate.

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