

Book Reviews

The Color of Credit: Mortgage Discrimination, Research Methodology, and Fair-Lending Enforcement, by Stephen L. Ross and John Yinger. Cambridge, Mass., and London: The MIT Press. 2002. Cloth, ISBN 0262182289, \$39.95. 432 pages.

Stephen Ross and John Yinger have published an important book of interest to academics and others doing theoretical, econometric, and fair-lending enforcement work related to mortgage-lending discrimination. Although much of the most technical material has been relegated to appendices, this is a scholarly tome. The general reader, uninitiated in basic probit regression analysis, would likely be better served by dipping into this literature elsewhere. Despite this caveat, even the econometrically challenged and those new to the subject matter can find much of value in the opening three chapters, which introduce and define the issues at stake while creating a framework for thinking about the mortgage-lending process. Similarly, those interested in the implications of this research for fair-lending enforcement will benefit from the final chapter, which addresses this topic. For those of us toiling in this research area, the entire book is required reading. In short, *The Color of Credit* seems destined to become as famous and controversial as the seminal studies it cites, re-evaluates, and critiques. It will become the starting point for a new generation of scholars interested in mortgage-lending discrimination as well as for the citizen activists and public officials who hope to improve our fair-lending enforcement system.

Over the course of ten chapters, Ross and Yinger make three very important contributions to the literature on mortgage-lending discrimination. First, they undertake a scrupulous re-evaluation of the famous Federal Reserve Bank of Boston study (Munnell, Browne, McEneaney, and Tootell 1996) as well as a comprehensive and painstaking review of its many critics. After careful consideration, Munnell et al.'s key finding—that minority mortgage loan applications in the Boston area in 1990 were rejected 18.2 percent of the time, while those of whites with similar incomes, credit histories, and other control variables were rejected only 10 percent of the time—is vindicated. Ross and Yinger summarize, “The large differences in loan approval between minority and white applicants that are identified by Munnell et al. cannot be explained by data errors, omitted variables, or the endogeneity of loan terms” (p. 163). They concede that no study is “irrefutable” but go on to say that the Boston Fed study strongly suggests racial discrimination on behalf of Boston’s mortgage lenders in 1990.

Ross and Yinger's second major contribution involves carefully describing and fatally critiquing the default approach to testing for mortgage-lending discrimination used by Gary Becker and others. Becker's approach was first put forward as an attempt to discredit the Boston Fed study and as the best test for discrimination. To many, it apparently seemed appealing to (1) view discrimination as holding minority applicants to higher underwriting standards than whites and (2) to assess this view by simply comparing whether these apparently higher quality minority borrowers default less often than white borrowers. Unfortunately for this approach to detect discrimination, some underwriting variables must be omitted. Omission of these variables creates biased results when the variables are correlated with minority status. As a result, the authors forcefully assert that "the available evidence overwhelmingly supports the proposition that it is difficult, if not impossible, to provide credible evidence about discrimination in mortgage lending based on data about defaults alone" (p. 272).

The authors' third major contribution is to show how easy it is for an apparently race-neutral credit-scoring system to generate adverse impact discrimination against minority applicants. To make their case, Ross and Yinger take the reader through a series of carefully described predicted loan performance equations. They show that if a lender attempts to avoid discrimination by naively dropping minority race from the equation—a variable that may capture important unobserved characteristics—they will in fact be doing the opposite. The attempt to *explicitly* remove bias by dropping the minority race variable from a credit-scoring technique will in fact create bias by *implicitly* bringing group membership back into the model and spreading out its affects across the remaining variables. The authors show that this bias will adversely affect minority group members and that the action cannot be justified on the grounds of business necessity. To further strengthen their case, the authors use a simulation technique to estimate the potentially large magnitude of the bias.

The book is not without its faults, especially from a heterodox economist perspective. Despite the dust jacket's claim to be "the most comprehensive review to date of what we know and what we do not know about mortgage-lending discrimination," the authors do not pretend to provide an exhaustive review of the literature on mortgage lending. Instead they focus on articles that best illustrate or deal with issues of bias created by omitted variables, sample selection, endogeneity, and the methodological problems caused by differences in underwriting standards across lenders. Thus, the authors fail to cite a single article published in this journal or any of its heterodox peers. Scholars who provide a political economy perspective on this issue—such as James Campen, Gary Dymski, Anne Shlay, and Richard Williams—are not to be found in the book's references. For reviews inclusive of this more heterodox literature see Dymski 1999 or Nesiba 1996.

These omissions are important for readers of this journal because the study of mortgage-lending discrimination seems particularly amenable to institutionalist research methods. Ross and Yinger unknowingly confess as much. At the beginning of chapter 2 (p. 15, emphasis added) they say, "Any analysis of mortgage lending discrim-

ination must be based on an *understanding of the institutions and practices* in the mortgage market, of recent *changes in those institutions and practices*, and of the legal definition of discrimination.” Unfortunately they do not say very much about how one goes about gaining this understanding. Most likely it requires a research approach that is holistic, that employs participant observation, and whose case study results can be used to describe patterns of best practice. This kind of research would serve as an important complement to the research done by scholars such as Ross and Yinger; it would deepen our understanding of mortgage market institutions and practices; and it would provide constructive guidance to lenders who wish to increase their lending to minority and low-income individuals and neighborhoods.

Reynold F. Nesiba
Augustana College, Sioux Falls, South Dakota

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Networking Knowledge for Information Societies: Institutions and Intervention, edited by Robin Mansell, Rohan Samarajiva, and Amy Mahan. Delft, the Netherlands: Delft University Press. ISBN 9040723001, \$77.50. 405 pages.

This book is a *Festschrift* for William Melody, who retired in 2002 as professor of economics of infrastructures at the Delft University of Technology in the Netherlands. Melody is an extraordinary institutional economist, and this volume is a testament to his contributions in the economics of information. Educated in the United States at the University of Nebraska and chief economist at the Federal Communications Commission early in his career, Melody held positions in Canada, the Annenberg School for Communications at the University of Pennsylvania, Simon Fraser University, the United Kingdom, Australia, and the Netherlands. In retirement, Melody continues as guest professor at the Technical University in Denmark.

This brief biographical sketch, coupled with the more than fifty wide-ranging contributors to this volume, is indicative of the impact Melody's work has had across disciplines and national borders. Though it seems unlikely, anyone who has somehow missed the work of Melody should forthwith become familiar with his contributions. This volume is a good starting point, but it will lead the reader to seek out Melody's published work (including articles in this journal).

The organization of the book may at first appear strange. The volume is divided into five parts: Inquiry, Change, Next, Bias, and What. As the editors note in the introduction, these "are chosen to encapsulate the central ideas in Melody's contributions to the creation, networking and application of knowledge about interactions between changing technologies and societies" (p. 8).

The section *Inquiry* comprises six articles centered on the issues of the role of universities in the information society and Melody's commitment to the idea that universities must inform the public policy process through research and teaching activities. This theme has increasingly become a focus of policy makers in the face of budget cuts and retrenchments in higher education.

The thirteen articles in the next section, *Change*, focus on Melody's analysis of the social and economic dynamics of the institutions through which people are mobilized into action. Harry Trebing contributes an article on a critical evaluation of utility deregulation in the United States. Several articles in this section focus on aspects of the telecom industry as well as the doctrine of natural monopoly and issues of privatization.

The *Next* section includes eleven articles centered on the emergence of information societies in a period characterized by the rapid spread of digital technologies. The articles in this section focus on the information society and its impact on a variety of cultures. The possibilities of stabilizing and destabilizing factors are the focus of this section.

The *Bias* section has eight articles. *Bias* means the emphasis on one aspect of an issue to the neglect of another. The term is used by Harold Innis, the Canadian political economist and historian, to describe distortions in economies that giving varying emphases to space and time and to different modes of information exchange and communication. In his work, Melody analyzed systemic biases in power relationships that become embedded in technologies and institutions in modern societies.

The *What* section is nine articles which concern the content of media and the possibilities for alternative media using older and newer technologies. Melody was long concerned with the impact of advertising on children and the purveying of information wares that are bought and sold in the marketplace and are circulated under arrangements that do not depend on the price mechanism.

This short review cannot do justice to either Melody's contributions or to the wealth of knowledge contained in this book. The issues dealt with in this volume are those that are at the heart of the institutionalist analysis of economy and society: the relationship of theory and policy, social stability, non-market resource allocation, the normative nature of economics, and the role of technology. This book, along with the

works of Melody, should be in all libraries, and scholars across numerous disciplines will find it an invaluable reference.

Ronnie J. Phillips
Colorado State University

Insatiable Is Not Sustainable, by Doug Brown. Westport, Conn.: Praeger Publishers. 2002. ISBN 0275974162, \$22.00. 232 pages.

As the title of this book suggests, an economy based upon attempts to satisfy insatiable wants is doomed to be unsustainable. Doug Brown argues that human society has gone through two major cultural paradigms: the culture of security and the current culture of insatiable freedom. He feels that the culture of insatiable freedom, epitomized by the well known “be all that you can be” slogan of U.S. Army recruiters, is the source of most of humanity’s social, economic, and environmental problems. He claims the only hope for the survival of modern society is an evolution into a third major paradigm: the culture of sustainability.

The first part of the book is an anthropological and historical look at how human societies and economic systems evolved from what Marshall Sahlins has categorized, as a culture of security, where the survival of the group (and individuals within it) was paramount to the culture of insatiable freedom, where the obsession to have more (and be more) dominates. Brown traces the origins of the latter culture to the Neolithic Revolution (10,000–8,000 B.C.), when agricultural surpluses and class differences contributed to the first awareness of the potential “to have more and to be more.” But the possibilities of economic growth and self-development were constrained by the social norms of the culture of security. Their full development would await the shift to a cultural paradigm of insatiable freedom associated with the capitalist economic system.

In the second part of the book, Brown explains the causes and consequences of this shift. He draws largely on Karl Polanyi’s framework in *The Great Transformation* that identified pre-capitalist economic systems as “embedded” within a larger social framework, while the capitalist economic system has been “disembedded” from this larger social framework. Brown notes, “With the emergence of capitalism in the sixteenth century economic activity was disembedded from the Culture of Security, and not only did this happen, but the old culture was replaced by a new one based on freedom, and buying and selling in markets and motivated by the individual’s desire both to avoid insecurity and to get ahead” (p. 67). Furthermore, the insecurity of this new market system makes this freedom (to compete in order to survive) an imperative that gets generalized to the culture as a whole. As a result,

[a]ll have the freedom to try to close the gap between what they are and what they could be. Each has the freedom to pursue insatiable self-realization. Each is

free to Be All You Can Be. Unlimited self-development is a real possibility for all who want it.

. . . Being more is the imperative pivotal to the functioning of capitalism. The insatiable self is both a product of capitalism's emergence and a cause. So there is both a material and ontological component with the emergence of capitalism. It allows for the pursuit of being more; it coerces the pursuit of being more; and it channels this into the realm of having more. The market economy, in other words, directly links the unlimited potential to be more to the insatiable desire to have more. (pp. 90-91)

Brown goes on to argue that both Karl Marx and John Stuart Mill were nineteenth century advocates of the insatiable self. In fact any thinker who recommends human self-improvement is seen as a supporter of the "Be All You Can Be" philosophy. Therefore, it comes as no surprise that Brown's analysis of the major problems of the early twenty-first century (poverty, inequality, environmental degradation) can all be traced to what he summarizes as the "Culture of Insatiable Freedom."

The solution to all these problems, presented in the third part of the book is, not surprisingly, the transition to a culture of sustainability. It is not clear how such a transition would take place, or what economic system(s) would support such a culture. However, Brown's professed role models for behavior in this new culture, discussed at length (chap. 6), are Mahatma Ghandi and Zorba the Greek!

I read this book expecting to be sympathetic to its major thesis that insatiable is not sustainable, yet I ended up being disappointed by what turned out to be a repetitive and often superficial series of proofs by assertion. Despite the book's often hectoring tone, no new insights were presented that would aid in understanding or solving economic or environmental problems. As a historian of economic thought, I also was annoyed by the presentation of the purported "insatiable self" views of Mill and Marx that were unconvincing and based almost entirely on secondary sources. I think one problem with the book in general may be that its intended audience is not clear. It is too academic in nature for a general audience, yet too popular in nature for an academic audience.

Stewart Long
California State University, Fullerton

The Greening of Business in Developing Countries: Rhetoric, Reality, and Prospects, edited by Peter Utting. London: Zed Books. 2002. Cloth, ISBN 1842770888, \$69.95; paper, \$27.50. 312 pages.

The behavior and consequences of business development (particularly transnational corporations) in the developing world is a topic of great interest to readers of this journal. During the past decade, there have been claims that such business is greening. Many

multinational corporations, for instance, have taken up the sustainable development banner. The extent of such corporate greening is the subject of a collection of essays assembled by Peter Utting.

The book is divided into two parts, each with five chapters. The first half is devoted to the empirical question of whether there really has been a greening. The chapters tend to be case study oriented and focus on specific studies of Mexican business movements, bioprospecting in Costa Rica, the Brazilian pulp industry, and corporate responsibility in Malaysia and Singapore. The second half of the book asks what types of policies can promote and enhance greening.

A major limitation of this book is its expansive subject matter. "Business" and "green" are too broadly defined to provide adequate focus for the book. The reader is constantly required to deal with shifting frames of reference between and within chapters. Discussions jump from international environmental technology transfer to environmental performance of transnational firms to the political activities and economic behavior of domestic firms and to the validity of corporate PR campaigns. Each topic is worthy of detailed conceptual and empirical analysis, and, in this sense, the book tries to do too much.

Taken separately, the individual chapters are of mixed quality. Some chapters focus on specific topics and present interesting pieces of empirical and conceptual research. These chapters succeed in highlighting the complexity and richness of a specific topic and leave the reader more curious and intrigued about the topic than they were before reading the chapter. Two examples of such chapters include "Environmental Management as an Indicator of Business Responsibility in Central America" by Lawrence Pratt and Emily D. Fintel and "Environmental Regulation of Transnational Corporations" by Michael Hansen.

Pratt and Fintel describe the results of a Latin American Center for Competitiveness and Sustainable Development study aimed at assessing the level of environmental efforts undertaken by large domestic and international firms in the region (part 1 of the book). While clearly discussing the challenges of coming up with meaningful indicators of environmental performance, the study finds evidence of low levels of corporate attention paid to environmental performance. These low levels, however, were set against interesting differences among firms. For instance, firms with strong international linkages are more likely to have active environmental programs than domestic firms. These findings raise interesting questions about the reasons for these observed patterns. While the study makes no systematic effort to isolate cause and effect, the authors do offer a variety of possible hypotheses as to why these patterns might exist. These reasons, ranging from the corporate structure to the unique political and cultural conditions in the region, offer a number of interesting lines of future research.

Hansen assesses the needs and merits of international regulation of transnational corporations (in part 2 of the book). The chapter begins with a historical summary of international efforts to create such a framework. Hansen then goes on to describe the

arguments and evidence of supporters and opponents of greater international regulation. While Hansen eventually argues for greater international regulation, the discussion succinctly and fairly synthesizes the complexities of the debate. This chapter would serve as good introductory reading in a course that investigates the growth and influence of transnational corporations.

Set against these chapters are others with clear ideological axes to grind. These chapters are more successful at inciting ideological passions than engaging the mind. They become tiresome reads where overzealous indignation raises more questions about the credibility of the analysis than it answers about the subject of corporate greening.

Perhaps the best example of such a chapter is “Disturbing Development: Conflicts between Corporate Environmentalism, the International Economic Order, and Sustainability” by Richard Welford. In the first half of the chapter, Welford rails against the growing chasm between the “international economic order” and “sustainable development.” Welford paints a caricature of the transnational corporate world. Such a picture can only be painted by someone unencumbered by evidence (or measured thought) and blinded by self-righteous zeal. Despite the immense evils attributed to transnational corporations, the reader will be surprised to read in the second half of the chapter that the author believes a number of practical policies can readily be “operationalized” to bridge this immense chasm. No radical restructuring to the international economic order needed. Yet, most of the policy recommendations are as equally divorced from the nuances of the real world as the assessment of the problem. For instance, Welford asserts that “sustainable production” policies should be implemented. Sustainable production occurs when “the value of what is being produced outweighs the value of that which is destroyed.” The reader is given nothing more: value is never defined much less any discussion concerning how such an idea could be implemented. Other policies have similar vague, “feel good” qualities. The only policies that could be readily operationalized (eco-labeling and environmental taxes) are completely orthodox.

In the introductory essay, Utting laments that the problem with this field of inquiry “is not only the ideological and polarized nature of the debate, but also the lack of reliable data and analysis” and the evidence that is provided is “highly anecdotal” (p. 10). Indeed. Unfortunately, this book represents only a modest improvement over this status quo.

Kurt Stephenson
Virginia Tech

Richard T. Ely: The Story of Economics in the United States, edited by Warren J. Samuels. *Research in the History of Economic Thought and Methodology*, vol. 20-C. Oxford: Elsevier Science Ltd. 2002. Cloth, ISBN 0762307943, \$95.00. 322 pages.

Warren J. Samuels deserves great credit for bringing to publication Richard T. Ely's fascinating history of American economic thought, reproducing Ely's 1931 draft manuscript. Ely, the dean of American economists at that time, had worked on this project on and off over a span of roughly fifty years, producing various drafts, but never was satisfied it was sufficiently complete and publishable. Samuels opens the volume with an excellent fifteen-page editorial introduction. Then follows the body of Ely's draft: twelve chapters and two appendices.

Chapter 1 is introductory and focuses mainly on the nature of economics. Chapter 2 addresses the economic ideas of Benjamin Franklin, Thomas Paine, Thomas Jefferson, and Alexander Hamilton. Chapter 3 is devoted to the ideas of Daniel Raymond, Alexander Everett, and Matthew Carey. Chapter 4 discusses evolution of the scientific approach to study of economics. Chapter 5 applies that scientific approach to economic debate on the question of free trade versus protectionism, since the tariff issue tended to dominate concerns about economic policy for so long.

Chapter 6 may be of special interest to other institutional economists, as it was to me. Ely makes a case for Henry C. Carey as "the first American economist who developed an indigenous and fairly complete system of political economy" (p. 113), and I would argue this also makes Carey America's first institutional economist. Ely states that for more than forty years, Carey "gave himself up to study and writing and, in particular, to the development of his system of political and social science. Too much praise cannot be awarded him for this wise employment of his later years" (p. 117). Carey held against "laissez-faire dogmatism" (p. 127) and in so many ways was an institutionalist that Ely's labeling Carey as the first American economist logically makes Carey America's first institutional economist. Cogent arguments can be made on behalf of others; Ely's assessment and my own study of Carey's work cause me to favor Carey. This chapter also discusses notable associates and followers of Carey, such as Horace Greeley and Francis Bowen.

Chapter 7 summarizes the English classical school in the United States, especially the work of John McVickar, Thomas Cooper, Willard Phillips, Henry Vethake, Francis Wayland, and Amasa Walker. Chapter 8 briefly treats Francis Bowen again before concentrating on the work of Henry George and Simon Newcomb. Chapter 9 speaks to economic thought in the South, emphasizing the work of J. N. Cardozo, Thomas R. Dew, and George Tucker. Chapter 10 defines the new life in American economics leading to and emanating from the founding of the American Economic Association. Chapter 11 is devoted to Francis A. Walker, and Chapter 12 to Simon N. Patten and John Bates Clark. Appendix 1 deals with collateral subjects, mostly American economists whose work Ely regards as less important.

Appendix 2 updates the entire field with Ely's insightful treatment of contemporary (1931) American economics. In my opinion this ranks with his chapter on Carey as one of the two sections in the book of greatest potential interest and value to today's institutional economists. This appendix, while revealing that Ely stayed current and well informed into the 1930s, is nonetheless rather uneven in its treatment of contemporary economists. For example, though Thorstein Veblen and John R. Commons are recognized for recent contributions, Commons perhaps more so, Ely could and perhaps should have written more about the contributions of both men. I'll grant that the fame of Veblen and Commons grew after 1931 and, at that date, more extensive treatment may not have been clearly justified. Yet, I feel Veblen deserved fuller treatment.

The chief weakness in Ely's draft, in my judgment, is that it is not as encyclopedic a history in its coverage of American economic thinkers as it could have been. There were lawyers such as Van Buren Denslow, clergymen such as Grover P. Osborne, and a host of full-fledged economists, who contributed something of temporary or lasting importance to American economic thought, yet Ely slights them. Readers will find numerous such contributors among the barely mentioned or missing. Ely had wholly understandable reasons for highlighting the individual economists whose outstanding work he ably discusses, but I fail to understand why he under-recognizes so many others who in my judgment are due some credit. I suppose he was trying to keep his history sharply focused and manageable, and consequently he does a splendid job of treating the economists he selected for inclusion. Yet other noteworthy contributors have been under-recognized, and that's a lamentable tradeoff.

Ely's 1931 draft also lacked an index, making it difficult to readily locate mentions of specific economists in whom readers might be interested. So the curious reader must examine carefully the entire 322 pages. An index would be helpful, and Elsevier Science Ltd presumably could have added an index. Of course this would have increased the cost of production and presumably the price, possibly reducing sales, another lamentable yet more understandable tradeoff.

This 1931 draft was the version of Ely's history most readily available to Samuels. Some years ago Rick Tilman called attention to the existence of another and somewhat different draft dated June 1932. Tilman alluded to even later drafts as Ely apparently continued working on the history into the late 1930s. One of the important differences in the June 1932 draft is inclusion of a fuller treatment of Thorstein Veblen, which Ely attributes to Grace Jaffe and is the subject of Tilman's article (1987). If the June 1932 draft can be located again and it differs from the 1931 draft in many respects, the June 1932 draft also might be worth publishing as a volume. At least someone should write a journal article comparing and contrasting the two versions. Ely's evolving views on the history of American economic thought are of incomparable value, and again kudos to Samuels for publishing the 1931 draft.

Gerald F. Vaughn
Retired, University of Delaware

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A New Guide to Post Keynesian Economics, edited by Richard P. F. Holt and Steven Pressman. London and New York: Routledge. 2001. Paper, ISBN: 0415229820, \$34.95. 134 pages.

In the 1970s, macroeconomic troubles ended the dominance of neoclassical Keynesianism in the realms of both theory and policy. While Keynesian economists resigned themselves to an unpleasant policy tradeoff between inflation and unemployment, much of the industrialized world witnessed an acceleration of price hikes *and* joblessness. One result was the rise of monetarism, along with emergence of what would soon be called "new classical" macroeconomics. Another was the coming together of economists who felt that more realistic theories, especially those built on commonly overlooked insights contained in the writings of John Maynard Keynes, could point toward a restoration of prosperity without high inflation.

A Guide to Post Keynesian Economics, edited by Alfred S. Eichner (1978), was the manifesto of this latter group. The *Guide* helped inspire and bind a generation of economists critical of neoclassical theory. Since that book was published, however, much has changed in the economics profession and the world. That's why *A New Guide to Post Keynesian Economics*, edited by Richard P. F. Holt and Steven Pressman, is a welcome addition to the literature on contemporary political economy.

The *New Guide* retains most of the attractive features of the volume that Eichner organized. It seeks to offer "an up-to-date introduction to Post Keynesian thought" that is accessible to university students as well as to professional economists (p. 2). It begins with a foreword by a prominent, senior figure associated with this intellectual tradition (Geoff C. Harcourt in the new book, Joan Robinson in the original), followed by an introduction that traces the development of Post Keynesian scholarship. Each core chapter compares neoclassical and Post Keynesian ideas and offers a discussion of public policy implications. Each book ends with a forward-looking chapter that outlines some of the work to be done in the years ahead.

The bulk of the subjects covered in Eichner's collection are also found in the *New Guide*. Each volume contains essays on pricing, income distribution, tax incidence, labor markets, money and inflation, macrodynamics, and international economics. Although the new book departs from the original by offering no chapter on the work of Piero Sraffa, his critique of marginal productivity theory is incorporated into the *New Guide's* distribution chapter. Similarly, although the *New Guide* has no essay on production theory, a central element of that chapter in Eichner's collection is the notion that markets have no inherent tendency toward coherence; the same point is made in

the new book in a chapter on methodology, written by Sheila Dow. (In many cases, authors of the new volume—who include L. Randall Wray, Marc Lavoie, John Smithin, James K. Galbraith, and Mark Setterfield—“were students or proteges” of contributors to the first *Guide* (p. 2).)

In addition to methodology, the *New Guide* adds chapters on uncertainty and on the state. The uncertainty essay, by J. Barkley Rosser Jr., stresses the unpredictability of much economic activity, the structural economic changes that occur when an economy operates in historical time, and the group dynamics and social conventions that influence how expectations are formed. By chipping away at the assumptions of public-choice and rational-expectations theorists, Pressman’s chapter on the state revives Keynes’ notion that government can be a positive force in the effort to improve overall economic performance.

While institutionalists will find much that they agree with throughout the *New Guide*, they might be particularly interested to observe that the rapidly emerging methodological foundation of Post Keynesianism is thoroughly compatible with their own. Post Keynesians view the economy as an open and evolving social system—one that exists in a world of fallible knowledge, is ordered by institutions and cultural norms, and can be studied by using a range of research methods. This methodological common ground opens the door to a future rich with institutionalist and Post Keynesian discussions and collaborations. Post Keynesians acknowledge that they must incorporate a better understanding of institutions into their work (pp. 76 and 98, for example), and institutionalists should either have something to share or be prepared to join with the Post Keynesians in real-world explorations. The room for collaboration will widen even further if Post Keynesians begin to address issues mentioned in the final chapter by Holt and Pressman, including the challenges of maintaining an aging population, sustaining productivity growth, reducing crime and poverty, and improving education, health care, and the environment.

Despite its many virtues, the *New Guide* lacks a key element that made the Eichner volume so compelling and successful. All chapters in Eichner’s collection gave some attention to the central economic problem of its day, the end of the 25-year “long boom” experienced by many industrial nations after World War II and the emergence of stagflation. In fact, a broad description of the Post Keynesian analysis of that problem was laid out in the original book’s foreword. That collection’s clear focus on a major economic problem played an important role in motivating students to follow the book’s complex debates and analyses. The *New Guide* does not have such a unifying theme.

The timing of the new book explains part of the problem. The *New Guide* was organized in the late 1990s, as the United States was in the midst of an unprecedented expansion that boosted the stock market, reduced unemployment and poverty, increased worker incomes, and eased wage inequality. At the same time, price inflation remained under control, productivity growth was robust, technological innovation was swift, and the federal government was once again running budget surpluses. Unlike in

the late 1970s, the real-world economic problems of the late 1990s didn't seem all that severe to most citizens in the English-speaking world.

Nevertheless, editors of the *New Guide* could have emphasized any of a number of issues. One approach would have been to focus on the national and international policies needed to secure stability and widespread prosperity in an increasingly integrated global economy. That emphasis might have drawn attention to the widening gap between the worlds of finance and industry in the most advanced nations, as well as to the alarming financial fragility evident in developing parts of the world. The *New Guide* doesn't discuss the economic significance of the "irrational exuberance" of the 1990s, however. And its chapter on macrodynamics ignores Hyman P. Minsky's theory of capitalist development (see, for example, Minsky 1990 and 1993), and instead states that Post Keynesian theory "has thus far paid little attention" to the relationship between finance and economic growth (p. 97).

Another approach to bringing coherence to the new volume would have been to pursue Joan Robinson's (1972) suggestion that economists should address the "second crisis of economic theory." According to Robinson, getting government to accept responsibility for stabilizing the economy and maintaining high employment was a response to the first crisis of economic theory, which centered on problems linked to the *level* of economic activity. The second crisis, meanwhile, arose from the inability of economics to focus on problems related to the *content* of economic activity. The *New Guide*, however, says almost nothing on this issue.

A third approach to unifying the volume would have been to stress how the economy has changed since the 1970s. Eichner's collection described investment decisions and wage and price determination taking place in a largely closed-economy setting, where there was a distinct "dual economy" (a core of huge, oligopolistic companies and a periphery of small, competitive enterprises) and powerful unions. The *New Guide* does not adequately describe the subsequent changes in the economic environment, their impact on economic performance (such as inflation and productivity growth), and their implications for Post Keynesian theory and policy. The book is even silent on the erosion of automatic fiscal-policy stabilizers in the United States (see Whalen and Wenger 2002), which is surprising given the emphasis on demand management in Post Keynesian scholarship.

Does the *New Guide* lack a focus on one of these themes because Post Keynesians are unprepared to address them? I hope not, but I'm not sure. John E. King, author of the chapter on labor markets, agrees that important business and labor changes point toward a reconsideration of Post Keynesian theory; "Unfortunately, little serious thinking has been done on these issues so far," he concludes (p. 76). If Post Keynesianism is to have a future that warrants an update of the Holt and Pressman book twenty years hence, it's time to get serious.

Charles J. Whalen
Interactivity Foundation

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Capitalist Development in the Twentieth Century: An Evolutionary-Keynesian Analysis, by John Cornwall and Wendy Cornwall. Cambridge: Cambridge University Press. 2001. Cloth, ISBN 0521341493, \$60.00.

It is ironic that, having seemingly emerged triumphant from the global ideological struggles of the twentieth century, "capitalism" is seldom mentioned by name in the economics profession. And yet capitalism—more specifically, the advanced capitalism found in the world's richest nations—is undoubtedly the implicit object of most economic analysis. John and Wendy Cornwall are more inclined to call a spade a spade. Their reference to capitalism in the very title of this book is a mark of the refreshing candor and iconoclasm with which they regard the topic of their investigation and much of the "dismal science" that precedes them.

A central theme of the book is that capitalism is not inherently self-regulating. First and foremost, this involves repudiation of the notion that aggregate demand (AD) passively adjusts to assure the utilization of productive resources in accordance with supply-determined equilibrium rates. Cornwall and Cornwall extend this basic Keynesian insight by postulating that AD also influences the development and productivity of factor inputs. Put differently, AD determines not just the proximity of actual to potential output but also the size of potential output itself. Hence we observe "Say's law in reverse"—supply adjusting to accommodate demand—even in the long run, at least for some admissible range of AD growth rates (see also Cornwall 1972 and Palley 2002).

Cornwall and Cornwall also reject the idea that the very structure of a capitalist economy, in the context of which income is generated, can be taken for granted as something that is "natural" and given and that automatically creates an orderly environment for economic activity at *any* level. On the contrary, they argue that the orderliness of economic activity owes substantially to the socially constructed institutions that are an essential structural feature of a capitalist economy. Examining the ways in which these institutions are created, reproduced, and destroyed is thus essential to understanding the workings and performance of capitalism.

These Keynesian and institutionalist themes are combined in an evolutionary framework of analysis in which macroeconomic outcomes arise from a dynamic, path-dependent process with no fixed equilibrium position. Specifically, a particular set of relatively enduring institutions gives rise to a discrete episode of better or worse macroeconomic performance by virtue of the influence of institutions on the Keynesian demand constraint that is the proximate determinant of performance. Essentially, institutions determine the extent to which an economy faces political, external (international trade and payments), and inflation constraints on its capacity to engineer the demand conditions conducive to rapid growth and full employment. But episodes of macroeconomic performance eventually have feedback effects on the very institutions from which they first arise. This causes institutional change that ultimately leads to a new episode of macroeconomic performance. In sum, institutions affect demand-constrained macroeconomic performance which affects institutions, in an ongoing, recursive fashion.

Cornwall and Cornwall then use this evolutionary-Keynesian model to explain the historical development of capitalism during the twentieth century. The inter-war years, the post-war Golden Age, and the period since 1973 are analyzed as three discrete episodes of alternating better or worse macroeconomic performance that are causally related to one another, in the sense that performance during each episode is shown to have induced institutional change that ultimately explains macroeconomic performance during the subsequent episode. Cornwall and Cornwall focus on the unemployment record when assessing actual capitalist macroeconomic performance, owing to their concern with the high social welfare losses associated with unemployment and the relative neglect of these welfare losses by economists and policy makers over the past thirty years. However, they also articulate a novel empirical model of productivity growth derived from their evolutionary-Keynesian framework that permits a subsidiary focus on per capita income growth in the course of their assessment of the post-war period.

As previously noted, a cornerstone of Cornwall and Cornwall's approach is that economic theory cannot legitimately begin by taking as given a competitive market "state of nature." Their analysis of recent capitalist history also reveals that trying to approximate this *laissez-faire* ideal is bad for capitalist macroeconomic performance. Instead, they show that episodes involving the greatest divergence from *laissez-faire* conditions have been associated with superior macroeconomic performance (for example, the Golden Age) and that during episodes of comparatively poor performance individual economies that least approximate the *laissez-faire* ideal fare best (for example, the Nordic economies during the period 1973–89). In particular, Cornwall and Cornwall emphasize the role of extra-market "social bargains" as a foundation for successful macroeconomic performance. These involve capital and labor entering into and maintaining a convention of "value sharing," according to which both parties respect a preconceived "fair" distribution of economic rewards. This relieves capitalism of the inflationary pressures that would otherwise result from rapid growth and full

employment, thus relaxing an important constraint on the demand conditions necessary to achieve these macroeconomic outcomes. But Cornwall and Cornwall stress that social bargains are not easily come by. The Golden Age social bargains were the product of two complementary pressures on the capitalist class (which, following Lindblom 1977, is regarded as the natural center of power in a capitalist economy, thanks to its ownership of the means of production and concomitant right to make autonomous decisions about where, when, and how to produce). The first of these pressures affected “liberal” capitalists and arose from the legacy of the Great Depression and World War II, the experience of which made a case for concessions to labor based on fairness. The second arose from the increased power of the labor movement and the rise of the Soviet Union, which compelled “conservative” capitalists to “concede and rule” in preference to risking extinction at the hands of an apparently viable alternative (non-capitalist) economic system.

In sum, social bargains are a necessary condition for the sort of egalitarian prosperity experienced during the Golden Age, and pressure on capital to concede some of its intrinsically asymmetric power vis-à-vis labor is a necessary condition for the creation of social bargains. This analysis ultimately sets Cornwall and Cornwall up for a bleak prognosis—as the authors themselves acknowledge in the final chapter of the book. After all, where is the evidence of hardship and self-sacrifice on the scale of the Great Depression and WWII that once convinced liberal capitalists that concessions to labor were fair? And in an age in which globalized capitalism is the “only game in town,” where is the strong labor movement and viable alternative economic system that once convinced conservative capitalists that concessions were a necessary act of self-preservation? Absent the pressures necessary for the reconstruction of social bargains, the prospects for a return to the egalitarian prosperity witnessed during the 1950s and 1960s seem remote indeed.

Cornwall and Cornwall are careful to compare and contrast their evolutionary-Keynesian framework with other traditions—including institutionalist economics—with which it shares important characteristics. One feature of their approach that should be of particular interest to readers of this journal is its combination of analytical and discursive methods of explication. The role of the former in economic analysis is contested by institutionalists, but Cornwall and Cornwall show how analytical techniques can be utilized without sacrificing the nonmechanistic vision of social processes associated with institutionalism.

The authors are to be congratulated for crafting a highly readable volume that combines a compelling account of recent capitalist history with contributions to the methods of evolutionary and Keynesian analyses. This book is essential reading for all who are interested in capitalist development and in the methods and application of non-mainstream macroeconomics.

Mark Setterfield
Trinity College, Connecticut

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Financial Markets, Money, and the Real World, by Paul Davidson. Northampton, Mass.: Edward Elgar. 2002. Cloth, ISBN 184064740X, \$95.00. 265 pages.

Financial Markets, Money, and the Real World is a compilation of much of Paul Davidson's most important work toward his lifelong goal of "resurrecting Keynes's analytical vision" (p. 3). Readers of Professor Davidson's previous books, most importantly *Money and the Real World* (1978) and *International Money and the Real World* (1992), are advised that the current volume contributes very substantial value added. For example, chapter 3 ("Uncertainty and Reality") contains a brilliant disquisition on the epistemological underpinnings of mainstream and heterodox models. The later chapters of the book present a fresh examination, from the Post Keynesian perspective, of contemporary issues such as persistent United States trade deficits, exchange rate volatility, destabilizing international capital flows, and proposals to reform the global financial architecture.

The distinctive feature of the economics we call Post Keynesian is a strong emphasis on the role of uncertainty in decisions to employ resources, to invest in new capital goods, or to store wealth. Following in the line of J. M. Keynes and G. L. S. Shackle, Davidson has taken the case for the non-equivalence of uncertainty and actuarial (or probabilistic) risk to a new level of cogency. He concurs with Paul Samuelson that the conceptual substructure of mainstream economics is supplied by the theory of stochastic processes, which treats an economic time series as a "finite realization" of an *ergodic* stochastic process. If economic reality is governed by ergodic stochastic processes, then

drawing a sample using past time series and/or current cross sectional market data is equivalent to drawing a sample from the universe of future market data. . . . Invoking the ergodic axiom means that the outcome at any future date is merely as statistical shadow of events that have already occurred; the future is written in today's historical "evidence." (p. 50)

The probability distributions that describe the future yield of investment, the prospects of war, or the job market for next year's Ph.D.s are, in an ergodic world, immutable or time independent. But in the real world agents take what Shackle (1955) called "crucial decisions"—that is, decisions that irrevocably alter the environment in which decision making takes place. Economic and social reality is nonergodic, creative or *transmutable* "in the sense that future economic outcomes may be permanently

changed in nature and substance by today's actions of individuals, groups (for example, unions, cartels and/or governments), often in ways not even perceived by the creators of change" (p. 52). In other words, *even if* future outcomes are accurately described by probability distributions, in a transmutable reality these distributions are *yet to be created* and hence not knowable at the moment when action must be taken. That the phenomena it seeks to understand inhabit a transmutable reality is the best definition I know of a social science.

International trade and finance issues dominate the second half of the book. In chapter 9, Davidson explains why exchange rate fluctuations are rarely effective in correcting trade imbalances. The critical assumption underlying the classical case for flexible exchange rate regimes—that is, near perfect substitutability between imports and exports, has been repeatedly disconfirmed by the evidence. Moreover, history shows that income changes dominate substitution effects with respect to current account balances.

The author moves on to digression on Thirwall's law, which demonstrates that the capacity of a nation's economy to expand without international payments problems is limited by the ratio of its trading partners' income elasticity of demand for its exports (e_{rw}) to the nation's income elasticity of demand for imports (e_a). If $(e_{rw})/(e_a) < 1$, and if there is a need to maintain balance of payments equilibrium, then the nation's growth rate must be less than that of its trading partners. The ratio is likely to be less than one for less-developed nations (LDCs) that export commodities with low income elasticities of demand. Davidson explains the implications:

[I]f the LDCs follow the conventional advice of classical economists and continue to develop only their comparative advantage industries and simultaneously try to maintain a position where the market value of imports just equals the market value of exports, then the LDCs are condemned to relative poverty, and the global inequality of income will become larger over time. (p. 160)

Chapters 12 and 13 are devoted to an examination of the deleterious effects of flexible exchange rates combined with free mobility of (financial) capital. Professor Davidson explicates Keynes's "incompatibility thesis" (that is, the incompatibility of flexible exchange rates, free capital mobility, and global full employment) and warns that "if the developed nations do not hang together on a currency-fire prevention system, then they may all hang separately in a replay of the international financial market crisis of the Great Depression" (p. 201).

A comprehensive proposal for reforming the international payments system is presented in chapter 14, which the author describes as an "updating of Keynes's original proposal for a postwar international monetary scheme" (p. 231). The key proviso is the creation of the international monetary clearing unit (IMCU), which would serve as the ultimate reserve asset for international liquidity. IMCUs could be held only by central banks of nations that comply with the agreed-upon rules of the clearing union. Central

banks would guarantee convertibility of ICMUs to domestic currency but not vice versa. One-way convertibility would give central banks the power to impose capital controls in the event of a speculative run on the domestic currency. The most intriguing aspect of the plan is a “trigger mechanism” which would force surplus countries to de-accumulate “excessive” credit balances with other members of the clearing union. The trigger mechanism explicitly targets industrialized nations that pursue export-led, beggar-thy-neighbor growth strategies.

In summary, *Financial Markets, Money, and the Real World* is a major achievement. Paul Davidson has in this book delivered a confutation to the dismissive claim about Post Keynesian economics that it offers nothing more than criticism of standard macroeconomics.

Christopher Brown
Arkansas State University

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The High Price of Materialism, by Tim Kasser. Cambridge: The MIT Press. 2002. Cloth, ISBN 026211268X, \$24.95. 149 pages.

Arguably, there is no more important economic issue than the one addressed in Tim Kasser’s book, *The High Price of Materialism*. Is it possible that materialistic values, while making us more productive, make us less happy? The implications for economics are sweeping: if the primacy of production is unwarranted, then the costs of greater leisure and equality, as well as those for individual, communal, social, and environmental safeguards, are routinely overstated. This is nothing new to institutionalists, but Kasser is a psychologist. There is no mention of the likes of John Kenneth Galbraith here. Instead, Kasser presents a fascinating and compelling array of psychological research that paints a frightening picture of materialism.

Kasser demonstrates links between materialism and such ills as dissatisfaction, anxiety, narcissism, pain, aggression, alienation, anti-social behavior, and self-destruction. These connections are reportedly robust and supported by researchers from around the world. A key feature of Kasser’s book is his use of experimental and other empirical research to support both his broader conclusions and each link in a psychological model explaining how materialism is harmful to people’s psychological well-being.

After a brief introduction, Kasser lays out the heart of his argument in the second chapter. This intriguing chapter could stand alone and be a useful resource for providing students with a counterargument to the assumptions and conclusions of

neoclassical consumer theory. One approach taken by Kasser and his colleagues was to use surveys to ascertain subjects' feelings about the importance of such things as financial success, status, and image as well as alternative values like self-acceptance, family, friends, and community. The results were used to construct an "Aspiration Index." Then, depending on the study, an additional survey, interviews with clinical psychologists, or diaries were used to gather information about subjects' behavior and well-being. These studies found high levels of materialism associated with low levels of self-actualization and vitality, along with high levels of depression, paranoia, and drug use. Materialistic subjects experienced fewer positive emotions, and, in general, materialistic pursuits "decreased the quality of their daily experience" (p. 12). Studies by others have found similar results for a variety of demographic groups in countries as different as Turkey, India, and China. In subsequent chapters, Kasser argues materialism harms people by interfering with their ability to satisfy fundamental psychological needs for safety, self-esteem, relationships, and autonomy. He takes these needs, each in turn, and points to empirical evidence demonstrating they are unmet in materialistic people.

Kasser identifies insecurity as a key cause of materialism. He points to studies showing higher rates of materialism in people whose childhoods were made less secure by broken homes, poverty, or war. Kasser and a colleague also performed experiments by artificially stimulating student's feelings of insecurity. Some students were asked to write about death, while students in a control group wrote about music. Both groups were then asked questions regarding their expected financial worth and consumption patterns fifteen years in the future. Those who had written about death expected to earn and spend more. For instance, compared with the control group, they expected to spend nearly twice as much on entertainment, leisure, and clothing (p. 41). Kasser argues this is evidence of a vicious cycle wherein "materialistic values are both a symptom of an underlying insecurity and a coping strategy taken on in an attempt to alleviate problems and satisfy needs" (p. 42).

Remaining unfulfilled psychological needs are more the consequence of materialism. Much of the considerable evidence suggests activities better suited for meeting psychological needs represent a high opportunity cost of materialistic pursuits. For example, one of Kasser's studies shows students' well-being hurt when they failed to make progress toward non-materialistic goals (like close relationships and personal growth), but it did not decline when they failed to meet their materialistic goals. Progress toward both types of goals was not really better than progress toward non-materialistic goals alone (pp. 44-46).

This book was written for a general audience. Academics and others will be frustrated by the aggravatingly casual way statistical relationships are discussed. There is almost no mention of the statistical significance or even the magnitude of these relationships. Presumably such information could be found in the many papers and books cited by Kasser. However, for a book based on empirical evidence, these omissions leave the reader with many questions.

Another problem with this type of research is the difficulty of categorizing people as materialistic. Different motivations could prompt similar responses, thereby making categorization ambiguous. To take one example, a desire to be one's own boss is interpreted as a desire for financial success and one indicator of materialism (p. 6). Clearly, the opposite could be true. Some might want to be their own boss so they could spend more time with family and escape the materialist demands of their current employer. While no response was sufficient, by itself, to categorize a subject as materialistic, there is enough ambiguity that researchers need to be very careful about any inferences they make.

Clearly, many economists will reject Kasser's argument. The anecdotal way he presents his findings makes the skeptic's job easier. Nevertheless, if economics is to be more science than religion, then we must continually critique our most fundamental precepts in light of our best scientific understanding. Kasser's book, in spite of its weaknesses, is a stark challenge to the central themes of neoclassical economics and to the very way we have organized our economy and society. It points to a wealth of psychological evidence that is consistent with the wisdom of the ages as well as much contemporary work being done by economists, sociologists, and others. At a minimum, Kasser's book should be met with further research. If such research corroborates the findings in *The High Price of Materialism*, what is called for is nothing less than a fundamental reorientation of society. This is an important area for inquiry, and Kasser's book is a good step in our rather slow start.

Terrel Gallaway
Southwest Missouri State University

Technological Revolutions and Financial Capital: The Dynamics of Bubbles and Golden Ages, by Carlota Perez. Edward Elgar. 2002. ISBN 1840649224. 182 pages + index.

If you are interested in business and economic cycles and in explanations of what shapes their patterns, this book is for you. The author, Carlota Perez, is one of the best-known consultants and researchers in the field of techno-economic paradigm shift. She does not provide here her first contribution to the understanding of long-term technological transformations; she goes a step further. She examines the interaction between financial capital and the upsurge of new technologies from their first beginnings to the time when they predominate in the structure and behavior of the economy.

Her study is based on Joseph Schumpeter's work (1939) and its notion of creative destruction. This Austrian economist, politician, and banker saw innovation not only as the force propelling progress but also as the cause of recurring recessions and in general of the cyclical behavior of growth rates and other economic magnitude. More importantly, for Perez's point of view, while Schumpeter interpreted the major waves of economic growth and technological transformation as "successive industrial rev-

olutions,” he insisted that these clusters of radical innovations also depended on financial capital.

Following Schumpeter’s argument, *Technological Revolutions and Financial Capital* is a journey through the creation of a cycle model based on the analysis of the changing relationship between finance capital and production capital when new technologies come out. It tries to make links between the types of financial innovation made and the specific technologies of the period in question. It is also supposed to explain why each technological revolution led to brilliant successes and innovation which share the stage with great manias and outrageous swindles. In accordance with other renowned business cycles, the author advocates that there is a certain sequence of events that recurs about every half century.

The model is based on three assumptions. First, technical change occurs by clusters of innovations forming successive and distinct technological revolutions that modernize the whole production structure. Second, financial and production capital are interrelated but functionally separate agents, each pursuing profits with different criteria and behaviors. Third, the socio-institutional framework has much greater inertia and resistance to change than the techno-economic sphere, which is spurred by competitive pressures.

Although the author is not a historian, she looks at the technological revolutions which occurred in the past two centuries to shape her interdisciplinary model. The historical record is used as a laboratory for testing the hypotheses. Yet, her demonstration is drawn from five revolutions: the industrial revolution; age of steam and railways; age of steel, electricity, and heavy engineering; age of oil, automobile, and mass production and; and age of information and telecommunications.

With this background in mind, Perez defends a four-phase cycle based on causal mechanisms that are in the nature of capitalism: technological revolution; financial bubble; collapse; golden age, and political unrest. She attaches two associated concepts that play a major role in the model. The “techno-economic paradigm” (an expression she coined), which stands for the new “common sense” guiding the diffusion of each revolution, and the “great surge of development,” which represents the process of installation and deployment of each revolution and its paradigm in the economic and social system.

The model-building arguments are developed over fifteen chapters put under three parts. Part 1 is devoted to the discussion of great surges of technology and technological revolutions. Part 2 examines the changing and recurrent behavior of financial capital in its relation to technological revolutions. The last part, short, discusses the internal forces that produce the recurring sequence. The book ends with the implication for theory and policy of the author’s model.

According to the results presented by the author, when a technological revolution irrupts in the scene, it does not just add some dynamic new industries to the previous production structure. Through the configuration of a techno-economic paradigm, it also provides the means for modernizing all the existing industries and activities. The

process of diffusion of both the revolution and its paradigm across the economy constitutes a great surge of development. Each surge has two distinct periods. The first two or three decades are the period of installation, during which the critical mass of the industries and infrastructures of the revolution are put in place against the resistance of the established paradigm and driven increasingly by the criteria and the turbulent dynamics of financial capital. At about mid surge there is a turning point when the tensions that had built up are surmounted, creating the conditions for the period of deployment. During this period, which also lasts two or three decades, the transformation potential of the revolution spreads across the economy, yielding its full development benefits. Growth is driven mainly by the longer-term criterion of production capital, with an institutional framework that tends to pursue a better balance between private and collective interests. This overall evolution by long leaps would be the manner in which progress takes place in capitalism, Perez argues.

Written in most cases with a stylized narrative as the chosen manner of presentation, the book is a much focussed one on the issue and consequently targets an interested audience on the matter. It adds another piece for thought to the Kitchin, Juglars, and Kondratiev explanations of the techno-economic evolution and cycles. Her approach and conclusion, which take into account the financial aspect and its contribution to the technological revolutions, comes at point to enhance the debate on the development of the Internet bubble as well as on the ICT sector after its recent decline.

Marie-Aimée Tourres

Institute of Strategic and International Studies, ISIS Malaysia

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Veblen and Modern America: Revolutionary Iconoclast, by Michael Spindler. London: Pluto Press. 2002. Cloth, ISBN 0745309607, \$69.96; paper, ISBN 0745309593, \$20.00. 184 pages.

The importance and influence of Thorstein Veblen's contribution to economics and social theory have been a matter of debate among economists for many years. Some believe that his importance was minor and his contributions to economic thought obtuse and insignificant. Others feel that he was a major social critic of the twentieth century and that his ideas still influence the way we interpret the world. Falling strongly into this latter camp is Michael Spindler's book.

Spindler begins by looking at the social and economic forces that affected Veblen during his lifetime. He came to maturity during the era known as the "Gilded Age," when America was rapidly industrializing in the wake of the end of the Civil War. Great

wealth was being amassed by industrialists while the workers in their factories labored in poverty. This wealth was often manifested through ostentatious display while the results of grinding poverty were ignored. Members of the Vanderbilt family, for example, built eight mansions in an eight-block area of New York City. At the same time, in other parts of the city, tens of thousands of people, many newly arrived to this country, were packed into tenement housing, whole families often sharing one room. This growing polarity between the haves and have-nots would continue through Veblen's lifetime and influence his view of the world. At the same time, there was a great increase in scientific inquiry both in social and physical sciences. Veblen read a good deal of the literature, and his writings later would show the many sources from which he drew his ideas, especially Charles Darwin. In 1892, Veblen arrived at the University of Chicago and began his career as academic and social critic.

Spindler splits Veblen's works into two parts, early and later. His early works began shortly after he became an academic in the 1890s and include his most famous work, *The Theory of the Leisure Class*, as well as *The Theory of Business Enterprise* and a number of other books, essays, and articles. In these works, Veblen introduced many of his favorite themes; conspicuous consumption, pecuniary emulation, the dichotomy of industry versus business, and the instinct of workmanship were all ideas that he proposed and expanded upon in the early period of his writings.

The line of demarcation between his early works and his later works coincided with the beginning of the first World War. Veblen was in Europe in 1914 and returned to the United States to write *Imperial Germany and the Industrial Revolution*. There followed a series of books and articles over the next fifteen years that included *The Vested Interests and the Common Man*, which was a collection of previously published essays, *The Engineers and the Price System*, and *Absentee Ownership and Business Enterprise in Recent Times*. He expanded on earlier themes and introduced new ideas, including the "soviet of technicians" and the illegitimacy of stock ownership. In *Absentee Ownership* he argued that America's greatest threat lay not in Bolshevism and the IWW but in the "One Big Union of financial interests" which controlled America's economy. Spindler argues that Veblen's later writings differ from his pre-war writings more in tone than in substance. In his early writings, he seemed more of a disinterested spectator making scientific observations on his subject matter. After he left the academic world for good in 1918, his writing became more acerbic and polemic. Now he was making judgments as well as observations.

Veblen was also the first to point out the factors that were changing American society from a production-driven economy to a consumer-driven one. America during and after the first World War saw a rise in the availability of durable goods including the automobile. The possession of goods became more than just a need to use them; it also was a measure of person's social status. Production of products became less important than the marketing of products. In this, Veblen saw the middle class emulating the leisure class.

Spindler reviews the influence of Veblen in two chapters. In one, he provides a quick literature search of books and articles pertaining to Veblen from the publication of *Leisure Class* to the present day along with a brief comment on their contents. Veblen was fairly well received during his lifetime and until the 1950s. His views on the inherent contradictions of business and industry seemed prescient after the crash of 1929, and he was held in wide esteem. In the 1950s and early 1960s, however, his reputation began to sink, in part due to the political climate of the era. Through the 1970s and 1980s, his significance was debated, and only during the past decade does there appear to be a revival of the interest that was shown in him during the early part of the twentieth century. Spindler, who teaches American literature, also devotes a chapter to tracing the influences Veblen had on American writers of fiction in the 1920s and 1930s. These show particularly in the novels of Sinclair Lewis, F. Scott Fitzgerald, and John Dos Passos.

This is an excellent book to introduce Veblen to readers who have had little or no previous contact with his ideas. It is not a biography or an in-depth study of his ideas. Rather it is a summary of them that whets the appetite to learn more about him.

Bob Dick
University of Nevada, Reno

Globalising Intellectual Property Rights: The TRIPs Agreement by Duncan Matthews. London: Routledge. 2002. Cloth, ISBN 041522327X, \$80.00. 202 pages.

Duncan Matthews explores the role of global corporate actors, multinational companies, in the evolution of the World Trade Organization's Agreement on Trade-Related Aspects of Intellectual Property Rights (TRIPs). He contends that global corporate actors, primarily from developed countries, exerted their influence through multilateral actions backed by actual or potential bilateral actions. This influence was first evident in the initial period of negotiations and continued through the implementation period. Further, Matthews proposes that the consensus leading to the TRIPs agreement is now fragmenting and being challenged by new global actors. These changing dynamics may, in turn, alter the direction of the global intellectual property rights (IPR) debate in the future.

The book begins by setting the historical context for the TRIPs negotiations. Global businesses based in developed countries had become dissatisfied with the existing international framework for intellectual property right protection. The World Intellectual Property Rights Organization (WIPO), established in 1967, had built upon previous treaties to provide a framework for global intellectual property rights but provided few means of redress for firms suffering from widespread IPR violations. In response, the United States had developed, and successfully applied, its own means of dealing with violations via bilateral actions, primarily Section 301 of the U.S. Trade Act of 1984. Meanwhile, European and Japanese interest in stronger protection had grown,

and their support strengthened the push for multilateral intellectual property rights negotiations during the Uruguay Round. The mutuality of purpose shared by corporate interests in the developed countries continued throughout the TRIPs negotiations. While not directly involved in negotiations, private corporations provided extensive input into the negotiation process by providing expertise and evidence not otherwise available to official negotiators. This level of expertise placed the developed nations at a negotiating advantage vis-à-vis the developing nations who eventually agreed to the TRIPs agreement after being granted other concessions in the agricultural and textile sectors.

While the TRIPs Council has official responsibility to oversee the implementation of TRIPs, the same private global actors that influenced the TRIPs negotiations have continued their influence through the implementation stage. Through their national institutions, they provide expertise, advice, facts, and scrutiny that continue to shape the evolution of TRIPs. At the same time, their influence has been constrained by other global interests. Intellectual property rights are one, but not the only, international issue, and at times nations have placed other global issues above the absolute enforcement of intellectual property rights. In addition, differences among the developing nations have arisen. For example there are ongoing disputes as to whether patents should be granted on a “first to invent” (the U.S. approach) or a “first to file” (others’ approach) basis. Conflicts have also arisen over the issue of generic drug production near the end of patent periods. At the same time, the developing countries have become more aggressive in expressing their opinions and guarding their interests, particularly when strong global IPRs threaten public health or are used to capture indigenous knowledge. The bottom line seems to be this: the global (developed nation) corporate actors that have previously been very influential in the evolution of TRIPs will see their influence wane in future negotiations.

At times this book seems more informative than provocative. Perhaps that is the intent. For example, within the story summarized above, Matthews provides specific details about the TRIPs agreement via two means. Chapter 3 is a provision-by-provision commentary on the agreement. He supplements this commentary with an appendix containing the full text of the TRIPs agreement. This allows readers so inclined to read the chapter in parallel with the actual text to gain a good grasp of the contents of the agreement, a very informative but not particularly engrossing exercise. On the other hand, one can learn a great deal about TRIPs and related issues by reading this book. It is well written, well documented, accessible, and concise. Some might find it useful in the classroom, and anyone needing a good overview of the global intellectual property rights issue should consider reading this book. Matthews does a good job of identifying unresolved and ongoing issues, and academic readers may find this book to be a good source of research ideas.

Richard V. Adkisson
New Mexico State University

The Myth of Ownership: Taxes and Justice, by Liam Murphy and Thomas Nagel. New York: Oxford University Press. Cloth, ISBN 0-19-515016-3, \$25.00. 228 pages.

Fight until death over taxes? Oh, no. Women, country, God, things like that. Taxes? No.

—Daniel Patrick Moynihan

Liam Murphy and Thomas Nagel, philosophers at New York University, wrote this book about tax equity. Their research is impressive, their reasoning precise. And their book should spark debates because they take enough controversial positions to irritate almost anybody. Their basic controversial position distinguishes wrong from right ways to assess tax equity. Wrong ways, they say, assume *prima facie* rights to pre-tax income. Right ways assess post-tax outcomes from the standpoint of social justice. As they develop their position, they criticize standard public finance principles, critically evaluate theories of social justice, and analyze specific questions of tax design. Their language may lead some to underestimate their contribution, and, since taxes are only one side of fiscal activity, their treatment suffers from incompleteness, but their contribution remains significant.

Their critique of horizontal equity illustrates their approach to public finance principles. They maintain that any attempt to define equal ability to pay must include not just market income but also a whole range of differential benefits from government—transfers, expenditures, regulations, contracts, law enforcement, judicial decisions, and so forth. But these differential benefits depend on taxation. In addition, private sector decisions depend on expected tax liabilities, that is, on the current tax structure. So, there is no empirical pre-tax income. Attempts to conjure up mythical pre-tax incomes generate images of random violent anarchy—against which almost any decent government offers positive net benefits. Their critique of vertical equity mirrors their case against horizontal equity, and when they add doubts entertained by economists, most traditional principles of tax equity disappear.

Only optimal taxation literature gains their qualified approval—for asking questions in the right way, by measuring post-tax income distributions against standards of social welfare. The problem, of course, lies in the arbitrary quality of utility functions—reflecting the absence of any universally accepted social welfare standard.

They review and criticize a range of such standards. Pareto optimal moves are easily legitimated, but such moves are rare. Positions that go beyond Pareto optimality include utilitarianism, weighted utilitarianism, libertarianism, John Rawls's difference principle, and guaranteed minimum living standards. Utilitarianism aims at increasing aggregate welfare. They criticize it for ignoring individual differences in the income distribution. Utilitarianism that weights the welfare of some people more than others' overcomes this objection. Rawls's difference principle proposes a more radical notion of fairness: that any increase at the top of the income distribution must be legitimated by an increase at the bottom. But weighted utilitarianism like Rawlsian justice probably aims at more redistribution than most people would accept. Their "sympathies lie" with

a position that guarantees a minimum income to all citizens (p. 140). Their sympathies do not lie with libertarians.

Libertarians start with the moral imperative that people should be allowed to use their abilities in any way they want so long as they do not transgress the freedom of others. The prepolitical right to property follows from this freedom and from the assumed operation of the free market. Libertarians thus give a “moral significance” to markets, a moral significance that Murphy and Nagel question (pp. 66–70). They agree that markets can allocate resources efficiently and that participants have a right to a fair return for their efforts. But they question the assumption that gross income measures that fair return. Market returns depend on many things—on physical and legal infrastructures, privileges of birth and education, market power, political power, and plain luck. Consequently, acceptance of property rights does not imply acceptance of rights to precise gross incomes generated in capitalist markets. Considering social welfare from another point of view, market rewards might be inadequate to induce people to enter occupations such as social work and teaching that have a high social value. Finally, since libertarians assume equality of opportunity, their position should logically require government to engage in massive income distributions to assure every participant the same chance of profiting from market processes. Even then, it might not be possible for markets to generate the morally significant income distribution that libertarians assume.

As they move to specific issues, the authors reject arguments for excluding saving from the tax base because consumption taxes fall on labor income, and for that reason, the same arguments for excluding saving apply to consumption. They choose wealth as the best base because it contributes to welfare in many ways—providing comfort, security against the future, status, and political power

With respect to progressivity, they accept, in a qualified way, the optimal tax literature’s suggestion—a negative income tax and progressive declining marginal rates. But they add that reductions in upper income rates without a guaranteed minimum income do more harm than good.

They propose replacing inheritance taxes with accessions taxes paid by heirs—modified by income averaging, exclusion of child support, and other adjustments. Repeating their general thesis, they argue that deductions and exemptions should be analyzed not from the standpoint of horizontal equity but from the desirability of post-tax outcomes.

They are proud of their title, but one could question its wisdom. The term “myth” has many meanings. Together with their portrayal of property as a legal convention (p. 8), the title is enough to raise the hackles of hard right critics who obscure the book’s contributions with the image of a pair of pied pipers leading us down the road to serfdom (Gordon 2002; Stephen Moore, “In Their Own Words,” *National Review*, April 23, 2002, Internet: National Review, <<http://www.nationalreview.com/moore/moore042302.asp>>).

There is incompleteness to the book. Murphy and Nagel do not justify their concern about poverty problems (pp. 135, 189–9)—though they have done so elsewhere (Nagel 1986, 135–207; Murphy 2000). Taxes are not the complete solution to poverty problems. Complete solutions must be sought in structures of laws, regulations, and expenditures legitimated within broad historical theories of democracy such as those developed in pragmatist traditions (Putnam 1995). Murphy and Nagel need to complete what they have begun—perhaps in another book titled something like “moral government in a nonideal world.”

Till that book comes out, this one should be on every public economics reading list.

J. Dennis Chasse

State University of New York: College at Brockport

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