Book Reviews


In *The Invisible Hand of U.S. Commercial Banking*, Margaret Polski provides an instructive account of the transformation in U.S. commercial banking during the latter part of the twentieth century. On one level, she uses this case study to explain the evolution from a highly regulated industry, where state and federal laws determined product lines, geographic locations, and prices, to a deregulated industry where firms compete with a variety of financial service providers. On a different level, Polski uses this case study to test the explanatory power of what she terms as theories of political economy available to study institutional and economic change.

Polski begins the book with a discussion of the theoretical underpinnings she has identified as available to study institutional and economic change: social welfare, distributional, and institutional. *Social welfare* theory begins with the existence of a market failure. Change occurs when a disinterested social planner designs a new regulation to promote efficient behavior. *Distributional* theory begins with government failure. Change comes about when groups engage in rent seeking by pressuring government officials to create regulations in return for votes. Polski thinks of *institutional* theory as the new institutional economics (NIE). In this theory, both markets and governments can fail to allocate resources efficiently, due to either information constraints or transaction costs. Change occurs when the actors modify the “rules of the game” in either the private or public sectors.

Drawing on a survey of case studies using these theories, Polski argues that they provide only partial explanations of economic and institutional change. In response, she identifies the strengths of each and adapts them to provide a more comprehensive explanation. This framework holds “constant many of the situational factors the empirical literature identifies, e.g. physical resources, prior economic experience, entrepreneurship, leadership, etc.” (p. 36). She postulates that change originates in an exogenous economic or institutional shock, such as new technology, a change in demand, or a new rule. The shock can instigate four sources of change: (1) changes in the beliefs of individuals who have the potential to “exert influence in other sectors”; (2) changes in the governance or economizing of firms; (3) changes in the economic environment that affect
the industry; and (4) changes in the institutional environment that take place in the public sector (p. 37). She conceives of this framework as drawing on a variety of methods of analysis, including “historical, quantitative, and decision-theoretic” (p. 39).

Polski uses this framework to describe changes in the commercial banking industry from 1960 to 2000. In each chapter, she begins with a historical overview from colonial times, first of the industry and next of regulation. She uses a structure-conduct-performance framework to document changes in the competitive environment. She also includes numerous time-series data to supplement her descriptions.

Polski highlights several key findings. First, change came from three sources: markets, governments, and institutions. Initially, private firms led the way by working around existing regulations. Regulators followed by easing laws and liberally interpreting existing laws, first on the state level and later on the federal level. Second, these changes resulted in profit, but not cost, efficiency. Third, the strength of the economy in particular sectors of the country led to different responses by commercial banks. Banks in low growth areas resisted change, while those in high growth areas actively sought regulatory reform.

This book is short. But as the partial summary indicates, its brevity masks the scope of its coverage and the depth of its analysis. In fact the richness and conciseness of the book makes reading almost every sentence crucial. At the same time, Polski writes with such clarity that my third-year money and banking students could easily read and learn from it, particularly the case study of commercial banking.

When Polski details the lessons of the case study, she recognizes one of the book’s shortcomings that is of particular interest to readers of JEI. She states that this study has taught her that scholars need to do more research on evolutionary economics, an observation that suggests to me that she is unaware of modern work in the old institutionalist tradition. I think this case study would benefit greatly if she learned more about the old institutional economics (OIE). She appears to share the concerns of OIE about the problems associated with restricting explanations of economic and institutional change to traditional, choice-theoretic models of market or government failure and the NIE models of principal-agent and transaction costs. At the same time, the framework she developed could not explain the origin of change and resorted to exogenous shocks to start the process. Not surprisingly, her description of this episode ended with puzzling findings that contradicted the predictions of the theoretical models on which she based her analysis.

A recent paper by John Groenewegen discussing the distinctions between NIE and OIE warned of this outcome. He stated: “The scope of the OIE is much broader and involves the history and context of firms, the method is multi- or even interdisciplinary, and the significance is more policy oriented. The scope of the NIE is very much in the tradition of mainstream economics of predicting optimal solutions, the method is axiomatic-deductive, and the significance is restricted to specific questions of comparative static nature and to specific conditions in which the selection of the fitter is assumed to
take place” (2004, 358). Polski’s approach in this case study is much more in the tradition of OIE. She gets into trouble when she tries to interpret her findings using an adaptation of the social welfare, distributional, and NIE theories of change. Because I find her narrative so rich and instructive, I would recommend that she rethink this episode using some of the ideas of the OIE. Her gifts of description and analysis combined with some new analytic tools could provide scholars with a much clearer understanding of the process of economic and institutional change, both in commercial banking and generally.

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References


As the study of social provisioning, institutional economics necessarily casts a wide net in its investigations. The market, state, and family are all within the bounds of institutionalist analysis, and much has been written about these spheres and their interrelations.

The age-old social practice of nepotism, however, has received scant attention from institutionalist scholars. Thorstein Veblen (1914), of course, saw significance in the instinctive “parental bent” from which nepotism springs. More recently, William Dugger’s (1989) Corporate Hegemony detailed how the corporate career game involves nepotism-soaked “sponsored mobility” (in contrast to the “contest mobility” of a meritocracy). Still, these are among the notable exceptions.

Yet institutionalists are not alone in overlooking this subject. According to Adam Bellow, a New York–based editor (and son of novelist Saul Bellow), he made a shocking finding when seeking to write an article on the topic a few years ago: “The first and only book explicitly devoted to nepotism is a seventeenth-century tract . . . translated as The History of the Popes’ Nephews” (p. 20). He adds, “I had to write the book I needed to read in order to write the shorter polemical work I had envisioned” (p. 21).

In Praise of Nepotism is that book, a volume that draws on scholarship in an array of disciplines and that traces nepotism across history. Although I quarrel with one of the author’s primary conclusions, I recommend the work highly to evolutionary economists.
Many Americans like to think the United States is a meritocracy. Bellow’s book, however, shows that nepotism is alive and flourishing in twenty-first century America. In fact, Bellow argues that the practice is in the midst of resurgence. His examples of family influences upon present-day employment and careers begin with politics (the presidential election in 2000 was just the tip of the iceberg), but attention is also given to business, the labor movement, the military, the arts, entertainment, sports, and numerous professions.

Of course, nepotism has always existed. It’s a human drive: normal, natural, and “basic to human survival” (p. 22). But nepotism, a preference for kin that often involves coercion as well as reciprocal altruism, is not merely a biological phenomenon: it’s also a cultural phenomenon. That means society influences how nepotism is practiced, even as those practices help shape society.

Bellow’s ambitious book demonstrates that nepotism can take many forms. He examines the practice in clan, caste, and tribal societies; in ancient Greece and Rome; in feudal Europe; and in the eras of Napoleon and M. A. Rothschild. Chapters on nepotism in America give some credence to the belief that U.S. history contains a powerful current of anti-nepotism, but the more complex picture that emerges shows a kaleidoscope of practices, often rooted in very different understandings of kinship. Indeed, even support for a merit-based society often rests on the belief that citizenship makes Americans part of a national family. (Christianity sought similarly to redefine kinship for centuries in Europe by means of the notion of a universal church.)

As long as the book focuses on description, its pages are illuminating. It is only when Bellow offers commentary on the present that the analysis falters. As the title suggests, Bellow praises the recent trend that elevates heredity over equal opportunity in employment. He admits some observers are worried that numerous career paths are becoming closed to outsiders due to a growing trend toward family succession, but he dismisses the concern without taking it seriously.

Bellow dispenses with the problematic side of the current trend by drawing a distinction between nepotism practiced well and that practiced poorly. When the beneficiaries of nepotism later demonstrate effective job performance, Bellow argues that nepotism was practiced well and that nobody has a reason to be troubled. In contrast, when beneficiaries prove unqualified, nepotism was clearly practiced poorly and receivers of its advantages will be “mercilessly hounded from the stage” (p. 508). Neither part of this hand waving is convincing.

Despite this shortcoming, I enthusiastically recommend the book for its contribution to the study of social provisioning. Evolutionary economists will benefit from thinking about how the practice of nepotism affects the institutions they are currently studying. Their students will benefit, too, not only from thinking about how the practice affects the contemporary economy but also from observing how institutionalized behavior norms evolve. Looking beyond Bellow’s shallow analysis of current trends, readers will find he admits that “nepotism unchecked by laws and strong ethical prescriptions does tend to run to extremes” and that the key question is, how can the
nepotistic urge be channeled “so that it does not obstruct our efforts to create a good society” (pp. 22–23)? His treatment of these crucial matters might not offer much more than a point of departure for discussion, but at least he has us talking about them.

Charles J. Whalen
Perspectives on Work

References


This book puts together several articles that provide a concise and clear presentation of current Post Keynesian developments in central banking. The book is divided into three well-defined parts, and, depending on his or her areas of interest, the reader will profit from the reading of any or all of them.

The first part of the book is a detailed critical presentation of the New Consensus, now dominant in academia. The first two articles by Marc Lavoie and Mark Setterfield critically analyze the core mathematical model of the New Consensus. They show that models that include Post Keynesian assumptions are mathematically more satisfactory. John Smithin argues for a monetary-policy goal oriented toward income distribution. In doing so, the central bank should target a small positive (optimally zero) real rate (and so continue its inflation targeting strategy) but should forget about the notion of natural interest rate. Finally, Philip Arestis and Malcolm Sawyer argue that the inflation goal proposed by the New Consensus is too narrow and that the recent decline in inflation is not due to the adoption of inflation targeting by some central banks.

The second part of the book concerns the transmission mechanisms of monetary policy. Robin Rowley and Brenda Spotton Visano critically review the official and unofficial theoretical arguments, and empirical evidence that has been provided in Canada to explain how the central bank influences inflation with its interest rate policy. The authors show that the transmission mechanisms are not straightforward. Marc-André Pigeon shows that the centrality of the communication process, only recently understood by central bankers, has long been recognized by John Maynard Keynes (1936, 203) and his followers. Thomas Rymes shows how the disappearance of the Quantity Theory of Money in the New Consensus, and so the causality from reserves to price, brings forward the relevance of Keynes for monetary policy. Louis-Philippe Rochon and Sergio Rossi demonstrate that even if reserve requirements are nil, reserves still matter for the
settlement of transactions between banks (Fullwiler 2004). They, then, show how this can be integrated in a monetary circuit and how this gives importance to the central bank. Mario Seccareccia and Marc Lavoie look theoretically and empirically at the relationship between short-term rates and long-term rates and study the capacity of the central bank to control the latter. Gennaro Zezza and Claudio Dos Santos extend the stock and flow model proposed by Lavoie and Wynne Godley by introducing the role of monetary and fiscal policies. They find that the interest-rate policy has uncertain effects on the economy.

The third and final part of the book focuses on economic history and history of economic thought. The first paper, by Thomas Palley, compares Rudiger Dornbush’s and James Tobin’s conclusions about the effectiveness of monetary policy in open economies. The author concludes that monetary policy should be given an active role because Dornbush’s hypotheses (perfect substitutability of financial assets, perfect capital mobility, and purchasing power parity) are not empirically verified. Robert Dimand compares Tobin’s and Hyman Minsky’s thoughts about the instability of capitalist economic systems. He shows that there is a tension in Tobin’s between equilibrium analysis and instability analysis and argues that Tobin can provide some insights about the cause of instability of monetary production economies. Robert Prasch reviews Allan Meltzer’s history of the Fed and praises his reliance on primary sources. However, he also shows that the book is limited in its analytical side due to its reliance on Monetarist arguments that have long been denied validity by Post Keynesians. The last article, by Jane Knodell, looks at the impacts of central banks on the early stage of industrialization (1830–1900 period) in seven countries. She shows that countries with a central bank had lower growth on average but this was not due to interest rates (real or nominal). Indeed, there was a positive or nil relationship between interest rate and growth, which confirms some work done by Post Keynesians (Minsky 1982).

In total, the book provides a good variety of articles capable of satisfying different readers regarding central banking. The main shortcoming of the book is that it does not include an article that analyzes the role of central banks for financial stability. There is a large literature in the New Consensus and in Post Keynesian analysis about this and how this relates to price stability and full employment (Tymoigne 2005). One debate, for example, is about the “Schwartz hypothesis” (Schwartz 1988, 1998), which assumes that price stability automatically generates financial stability (in addition to higher and more stable economic growth). One could also argue that promoting and managing financial stability should be the unique role of a central bank, leaving distribution, inflation, and employment to other public institutions. If this is the case, the stock-flow analysis, by allowing an exhaustive account of financial interrelations, is a great modeling tool. This can be contrasted with the first two papers of the book which, by staying in the realm of the New Consensus, completely leave aside the financial side of the economic system.

Most of the authors have been pleased by the assumption of endogenous money in the New Consensus and their presupposed abandonment of the Quantity Theory of
Money. I would be more reserved about this move for three reasons. First, the quantitative equation is still looming at the back of the mainstream’s mindset as a medium-/long-term guide for monetary policy. Laurence Meyer (2001) perfectly made this point by showing how the central bank should determine a “reference value” for the growth of money supply \( (m^*) \). This value should be consistent with the inflation target of the central bank \( \pi^T \), the prevalent natural growth rate \( q_n \), and the existing trend of velocity \( \bar{v} \):

\[
m^* = \pi^T + q_n - \bar{v}
\]

If \( m > m^* \), then it shows that actual inflation will be above target and this provides medium-term guidance for central bankers.

Second, as many authors note, the endogenous money approach was adopted only after reality forced the hand of mainstream economists. It is what we could call an “ad hoc” endogeneity of money. The true nature of money is still to be a commodity that emerged to facilitate exchange. Money is not naturally endogenous, to take Rochon’s terminology (2003), because money is not a debt relationship.

Third, the neutrality of money is still present in the long run via the NAIRU and the equilibrium natural interest rate. Money has no use in itself, and a real exchange economy with money is considered to be equivalent to a monetary production economy.

In conclusion, the New Consensus and the Post Keynesian analysis are far apart. The book shows well that the latter is not only critical of the former but is also able to propose alternative recommendations for what a central bank should do.

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References

What can various social science disciplines tell us about the process of economic development? And what is the relationship between early insights and contemporary approaches to the complex and seemingly intractable problems confronting poor countries? In this provocative book, Vernon Ruttan addresses these questions by providing a compelling synthesis of the key contributions of social science to the discourse on economic development.

The book focuses on the centrality of institutions in the process of economic development and identifies insights from anthropology, political science, and sociology that strengthen our understanding of institutional change. The importance of institutions to the process of economic development is analyzed in the context of Ruttan’s theory of induced institutional innovation, in which changing endogenous variables such as technical change and cultural endowments are viewed as important sources of institutional innovation.

Ruttan’s careful but isolated treatment of the various social science disciplines and their subfields has some drawbacks. In reviewing contributions from cultural anthropology, for example, Ruttan notes the potential for the materialist approach to provide insights into microeconomic issues bearing on development such as the production and consumption decisions of indigenous or peasant households. But as Ruttan rightly notes, some anthropological traditions easily view production and consumption patterns as components of culture, which encompasses farming practices, religious rituals, and beliefs about nature. Isolated insights from various theoretical perspectives without an analysis of their cumulative import may lead to an underestimation of the utility of anthropology (or other disciplines) for resolving microeconomic problems, which Ruttan argues lie at the heart of the development process.

With respect to sociology, Ruttan emphasizes the structural-functionalist perspective advanced by Talcott Parsons and James Coleman’s *Foundations of Social Theory*. Theories of social capital developed from Coleman’s work address issues that are central to Ruttan’s theory of induced institutional innovation such as intercultural interaction, social capital/cohesion, and trust. These factors are integral to the process of nation building in multiethnic polities (Ritzen, Easterly, and Woolcock 2001) and constitute important considerations for understanding how institutions evolve. Of course, elements of Coleman’s work can be traced to the intellectual foundation established by Parsons’ work on the organization of societies. Sociologists clearly bring an understanding of society to economic development that has profound implications for the design of institutions, and Ruttan anticipates much more from the discipline.

The foundation for sustainable economic planning remains a stable political system. The debate over the relationship between political institutions and economic
development centers on the comparative effects of democracy and authoritarianism on prospects for economic development and growth. Against this background, Ruttan addresses an important yet often ignored issue, namely that political development has to be measured in terms of the concentration and distribution of political power.

In developing multiethnic societies, political power can be concentrated in a single ethnic group or among a select number of ethnic groups, influencing both economic allocation and distribution. Institutional innovation is deeply linked with the distribution of political resources, particularly political power. Ruttan identifies the need in political science for further study of political power, but the successful design of democratic governance is also an especially important variable in heterogeneous societies where political mobilization tends to occur along ethnic or religious lines (Okediji 2003). It bears emphasizing that to the extent such power is managed within a democratic governance structure, the social returns from institutional innovation will depend largely on how the practice of democracy assuages ethnic competition. In sub-Saharan African countries, ethnicity and religion are profoundly interwoven, creating a complex array of cultural endowments that make institutional innovation difficult to sustain. Ruttan does not deal with this problem, but he does give attention to the role of religion in shaping the political environment in which economic markets must function, focusing on East Asia, South Asia, and the Middle East.

Finally, Ruttan addresses the important relationship between the role of technical assistance, diffusion of technology, foreign assistance, and economic development. Economists have long recognized the role of technology transfer in economic development, but Ruttan’s conclusion that an insufficient amount of research has been conducted on this issue confirms a significant gap in economic development literature. Intellectual property rights (IPRs) are the critical foundation for innovation, technology transfer, and diffusion but as a subject of study they are largely the domain of legal scholars. Analyzing how proprietary rights in the fruits of innovation affect the capacity of poor countries to access, utilize, and adapt technology to local environments and how such rights fit within existing institutions is a scholarly task that requires an intellectual engagement between law and the social sciences.

In connection with this, Ruttan’s theory of induced institutional innovation could be enriched by insights from a distinct strand of critical legal scholarship that examines how the rule-of-law variable, de-linked from any social or cultural institution in developing countries, has in fact served to undermine cultural cohesion and social networks, thus destabilizing the institutional environment of poor countries (Anghie 2002). Legal institutions are crucial to the process of economic development, but the precise formulation of legal concepts must reflect the endogenous sources of institutional change that are central to Ruttan’s theory.

Economic development is a dynamic process central to the sustainability of markets and, ultimately, the success of human society. Ruttan’s remarkable analysis of the intellectual history of this process—the overlaps, synergies, strengths, and weaknesses of various disciplinary insights—provides a rich resource depicting how far our understanding
has evolved and, importantly, of how much more work remains to be done. His singular contribution reflects the challenges to understanding how best to address the pressing human issues of our time.

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References


The author claims this slender volume represents the first “comprehensive treatment of the personal distribution of wealth in the modern sense” (p. xi). Michael Schneider has covered numerous sides of the wealth distribution question—from the meaning and measurement of wealth to redistributive policy—with considerable skill and clarity. Be advised, however, that no real effort has been made here (aside from short references to inheritance) to identify and explicate institutional sources of wealth inequality as well as its intergenerational transmission.

Wealth is notoriously difficult to define and measure. Variable definition here (as is so often the case in the social sciences) is conditioned by the availability of data sufficiently uniform so as to enable intertemporal and cross-country comparisons. Schneider’s exclusion of “human capital” from his definition of personal wealth—“the value of tangible assets owned by the individual/household and the net total of the individual/household’s claims on tangible assets” (p. 2)—will raise the hackles of some economists. Is a new-mint neurosurgeon with outstanding educational loans truly less wealthy than a retired bus driver with a paid-off mortgage and $300,000 in pension assets?

Chapter 3 (“Empirical Studies of the Distribution of Wealth”) supplies a valuable primer on techniques used to estimate the personal distribution of wealth for the USA, Canada, Western Europe, Scandinavia, Japan, and Korea. The most common approach, called the “estate multiplier,” begins with estate data gathered for tax pur-
poses or probate inventories (which in a given year cover only a small percentage of indi-
viduals) and arrives at population estimates by multiplying each case by the reciprocal of
the mortality rate for a person of that specific age and gender. Schneider does not men-
tion that U.S. estimates are biased downward because estate tax records omit wealth
held in trust (which is substantial and tends to be concentrated among the very rich).
Counting the present value of social security benefits as wealth would make the distribu-
tion of wealth appear more equitable, even though wealthier people live longer and thus
collect more benefits.

The statistics displayed by Schneider in chapter 3 serve to fortify and embellish a
position shared by virtually all social scientists: Wealth is tightly held in the industrial-
ized or developed countries and is distributed more unevenly across individuals (or
households) than income (Gini ratios for wealth are typically double those for income).
Some highlights: Inland Revenue Statistics indicate that in the year 2000, the top 5 per-
cent of U.K. residents controlled 42 percent of total wealth, whereas the bottom 50 per-
cent held a mere 6 percent. For the USA Schneider relies on the estimates of Edward
Wolff and Arthur Kennikell, which are remarkably close for 1983–98. Wolff calculates
a Gini ratio for wealth of 0.799 for the year 1995. Kennikell’s estimate is 0.785. Both
estimate that the top half percent controlled more than one quarter of total wealth in
the USA in 1995. Even in Sweden, the putative model of equality, the distribution of
wealth is highly skewed.

The absence of cross-sectional data in this book is disappointing, as these are useful
in uncovering important questions as well as forming tentative hypotheses about the
causes of wealth inequality. It is known, for example, that the distribution of wealth
among women is substantially more concentrated than it is among men. Brittain (1978)
attributed this to the low probability of obtaining a large bequest or a wealthy
spouse—the primary means by which women accumulate fortunes.

Chapter 4 contains a neat theoretical discussion of the factors that explain differ-
ences in wealth between persons. These include variations in earned income, age, thrift-
iness, inherited wealth, and the rate of return on wealth. There is no attempt by means
of econometrics to assess the relative importance of these factors in accounting for real
world wealth disparities, though Schneider’s reading of the literature leads him to con-
jecture that “inequality in the distribution of income accounts for less than half of
inequality in the distribution of wealth, that age differences account for only part of the
gap between inequality in the distribution of income and inequality in the distribution
of wealth, and that non-uniform inheritance makes a significant contribution to this
gap” (p. 68).

Chapter 5 is concerned with the principles that might be applied to “rank” alterna-
tive distributions of wealth. Schneider declares there is no objective standard for deter-
mining if one degree of wealth inequality is superior to another as all such judgments
are value laden. The objection might be raised that positive criteria can be found to
assess whether a given distribution of wealth is instrumental in achieving social desider-
ata—for example, cooperativeness or cohesion, innovativeness, full employment, or
macroeconomic stability (in the author’s defense, he does examine the equality–eco-
nomic efficiency trade-off in chapter 7). The author classifies the conservative, libertar-
ian, egalitarian, and utilitarian views as “single value” systems, meaning “judging society 
by one criteria only” (p. 71). Conservatives resist change because it (ostensibly) threatens 
to undermine the most sacrosanct of institutions—property. With regard to the egalitar-
ian view, does it mean “equality of welfare” or “equality of resources”? Pursuit of the for-
mer fails to allow for “voluntary” inequality. Amatya Sen argues for “equality of 
capabilities,” which is not in every case equivalent to equality of resources: “Equality of 
capabilities... requires some inequality of resources, as in order to have access to the 
same capability a person who is handicapped, for example, will generally require greater 
resources than one who is not” (p. 79).

Most JEI readers will derive an unexceptional return on time invested in this work. 
There is little of direct relevance to the current debate over the repeal of the estate tax, 
for instance. Schneider’s book does offer a beneficial introduction to the topic of wealth 
distribution for students. It is also appropriate as a basic text in courses covering income 
or wealth inequality.

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Reference
1978.

pages.

This is an interesting and stimulating book. It argues that innovation studies have so far 
neglected an important dimension of the innovative process, which the authors call the 
interpretive dimension. This refers to managers’ capability of bringing together people of 
different backgrounds (e.g., engineers, product designers, advanced users), engage them 
in constructive discussions about new products, manage the confusion and ambiguity 
that may inevitably arise in the interactions between heterogenous agents, interpret such 
ambiguity, and eventually point to new technological trajectories that the innovative pro-
cess should lead to. These interpretive capabilities have been neglected by the literature 
on innovation management, which has so far mostly focused on the analytical dimension 
of the innovative process.

The analytical dimension is of course important; it refers to the rational prob-
lem-solving skills that the actors engaged in innovative activities must necessarily be
endowed with. In a complex, rapidly changing, and fundamentally uncertain business environment, however, analytical problem-solving skills must necessarily be complemented by a diverse set of broader nonanalytical capabilities. In fact, radical uncertainty makes it frequently hard even to identify the objective of problem-solving activities. The identification of objectives, the definition of problems, and the opening up of new technological trajectories can only be achieved by the managers’ use of their interpretive skills.

Richard Lester and Michael Piore develop this interesting argument by describing the results of three case studies on innovation and product design in rather different industries, namely cellular telephones, blue jeans, and medical devices. What these different cases have in common is that in all of them new product development has required a high degree of integration, obtained through conversations among people with different backgrounds. This type of interaction among heterogenous agents frequently generates ambiguity, which is the crucial resource out of which innovation is created. These conversations must therefore be fostered, and the resulting ambiguity must be managed, interpreted, and oriented toward new, promising directions. This is precisely what creative managers should do, not simply solving complex analytical problems but also initiating, encouraging, fostering, and interpreting conversations between different actors about new products’ design.

The missing dimension of the innovative process—interpretation—should therefore complement the traditional approach—analysis—in the minds of innovative managers, academic scholars, and policy makers. However, there exists a fundamental opposition between these two perspectives, and the combination of analysis and interpretation cannot easily be realized. The basic problem is that, in an economic environment characterized by rapid changes and increasing global competition, interpretive spaces do not naturally arise, and they risk being displaced by short-sighted opportunistic behavior. These spaces, which the authors call public spaces, must therefore be created, cultivated, and protected so to preserve and reproduce the most crucial resources of the knowledge economy, creativity, interpretation, and novelty.

The paradox of the modern economic world, and of the U.S. economy in particular, is that precisely in a period when the need to cultivate public spaces has grown bigger, the free market ideology and the related policy strategy have become dominant, progressively eroding those very public spaces where interpretive capabilities should flourish. The book concludes that the best way to foster the innovative performance and the future competitiveness of the U.S. economy is not by strengthening and extending the free market system even further but rather by protecting public spaces that may promote open conversations, interpretation, and creativity, such as university research, the educational system, the regulatory process, and the IPRs regime.

The basic argument developed in this book is original, and it challenges the dominant perspective on innovation management and policy. The scope of the book is broad, ranging from micro-level case studies to macro-level discussions of country com-
petitiveness and the related policy implications. The readership of the book could also be wide, ranging from managers to product designers, policy makers, academic scholars, and students. The broad and stimulating discussion carried out in the book leads to reflection upon some aspects that have been briefly touched upon but not analyzed in detail. There are four points, in particular, that the book should have discussed at greater length. They possibly represent directions for future research.

The first point refers to the micro level of analysis. Analytical and interpretive skills represent different capabilities of microeconomic agents. What are the psychological roots of these two distinct types of human skills? How would it be possible to develop interpretive skills of individuals and combine them with their analytical capabilities? And what, more precisely, could the education system do in this respect? The second aspect refers to the sectoral level. Several innovation studies have shown that innovation is to a great extent a sector-specific activity and that, in particular, the characteristics of the knowledge base differ significantly across industries. This suggests that the appropriate mix of analytical and interpretive skills needed to carry out innovative activities could be very different among sectors, depending on the specific nature of each industry’s knowledge base. The consideration of the sector-specific nature of the knowledge base could therefore enrich the perspective presented in the book.

The third point refers to the macroeconomic level. The book focuses on the U.S. economy, and it points out the danger that this may in the future become less competitive if it will not take a broader approach to innovation. But the same argument may easily be applied to many other industrialized countries in the world economy. The dominance of the analytical perspective is in fact deeply rooted in the Western world’s scientific tradition, and it is really difficult to find a country in the industrialized world where the analytical perspective is not dominant in the business sector or where interpretive public spaces are not currently being threatened. The argument about the competitiveness of the U.S. economy should therefore be formulated in comparative terms and possibly point out economies, if any, that could in the future become more competitive than the USA by making use of their greater stock of interpretive capabilities. Can the current rise of emerging regions in the world (e.g., Asian NICs, China, India) be in any way related to this discussion about analysis versus interpretation?

The fourth and final consideration is more general, and it refers to the theoretical level. The core argument of the book—that innovation arises from open-ended conversations and interactions among heterogenous agents in a radically uncertain economic environment—is in fact quite in line with the evolutionary economic approach. Evolutionary economics theorizes a world where it is the interaction between heterogenous agents that creates novelty and where entirely new technological paradigms and trajectories can be found when innovative firms undertake search activities in radically new directions. Such a creative search process does not only entail analytical problem-solving abilities, it may be thought, but it also requires open-ended interpretive capabilities. The study of the relation between evolutionary economics and the analysis versus interpreta-
tion perspective is a missing dimension of the book, and it seems to be a fascinating
direction for future research.

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When All Else Fails: Government as the Ultimate Risk Manager, by David A. Moss. Cam-

This is a useful and worthwhile book for anybody interested the evolution of U.S. gov-
ernment risk management or effective policy design. Professor David Moss argues that
the U.S. government has since its inception acted as a manager of risk due to its unique
ability to enforce action. Moss goes on to define risk management policy as “any govern-
ment action designed to reduce or to reallocate risk” (p. 1). The general thesis of the book
is that government has facilitated economic development and growth by managing the
risk associated with the economic process. Indeed, Moss goes so far as to state that with-
out this intervention “it is doubtful that a modern industrial economy could have taken
root in America” (p. 1).

Moss develops his thesis of government as the ultimate risk manager from the
assumption that government policy makers are problem solvers (pp. 20–21). To his
credit Moss continually justifies this hypothesis by including the comments and testi-
mony of legislators. Even a casual glance through the notes section will reveal numerous
references to the U.S. Congressional records. Indeed, Moss’s research is convincing that
the “problem solver” assumption is justified more often than not. This does not mean
that political opportunism has been overlooked where it has played a role in policy out-
comes. However, the overall impression that one gets is that lasting policy actions are
the result of perceived problems and not simply political opportunism.

The basic layout of the book is straightforward. A brief historical treatment of the
study of risk including an overview of the necessary mathematical tools sets up the rest
of the book, which focuses on specific economic problems in their historical context.
Chapters on limited liability, money, bankruptcy, workers’ insurance, social security,
and product liability make up the bulk of the text with a concluding chapter entitled
“Security for All.” With the exception of the last chapter the topics are self explana-
tory—in each case Moss shows how government responded to perceived economic prob-
lems by managing the associated risk either by reducing it all together, shifting it onto
more able parties, or spreading it across the population. The last chapter hypothesizes
on future areas that may demand government attention (e.g., health care) while at the
same time mulling the American attitude about government intervention in the econ-
omy. This last point reveals a secondary thesis that runs through Moss’ book, that is,
that Americans will generally not support direct intervention into markets but will often
favor the government support of markets through risk management. Indeed, this is an interesting idea that would lend itself to future study as it is not pursued with any vigor by Moss.

Professor Moss’s book is primarily historical and divided into three historical phases. The first phase, business security (to 1900), includes the introduction of limited liability, banking regulation, bankruptcy law, and the predictable enforcement of property rights. The second phase, security for workers (1900–1960), includes unemployment insurance, social security, and workers’ compensation laws. The third and final phase, which Moss refers to as security for all (since 1960), includes federal disaster insurance, consumer safety laws, credit card liability, and, Moss argues, in the likely near future national health insurance.

Moss’s three-phase format reveals an interesting distinction between phase I and II and phase III policies. He clearly shows that the majority of phase I and phase II policies came as a result of overwhelming popular demand. For example, in the section on workers’ compensation (pp. 162–169) Moss draws heavily from American Association for Labor Legislation (AALL) statements that reflect the concerns of both business and workers. This is not to say that the policy did not have its detractors (several alternative opinions are mentioned) but that the final legislation was able to give something to all parties. However, Moss concedes that this was not the case for components of phase III legislation. In particular Moss argues that product liability law came from the legal profession rather than individual citizens, consumers, and their elected officials (p. 230). Yet, having drawn this distinction, it seems that Moss underappreciates the significance of this difference. While phase I and phase II reforms have become almost unshakable components of American economy (witness the current administration’s failure to modify social security) many phase III reforms have faced significant political opposition (e.g., environmental laws). Moss, however, does not make much of this distinction nor does he deal with the question of long-run political viability. Yet it seems significant that the impetus for these reforms was different. That is, one can seemingly draw the conclusion that without grass roots demand from all relevant parties—even where there is popular support—the political viability of reforms can be fleeting.

Another small point of contention is Moss’s treatment of human capital markets. Moss argues that human capital markets are incomplete because “individuals cannot credibly commit to turn over their future incomes to others” (p. 46). Moss argues that this is the case because individuals have the right to declare bankruptcy. Yet it is not clear how this is significantly different from a modern corporation which can also free itself of debt through bankruptcy. The assurance of future repayment would seem to be a separate issue from a contractual obligation on potential future earnings. Again, this is a small point that has little to do with the wider issues presented in the book, but it does point out the one weak point in the book, that being its limited analysis. Moss’s book offers a tremendous amount of information about the political history and economic reasoning behind various reforms, yet it does fairly little analysis on what these reforms mean or what specifically has contributed to their lasting success (or failure). Still, Moss
deserves congratulations for showing government policy to be more than self-interested rent seeking and for revealing the reasoning behind government risk management—a recommended book.

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This volume has a history, and it is important to understand its history to appreciate the essays in the volume. In the 1980s in the city of Trieste, Italy, a two-week annual Post Keynesian workshop was organized by Sergio Parrinello, Pierangelo Garegnani, and Jan Kregel during the month of August. The purpose of the annual workshop was to provide an international gathering of Post Keynesians and fellow travelers with the primary goal of introducing graduate students and young professors to Post Keynesian economics. During the mid 1980s tensions between the more traditional Keynesians and Sraffians became apparent, leading to a level of antagonism that spread into the sessions. This along with the difficulty of getting finance to support the workshop led to the end of the Trieste workshops. In 1988, under the direction of Paul Davidson, a second phase of the workshops began, this time in Knoxville, Tennessee. The tensions between the Sraffians and traditional Keynesians that plagued Trieste were not as apparent in Knoxville, but the program did seem to represent more of the issues that traditional Post Keynesians were interested in. With the retirement of Paul Davidson and again the difficulty of financing the summer school, the second phase of these workshops had its last conference in 2000. The third phase, which this volume represents, started in 2002 at the University of Missouri at Kansas City with the financial support of the Center for Full Employment and Price Stability, which is also housed at the University of Missouri.

Starting with the Knoxville workshops Edward Elgar traditionally has published a volume of the very best papers from each of the conferences. This volume continues that tradition with the very best papers from the first Post Keynesian conference at the University of Missouri.

The chapters of this volume are broken into six sections: In the first section, “Post Keynesian Perspectives on Current Economic Policy,” we have the first chapter, written by Eckhard Hein, who takes a well-known Post Keynesian view that inflation is not a monetary issue but a conflict over distribution in the private sector. The second chapter, by Hassan Bourgrine, also looks at distribution but by focusing on the relationship between equal opportunity and economic growth. A final chapter by Robert W. Parentau evaluates the 2000 U.S. wealth bubble from a Post Keynesian macro perspective. In the second section, “Post Keynesian Approaches to Monetary Theory and Pol-
icy,” the first chapter by Claudio Sardoni looks at electronic money and its effects on central bank policy. The next paper, by Thomas Swanke, adds to the debate of an area that Post Keynesians are exploring these days, and that is the neo-Chartalist view of money. Stephanie Bell’s chapter follows the neo-Chartalist theme and adds some discussion of Abba Lerner’s “functional finance.”

The third section of the book, “Post Keynesian Contributions on Development,” starts off with Etelberto Ortiz questioning the neoclassical approach to the economic crisis that developing countries like Argentina and Brazil are facing today. Arturo Huerta continues this questioning in his chapter “Mexico: Strong Currency and Weak Economy.” The next chapter in this section, by Rumen V. Gechev, looks at the transition of Eastern European countries and questions the neoclassical use of “shock therapy” instead of a gradual approach that recognizes differences between Eastern countries. The final chapter, “Allied, German, and Latin Perspectives on Inflation,” by Alcino Camara and Matias Vernengo, questions the neoclassical view of inflation based on the quantity theory of money and supports the PK position that inflation is based on distributive conflict.

The next section, “Kaleckian Perspectives on Growth, Inflation, and Distribution,” deals with many of the economic contributions of Michal Kalecki. The first chapter, by Harry Bloch and David Sapsford, looks at the causes of inflation by focusing on the prices of primary commodities through a structural Kalecki model of pricing. Jerry Courvisanos and Bart Verspagen in their chapter use Kaleckian theory to analyze innovation in capitalist development and to understand the role of innovation in the context of Post Keynesian theory. The final chapter, “Kalecki’s Theory of Income Distribution: The Answer to a Maiden’s Prayers?” by Tony Laramie, Doug Mair, and Peter Reynolds, provides empirical evidence to support Kalecki’s theory of price/cost mark-ups.

The final two sections of the volume cover areas that are of strong interest to Post Keynesians and always covered at these workshops—“Methodology” and “Issues in the History of Thought.” In the methodology section we have a paper by Richard Arena which looks at the views of Keynes and Hayek on individual beliefs and markets. James Juniper’s paper provides a Keynesian perspective on risk-sensitive control theory. The next paper, by Paul Downward and Andrew Mearman, pursues a contentious issue in PK thought and that is the role of econometrics. In the final section of the volume on history of thought we have a paper by Claude Gnos that shows the New Keynesian approach to employment is different from the Post Keynesian. Antonio Carlos Macedo e Silva’s paper evaluates interpretations of Say’s law, and finally Dimitri Uzunidis and Blandine Laperche’s chapter ends the volume appropriately with a paper on Joan Robinson and the power of the firm.

Needless to say the quality of these essays varies, but the benefit of this volume and the previous volumes is the opportunity to see how Post Keynesians today are dealing with a variety of contemporary and prevailing issues that confront the world economy and policy makers. It is clear, as these papers show, that Post Keynesian economics as developed in the Cambridge tradition of J. M. Keynes, Piero Sraffa, and Joan Robinson
is alive and well and providing major insights and understanding of the many current economic topics and debates that we are facing today.

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Simon Head, the director of the Project on Technology and the Workplace at the Century Foundation, argues that a significant factor in the celebrated increase in American productivity over the last ten years was neither the wonders of “labor-saving” technology nor some quasi-mystical “New Economy.” Drawing upon a clear understanding of the history of manufacturing technology, old-fashioned field work including several case studies, and a close reading of the popular and academic management literature, Simon argues that a substantial portion of the late 1990s improvement in productivity can be attributed to something characteristic of the “Old Economy”—the intensification of work or, more colloquially, “speed-up.” In short, Fordism is alive and well and permeating the service sector of the American economy.

Head’s core insight is that corporate “re-engineering” is just another name for bringing Taylorism to the service sector. He argues that what is actually new about the “New Economy” is the extension of the methods of mass production to services. Previously the constraint, from the perspective of management, was to find a cost-effective way to monitor and thereby control the performance of the service provider. In the factory this was accomplished when the logic and speed of the production process was built into an assembly line that could be controlled by management. Thanks to the computer, this assembly line experience can now be brought to the service sector as management is now able to more closely monitor their workforce. Today’s computers can instantly identify deviations from authorized practices. Workplace autonomy, privacy, personal style, pace, and initiative are now increasingly under the direct supervision and control of management.

Head successfully demonstrates that over the last ten years the application of information technology (IT) to the service sector has been shaped by management’s superior bargaining power: “The massed cubicles of the call center are digital assembly lines on which standardization, measurement, and control come together to create a workplace of relentless discipline and pressure” (p. 98). But he also stresses that it does not have to be this way:

Agents do not have to be “simple conduits,” their work governed by the digital script. Nor does their talking, walking, eating, and resting have to be measured and controlled to the last second. The electronic eye can always be dimmed to
allow agents some freedom to deal with customers as they judge best. But for that to happen, managers have to give up some of the enormous power that IT has handed them. (pp. 98–99)

As with mass production on the assembly line, the “intelligence” of the work process has been ceded to those who design and control it: management. That such de-skilling is not an accidental by-product or unintended consequence of modern reengineering is well-known to the management consultants who have developed, promoted, instituted, and profited from it. This quality is, Head observes, something that they routinely discuss among themselves. Quoting extensively from trade publications and reports by prominent re-engineering consultants, he shows how deliberately such changes are being imposed the American workplace (pp. 68–79).

What employees have lost, besides skills and the wage premiums they command, is the sense of a “career” as opposed to a “job” (pp. 54–57). Additionally, work intensity and alienation are increased as every operation, spoken word, and action is carefully pre-scripted and monitored. In light of these emerging workplace realities it is a tribute to “New Economy” propaganda, and those who are paid to promote it, that these re-engineered workplaces could ever be credibly described as “empowering the worker.”

In addition to the deleterious effects of these new workplace processes on employees, Head considers some of their effects on customers. What most thinking people know, and what re-engineers must routinely deny, is that not all services or processes can be broken down into minute subprocesses and then reconfigured, standardized, and subjected to a regimented speed-up. To illustrate this point two chapters recount the breath-taking misapplication of re-engineering to medical services. Those not cognizant of recent trends in the provision of medical services will be shocked at the new methods and procedures that have been introduced by health maintenance organizations (HMOs) in the name of “cost savings.” The consequence is no less than a fundamental change in the identity, qualifications, and agenda of the persons who make decisions concerning treatment and medication. One of the laughable ironies of our time is that the empirical studies now indicate that cost containment measures in medicine have themselves become very expensive (pp. 136–140).

Mainstream economics has long persisted in thinking of technology as a largely exogenous force that shares its blessings with one and all. By contrast institutionalists, feminists, Marxists, and others have long understood that in workplaces featuring asymmetric bargaining power technology has a strong tendency to reflect the needs and interests of the powerful. Additionally, by exclusively focusing on measures of output, economists often miss the dynamics of the workplace, thereby confusing instances of speed-up and cost shifting with the more socially desirable process of actually increasing productivity.

The strength of Head’s book is its foundation in actual workplace practices, rather than the a priori models of technical change favored by most of today’s economists. He interviewed workers, managers, and consultants in addition to reading and quoting
extensively from the trade literature. Although he does not make this connection, Head’s findings nicely complement and update the insights of the late David Gordon—whose 1996 book appears even more prescient today.

Head implicitly understands another issue long appreciated by institutionalists. This is that productivity improvements gained at the expense of the lives and livelihood of the workforce, whatever their short-term quantitative advantages, are likely to be counter-productive over the medium and long term. Several reasons can be presented. One is that employees who work directly with customers will, in the course of performing their daily tasks, inevitably develop a better understanding of the patterns of customers’ concerns than a distant manager or re-engineering consultant. A second is the invisible, but real, cost of demoralized employees to the firm. De-skilling, speed-up, and repeated downsizings have induced employees to repress what Thorstein Veblen referred to as our “instinct of workmanship” in favor of perceiving jobs as a paycheck—and an increasingly uncertain one at that. Each of these inevitably impacts performance and customer satisfaction.

This is a compelling book that presents a sophisticated understanding of the forces and ideologies driving technical change in the modern workplace. Although written by a former journalist, it is an important resource for those economists who think of the workplace as an institution—one that features power relations and is evolving over time.

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Reference


The globalization phenomenon is a hard one to spin into a nutshell. That hasn’t stopped smart guys like Francis Fukuyama from proclaiming the “end of history” or Lester Thurow from promising that the global economy will ultimately dissolve our national economies. These hoots of hyperbole have by now become clichés, leading one to think that the more absolutist the less reliable the promulgation. The current volume avoids such self-satisfied manifestos and instead seems to employ an inverted cliché, “the more things stay the same, the more they change.” The academic and popular conversation about globalization has become a battleground for competing ideas and ideologies by sporting countless dichotomies and dualistic arguments, so that the rank-and-file smart
guys feel compelled to take a side and declare the process must be one thing or another, this or that. A favorite thread of the globalization discussion is that one is either a free trader or a protectionist, with the underlying assumption being that any form of economic nationalism is thinly disguised protectionism and therefore anathema to free trade and the neoliberal imperative. The editors step into the breach for us and try to disabuse readers of the notion that economic nationalism and neoliberalism are mutually exclusive or incompatible.

Neoliberalism is by now familiar to anyone interested in the globalization debate; briefly, it espouses the singular importance of individual liberty while emphasizing the supreme significance of economic growth. Economic nationalism refers to policies guided by the idea of protecting the domestic economy from dangers including but not limited to the ravages of free trade. Such policies include tariffs and other restrictions on free trade that are odious to neoliberals yet might be in the interest of economic growth. The editors explain that economic nationalism is really a complex set of relationships between nation and economy that can indeed include neoliberalism in its various forms and provide not just theory but empirical narratives to demonstrate their point. The volume offers nine such narratives divided into four parts: “Economic Nationalism in the Post-Soviet Context,” “Developmental States and Economic Nationalism in East Asia,” “Monetary Policy Liberalism and Economic Nationalism,” and “New Forms of Economic Nationalism in a Globalizing World.” Interestingly, seven of the volume’s contributors are associated with universities outside of the United States and the remaining three are associated with American universities but also have international affiliations. This may account for their freedom from the American ethnocentrism that characterizes pundits such as Fukuyama or Thurow.

In the first contribution, “Nationalism and International Political Economy,” Rawi Abdelal focuses on the post-Soviet national identity crises by comparing and contrasting Lithuania, Belarus, and Ukraine as they tried to balance their changing relationship with the West and imperatives toward regional reintegration. Andrei Tsygankov unravels some of the complexity of Ukraine’s evolving quest for an appropriate geoeconomic identity as Little Russia or a post-Imperial Soviet in policy makers’ conception of that nation’s new role in “The Return to Eurasia.” Maya Eichler concludes the section with her chapter, “Explaining Postcommunist Transformations,” by analyzing both Russian and Ukrainian struggles between national identities and economic transformation, illustrating that economic nationalism applies in a variety of situations, and even in opposite cases.

Chapter 4 introduces a different theme with a delightfully tongue-in-cheek article by Meredith Woo Cummings, “Back to Basics,” offering incisive insights into the seemingly paradoxical relationship between economic nationalism, anti-communism, and the export-led growth enjoyed by East Asia since the 1970s.

Derek Hall next contributes his thoughts to help resolve the contradiction between liberalization and economic nationalism in “Japanese Spirit, Western Economics” with the explanation that in Japan, liberalizing policies are justified in terms of their contri-
bution to national strength and prestige, while economic nationalism motivates liberal-
ization. Klaus Muller redirects the focus to Germany with “Nationalist Undercurrents
in German Economic Liberalism.” He attempts to show, in a case not so dissimilar from
Japan, that advocacy for economic liberalism, national identity, and pride were tied
together because of that country’s exceptionalism as a bifurcated and later reunified
nation. The puzzle over Quebec’s quest to attain nationhood while eschewing a national
currency is solved in the next article by Eric Helleiner, “Why Would Nationalists Not
Want a National Currency?” In fact, the plethora of competing and intersecting goals is
well demonstrated in this chapter where liberalism and economic nationalism seem to
occupy the same space: Quebec’s drive for independence. Here nationalists argue for
stronger ties with the USA and less dependence on Canada but paradoxically against
a national currency, reasoning that monetary sovereignty is futile in today’s volatile
financial markets and would compromise national economic stability.

The final section brings us to the last two chapters’ thought-provoking themes and
additional insights into national economic goals and their relationship to
neoliberalism. Patricia Goff in “It’s Got to be Sheep’s Milk or Nothing” illustrates how a
strong sense of national identity can prevail upon government to intervene in markets
and open economies in such a way as to preserve a nation’s sense of itself, thus turning
the textbook explanation of protectionism on its head. Saving the most innovative con-
tribution for last, the editors offer Jacqui True’s tour of nation branding in “Country
before Money.” In this scenario economic nationalism was employed to promote tour-
ism, bolster exports, and attract foreign investment in New Zealand, where “market lib-
eralization [sic] in fact made the nationalist campaign possible” (p. 217).

In case we missed the common threads and themes that weave the volume together,
editor Helleiner offers us a comprehensive “Conclusion: The Meaning and Contempo-
rary Significance of Economic Nationalism.” Helleiner provides a fine ending to a vol-
ume that goes a long way toward reframing the debate between free traders and
protectionists, consumers and producers, neoclassical economists and heterodox think-
ers, the smart guys and the rest of us.

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