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Book Reviews

Editor's Note: Guidelines for Selecting Books to Review

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

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

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

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

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

A General Economics and Teaching

Explorations in Pragmatic Economics: Selected Papers of George A. Akerlof (and Co-authors). By George A. Akerlof. Oxford and New York: Oxford University Press, 2005. Pp. ix, 514. $110.00, cloth; $45.00, paper. ISBN 0–19–925390–0, cloth; 0–19–925391–9, pbk.

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The influential body of work produced by George Akerlof has turned him into an economist on the verge of a retrospective for some time now. *Explorations in Pragmatic Economics* provides that long awaited compilation of favorite hits by Akerlof and his coauthors. The underlying theme of the book is the interplay between micro and macro behavior. Minor defections from statistical sophistication, a small amount of stickiness in updating prices, or a slight tendency to hold self-confirming beliefs are only some of the micro phenomena that are analyzed and shown to generate potentially significant macroeconomic consequences.

As a compilation of papers, the book is by nature somewhat eclectic. In fact, in analogy to Leibniz's *Monadology,* the book could be thought of as Akerlof's *Nomadology,* consisting of papers journeying from area to area in economics. While it is not structured as a text book, it could certainly be a natural accompanying text for various advanced graduate classes. It is also interesting from a "history of thought" point of view. Indeed, the recent behavioral uprise has introduced and reintroduced many of the phenomena the articles in the book suggest (e.g., the ideas that agents hold self-confirming beliefs or have a taste for immediate gratification). In that respect, some of the ideas presented throughout the book will appear astonishingly familiar even to those who have not been weaned on the articles themselves.

The wonderful introductory chapter is useful in summarizing and linking the different papers. It also spells out Akerlof's dictum that economists should subject their beautiful theories to sprinkles of empirics and loitering with facts regarding the details of microeconomic behavior. Certainly, when one goes through the articles, one is left with the sense that empirical work served as a trigger or motivation for much of the (alas, parsimonious) formal modeling. For the reader who is short on time, I highly recommend the introduction as a stand alone piece.

The book is divided into segments broadly categorized as micro- and macroeconomics, each presenting several conglomerates of related papers. In what follows, I shall try to briefly describe to the aspiring reader each of the major pieces presented in the book.

The microeconomic part of the book starts with a discussion of information asymmetries and presents one of the most notable (and literal) exercises in turning lemons into fantastic lemonade, "The Market for Lemons." This important paper introduced the idea that asymmetric information may cause some markets to disappear in the absence of long-term guarantees. Indeed, if a commodity's quality is known only to the seller but not to potential buyers, the seller has incentives to pass off a low-quality good (a "lemon") as a higher-quality one. Sophisticated consumers take these incentives into account. Thus, the mere fact that a commodity (say, a used car) is on the market suggests its potential undesirability (its potential for constituting a "lemon"). Consequently, in the absence of long-term guarantees by the seller, consumers may be quite wary of purchasing such commodities, and the corresponding markets may be restricted or vanish altogether. This idea has been applied to numerous realms ranging from used cars to capital markets to online dating. Beyond introducing a new methodology to the bastion of economics of complete information, the paper is also a professional exemplar for the jaded. This mighty piece has apparently fallen several times through the cracks of refereeing processes at a few of the top journals until it finally got published in the distinguished *Quarterly Journal of Economics.* It is possibly the most influential paper (as of yet) appearing in the volume.

The sequence of papers that follow do not directly tie to the lemons paper (as Akerlof admits, partly because of that paper's rocky reception), though follow themes in their own right. In particular, the second theme of the book, presented in chapters 2–4, deals with issues pertaining to identity and their importance to economic phenomena. In two papers, the reader finds the "dent" in "identity" in the form of discrimination. In these papers, one's identity with a group (a caste) may lead to economically suboptimal equilibria. Individuals may be willing to obey the caste codes because of sufficient punishment norms for defection within the caste. Thus, the notion of "caste equilibrium" is coined and two important macroeconomic implications are analyzed. First, caste equilibrium can entail a gap between supply and demand that explains what would appear as discriminatory unemployment. Second, the models help analyze the type of codes that can be sustained by castes.

The third paper under this theme ("Economics and Identity," coauthored with Rachel Kranton) provides a more general framework for thinking about identity in economic contexts. It draws on insights from sociology, and illuminates the important aspects of identity formation, as well as makes the case that thinking about identity is crucial for understanding a plethora of economic symptoms across fields, particularly in labor economics (e.g., why women (men) are not supposed to engage in male (female) type jobs, how racial discrimination occurs, and so on).

The third set of essays, chapters 5–7, studies income redistribution and family structure, and utilizes some of the adverse selection techniques that were pioneered in the lemons paper. "The Economics of 'Tagging'" points to the difficulties stemming from asymmetric information in the U.S. welfare system. Since the government does not know incomes a priori, it "tags" groups of people who are particularly likely to be needy. This policy has many unfortunate consequences and the paper argues for earned income credits as an alternative policy. Low skill, misfortune, and family structure are a troika of significant components in the determination of economic neediness, and the papers that follow tackle the endogenous formation of impoverished groups by studying the last of the three.

Empirically, the introduction of oral contraception and legalization of abortion in the late 1960s and early 1970s was accompanied by significant declines in the rates of "shot-gun" marriages occurring during pregnancy. Akerlof's economic brethren have argued that the changes in that time empowered women, and made a woman's pregnancy less of the "man's fault." Consequently, out-of-wedlock births became more common, their associated stigma weakened, and a curious feedback loop was created. Nonetheless, as Akerlof and coauthors point out, these arguments are confounded with another historical transition—the secularization of sexual relations—that may have trumped the custom of marriage upon conception. The consequences for welfare, especially in view of "tagging" policies are dramatic. Indeed, restricting assistance to single mothers would hardly affect the scope of out-of-wedlock births, but would seriously decrease the income and welfare of unfortunate mothers and children. The analysis crystallizes the interplay between norms, identity, and social tagging.

The fourth group of papers, presented in chapters 8–10, considers the wide paradigm of economics and psychology. In two of the papers, the underlying trade-off is between holding beliefs (e.g., regarding safety measures on the job or financial returns) that are accurate and holding beliefs that are pleasant. Even a slight taste for self-comforting beliefs may have dramatic effects in various contexts. This is especially relevant for collective decision settings. Each atomic individual has little influence on outcomes and therefore may as well select beliefs that are affirming rather than correct, but the aggregate effect may be substantial. These two papers have a clear link to the identity papers, the latter suggesting a potential channel by which (possibly erroneous) beliefs are formed.

The idea that people exhibit a taste for immediate gratification is now part of the standard discourse of economics and psychology. It is thus befitting for the volume to contain a paper studying the implications of present biases and issues of self-control. Again, the paper builds up on a slight taste for immediate gratification and illustrates its potentially grand effects on procrastination, notably important for saving behavior.

The microeconomics section of the book concludes with a paper on financial mischief. In "Looting: The Economic Underworld of Bankruptcy for Profit," coauthored with Paul Romer, one particular managerial malady is identified. Accounting rules permitting, managers and owners may have incentives to loot a company (pay excessive dividends) in one period if they know the firm will declare bankruptcy later on. Indeed, the return from a marginal dollar once a firm is bankrupt is zero, and a "morbid" equilibrium exists. Following the general theme of the book, a small divergence between accounting and economic definitions may have great impacts for the survival of firms.

The macroeconomics half of the volume comprises four topics. The first, appearing in chapters 12–14, illustrates the impacts of small frictions on the economy. It starts with the maxim of monetary neutrality—expected changes in money supply are classically anticipated to be accompanied by matching changes in wages and prices that leave real variables unchanged. However, if, for instance, wage or price settings are staggered, the neutrality result may evaporate. This is illustrated in a few contexts. In the first paper firms simply alternate in introducing price changes. In the following few papers, banks use target-threshold rules, which connect money demand and the flow of funds. Furthermore, target-threshold demands for money explain why both fiscal and monetary policy are effective in changing aggregate demand in the short run, turning (to the paper) "Irving Fisher on His Head."

The second set of papers, chapters 15–17, deals with unemployment and the ultimate question of why involuntary unemployment exists. Indeed, a classical economist would expect wages to equilibrate demand and supply of labor so that those who want to work, will work, albeit for a possibly very low wage. In "Jobs as Dam Sites," a simple response a là Ricardo is given. Workers with sufficiently low skills, even if willing to work, would make employment, even at zero wage, potentially noneconomical. Just like the construction of a low quality dam, cheap as it may be, at a prime site may be noneconomical. In the papers that follow, an alternative style of explanation for voluntary unemployment is given.

Namely, the idea that firms may be reluctant to reduce wages below a certain point caring about workers' morale, as a consequence of fairness, or a reciprocal gift exchange norm (i.e., the experimentally grounded idea that paying workers well will make them exert effort in return) vis-à-vis the workers. These "morale hazard" explanations for wages that exceed market clearing levels have strong conceptual ties to the microeconomic papers dealing with identity. They both stress one's internal psychological ideals as opposed to pure economic considerations.

The penultimate suite of papers, appearing in chapters 17–19, combines some of the previous ideas regarding sluggish adaptation and analyzes the nature of macroeconomic equilibria. Classical economics adheres to the idea that changes in money supply should have no effect on economic equilibria, in particular on output or unemployment. However, a sluggish response by firms (or workers) may result in significant changes to the equilibrium at play and push money neutrality into a very dusky horizon. Similarly, a small amount of money illusion derived from, say, firms' response to workers' dislike of wage cuts, can produce an interesting tradeoff between inflation and unemployment even in the long-run, and cause the prevailing Phillips curve to be nonvertical, contrary to classical economics' credo. These papers illustrate, yet again, the significance of micro-level reactions to macro-level end results.

The closing paper of the book is Akerlof's 2001 Nobel lecture, "Behavioral Macroeconomics and Macroeconomic Behavior," that argues for the natural appeal of behavioral elements in macroeconomic analysis. Ironically, Keynes's *General Theory* is filled with psychological explanations of observed phenomena. The economists' weapons of math destruction have tamed Keynes's contributions and transformed it to what is known nowadays as classical economics. The chapter illustrates how behavioral explanations can fill important empirical gaps using psychological observations on reciprocity, fairness, loss aversion, herding, etc.

To conclude, the book compiles some of the most innovative articles written in the past few decades. It is a must read (or at least, a must skim) for any economist, be it a micro, macro, behavioral, or misbehavioral one.

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F International Economics

A Corporate Solution to Global Poverty: How Multinationals Can Help the Poor and Invigorate Their Own Legitimacy. By George Lodge and Craig Wilson. Princeton and Oxford: Princeton University Press, 2006. Pp. xii, 198. $27.95. ISBN 0–691–12229–6.
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George Lodge and Craig Wilson have tackled an important and timely topic. The past decade has seen much new thinking on development and poverty alleviation issues. Much of this work—by economists, development organizations, civil society organizations, entrepreneurs, and even large firms—advocates new and innovative approaches to development.

The time is ripe for a book that synthesizes this research and proposes a mechanism for institutionalizing it. The book jacket provided hope—hyping the authors' recommendation to create a "World Development Corporation" that will coordinate the efforts of various constituencies focused on development.

Unfortunately, the book falls far short of its potential. The argument, examples, and rhetoric all seem stuck in a 1970s time warp. There is little new beyond a proposed shift in responsibility from development agencies, who have failed, to MNCs, who the authors hope will stop acting like corporations and sign on to the agenda set by NGOs and self appointed global elites. William Easterly[1] and others have voluminously documented that this top-down, let-the-elites-save-you approach has led to little development while wasting billions. Unfortunately, this work is not even mentioned.

Lodge, an emeritus Professor at Harvard Business School, and Wilson, an economist with the International Finance Corporation, present a three-part argument, which proceeds as follows:

(1) *The persistence of poverty amid plenty is the overriding problem in the international economy today.*

(2) *Large firms are suffering from an emerging "legitimacy gap" because they are narrowly*

[1] Easterly has written two important books that make his case. See *The Elusive Quest for Growth: Economists Adventures and Misadventures in the Tropics* and *The White Man's Burden: Why the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good.*

focused on doing business and making money, not on addressing social problems.

(3) *Firms can regain their legitimacy with elites and NGOs if they shift their focus from growth and profits to addressing "communitarian" values—especially poverty reduction in developing countries.* This is to be accomplished through the creation of a World Development Corporation, a "non-profit corporation established under the auspices of the United Nations [that] will harness the skills, capabilities, and resources of leading global corporations to reduce poverty and improve living standards in developing countries" (p. 157).

The book is organized into three sections—which do not parallel the argument presented above. The first section, "The Legitimacy Gap," presents the first and second parts of the argument above. Chapter 1 discusses the problem of poverty in the world. They make a reasonable, if primarily rhetorical, case that development has been uneven and that some groups (Africans, Muslims, groups targeted by World Bank and development assistance programs) have been left behind as globalization, on average, raises living standards. The second chapter addresses the legitimacy of business and is much less convincing. The chapter starts by noting that firms operate in a larger environment that, at least in part, depends on public opinion. They then assert (via a quick reference to Adolf A. Berle and Gardiner C. Means 1967) that "the corporation has no owners . . . is not real property and thus sits in ideological limbo" (p. 22). This provides the takeoff for a long discourse on the shortcomings of "individualism" (which the authors deride) as compared with the benefits of "communitarianism" (which they laud). This philosophical discussion accounts for the bulk of the chapter (pp. 24–37). The positive references to "spaceship earth" and "a holistic consciousness" (both p. 36) are good illustrations of the general rhetorical approach.

The second section of the book, "Reactions, Responses, and Responsibilities," provides a useful overview of key players in international opinion leadership. They devote a chapter each to NGOs, corporations, and international development organizations such as the World Bank, the United Nations, and the U.S. Agency for International Development. This section provides a useful primer for readers who are not familiar with these organizations.

The third and final section, "Global Poverty Reduction and the Role of Big Business," proposes the creation of a World Development Corporation that would coordinate and channel corporate resources to those most in need.

Evaluation

While the book raises some interesting points about poverty, it ultimately disappoints for a variety of reasons.

First, their argument is almost entirely rhetorical, with little or no data to support key assertions. The overall tone comes across as naïve paternalism—i.e., *if only large firms did (what we define as) the right thing, the world would be a better place.* They use the passive voice extensively, making assertions without citations or data and expecting the reader to accept them as fact. For example:

- "Of paramount concern to many is the failure of globalization to reduce poverty and lessen wealth inequality in developing countries" (p. 9).
- "GM and the thousands of companies like it are collectives floating in philosophic limbo, dangerously vulnerable to the charge of illegitimacy and to the charge that they are beyond community control" (p. 32).
- "Sticking with the myth of the corporation as property as a passport to legitimacy has become increasingly precarious" (p. 33).
- "The lack of a civil society imprimatur amounts to an absence of legitimacy" (p. 54–55).

In the sense that someone believes these statements, they may be true. But, without references to data, surveys, or some solid foundation beyond the authors' perception, they provide a thin reed upon which to build an argument.

To pick one significant example, the authors provide almost no support to their foundational idea of an emerging legitimacy gap that must be addressed. The book presents dated anecdotes about bad business behavior (slavery, p. 21; an anonymous river polluter in 1968, p. 23; AT&T's poor treatment of women in the 1960s, p. 27; Allied Chemical polluting rivers in the 1970s, p. 22). But the examples come across as both vague and decades out of date. It's like hearing protesters today reciting Vietnam-era chants against the military industrial complex. There may be a kernel of truth in there somewhere, but that kernel is difficult to identify amidst the fog of rhetoric and sloganeering. The fog left this reader exhausted and skeptical.

To be fair, the book briefly mentions a World Economic Forum survey (p. 24) that indicates that global leaders expect multinationals to do more to reduce global poverty. But no evidence is presented that failing to focus on social issues has harmed even a single firm's ability to operate. Legitimacy is not defined or measured and, beyond selected quotes from professional activists, no evidence is provided that lack of legitimacy is reaching crisis levels.

To gauge the validity of the legitimacy gap claim, I examined *Fortune Magazine's* "Most Admired Corporations" listing.[2] The listing fails to reveal even a casual correlation between social focus and "most admired" status. At most, two of the top twenty most admired firms (Starbucks and perhaps Johnson & Johnson) emphasize their social mission. They are vastly outnumbered on the list by firms that downplay social factors in favor of primarily business objectives—including Walmart, Home Depot, Microsoft, and Goldman Sachs. This is by no means a definitive analysis, but it is one more data point than the authors provide.

Second, the authors have an unquestioned faith in the legitimacy of claims by NGOs and political elites. They assert that NGOs "form the basis of world opinion" (p. 46), that they act as "watchdogs and monitors" (p. 53), that the "lack of a civil society imprimatur amounts to an absence of legitimacy" (p. 55), and that NGOs "reflect a rising global consciousness" (p. 70).

NGOs are generally presented as pristine representatives of community needs whose demands firms must satisfy to gain legitimacy. There is little consideration that NGO claims might be inflated or strategic. Nor do the authors consider the fact that such claims are often in conflict with each other. For example, how should a firm reconcile demands for economic development with calls for preservation of the environment; or for respecting local cultures versus ensuring the rights on women, children and minorities? The authors never address, or even acknowledge, such troublesome issues.

Their position seems to be that, because private property rights are not absolute—a position that few would dispute—these rights mean nothing and should be subject to the whims of vaguely defined communities—as operationalized by NGO demands. Their approach strikes this reviewer as a recipe for chaos and ever escalating demands that corporations "do more." I'm sure that's not what the authors intend, but they never propose any basis for limiting those demands. Absent that limit, the communitarian waters appear to be dangerous for any organization with valuable resources that could be claimed by various communities.

Third, the book presents a straw man caricature of both business decision makers and the economics profession. Business executives and economists are portrayed as being mystified by intangible assets (p. 82), payback over multiple periods (also p. 82), and the importance of institutions (p. 38). This is either groundless rhetoric or a serious misunderstanding of business and economics research.

Similarly, their grasp of development economics is worrisome. The authors assert that the most successful economies in the post World War II era are those where the state led development (p. 36). This view may have been reasonable during Japan and Korea's miracle years in the 1960s and 1970s, but it is hard to square with events over the past twenty-five years. To take two relatively current cases, consider China and India. In each, the sectors and geographies with the most rapid growth in output and competitiveness were those where the state had the lightest hand (software and BPO in India, and foreign-invested manufacturing in China). Areas where the state intervened (such as agriculture in India, and heavy industry, banking, and retail distribution in both countries) have trailed badly. This is not to say there was no state role in the successful sectors, but the correlation between state involvement and sector success is clearly and strongly negative.

The authors make the case that openness to globalization is important to increasing living standards and that MNCs play an important role in raising living standards (pp. 16–17). But they are clearly uncomfortable with the idea of market forces determining economic outcomes. They continually return to questions such as "who is

[2] Available at http://money.cnn.com/magazines/fortune/mostadmired/top20/.

deciding its [globalization's] purposes and priorities? How are the decisions made? Who will benefit? At what cost?" (p. 11). In every case, their answer is that it should be bureaucrats and NGOs, not firms or the market.

They also note that "those countries with the weakest links to the outside world are the poorest," and that "the problem of FDI inflows to developing countries and the benefits that flow from them are not evenly spread among or within developing countries . . . This is a clear form of market failure" (p. 115). It's hard to see how. If countries like Zimbabwe, Venezuela, Bolivia, and Uzbekistan choose to impoverish themselves through bad policies and the scapegoating of multinationals for political purposes, it is hardly a "market failure" for MNCs to withdraw. This strikes me as a non sequitur. It's a bit like saying "Bill Belichick has done wonders for the New England Patriots, but he really deserves criticism for not doing a better job of promoting World Cup Soccer."

There are similar problems with the book's treatment of foreign direct investment. Chapter 2 portrays FDI as key to development. It certainly helps, but research over the past decade has shown that local investment, entrepreneurship, and turnover in the population of firms to be much more important than FDI.[3]

Fourth, the authors appear to be almost entirely unaware of a vast amount of research and new insights on development and poverty reduction that has been published in recent years. There are at least three strands of this work.

How development assistance should be structured—Jeffrey D. Sachs (2005) and Easterly[4] have engaged in a spirited debate over the past few years about how to design, implement, and evaluate development assistance programs. Easterly, a former World Bank economist and currently a Professor at NYU, argues that most development programs fail to take account of incentives on the ground and consistently miss their targets. He argues for bottom up programs, with designed-in experimentation and the expectation of many failures amid a few successes that can be built on. Sachs, the well known economist who heads Columbia University's Earth Institute, focuses on situations where business incentives seem to not apply—for example health programs, education, and soil chemistry. While Easterly and Sachs are the big names in this space, Robert Calderisi (2006) and David K. Leonard and Scott Straus (2003) have also made important contributions, arguing against development assistance as currently structured.

How to do business in the informal sector (i.e., operating at the base of the economic pyramid)—coming from a different direction, best-selling business books by C. K. Prahalad (2004) and Stuart L. Hart (2005) have made the case that businesses should engage with the poor, both to improve lives and because it is good business. Cornell University's Center for Sustainable Global Enterprise (www.johnson.cornell.edu/sge/resource.html), the World Resources Institute (www.nextbillion.net), and the University of Michigan's William Davidson Institute (www.wdi.umich.edu/ResearchInitiatives/BasePyramid/Resources) all have resource centers with case studies, articles, books, and working papers on these topics.

Using business tools to alleviate poverty and address social issues—there has been extensive activity in both the social and corporate sectors on this topic. For example:

- CitiBank and Duetsche Bank Group have made major commitments to invest in microlending organizations looking for profitable growth.
- Amul Dairy (www.amul.com), Hindustan Lever's Project Shakti (www.hllshakti.com), and Honeycare (www.honeycareafrica.com) have all developed innovative approaches to aggregating supply and/or distribution to lower transactions costs and reach people at the Bottom of the Pyramid. All three are focused on commercial operations, not charity.
- The Clinton Foundation's HIV/AIDS Initiative (http://www.clintonfoundation.org/cf-pgm-hs-ai-home.htm) has worked with pharmaceutical companies and developing country governments to restructure the market for and delivery of HIV, TB, and Malaria drugs to BoP populations, something that years of flashy NGO protests failed to accomplish.
- Organizations such as Acumen Fund (www.acumen.org), Care Enterprise Partners (www.care.ca/CEP/), and Ashoka:

[3] See, for example, Mark J. Roberts and James R. Tybout 1996 or Robert E. Kennedy 1997.

[4] See Easterly above.

Investors for the Public (www.ashoka.org) focus on matching socially minded investors with organizations that produce high social returns.

Unfortunately, the book doesn't recognize or build on any of this new thinking.

Fifth, the book is quite vague regarding the specific obstacles that keep MNCs from working in developing countries, or how the proposed World Development Corporation (WDC) would improve the situation.

There are many legitimate reasons that large multinational firms don't focus primarily on social outcomes in the world's poorest communities. These include, among other issues:

- The fact that they raised money from shareholders with the agreement that they would pursue economic value creation on shareholders behalf;
- National regulations that keep MNCs out or make it impossible to operate profitably;
- The risk of expropriation;
- The risk that some level of involvement will inevitably invite calls to "do more," etc.

Unfortunately, the authors provide almost no details on how the WDC would address any of these issues. It comes across as a feel-good admonition (i.e., "let's do something"), but one with no underlying foundation. This would make for a nice discussion over lunch at the faculty club, but is painfully naïve given existing stakeholders in corporations, the long record of failures by the UN and other development agencies, and the complexities of operating in developing countries.

Finally, as noted at the beginning of this review, the book offers what is essentially a deep pockets argument. Poverty is an enduring problem and those charged with addressing it (national governments and development organizations) have largely failed. So the book seeks to shift the burden elsewhere. The book returns to this theme time and again:

- The resources of the world's MNCs are central to solving this challenge [poverty]. "These are challenges that go beyond the capacity of the public sector. To help address these challenges, the private sector has to take some responsibility for economic and social development as well." Quoting Peter Woicke of the International Finance Corp (p. 18).
- "Recently, these agencies [development organizations] together with their NGO counterparts in civil society, have belatedly come to the realization that the power of MNCs can considerably augment their own disappointing efforts to reduce global poverty...they have failed to harness effectively the collective power of the MNCs" (p. 92).
- "The WDC is needed to fill the gap between the poverty-reducing intentions of the international development agencies and the poor countries they are supposed to help . . . [it] is a missing link between those agencies and the multinational corporations that have the capacity, the resources, and increasingly the will to do the investing" (pp. 158–59).

In the end, the authors' argument boils down to a type of Willie Sutton wisdom. Reportedly, when the famous bank robber was asked why he robbed banks, he responded, "because that's where the money is."

MNCs have abundant resources (capital, talent, products) that Lodge and Wilson believe should be put to better use than pursuing economic value on behalf of shareholders. They therefore advocate changing the rules of the game to direct those resources to their "communitarian" goals. Perhaps Willie Sutton felt the same way about cash sitting unused in the bank. This mission change might or might not have an effect on global poverty, but the authors never make the case as to why this fundamental shift in focus would be of benefit to MNC's owners and employees. Nor do they grapple with the possibility that it might change the very nature of these organizations, perhaps not for the better.

There is clearly something happening in the fields of development and poverty reduction. This is an exciting time for business operating in developing countries, for development organizations considering the design of their assistance programs, and for academics considering these programs' impact. There is still a good book to be written that captures this creative tumult. Unfortunately, this book is not it.

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A Strategy for IMF Reform. By Edwin M. Truman. Policy Analyses in International Economics, no. 77. Washington, D.C.: Institute for International Economics, 2006. Pp. xii, 128. \$23.95, paper. ISBN 0–88132–398–5.
JEL 2006–0540

Reforming the IMF for the 21st Century. Edited by Edwin M. Truman. Special Report 19. Washington, D.C.: Institute for International Economics, 2006. Pp. xiii, 559. \$27.95, paper. ISBN 0–88132–387–X. *JEL 2006–0946*

The world economy has, to put it mildly, seen dramatic changes in the past two decades. Private capital markets have exploded in size, complexity, and global scope. Long-moribund countries have stumbled to life economically, in Asia, central Europe, and Africa. These transformations, which will continue, have made it imperative for the international financial system to adapt just as dramatically. The official "architecture," of which the International Monetary Fund (IMF) has been both the foundation and the protective roof since the end of World War II, can support this ongoing mansionization only if it is thoroughly modernized.

Literature on IMF reform is not in short supply, but much of it is of little practical value. The difficulty is that the topic has a technical dimension that eludes many analysts, a bureaucratic dimension that is mystifying even to many insiders, and a political dimension that injects emotional advocacy into the debate. A shelf of recent books that clear these hurdles reasonably well would include Colin I. Bradford and Johannes F. Linn (2007), Peter B. Kenen et al. (2004), Gustav Ranis, James Raymond Vreeland, and Stephen Kosack (2006), and Ngaire Woods (2006). Each presents points of view that illuminate certain aspects of the reform discussion, and each one places the debate about the IMF within the broader context of reform of the international financial system. The IMF itself has developed a "medium-term strategy" for reforming its operations and its governance that is more firmly grounded in the art of the possible than is most of the external literature (IMF 2005). The challenge now is to build on these specific contributions to develop a comprehensive strategy that is both bold and practical and that will give the system the flexibility it needs to respond to today's problems and to be prepared for tomorrow's.

Edwin (Ted) Truman has responded to this challenge with a linked pair of volumes. Most of the longer (edited) book is a collection of papers that were presented at a conference held at the Institute for International Economics in Washington in September 2005. It includes several papers by economists with strong academic and practical policy experience, including Barry Eichengreen, Kristin Forbes, Michael Mussa, Steven Radelet, and John Taylor. Other authors are stars of the Washington think-tank circuit, including Fred Bergsten, Nancy Birdsall, William Cline, Morris Goldstein, Desmond Lachman, and John Williamson. The list also includes good representation of experts from Europe (Lorenzo Bini Smaghi and Tommaso Padoa-Schioppa), Asia (Yu Yongding), and Latin America (Ariel Buira and Martin Redrado). Roughly a third of the twenty-nine contributors have worked at the IMF at some point in their careers, a ratio that lends a strong does of realism to the collection without tainting it with excessive proximity.

Most of the conference papers are short expositions of specific suggestions for reforming or strengthening the IMF. Goldstein calls for the IMF to get tough with currency manipulators, of which he presently counts China to be the worst offender. Williamson repeats his long-standing appeal for a system of reference ranges for exchange rates. Several papers propose new lending facilities within the IMF to direct resources toward high-priority goals. Buira, Lachman, and Karin Lissakers offer suggestions for improving the financial resources of the IMF. Radelet sets out a framework for restructuring the IMF's assistance to low-income countries. Randall Henning wants regional entities such as the European Union to become members of the IMF. Miles Kahler wants a more open selection process for IMF management and a more independent Executive Board. Fred Bergsten urges replacing the "illegitimate" and "ill-equipped" G-7 as a "steering committee" for the world economy with a broader group of finance officials that he dubs the "F-16" (pp. 282–90).

The last section of the book constitutes a panel discussion on the larger issues of IMF reform. Each of the five panelists makes substantive and thought-provoking contributions. Eichengreen argues that the IMF "is adrift on the fundamental . . . issues of the day" (p. 499), and he concludes that the overriding issue is the need for better intellectual leadership from within the IMF. Mohamed El-Erian sketches a wish list of reforms aimed at making the IMF a "trusted advisor" for national governments and a "center of excellence on . . . macroeconomic and financial issues," with adequate financing and "a more legitimate governance structure" than it now has (p. 507). Padoa-Schioppa decries what he sees as a decline in the quality of political leadership in the world, especially in Europe, and the effect that it has had on the IMF and other multilateral institutions. Yu Yongding presents a Chinese view on exchange rate management in counterpoint to Goldstein, and he argues for more inclusive governance to overcome the damage that he believes the IMF has done to its own reputation in recent years, especially in East Asia. Truman concludes with a broad overview.

In contrast to these short and pithy papers, the conference volume opens with a 126-page essay on IMF reform by the editor. Here, Truman summarizes the many points made by others throughout the volume, and he sets out his own comprehensive agenda. With just a few edits, this section has been reproduced on its own to form the second book under review, *A Strategy for IMF Reform.* Anyone who has the conference volume does not need the other, but the small one will suffice for anyone who just wants the essence of the various arguments.

The core of Truman's strategy is a governance reform in which the established but relatively stagnant advanced economies (mostly in Europe) would lose representation and voting power ("chairs and shares") in favor of the emerging powers (mostly in Asia) that have grown rapidly in recent decades. The European Union, in this scheme, would retain its influence by consolidating its representation in a single seat on the Executive Board. Without some such reform, the institution's policy advice will increasingly be denigrated as illegitimate and will ultimately be ignored. Truman supplements this generally sensible but politically charged suggestion with a detailed and wide-ranging list of operational recommendations for strengthening surveillance and lending and for putting the Fund's financing on a solid footing. Most of those recommendations flow from the conference papers, but Truman also draws nicely on the earlier literature and on his own long experience in dealing with the IMF as a senior official of the Federal Reserve Board and the U.S. Treasury.

Where does this strategy fall short? Obviously, it will not convince or satisfy the legion of "academics and op-ed columnists [who] have based their careers on criticizing the IMF" (Eichengreen, p. 498). These books are aimed instead at convincing the mainstream of economists, political scientists, and policymakers that the IMF—and the international financial system—can be and must be brought up to date in a positive way. Although any well informed reader will—and should—find many points with which to quibble, perhaps even violently so, most will find the broad canvas to be well painted.

Two limitations stand out. First, implementation of Truman's reform strategy will require a wide political consensus that will not be easy to achieve. Truman is sensitive to that problem, but none of the contributors offers a full solution to it. Second, the world economy is continuing to change, and the evolution of the next twenty years may well be even more dramatic than the last two decades have seen. For example, what happens when sub-Saharan Africa, which has long been the epicenter of poverty and economic stagnation, becomes the next East Asia? Even if all of these books' proposals were put in effect tomorrow and proved successful, how can the system be saved from having to play catch-up again? This answer to this and other prospective dynamic questions will have to wait.

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Is War Necessary for Economic Growth? Military Procurement and Technology Development. By Vernon W. Ruttan. Oxford and New York: Oxford University Press, 2006. Pp. xi, 219. $45.00. ISBN 0–19–518804–7.

JEL 2006–1344

This interesting book can be read in two ways: first as an exposition of the important place that military procurement has played in the development of some key twentieth century technologies; second, as an argument that the threat of war may be necessary to induce the R&D needed to create the fundamental new technologies that drive long run growth.

Read in the first way, the book is a companion to Vernon W. Ruttan (2001), in which he concluded that ". . . the public sector had played an important role in the research and technology development for almost every industry in which the United States was, in the late twentieth century, globally competitive" (Ruttan 2006, p. vii). In the present book, he concentrates specifically on military procurement and government-supported R&D as a source of U.S. technological advancement. Six chapters develop this theme in relation to a series of new technologies, all of which he terms "general-purpose" (GPTs). In chapter 2, he argues: "The emergence of an independent machine tool industry in the United States around the middle of the nineteenth century and of mass production in first decades of the twentieth were the direct consequences of the investment by the U.S. War Department during the first half of the nineteenth century in the invention of armaments, in the development of machines, and in machine methods of manufacturing" (p. 31). Chapter 3 discusses the publicly financed research administered by the NACA that developed some key interwar aircraft technologies, and the importance of military procurement in the development of the Boeing 707 and 747. Chapter 4 discusses nuclear energy, which developed from the government-financed Manhattan project. Chapters 5 and 6 deal with computers and the internet, the former being heavily subsidized by the military during the Second World War and the later being created by the military. Chapter 7 deals with the space industries, which received heavy government backing during and long after World War II.

No one who has studied this book, and its 2001 predecessor, should be willing to pronounce the common thought-suppressing dictum "governments cannot pick winners." Clearly, governments have picked and backed some spectacular winners. But if we are to assess the importance of public support in developing these new GPTs, we need more than the material in this book.

First, the development of some of the technologies would have been long delayed, if not precluded, if it were left solely to private-sector R&D. Prime examples are the internet and space-related technologies. Others would have been merely slowed, although it is hard to know by how much. For example, both electronic computers and aircraft (propeller and jet driven) were being researched by private-sector firms and universities. Without military and other government support, these technologies would no doubt have been developed more slowly but it is hard to believe that the path of their developmental trajectories would have been substantially altered.

Second, the book deals almost exclusively with developments in the United States. If one is interested in U.S. competitiveness and technological leadership, that may be an acceptable limitation. But for those interested in economic growth more broadly, it is not. In today's globalized world, the great bulk of technological advances are made in a small subset of the advanced countries and then diffuse to those others that have the appropriate receptor capacity. There may be a persistent gap in per capita income levels between leaders and advanced-country followers but there appears to be no long term gap in their growth rates. So neither world nor U.S. economic growth might be seriously hampered if the proportion of world technological advances fell in the United States and rose elsewhere. The book's exclusive concentration on the United States thus does not help in assessing whether less U.S. publicly financed R&D would lead to a slowing of world growth rates or merely a geographical redistribution of the sources of technological advance. Consider one example. Ruttan correctly judges that U.S. military procurement played an important part in the development of the Boeing 707, the basis of the United States' initial competitive advantage in long haul commercial jet aircraft, and in the 747 that helped the United States maintain that lead. But he does not consider what would have happened without that U.S. support. The French developed the first successful medium range commercial jet aircraft in the Caravel. Although the British lost their early lead in jet transport when metal fatigue unexpectedly ended the commercial life of the Comet 1, this did not prevent them from developing a long range passenger jet. The VC-10 was a magnificent aircraft with 4 engines mounted in the tail rather than below the wings. Unfortunately, for the British, its operating costs were just high enough to tip the commercial scales in favor of the 707. But absent the 707, the VC-10 would have been the jet to first establish long range flights as commonplace, and successive versions would have progressively lowered its operating costs. The moral to this story is that unless we know what was happening in other technologically advanced countries, it is hard to judge the effect that U.S. defense-related assistance had on the world's growth of technological knowledge, let alone the overall rate of world, or U.S., economic growth.

Ruttan does mention the rest of the world when he concludes that "... American, *and the global,* technological landscape in which we live today would be vastly different in the absence of military and defense-related contributions to commercial technology development" (p. 162, emphasis added). If by "technological landscape" he means the geographical distribution of the sources of technological change, one cannot disagree. But if he means the overall rate of technological change and economic growth, we cannot assess his claim without knowing how much of the competing developments elsewhere in the world were financed for defense-related reasons.

Third, the book looks solely at government successes. As well as backing some major winners, governments throughout the advanced world have backed some spectacular losers. This observation raises two questions. First, what is the cost–benefit balance where failures are regarded as "costs" and successes as "benefits"? Clearly, the book does not provide the raw material for such an assessment. Second, can we gain any insight into the factors that tend to make for success or failure when governments select technologies for backing? By comparing a series of cases where governments picked winners (as studied, e.g., by Ruttan) and where they picked losers (as not studied by Ruttan), my coauthors and I have been tried to identify the conditions that tend to favor success or failure in such endeavors. (See in particular Richard G. Lipsey and Kenneth I. Carlaw 1996 and Lipsey, Carlaw, and Clifford T. Bekar 2005).

The second way in which the book can be read, the one the author chooses to emphasize, asks if defense-related support is necessary for long-term economic growth. The argument is as follows: (1) GPTs are drivers of long term economic growth; (2) most modern GPTs have received substantial defense-related support in their early stages; (3) the power of any one GPT to generate growth wanes as it "matures" in the sense that most of its potential spillovers become exploited; (4) structural changes in the defense-related sectors may seriously curtail the needed public support in the future; (5) such support will be unlikely without the threat of a major war?

He takes point (1) for granted, although not all economists would. We have argued this point in detail in Lipsey, Carlaw, and Bekar (2005) where we define a GPT as a technology that comes to be widely used for multiple purposes and to have many spillovers that enable myriad other derivative and dependent technologies. Point (2) is the subject of his chapters 2–7, as discussed above. He introduces point (3) by saying: "A major deficiency in the induced, evolutionary, and path-dependency literature on technical innovation . . . is inadequate attention to the problem of technological maturity. After experiencing rapid or even explosive development along an initial trajectory, the older general-purpose technologies . . . have often experienced a period of technological maturity or stagnation" (p. 163). He argues that, since GPTs eventually reach maturity, new GPTs are the essential driving force of sustained economic growth, so anything that stops or drastically slows their development will drastically slow the pace of economic growth. As to the alleged neglect of this characteristic, I offer one of the many possible conflicting quotations from our work. It comes during the discussion of the first of a series of models of GPT-induced growth: "This two-sector version of the model illustrates the rejuvenating power of GPTs. In the absence of the arrival of a new GPT . . . the growth rate . . . converge[s] to zero asymptotically. . . . However, the arrival of a new GPT encourages further applied R&D and rejuvenates growth" (Lipsey, Carlaw, and Bekar 2005, p. 449).

With respect to point (4), Ruttan's asks if ". . . changes in the structure of the U.S. economy and of the defense industries and the defense industrial base preclude military and defense-related R&D and procurement from continuing to play an important role in the generation of new general-purpose technologies" (p. 166). Although he does not say so explicitly, it seems from his discussion of the decline of spin-offs, the demise of the dual-use model (military and commercial) for military R&D, and the process of consolidation in which competition among many contractors is no longer encouraged by the DoD, that his answer is "Yes."

He raises point (5) by asking ". . . whether a major war, or threat of a major war, is necessary to induce the U.S. political and economic institutions to commit the very large resources necessary to generate or sustain the development of major new general-purpose technologies" (p. 176). This, he says, generates three subquestions. First, can the private sector generate the needed resources? He answers that it cannot because, among other reasons, the gains are so diffuse that they are difficult to capture by the firm conducting the research and because there is a long gestation period before commercial gains are substantial. Second, might a more aggressive policy of targeting public support for commercially oriented R&D provide the necessary financing? Ruttan is skeptical of this avenue because although in the past such programs ". . . have generated substantial economic benefits, . . . even the most successful programs must be evaluated in terms of their contributions to evolutionary rather than revolutionary changes in technology" (p. 182). The third is ". . . whether military and defense-related R&D can again become a source of major new general-purpose technologies" (p. 183). And he answers "no" largely for reasons given in his discussion of point (4) above.

If we grant all of Ruttan's arguments about new GPTs being needed to sustain the growth process and the inability of the military industrial base to generate the necessary R&D in the absence of the threat of war, doubts still remain. First, war is not the only threat that can lead to a massive mobilization of resources. What is required is the public perception of a clear and present danger and there are threats on the horizon at least as serious as war. The evidence of climate change is all around us. Whether or not a serious danger is perceived by the public soon enough to avert a major disaster, the demand for resources and new technologies to control the causes and to cope with the consequences will be enormous. Even if climate change does not turn out to be as serious as most scientists believe, the day is approaching when supplies of petroleum begin to dwindle significantly. A steadily rising price of petroleum will then unleash a major R&D effort to replace it, not only as a fuel but also in its myriad byproduct uses.

Second, much of the public support for R&D in other industrialized countries, such as France and the United Kingdom, has come not from defense-related concerns but from concerns about their national competitiveness and rates of growth. Although public support may be necessary for the development of new fundamental technologies in these countries, the threat of war probably is not. (For an analysis of the success and failures of some such policies, see Lipsey and Carlaw 1996.)

Third, electricity provides an obvious counter example to the proposition, implicit in Ruttan's argument, that public support is always needed for the development of new GPTs. Electricity's extended development trajectory, starting with the publication of Gilbert's *de Magenta* in 1600 and ending with the invention of the dynamo in 1867, was without defense-related assistance. There is insufficient space here to investigate the question of the types of GPTs that do and do not require extensive public support in their early stages—the laser and the internal combustion engine seem to be other counter examples to the thesis of the necessity of major public support of new GPTs. As electricity and steam engine show, long gestation periods are not enough to make public support necessary, Nor are substantial nonappropriable benefits, which are a characteristic of all GPTs, since what is needed for private development is that the small portion of the massive benefits that can be captured by the private developers is sufficient to repay the risks involved in undertaking the necessary R&D. In the absence of a full analysis of this issue, it is hard to predict what happen if major public support were withdrawn from the development of further GPTs.

In conclusion, we owe Ruttan a debt of gratitude for demonstrating yet again the importance of public sector support in the development of many major technologies and for raising, if not settling, the key issue of how important future defense-related support needs to be, and will be, in developing future GPTs and sustaining long-term economic growth. If he has not settled these issues, by raising them he has pointed the way to further potentially fruitful research, which could be the subject of a valuable conference volume.

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Individual Accounts for Social Security Reform: International Perspectives on the U.S. Debate. By John Turner. Kalamazoo, Mich.: W. E. Upjohn Institute for Employment Research, 2006. Pp. ix, 195. $40.00, cloth; $18.00, paper. ISBN 0–88099–283–2, cloth; 0–88099–282–4, pbk. *JEL 2006–0977*

While reforming Social Security might appear to the casual observer to be on hold, the recent midterm elections have also improved the Democrats' negotiating position. *Individual Accounts for Social Security Reform* is a good introduction for readers with little or no familiarity with the Social Security reform debate. Many new Democratic staffers will find much in common with the author's positions on the issue; the book should top their reading list.

The book draws on some international evidence for the expressed purpose of influencing the United States Social Security reform debate over the adoption of individual accounts, a key element in President Bush's mantra to modernize Social Security. John Turner begins by categorizing different public and private pension models around the world according to their main properties including whether they are voluntary or mandated; whether the accounts are managed by individuals or by the government; and, several other characteristics. In doing so, a useful taxonomy of different pension programs emerges.

Turner notes in the preface that he wrote the book on his own time and that his opinions are not necessarily those of his employer, the AARP. But the book is largely consistent with AARP's lobbying positions. To his credit, Turner avoids the misleading "privatization" label that opponents of personal accounts have used with recent political success. Yet, his analysis is a not-too-subtle attack on individual accounts.

Turner, in particular, uncovers every possible risk that can be associated with individual accounts including market risk, agency risks (high administrative fees), poor portfolio choice risk, tax policy risk, disability risk, and a litany of other risks. While most of the book focuses on these risks, he does not really prioritize them or explain how many of these risks can be mitigated with a sensibly specified reform. To be sure, individual accounts will always be susceptible to systemic (market) risk even they hold a well diversified portfolio; proponents of personal accounts err when they say otherwise. But individual accounts need not be as scary as Turner suggests either. Indeed, the three model plans proposed by the

President's 2001 Commission to Strengthen Social Security dealt sufficiently with most of the risks that Turner discusses.

While highlighting the risks in individual accounts, Turner attempts to minimize the risks associate with the traditional Social Security system. He devotes only a single, fairly cryptic paragraph to explaining how traditional pay-as-you-go systems are subject to changes in the ratio of workers to retirees. But he fails to clarify how attempting to pre-fund the imminent significant demographic changes inside of the U.S. Social Security system requires building up a Trust Fund, which is subject to enormous policy risk. Indeed, some empirical evidence suggests that additions to the U.S. Social Security Trust Fund during the past several decades have contributed very little, if at all, to an increase in national savings and, therefore, have not helped the nation prepare for the retirement of the baby boomer generation (Sita Nataraj and John B. Shoven 2004; Kent Smetters 2004).

The U.S. federal government focuses on the so-called "unified budget surplus" that includes additions to the Social Security Trust Fund. Targeting a zero or a fixed budget deficit, consistent with the usual rhetoric by policymakers, essentially guarantees that additions to the Trust Fund are spent elsewhere in the federal budget, either in the form of non-Social Security tax reductions or spending increases.

Indeed, one of the strongest arguments for personal accounts is they help deny policymakers the opportunity to squander retirement resources on other budget priorities. Of course, personal accounts are not a guaranteed "lock box" either since workers might demand pre-retirement access, as with 401(k) accounts. But policymakers would likely be more protective of this perceived first-tier source of retirement income than income from second tiers, such as 401(k)'s. International evidence largely supports this claim.

Turner also pushes another AARP theme regarding the specific design of the personal accounts, in particular, whether they are constructed as a "carve out" or an "add on." A "carve out" account is created by "carving out" some of the payroll tax for deposit into an individual account. An "add on" account imposes a new payroll tax on workers in addition to the existing payroll tax.

A carve-out type of reform is generally complemented with a reduction in the traditional Social Security benefit since workers pay less into the traditional system. An add-on account is typically constructed to provide a new source of retirement income on top of the traditional benefit. Whereas an add-on account injects new money into the retirement system, a carve out account could either be fiscally neutral in present value or, as in two of President's 2001 Commission plans, actually reduce the government's net revenue. (These two plans, however, include other features that reduce the growth rate of spending. In one of the Commission's plans, the carve-out account actually increases the government's net revenue in present value.)

Both Turner and his employer, the AARP, argue that carve-out accounts are not an acceptable policy option. The AARP is fairly blunt: "diverting Social Security revenues into individual accounts shifts risk to the individual and hurts the financial status of Social Security itself."[1] The AARP, of course, does not oppose asset accumulation in addition to Social Security, as in an add-on account: "Social Security was never intended to be your only source of retirement income—just the safe, reliable piece of a smart retirement plan. Ideally, you should build on Social Security's base with a pension, an IRA, a 401(k) or other investments. When added to Social Security, these kinds of private investments help provide a more adequate retirement income."[2]

The distinction by Turner and the AARP between carve-out and add-on accounts, however, seems more based on the "principle" of not altering Social Security benefits rather than on a careful analysis of the appropriate level of retirement income that should be provided by a traditional Social Security system versus other sources.

Suppose, for example, that Social Security benefits happened to be equal to 80 percent of their current levels. To increase retirement income, suppose that lawmakers then decided to institute a new add-on account so that the new combined annuitized benefit equaled the current level. I assume that Turner and the AARP would support

[1] http://www.aarp.org/money/social_security/a2003–03–26-ssprivatization.html [last checked, December 29, 2006].

[2] Ibid.

this reform. But such a system would be identical to the current system with a 20 percent carve-out account.

Turner provides no framework for understanding how much retirement income should come from a traditional Social Security system and how much should come from additional resources. His underlying principle seems to be clear and quite simple: Don't mess with Social Security.

Turner writes that "[p]erhaps the chief rationale against mandating carve-out accounts is that they place too great a burden of financial risk on low-income workers, especially when the plans replace part of a traditional social security program, reducing the base level of benefits" (p. 34). To be sure, a naive carve-out plan would likely expose low-income workers to an unacceptably small retirement benefit if the accounts were allowed to be invested in risky assets. But many carve-out plans also substantially increased low-income worker benefits under the traditional system, including the President's 2001 Commission plans. A carve-out was chosen by the Commission precisely to help avoid requiring low-income Americans from saving additional resources for retirement during a period in their lifetime when they are likely liquidity constrained.

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Rumors of Baseball's Demise: How the Balance of Competition Swung and the Critics Missed. By Robert Cull. Jefferson, N.C. and London: McFarland, 2006. Pp. vii, 239. $29.95, paper. ISBN 0–7864–2251–3. *JEL 2006–1401*

Competitive balance in sports leagues seems to have become the favorite topic of debate among sports economists. As do sports, debates stir passion. Debates also tend to call forth straw men, partial truths, cooked data, and occasional mindless empiricism.

Two recent books discuss competitive balance in baseball, *The Wages of Wins* by David J. Berri, Martin B. Schmidt, and Stacey L. Brook, and *Rumors of Baseball's Demise* by Robert Cull. Although the books have different analyses, they each invoke the same straw man to motivate much of their presentation; to wit, they claim that the mainstream view is that baseball suffers from serious problems of competitive imbalance.

Berri, Schmidt, and Brook, drawing from the work of Stephen Jay Gould (1986) and Andrew Zimbalist (1992), argue that the principal reason why baseball's competitive balance has steadily improved over the decades is talent compression. That is, a smaller and smaller share of the expanding population from which baseball draws its talent makes it to the major leagues. As such, given the presumed normal distribution of baseball-related skills, the difference between the best and worst players is narrowing. This both shrinks the talent gap among teams and makes it harder to identify (and, hence, to trade for or purchase) the top performing players, lessening what might otherwise be a significant advantage of the high-revenue teams.

Cull, in contrast, ignores talent compression altogether and asserts three different explanations: first, the introduction of baseball's amateur draft in 1965, as well as a series of subsequent reforms that strengthened its impact; second, what he calls "pitcher variability"; and, third, baseball's "compensation structures." Let me consider each of these factors in turn.

Cull begins his discussion of the draft with an interesting presentation of data on the number of minor league affiliates each club had in the decades before 1965. Although Cull doesn't call attention to it, it turns out that the number of affiliates was closely correlated with a team's city size and also with a team's performance. As the number of affiliates began to be standardized after 1965, it may have been the closing of the discrepancy in the size of teams' minor league systems rather than the amateur draft itself that had the leveling effect.

Cull traces the evolution of the amateur draft, showing how the selection of players shifted away from high school and toward college graduates after 1981. The shift occurred, he argues, primarily because teams grew increasingly aware that the performance of college players was more predictable. While Cull highlights some important developments, he fails to demonstrate whether the draft itself actually promoted balance. He sidesteps the academic literature on the Coase Theorem that basically concludes that the draft should have a minimal impact. He also sidesteps the reality that the reverse-order draft gives the bottom teams at most one pick (of dubious benefit) before the top teams. Finally, he does not consider the possible impact of the large-scale introduction of Latin American and other foreign players over the last fifteen years.

Cull's discussion of pitcher variability is also less than compelling. He starts by asserting that pitching is more important than hitting for winning a title and purports to demonstrate this empirically by showing that most of the time championship teams had staff ERAs that ranked among the top in their league. Cull offers no standard statistical tests here.

Cull proceeds to argue that pitchers' performance varies sharply from year to year, making it more difficult (a) to predict which pitchers will be successful and (b) to hold on to a winning team. His evidence is based on the metric of whether or not pitchers are in the top 25 percent of win percentages for two consecutive years. It is not at all clear why he uses this criterion. Recent statistical evidence suggests that pitching performance is most cleanly measured by ERA, strikeout–walk ratio, or hits-plus-walks per inning. Obviously, a pitcher's win percentage depends much more on the offensive and defensive support he receives than other measures and, hence, has much more noise. In any event, it is true that pitchers' performance has become more variable over the years, but so has hitters' performance. Both of these outcomes might be related to talent compression or other factors.

Finally, Cull argues that the statistical correlation between team payroll and team performance is weak. Here too he makes some worthy, though not novel, observations. For instance, while the correlation coefficient between payroll and performance has been statistically significant since the early 1990s, the causality may be running from performance to payroll rather than vice versa. Deciphering the direction of causality, he notes, is often confounded by statistical tests using end-of-year payroll data. This is because teams in the playoff hunt (i.e., teams already performing well) will frequently add players (and payroll) after the All-Star break, while teams performing poorly will often subtract players (particularly those with high salaries). Further, teams that win see the market value of their players rise, so player contracts go up in following years.

But, as elsewhere, Cull does not marshal his evidence as well as he could. He does not do Granger causality tests. He divides MSA population by two for cities with two teams, instead of allowing the effect of a second team to weight itself via a dummy variable. There is plenty of econometric evidence that having more than one team in the city deepens the baseball culture in a town and expands the city's fan base. One anecdotal piece of evidence, however, is as telling as anything. When the Giants and Dodgers left New York following the 1957 season, the Yankees' attendance actually dropped in 1958 relative to 1957—despite the facts that (a) the team had two fewer "competitors" in New York in 1958 and (b) the team went to the World Series in 1957 and won the World Series in 1958. He also does not consider the effect of new stadiums and player development expenditures on team performance.

Cull culminates by savaging baseball commissioner Bud Selig. While anyone who has followed baseball since 1992 knows that the commissioner is far from being beyond reproach,[3] Cull's assault is not justified.

Cull writes that Selig has overstated the problem of competitive balance. The fact is that most of Selig's comments as well as those of other authors cited by Cull refer to a period when it was difficult to overstate the magnitude of the problem. From 1995 to 2001, only eleven of MLB's thirty teams made it to the League Championship Series and no team outside the top quartile in payrolls won a single World Series game. Selig had reason to be concerned about balance, as he did to be concerned about the financial circumstances of many teams from smaller markets and in older facilities. In fact, Selig, to his credit, has changed his tune. Since 2002, he consistently has described MLB's economic model and its outcomes as successful.

To be sure, Selig's main analytical foray into the competitive balance issue was more insightful than much of the academic writing on it. He stated that the competitive balance goal was for fans of each team to have faith and hope at the

[3] Cf. Zimbalist 2006.

beginning of each season. This perspective properly focuses attention on the fans. It is the fans' reaction, after all, that makes balance important. Sports economists tend to use measures such as the ratio of the actual to the idealized standard deviation to measure balance which might make sense statistically, but have little meaning to the fans. If fans don't respond to it, then it won't impact attendance and there is little reason for the team owners to do anything.[4]

Statistical measurements are fine as long as they are grounded in institutional reality and sound theory. Cull's treatment, while interesting and insightful at points, misses this basic connection and leaves his readers with less than a home run.

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N Economic History

Among Empires: American Ascendancy and Its Predecessors. By Charles S. Maier. Cambridge and London: Harvard University Press, 2006. Pp. 373. $27.95. ISBN 0–674–02189–4.

JEL 2006–1439

Among Empires: American Ascendancy and Its Predecessors, by Charles S. Maier, the Leverett Saltonstall Professor of History at Harvard University and a distinguished scholar of international relations, is essentially two related essays on the theme of empires. One is a generalized discussion of the rise and fall of approximately three dozen empires, over an extremely broad historical and geographic span. It is an attempt to present a comparative perspective on the types of problems that past empires have had to deal with, and it does provide a rather cautionary warning that each of these empires had come to an end, after periods varying from only a small number of years to about fourteen centuries. The second essay deals with the rise of the American empire, particularly after the end of World War II, with some attention to developments during the continental expansion of the nineteenth century and with the imperialists after 1898. It also includes some discussion of the future prospects for the present American empire, given the current world situation and "the openness of the moment" (p. 295).

According to Maier, it is generally argued that empires represent expansion "by conquest or coercion" and the ability to "control the political loyalty of the territories it subjugates," whether by direct power or by the installing of "compliant native leaders" (pp. 24–25). By these standards, he claims, the United States has not, until recently, met the requirements of empire since it "has not engaged in a sustained program of conquest overseas" (p. 25) and since not all political decision making in the territories was originated in Washington. Yet it did have considerable influence throughout the world due to its military power, its economic strength, and its cultural and ideological influences. Among the numerous interesting aspects of empires considered by Maier are their effects on internal liberties as well as liberties in the colonies; differences between land empires and maritime empires, which have an influence on the ratio of metropolitan to colonial population; and the nature of political controls coming from settlers, from the metropolis, and from local residents. The basic characteristics of an empire in contrast with a nation are discussed particularly in regard to the question of possible differences in the extent of civil liberties in nations and in empires, but this comparison is not examined in great detail. Empires often involved violence, particularly in frontier or border areas, where either they met with rival empires or else the role of distance limited ability to control some fragments of the empire. Based on the works of Edelstein, Davis and Huttenback, and O'Brien, Maier suggests

[4] This theoretical point is developed in Zimbalist 2002.

that the costs of law, defense, and administration possibly did more to redistribute income to the elites than to provide benefits to the overall domestic society. Although the number of military and civilian deaths in colonies and otherwise has remained high throughout the twentieth century, for some of the nations which had liberal regimes, "colonial empires perished because the force needed to preserve them seemed unacceptable to a paralyzing fraction of domestic opinion" (p. 61). Given these quite different aspects of empire, and the shifts in ideological beliefs over time, completely satisfactory generalizations are difficult, though, of course the factors of expansion, violence, and rise and fall, are characteristic of most. In a brief "Afterword," Maier traces out the sequence of empires in the Euro–Asian continent from 2472 BCE to date, although the nature of the transitions, whether military or political, is not detailed nor is it clear what differences could have due to whether there were none,one, two, or three, or more competing empires. Thus, although there are many fascinating insights into the history of empires, there are questions of interest that remain for other scholars, as we would expect from a self-described "extended essay" (p. 3).

The discussion of "America's Turn" focuses on the years of the twentieth century, particularly after 1945, with the American ascendancy, the successful conflict with the Soviet empire, and the relative decline of the British. To Maier, a key set of forces are economic—particularly Fordism and international currency controls, while more recently "the advent of electronic informatics" (p. 278) seems to provide hope for a continued American economic edge. He divides the success of the post–WWII American empire into two periods, with different internal and external features. The "Empire of Production" lasted from the 1940s to 1970s, influenced by standardized mass production and the low price of energy. Then, after a period of disarray in the 1970s the "Empire of Consumption" emerged, with its low savings rate, federal budget deficits, and deficit in the current account. In regard to the ideological aims, such as spreading democracy, Maier sees some pressure toward an American desire for empire, as would a need to combat terrorism even if these "eroded individual liberties" (p. 294). While a possible empire via the "Bush dynasty" (p. 291), is noted, one strand of the desire for empire is not discussed, however. This is the current Democrat–Liberal desire to provide benefits to less-developed nations via labor standards, social concerns such as ending female genital circumcision, and contemporary antislavery measures. Thus the current debate seems less about the desirability of interventions elsewhere than the specifics of what is to be intervened against. Clearly, however, these concerns would differ from these earlier empires, typified most extremely by the Mongols under Chinggis Khan, with their extensive destruction of people and other resources.

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O Economic Development, Technological Change, and Growth

Institutional Reforms: The Case of Colombia. Edited by Alberto Alesina. Cambridge and London: MIT Press, 2005. Pp. x, 373. $35.00. ISBN 0–262–51182–7. *JEL 2005–1381*

The Political Economy of Protection: Theory and the Chilean Experience. By Daniel Lederman. Social Science History series. Stanford: Stanford University Press, 2005. Pp. ix, 191. $55.00. ISBN 0–8047–4917–5. *JEL 2005–0952*

Daniel Lederman's *The Political Economy of Protection: Theory and the Chilean Experience* is a good source of historical information for anybody wanting to learn about Chilean trade policy. It provides an overview of two hundred years of trade policy, openness, and some of the characters involved in policy making from Chile's independence in the early nineteenth century until the present, with a detailed focus on the liberalization process started in 1974 by the Pinochet administration and carried on by the succeeding democratic governments.

The historical account is launched in the context of a "search for policy cycles" of protection and liberalization that uses recently compiled data on imports, exports, GDP, exchange rates, and price indices ranging from 1810 to 1994. We are presented with graphical overviews of the evolution of imports and exports over GDP, their Hodrick–Prescotted trend components, variation in the terms of trade, and import and export exchange rates.

We learn that openness (imports + exports / GDP) evolves more or less in the same fashion as in so many Western economies: increasing, big drop around World War I and the Great Depression, increasing again but as of 1994 not reaching yet its pre–Great Depression level. The worldwide increase in consumption of nontradables tampers with intertemporal comparisons of openness and it would be more informative to use the tradable components of GDP. But there is of course a good chance that total GDP is as good as it gets when it comes to data from 1810.

Remarkably, the all time high of exports/GDP occurs during 1918 with a catastrophic fall in the following year, accompanied by the largest ever fall in terms of trade. As it turns out, world peace was the culprit: the Chilean nitrates industry flourished while demand for gunpowder was high. More details like this follow as the account goes on.

What is the motivation so far? Well, there is a search for policy cycles going on and that means identifying periods during which policy was protectionist and periods during which policy was liberal (in the economic British sense). As argued by Lederman, different authors have not been able to agree, for example, on which the period of import substitution was. He seeks to give his own classification of trade policy periods.

After looking at openness and terms of trade, the focus turns to trade policy. The appendix includes a detailed four-page table that lists trade policy related events starting at colonial times, when Spanish colonies were only allowed to trade with Spain, and ending in 1973 (the period 1973–99, which starts with the Pinochet administration, is discussed in more detail later on). Here the reader can find information such as tariff increases and cuts, production subsidies, and exchange rate controls. Each policy measure is classified into protectionist or liberal, and aggregated into graphs that plot the frequency of each two types of policy over time. The graphs present relatively clear patterns of alternating dominance of protectionism and liberalism.

Using these tools together with historical narrative about predominant ideas, presidential elections, interest groups, and overall trade and industrial policy, Lederman concludes that Chile went through a period of *open economy* (until the early twentieth century), *delegitimization of liberalism* (1911–27), *institutionalization of protectionism* (1927–56), *delegitimization of protectionism* (1956–73), and *liberalization* (from 1974 on). This nomenclature follows the work of political scientist Judy Goldstein. In Goldstein's view, policy decisions are made in response to dominant economic policy "ideas." Ideas follow cycles of "legitimization" and "delegitimization" according to their success and failure. Ideas get delegitimized when they are associated with economic crises; this opens a window of opportunity for new ideas; if new ideas are associated with success, they become legitimized and are supported by different parties—until a crisis strikes again.

The categorization of Chilean trade policy history into periods of protection and liberalism is followed by two empirical exercises. The first exercise are several structural break tests that seek to identify abrupt changes in the indicators of openness (exports, imports, and total trade flows) and their rates of growth during the period 1810–1995. Results indicate that there was a structural break during the Great Depression but nothing during Chile's 1970s liberalization.

These results are not surprising. Structural break tests are not flexible enough to identify uneven policy cycles of protectionism and liberalism. The Great Depression was indeed abrupt and had immediate effects. The 1970s liberalization, on the other hand, was gradual and the large increase in exports and imports stretched out over several years. Thus, the tests pick up the Great Depression but not the 1970s liberalization.

The second exercise looks into identifying the time-series determinants of Chilean trade policy through two different Probit models where each year is one observation and liberalization (yes or no) and protection (yes or no) are the dichotomous dependent variables. The classification of each year into liberalization, protection, and no-change comes from the trade policy events table in the appendix and includes, among others, changes in tariffs, nontariff barriers, export promotions and capital controls. Some of the explanatory variables are lagged fiscal balance, trade balance, GDP growth, employment, inflation, and import penetration.

An alternative specification for this exercise could have been one multinomial model with three outcomes—liberalization, protection and no-change—instead of two separate binary models. A multinomial specification would have taken the correlation between liberalization and protection into consideration. Another possible addition is the time dependence of liberalization and protection. As it is, the exercise looks at one-year-lagged explanatory variables and does not allow for a "liberalization (or protection) spree."

The last main chapter describes the past thirty years of Chilean trade policy and its politics in great detail. It starts with the military coup of 1974. The Pinochet administration pushed forward an aggressive liberalization process. In only five years, the average tariff went down from 90 percent in 1974 to a uniform 10 percent in 1979. Quantitative import restrictions were eliminated. There was a step back to protectionism in the midst of an economic crisis during 1983–85 but the liberalization efforts rapidly went back into place and were not interrupted by the return to democracy in 1990. The uniform tariff level continued its declining trend and reached 6 percent in 2003. The account of the period is extensive and informative in aspects other than merely trade policy. It includes details on stabilization efforts, development programs, exchange rate regimes, key players within the government, and the ideologies of advisors.

Lederman's view of Chile's political economy of trade policy during the past thirty years is virtually the same as that of the historical analysis in the previous chapters: much can be explained by dominant economic ideas. This leaves out an analysis focused on interest groups—a prolific area of research in the political economy of trade policy literature. Lederman argues that the pressures of interest groups were minimized by the shock therapy characteristics of the reforms together with the fact that a uniform tariff was applied.

Throughout the whole book, the approach is more focused on the time series aspect of trade policy (periods of liberalization versus protection) rather than on the across-industry details. In light of this, this book will be an ideal source of information for the reader seeking to learn about the long term evolution of protectionism in Chile.

The volume edited by Alberto Alesina looks at another Latin American country: Colombia. Like many other countries in the region, Colombia underwent big market-oriented reforms during the 1990s. So, upon reading the title of the book, *Institutional Reforms: The Case of Colombia,* it may appear obvious what the book is going to be about. Yet it turns out that the title refers not so much to past reforms but rather to the, in Alesina's words, self-evident *need* for more institutional reforms.

After an introduction on the economic history of Colombia during the past thirty years, the book is organized in chapters each covering a different topic: separation of powers, the electoral and party system, crime, decentralization, the budgetary process, education, social programs, and the Central Bank. Most chapters are authored by a team including at least one expert from Colombia, most of them from Universidad de los Andes and from Fedesarrollo, a policy oriented research institution in Bogotá. Following a common structure, all chapters start with a description of a current situation and end with very concrete proposals for reforms. Also following a common view, most chapters are critical of the 1991 Constitution and find it guilty of being too rigid, interventionist, and inconsistent with contemporaneous policy measures and socioeconomic reality.

The chapter on separation of powers, by Maurice Kugler and Howard Rosenthal, deals with the political system, which in practice means that they deal with a whole lot of things. Perhaps the most notorious issue is the existence of Constitutional Courts that often act "in an activist manner" to overturn legislation and have a strong influence on economic policy. This activism, that according to the authors is often detrimental, arises in order to guarantee very literal and detailed constitutional principles (such as indexation!) and also as a result of the individual right to contend legislation with the highest courts. The authors propose the requirement of a supermajority of seven out of nine members for the Constitutional Court to overturn legislation passed by the executive and Congress. Other proposals are related to establishing court hierarchies, to the elimination of secret voting by judges and congressmen, to modifications of the appointment and tenure system for higher court magistrates, to giving fast-track powers to the executive, and to the size of the chambers. It should be noted that due to their constitutional nature most proposals require a reform of the Constitution. There is no discussion about the viability of a new constitutional reform (the last one was in 2001).

The natural sequel is a description of the electoral and party system. This is done by Gérard Roland and Juan Gonzalo Zapata. The situation is basically that excessive fragmentation of the Senate and House of Representatives leads to clientelistic behavior whereby representatives seek to satisfy the objectives of small electorates and compromise the action of the Executive—the chapter mentions several historical examples of presidents who have tried to implement (presumably beneficial) nationwide reforms that were rejected by a pro-status quo Congress. Roland and Zapata delve into the intricacies of seat quota allocation formulas of congressional elections in Colombia and other countries and come up with a suggestion to replace the current LR-Hare (largest remainders) system by a two-tier system, similar to systems currently used in several European countries, where reminders are aggregated over jurisdictions. The aggregation of remainders at the national level would presumably yield a less fragmented Congress by forcing parties to present a sole national district list of candidates.

Alesina and several contributors agree that crime is probably the "major cause of all problems in Colombia." As such, crime is the focus of the following chapter, authored by Steven Levitt and Mauricio Rubio. After playing with interesting statistics and regressions on the evolution of different crime rates over time and across different countries, on the rates of investigation, trial, sentences, and unsolved cases of different types of crime, and on income distribution, the authors conclude that drug dealing is the cause of the high crime rates and that there should be a shift of crime-fighting resources from less violent crimes to more violent crimes (Interestingly, Colombia is one of the countries with highest homicides rates in the world yet property and domestic crime rates are similar to other Latin American countries).

The remaining chapters deal with issues related to economic policy. Alesina, Alberto Carrasquilla, and Juan José Echavarría write about fiscal decentralization—a choice of topic motivated by an increasing fiscal imbalance. The majority of tax revenue is collected by the central governments. Funds are allocated to regional and local governments for expenditure subject to very tight spending rules that leave them unable to tailor to specific local needs. On the other hand, the rules dealing with regional debt are relatively lenient and have led to bailouts by the central government. All these issues are discussed in detail, together with the general theory of fiscal decentralization and proposals for reform related to limiting local borrowing and allowing for more flexibility in the allocation of expenditure. The authors claim that some of these proposals were incorporated in the Constitutional reform of 2001. On a closely related matter, Ulpiano Ayala and Roberto Perotti describe in the following chapter the multiplicity of formal documents, definitional liberties, and accounting tricks that contribute to the nontransparency of the budgetary process. They make concrete suggestions—among the most concrete throughout the book—that could conduce to a healthier fiscal policy.

The chapter on public education is written by George Borjas and Olga Acosta. Education is a huge component of public employment and expenditure and the sector was recently reformed with the background target of offering nine grades of education to the whole population. The authors present statistics and regressions on employment, teacher-student ratios, expenditure, salaries and pensions, and even teacher migrations, and discuss the sector in fairly good detail including the hiring process, pension system, and labor union. One key problem seems to be that teacher wages are set nationwide, ignoring differences in purchasing power across regions and leaving the local governments with no saying in the matter (while local governments are in charge of hiring the majority of teachers). Overall the picture is that incentive schemes could be improved and that things could be done more efficiently. Given the size and importance of the sector and apparent past conflicts, it is not clear, though, and is not discussed much in the chapter, whether it would be politically viable to introduce further reforms on how wages are set and on the pension system.

Perotti focuses on social spending. He gives a detailed account of pension plans, programs toward family and children (mostly child care programs) and employment programs. Overall he addresses a huge number of issues with evident familiarity with Colombia's social programs and institutions. Beyond commenting on the shortcomings of the implementations of these programs and making suggestions for improvement, Perotti's main concern is that public expenditure (including education, health, and the programs mentioned above) generally fails to reach the very poor. Individuals in rural areas have hard time accessing education and health and are outside of the formal pension system. Public assistance safety nets are often implemented through community involvement, which also excludes the rural poor and urban indigents.

In the final chapter, Alesina, Carrasquilla, and Roberto Steiner write about the Central Bank, its improvement in terms of independence after the Constitutional reform of 1991, and the desirability for yet more independence. The authors find that the main points of conflict are that the treasury minister is the president and a voting member of the Central Bank board, and that the government is involved in exchange rate policies (and thus monetary policy) and the management of the financial system. Criticisms and proposals are in line with current views and trends on central bank independence.

Summing up, the book covers a great deal of topics from a perspective of diagnosing and prescribing. It is a study of those aspects of Colombian political and economic institutions that the editor and authors judge to be *in need of reforms.* The nature of the recommendations naturally varies with the nature of the problems. Most proposals related to political institutions involve a constitutional reform. Proposals affecting large influential groups, be their persuasion power legal—such as the teachers' union—or illegal—such as drug dealers, could be harder to implement. The original drafts of the chapters have been circulating for several years and Alesina argues that several proposals have been implemented.

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Software Patents: Economic Impacts and Policy Implications. By Knut Blind, Jakob Edler, and Michael Friedewald. New Horizons in Intellectual Property. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2005. Pp. xi, 204. $95.00. ISBN 1–84542–488–3.
JEL 2006–1049

The Democratization of Invention: Patents and Copyrights in American Economic Development, 1790–1920. By B. Zorina Khan. NBER Series on Long-term Factors in Economic Development. Cambridge and New York: Cambridge University Press, 2005. Pp. xvi, 322. $60.00. ISBN 0–521–81135–X. *JEL 2006–1098*

I am not sure there is any sort of "iron law," but there does seem to be a correlation of some sort: the literature on the economics of patents grows in rough proportion to the number of patents actually issued around the world. (The economic literature on copyrights is growing too, though it is too young at this point to say whether the iron law applies.) So the two books under review here, for all their differences, represent two data points on a much larger trend line. But they also stand for something more: they speak to some essential themes in this larger literature, in particular themes concerning history and comparative policy that are often mentioned in economic treatments of the patent system. Because these themes are so important, and because these two books sound them out so well, I will keep them front and center in this review.

Many of the differences in perspectives offered by these two books stem from the fact that Professor B. Zorina Khan is American, and the volume by Knut Blind, Jakob Edler, and Michael Friedewald hails from Germany. Khan is far more optimistic about the overall effect of intellectual property protection on economic development; Blind, Edler, and Friedewald are decidedly cooler. While part of the disagreement is undoubtedly due to the fact that Blind, Edler, and Friedewald write only about the software industry—an industry in which patents have been controversial for decades[1]—while Khan considers the entire American economy in the nineteenth century, I would argue that their differences run deeper. They are partly the product of history, in my view. Khan's book retells the canonical American success story of the nineteenth century,[2] this time through the lens of one

[1] See, e.g., *Aerotel v. Telco, Ltd.*, EWCA Civ. 1371 (Court of Appeal 2006) (Jacob, L. J.) (providing an excellent summary of legal cases in Europe since the 1970s dealing, mostly skeptically, with software patents).

[2] Of course, the characteristic American belief in the power and social utility if technology begins in the eighteenth century, as described so well by Lawrence A. Peskin (2003). Peskin emphasizes the rhetoric of American self-sufficiency, and the association of industry with rural virtue and energy (Peskin 2003, 136–37).

legal field, intellectual property. Blind, Edler, and Friedewald tell a much more cautious story about the potentially harmful effects of patents on the software industry. In this the German authors echo a long continental tradition of more cautious optimism about the economic effects of patents. Thus, for this reviewer at least, the comparative policy aspects of the two books flow in part from the different historical experiences with intellectual property protection in the United States and continental Europe.

Khan: Democracy and Creativity during the "Long Nineteenth Century"

Before Professor Kahn's book, economic historians had mostly limited their discussions of intellectual property policy to specific incidents and sectors. The famous article by Fritz Machlup and Edith Penrose (1950) on the "patent controversy" in Europe in the nineteenth century is a good example; it summarized the rising tide of (mostly anti-patent) opinion in the newly professionalizing ranks of economists in nineteenth century Europe. Recent work by Johann Peter Murmann (2003) on the history of the European chemical industry is another good example. But, with the exception of several fine studies of British patent policy and the "first" industrial revolution,[3] occasional references in the work of the new institutional economists, and various legal-centric histories, little systematic work had been done analyzing the economic effects of intellectual property protection in any kind of long historical perspective. Professor Khan's considerable achievement has now changed all that.

Khan writes from a very distinct tradition, and it is important to keep this in mind in reading and evaluating her book. She is an American economic historian trained at UCLA, and her background shows through on virtually every page of the book. Her mentors and heroes include Naomi Lamoreaux, Ken Sokoloff, and Harold Demsetz of UCLA and Joel Mokyr of Northwestern (p. xv). These are hardheaded economists who apply their skills to history in various ways: scholars whose work is permeated with quantitative data and arguments drawn straight from the logic of micro- and institutional economics. Khan draws from this tradition in evaluating the effectiveness of the American intellectual property system during the "long nineteenth century" (1790–1920). She moves easily from discussion of the incentive effects of legal rules to macro-level assessments of the system as a whole. In service of the latter, she marshals a wealth of data, much of it new, and painstakingly gathered, in support of her overall conclusions. The tight logic of the argument and the force of the quantitative backing they receive add up to a very convincing set of conclusions. Both in the methodology employed and in the clean, graceful writing style, she has set the bar very high indeed for those who would follow in the economic history of intellectual property.

So what are her basic conclusions? Three stand out. First, intellectual property law in the United States had a definite and mostly positive impact on economic growth during the extended nineteenth century. Second, this contrasts with the situation in Europe, where intellectual property protection was less effective and thus contributed to somewhat less robust growth there. And third, the greatest divergence in national policies centered on the class of people who benefited from intellectual property protection. This, her most original contribution, is captured in the book title: her thesis is that the more accessible, democratic character of the U.S. intellectual property system is what set it apart from its European counterparts, both in style and effectiveness. For Khan, the U.S. system did a better job of releasing the inventive and creative energies of its citizens. And in her view, this provided a notable boost to the overall economic "take-off" process at work in nineteenth century America.[4]

A word of caution is in order regarding Khan's approach. She believes that the legal system during the formative years of the American Republic was well-nigh perfect in its balancing of various social and economic interests. It is not too much to say that Khan is a rousing cheerleader for the major figures in the drama she describes. Two examples: the founding generation (particularly

[3] Henry I. Dutton (1984) and Christine MacLeod (1988).

[4] It should be pointed out that while there are some indications that European economies struggled to adapt to industrialization, as described for example by Malcolm I. Thomis (1976), the same was true in the United States; and also, economic growth in Europe was quite healthy overall during the period Kahn is interested in, as described for example by David S. Landes (2003).

James Madison); and Justice Joseph Story of the U.S. Supreme Court (whose early opinions in patent cases set the tone for a positive, pro-innovation view of patents that remained a distinctively American voice until the late nineteenth century). Figures such as these inspire Khan's great admiration: "The economic history of intellectual property laws and their enforcement leads to the inevitable conclusion that the federal judiciary and the U.S. legal system played a central role in facilitating social and economic progress during the nineteenth century. . . . [T]he judiciary objectively weighed costs and benefits, and ultimately the decisions that prevailed promoted social welfare rather than the interests of any single group" (p. 11). Now I am a big fan of the founders, and of Justice Story in particular. And I believe that much of the writing in legal academia is far too skeptical and critical, inclined still to the view that law often provides a convenient cover for the exercise of raw political power by elites. Even so, Khan may go a bit too far in her rosy assessment of the early days of U.S. intellectual property policy. Detailed histories of specific early inventions (the steamboat and the cotton gin, to name two) are enough to call into question just how perfect the early system was. And even if the *aspirations* of the "founding giants" were sound and true, one might admit that the analytical apparatus they brought to bear on policy questions was quite rudimentary. Thus while I agree with the overall drift of Khan's argument—and while I see some utility in her rhetorical excess, as a needed antidote to the overly critical "conventional wisdom" among many legal academics—I cannot quite agree with her tone all the way down the line.

Although the book title includes both patents and copyrights, most of the substantive chapters (six out of eight, by my count) are concerned with patent law. Though relatively brief, the coverage of copyright issues is notable in two respects. The first is this: it represents some of the most in-depth coverage of the economic history of copyright yet attempted (this field long having been a poor cousin to the analysis of patent- and invention-related issues by economists and economic historians). Second, it is of a piece with the general tone of the patent chapters. The description of U.S. copyright law is so glowing it borders on the Pollyanish. Federal policy regarding copyright protection in the United States was notoriously miserly in the nineteenth century—so much so that foreign authors such as Charles Dickens complained bitterly about it. The primary objection was that the United States failed to respect foreign copyrights, and as a consequence U.S. authors received no foreign copyrights for their works. In Khan's telling, this was a rational (indeed, optimal) policy: "[D]uring the period when the U.S. was itself a developing country, it regarded widespread copyright 'piracy' of foreign materials as international fair use" (p. 225). Khan provides solid backing for the widespread anecdotal evidence that the U.S. publishing industry adapted to weak protection by specializing in "pirated" editions of foreign books. (This chapter will be especially useful for policy advocates who argue that U.S. trade negotiators are ignoring their own history when they berate developing countries for having weak intellectual property systems.) As a consequence, the descriptive aspects of these chapters are really quite a fine contribution.

But the normative conclusions are debatable on a number of grounds. Particularly questionable is Khan's analysis of data on the number of U.S. citizens who chose to make a living as an author (particularly of fiction, a type of writing with an inherently more international potential market, compared to nonfiction works, dominated by fields such as geography and law, with a highly local dimension). She uses her carefully constructed tables and regressions (again, a model of painstaking historical/empirical scholarship) to argue that the eventual accession of the United States to the international copyright regime in 1891 did not result in a large increase in the number of U.S. citizens identifying themselves as professional authors of fiction (p. 274). But a careful look at the evidence shows that the opposite inference is at least equally plausible. Khan argues that there was significant growth in the number of authors and "professional authors" in the 1840–60 birth cohort of writers of fiction books and that, because fiction authors begin their careers on average "in the[ir] early thirties" (p. 274), this demonstrates that the 1891 change in copyright law had little effect on this segment of the market. But three stark facts stand out. First, the median year in the birth cohort under discussion is 1850, and the average entry age for fiction authors is 34.8 years, call it 35. So the median author from this cohort entered the fiction field around 1885, during the period when publishers knew quite well that international copyright protection was on the horizon.[5] (Since the number of authors was growing throughout the period under study, the median year when weighted by number of entrants would occur even later.) Second, there was noticeable growth in the ratio of "professional authors" to all authors (which includes part-timers and professionals) in the first birth cohort to enter the field after the 1891 reform (those born in 1870–89): professionals grew from 17.6 to 18.2 percent during this period. And third, in the midst of this mixed empirical story, there are the voices of numerous actual authors and publishers based in the United States—including the aforementioned Putnam, and Edgar Allen Poe (p. 272)—complaining bitterly about the lack of international copyright protection and its effect on their careers and work.[6] Khan ignores this evidence, citing instead modern theories about how an increase in piracy can indirectly stimulate the market for "complementary works" such as lecture tours—shades of the argument popular today that online music filesharing is actually good for the music industry. Many actual musicians, echoing the complaints of Dickens and others from long ago, might well beg to differ. At any rate, if her observations were correct, we would expect (in a world of rational profit maximizers) that some subsequent authors would have experimented with the "Dickens model," by giving away their books to stimulate the market for lectures and the like. If anyone did, I have never heard of it. Maybe authors in this period were just missing out on a good thing. On the other hand, maybe not. Maybe James Joyce, T. S. Eliot, and all the other post-1891 authors who assiduously sought and protected their U.S. copyrights knew something about the market for international copyrighted works that modern scholars—for all their knowledge of network externalities and bandwagon effects (Khan, p. 274)—have overlooked. It's a thought.

[5] See the statement by U.S. publisher G. H. Putnam in a trade press article from 1879: "An international copyright is the first step toward that long-awaited-for Great American Novel" (Quoted by Khan, p. 265).

[6] For another recent book arguing persuasively that music composers responded favorably to enhanced copyright protection, see Frederic M. Scherer (2004).

American versus European Views on Intellectual Property

This talk of international copyright matters leads to the volume by Blind, Edler, and Friedewald on software patents. Here, just as with nineteenth century copyrights, Europe and the United States are quite at odds over an important question of intellectual property policy. Only this time, the roles are reversed: the United States aggressively permits software patents, while in Europe they are harder to obtain, issued on a narrower class of inventions, and generally subject to more scrutiny and criticism. The reasons for this disparate treatment are quite interesting in their own right, although the Blind, Edler, and Friedewald volume does not really touch on them. These authors instead take this difference for granted. Their interest is strictly with European software companies. So I will focus on that, only returning to comparative issues in the conclusion.

The main points of the Blind, Edler, and Friedewald volume can be stated succinctly: companies that produce software in Europe "want neither an extension of patenting [from the level extant in Europe in 2001] nor an exclusion of software-based inventions from patenting" (p. 3). The general consensus among the European firms surveyed was general skepticism about the extension of patent protection to "software per se" in the style of the United States. But within the broad consensus view, there are some interesting contrasts and countercurrents. I will describe them in the following paragraphs. But first, I need to explore the makeup and timing of the Blind, Edler, and Friedewald data sample, because this has an important bearing on some of the divergent trends that emerge out of this most useful study.

Blind and coauthors were evidently impressed by the strength of anecdotal evidence about the resistance of European software companies to the strengthening of software patents. Impressed, but frustrated, I should say, for right at the outset they mention that their book was motivated by a desire to get some firm empirical backing for all the anecdotal information swirling around (p. 3). This impelled them into a substantial empirical research project: with the backing of a German government agency (the Federal Ministry for Economics and Labor) they solicited internet questionnaire responses from 1,200 German companies, eventually receiving 286 usable responses (table, p. 40). Of these, 196 were from software companies proper, 67 were from manufacturing companies that incorporate a substantial software component into their products (the so-called "secondary sector"), and 23 were companies located abroad. The authors supplemented this broad-based empirical survey with 22 detailed case studies, involving extensive interviews and follow-up questioning (p. 3). This last feature is a most useful aspect of the study, as it allowed more in-depth exploration of the thinking and strategy of the respondent firms.

Overall, as I said earlier, German companies involved in the software industry seem quite content with the "European equilibrium" regarding patentability as it existed in 2001 (and still largely exists today). In the words of the authors, they "speak out most strongly for retaining the status quo" (p. 88). This is not really surprising. And although providing ample empirical backing for a widely shared, anecdotally based belief is an important contribution, it would not in itself make the Blind, Edler, and Friedewald volume really *interesting*. But underneath this consensus there are some fascinating contrasts and countercurrents. These features, which have to be teased out of the data and case studies, are what would lead me to recommend the volume to a friend or colleague interested in the European software patent controversy, or the economic aspects of patents in general. In particular, close attention to the survey responses and interviews reveals three very interesting themes: (1) more mature companies worry about patents less than smaller, younger ones; (2) companies vary significantly in their ability to capitalize on the novel business strategies made possible with the advent of patents; and (3) the specific concerns of software companies provide a very useful guide to policymakers called on to make micro-adjustments in the patent regime as it applies to software.

Taken as a whole, these themes, lurking beneath the surface of the Blind volume's major findings, begin to fill out a more shaded view of the economic effects of patents, and thus contrast nicely with the vision laid out by Professor Kahn in her book, which seems by comparison more satisfying theoretically but factually more monochromatic.

The first of the three countercurrents in the Blind, Edler, and Friedewald volume concerns company maturity. Put simply, software firms that have been around longer are more accustomed to the ways of patents, and consequently greet patents with less concern. This is captured in a table in Blind's book, which shows the age of the IP departments in the firms responding to the survey. The preponderance of pure software firms ("primary" software companies) have very young IP departments (1–5 years old in 2001), while fully half of the "secondary" software firms have IP departments older than 20 years (table, p. 68). This carries over when the data are looked at by company size: the secondary software firms (again, manufacturing companies that incorporate software into their products) are in general larger than the "primary" or dedicated software companies, and there are far more very small firms (1–19 employees) among the primary software companies (36 percent), as compared to secondary software firms (19 percent) (table 3.2, p. 74). Age and size have a good deal to do with firms' attitudes toward and deployment of patents. As the authors say, "[a] positive correlation [can] . . . be drawn between the age, the company size (according to the number of employees) and the export activities on the one hand, and the propensity to patent on the other hand" (p. 71); and "[t]he use of patents increases with increasing company size . . . " (p. 75). This corresponds with other facts presented in the book, and with other information about the software industry: Younger and smaller firms are traditionally more afraid of patents. Indeed, they employ "protective measures" of all kinds far less than larger firms (p. 76). The authors note too that, although some companies report a fear that patents will impede the innovation dynamics of the software industry, "[t]his fear is significantly weaker in the firms of the secondary sector, which have worked with patents for decades in their areas of major activity" (p. 79). This is a crucial fact to keep in mind, because the large empirical part of the Blind, Edler, and Friedewald study is a *survey*: it is based on questions asking about the experience and opinions (including predictions) of the firms that responded to the authors' questionnaire. The point is that cautious opinions regarding patents are to some extent a function of the age and composition of firms—of industry structure, in other words.

If there is anything to this point, it counsels against reading too much of a comparative angle into the findings of Blind, Edler, and Friedewald. It would be easy, for example, to relate the European caution regarding patents to some of the themes Kahn emphasizes in her book, including differing ideologies concerning patents and intellectual property in Europe versus the United States. Someone taking this tack might draw on Kahn's discussion of the more "democratic" features of the U.S. system to argue that an industry dominated by small companies might feel less welcome in the European patent system, given the orientation of that system to larger companies. I think in the end there is something to be said for this view. But one must be careful not to make too much of it. For even in the United States, many smaller software companies continue to be less enthusiastic about patents than the larger firms in the industry. And also, software is perhaps the quintessential global industry. Hence the views of software firms in Germany and the United States are probably far more affected by the business they are in than the country they are in while engaging in that business.

Nevertheless, differences in national attitudes can probably bear some of the weight of explanation for the divergence in acceptance of patents in the German and U.S. software industries. Assuming this to be true permits me to venture an assertion: the fears of the German industry may well be overblown. I have studied the course of software patents in the United States, where similar fears were quite common during the early days of software patenting. It is safe to say that the dire predictions about the demise of the industry at the hands of runaway patents have not come true, and that the U.S. software industry continues on its robust growth trajectory well into the "patent era" (Robert P. Merges 2006 and forthcoming). It is also safe to say that individual companies—large and small—have adapted reasonably well to the advent of patents, and indications are that patents are being incorporated effectively into firm-level operations and strategy. Put simply, while there is no real proof that they have been outright good for the industry, they have certainly not killed it, and many software firms have found some good uses for patents (Ronald J. Mann 2005).

Divergences in firm-level strategies represent the second countercurrent in the Blind book. Here we find a very interesting contrast with Kahn's book, which by its nature describes the effects of patents at a much more "macro" or aggregate level. When firms were asked what purposes they thought patents could serve, they responded with a number of interesting answers. Many said that patents were fairly effective at protecting the firm from imitation by competitors (3.5 effectiveness on average, out of 5 for primary software firms, higher for the secondary firms); that patents could to some degree help increase a company's value (over 3 effectiveness on average out of 5); and might improve access to capital (2.5 out of 5). But—and here is where the case studies really shine—the aggregate numbers mask some interesting differences in firm experience and strategies. Some of the interview reports create the impression of lightbulbs going on in the minds of software company executives. One of the twenty-two companies interviewed, for example, reports that although it did not in the past seek patents, "an important customer in the hardware field adopted the unprotected features of the [interviewed] firm in its own software program and integrated it into its whole system" (p. 123). My own research shows that this is not unusual: patents can help when an erstwhile partner attempts to carve a firm out of the "value chain" by copying its core technical assets (Merges 2005). And indeed, many (predominantly medium-sized) companies reported the hope that software patents will "positively influence the cooperation possibilities of their firm, in that the trade with more strongly coded property rights will reduce possible difficulties in collaborating with other firms" (p. 96). Another company, said to be a successful firm in the competitive field of "automation, measurement, and control engineering" (p. 122), deploys its patents both to protect "market share" against its much larger rivals and "as an effective instrument to underline clearly [its] technological leadership over its competitors" (p. 122). These interview fragments are of course not determinative. But they do suggest that some firms have figured out how to use patents to distinguish themselves from competitors. Perhaps the company interviewed after it was "burned" by its customer will become a leader in structuring technology transfer agreements around a core of patent assets. Perhaps the other firm will continue to rely on patents as a bulwark against larger competitors. And if not these particular firms, then perhaps others will deploy patents creatively. The point is this:

enough firms may adopt strategies of this sort that it becomes a moot point whether patents *in the abstract* will help or hurt the software industry. When patents appear on the scene, some firms will figure out how to use them effectively. While this would not of course prove that patents are good for the industry, it would help to show why it is risky to predict doom and gloom from the advent of patents, or from other regulatory and legal shocks as well. Indeed, this kind of adaptability makes it very difficult for social scientists to do their jobs at all. Entrepreneurs seem to adapt to their environment faster than we can study how the environment is changing and what those changes mean.

We have covered the first two countercurrents, national differences and firm-level strategy. All that remains is the third: suggestions about the policies that are appropriate in helping the software industry adapt to the advent of patents. To begin, it must be recalled that the firms in the Blind survey say they are content enough with limited, moderate patent protection for software-related inventions. They do not advocate abolition of patents on all aspects of software, nor do they support more liberal claiming of software as in the United States (p. 88). I mentioned these primary findings of the Blind book at the outset of this section. What interests me now is what lies behind this desire to maintain what I have called the "European equilibrium" regarding software patents. Or, to put it somewhat more provocatively, what is it that prevents European firms from wholeheartedly embracing patents for all aspects of software? The Blind, Edler, and Friedewald volume, though not specifically directed toward an answer to this question, provides some very helpful guidance on this issue. And it is potentially influential guidance. These industry-specific concerns could assist policymakers to implement a patent regime that is more responsive to the needs of the software industry. The reasons why greater patent protection is resisted, in other words, might be transmuted, even in a world where software patents become more common, into useful policies designed to soften any negative impact patents might have on the industry.

Blind, Edler, and Friedewald uncover two overriding problems with software patents that could be addressed through wise public policies. The first is low quality patents. The survey response which lists "dynamic of innovation activities" as one of the effects of patents comes up with a quite negative score in the empirical data (p. 98), meaning that many companies are quite worried that patents will negatively affect their research and development activities. Obviously, an emphasis on preventing weak patents from issuing will help to address this concern. What usually worries software firms is that too many patents will issue that cover too many discrete features of software products. Restricting patents to truly meritorious inventions can go a long way toward addressing this issue. Minor features, under a wise and effective patent regime, will seldom be patented.

The second problem is the feared effects of patents on interoperability. Many respondents worry that patents will interfere with the ability of different software components to interact and interface with each other (p. 99). These are valid concerns, with far-reaching consequences. Not only is it crucial for different software to interact at the functional level, but interoperability policy can exert crucial influence on software industry structure. This is the great lesson of the longstanding legal battles involving Microsoft. And of course, one motive that leads European regulators to scrutinize this area quite carefully is the belief that liberal interoperability policy is essential to the survival and health of the European software industry, given that large foreign (mostly U.S.) companies own and control several essential "backbone" technologies in the software industry (Windows and iPod/iTune, to name two prominent examples). What the Blind volume makes crystal clear is that these concerns must be addressed before European software firms will be comfortable with a robust regime of patent protection in their industry. Fortunately for them, many of the policies necessary to encourage effective interoperability are well understood. What is needed is a sensitivity to the importance of interoperability, which can be effectuated through a number of discrete legal doctrines and policies: rules relating to estoppel and implied licensing, injunctions, damages, and antitrust/misuse defenses. The important point is this: these policies can be implemented regardless of which aspects of software are patentable. They are ex post rules, which regulate not which patents issue but how those patents are deployed. They are designed to guard against the kinds of harm—such as the strategic blocking of interoperability—that industry members are afraid will be caused by the spread of software patents.

Conclusion

There is no doubt that some of the fears of the software companies are realistic. And it may be—though I myself have come to doubt it—that patents for software overall are a bad idea. But for a number of reasons, patents will in all likelihood continue to creep into the software industry, even in Europe where they are often despised. Blind, Edler, and Friedewald provide hints about why this is so in their interview data; one company whose primary asset is innovative software happens to operate in the automation engineering field. In the interviews, this company's patents "belong in the area of measurement and control engineering, although the patented processes are always realized in the form of software" (p. 122). This firm is not concerned about the European rules, because "it can always formulate all [patent] claims in the language of automation technology," even though for this firm "software of sufficient innovativeness and with technical content is regarded equally as an engineering feat" (p. 122). In other words, under current European rules, which resemble the regime in the United States in the 1980s and early 1990s, this software firm can obtain patents because all its software has a clear "hardware dimension." But another firm specializing in software that is in some sense further removed from computer hardware has to either "characterize" its technology so as to qualify for patents, or push ever outward the boundaries of software protection. In the United States, these "lines in the sand" have proven very hard to construct and defend, at least on a consistent and principled basis. Software is so evanescent, and can be coded, implemented, and described in so many diverse ways that it is difficult to impose defensible boundaries signifying that the software on "one side of the line" is patentable, while that on "the other side" is not.

So is there any hope that the fears expressed by respondents to the Blind, Edler, and Friedewald survey will be addressed? The answer is yes, and here is where a return to that pragmatic and balanced spirit so admired by Professor Kahn in her book could really save the day. The best we are likely to have is a series of policies that mitigate the deleterious effects of patents on the software industry. Such policies will include (1) careful policing of the quality of patents, with an eye toward minimizing the deleterious effects of too many patents of dubious merit; and (2) sensitivity to the interoperability issues that are of such importance to the software industry.

Of course, in the end getting these issues right is mostly an empirical question. But because of the difficulty of obtaining rock-solid empirical evidence on the "big questions," the best we can do is often a combination of tantalizing but not definitive empirical work, some case studies and historical/comparative research, and good old-fashioned theorizing from first principles. From this point of view, we have a long way to go. But the volume under review makes a solid contribution. It ought to encourage similar efforts, in the United States and elsewhere. Indeed, because of the relative lack of information in this area, we might well call it a case of Blind, Edler, and Friedewald leading the blind. Or something to that effect. . . .

While more data are being gathered, we would do well to remember the basic principles stressed by Kahn. Her faith in the abiding logic of property rights and markets, together with her interest in institutional detail, provides just the right perspective on the vexing problem of software patents examined by Blind, Edler, and Friedewald. A dose of that Kahnian optimism might free the Europeans of their deep-seated concerns about software patents. In any event, her recounting of the success story of the U.S. economy over the "long nineteenth century" ought to reinvigorate our faith in the dynamism of economic growth. With patents or without them, because of them or in spite of them, the software industry is likely to face a bright future. Intellectual property policy might help, as Kahn argues it did in the United States. But it is not likely to be decisive. For those of us interested in legal policy, that is not only humbling; it is also a huge relief.

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Biography of a Subject: An Evolution of Development Economics. By Gerald M. Meier. Oxford and New York: Oxford University Press, 2005. Pp. viii, 250. ISBN 0–19–517002–4, cloth; 0–19–517003–2, pbk. *JEL 2005–0747*

Gerald Meier is the James Boswell of "development economics." The last term is advisedly put in quotes, for as I argued over two decades ago (Deepak Lal 1983) "development economics" is different from the economics of developing countries. The former is the attempt to develop a "new" economics and denies the "mono-economics" claim of the latter that traditional economics is applicable to developing countries in the same way as it is to developed ones (Albert O. Hirschman 1982). As Meier notes "it was not until the 1950s that development economics emerged as a special subdiscipline of economics" (p. 12). However, I have argued that, by promoting the *Dirigiste Dogma,* it did great damage to the prospects of the world's poor. Meier's collection of snippets of the major writings on both "development economics" and the economics of developing countries in his various editions of *Leading Issues in Economic Development,* charted and provided a running commentary on these debates. They were of great value to both students and their instructors in the growing number of university courses on economic development. So one would have hoped for a more even handed approach if his new book were to be a rounded biography of writings on economic development. But this turns out not to be so.

Till the end of the 1980s, Meier rightly notes, the "orthodox reaction" of mainstream economists had won this battle with "development economics." The first half of Meier's book charts this familiar ground, which has been covered by many others and it is by and large uncontroversial, though I. M. D. Little (1982)—who he cites—and the present reviewer, who he does not—arguably did so more succinctly and analytically. This part of Meier's book reminded me of the characterization of the published record of two millennia of Chinese history by William J. F. Jenner (1992): "that [it] rarely tells an outright lie but passes on the views of earlier bureaucrats as modified by later bureaucrats, and deals mainly with matters of concern to the monarchy and to officialdom" (p. 5). Substitute "development economist" for monarch and bureaucrat, and you have a fair description of this part of the book. Meier's self- appointed task of showing that his subject—development economics—is alive, necessarily involves air brushing much of the critique and contributions of the mainstream economics of developing countries from his biography. Perhaps that explains the strange subtitle of the book "An Evolution of Development Economics." Why "an" and not "the"? What are the other evolutions of the subject?

It is in the second half of the book about what Meier claims is "The New Development Economics" that his judgment really goes awry and the purpose of this book becomes clear. The heroes of this part are Joseph E. Stiglitz and Dani Rodrik. Meier claims that the new development economics is based on Stiglitz's imperfect information paradigm, which is illustrated by analyses of share cropping and inter related factor markets. I am surprised to learn that this is "new" and departs from mainstream neo-classical economics. The earliest paper I know on these themes (and which also had empirical content) was by Pranab K. Bardhan and Ashok Rudra (1978), whilst Bardhan (1980) and Kaushik Basu (1983) provide surveys of this literature on rural organization. They show how many otherwise puzzling and seemingly inefficient institutions can be explained as second best adaptations to an uncertain and imperfect environment. This was well before the so called "new development economics" emerged, according to Meier in the 1990's, as a reaction against the orthodox neoclassical resurgence of the 1980s.

It is not any great new theoretical insights, but the policy conclusions that Meier claims the "new development economics" draws from models of imperfect information, "co-ordination failures, multiple equilibria, and poverty traps" (p. 119) which really excites him. He writes: "With imperfect information and incomplete markets, the economy is constrained Pareto inefficient—that is, a set of taxes and subsidies exists that can make everyone better off" (p. 120). This echoes a similar claim made by Bruce C. Greenwald and Stiglitz (1986). But they concede: "It might be noted that we ignore any discussion of the political processes by which the tax-subsidy schemes described below might be effected. Critics may claim that as a result we have not really shown that a Pareto improvement is actually possible" (note 7, p. 234). Whilst on their claim of the existence of Pareto- improving government interventions, they conclude that: "we have considered relatively simple models, in which there is usually a single distortion . . . though the basic qualitative proposition, that markets are constrained Pareto efficient, would obviously remain in a more general formulation, the simplicity of the policy prescriptions would disappear. Does this make our analysis of little policy relevance? The same objection can, of course, be raised against standard optimal tax theory. (Some critics might say, so much the worse for both)" (p. 258). Quite!

Meier commends Rodrik (1995) for emphasizing "co-ordination failures" and in demonstrating "how the South Korean and Taiwanese governments got interventions right" (p. 124). He claims that this view and the Kevin M. Murphy, Andrei Shleifer, and Robert W. Vishny (1989) modeling of the Big Push vindicates the old "development economics" of Nurkse and Rosenstein-Rodan. He supports Paul R. Krugman's (1992) belief that in its earlier incarnation it was not persuasive because its ideas were not formalized in mathematics. But as Stiglitz, (Krugman's discussant), rightly noted: "That we can write down a model of a phenomenon proves almost nothing. It does not make the idea right or wrong, important or unimportant." As regards Rodrik's views about smart dirigisme in Korea and Taiwan, Little (1994) convincingly showed that social rates of return to investment were inversely correlated with the degree of dirigisme.

This raises the question of one of the strangest omissions in Meier's biography: a book which explicitly dealt with the question of whether the economic success of Korea and Taiwan was because of, or despite, their *dirigisme.* Meier rightly notes the contributions of Hla Myint, particularly on economic organization in the course of development, but falls silent on the last book Myint coauthored with the present reviewer (Lal and Myint 1996). Its subject matter is relevant to Meier's concerns. It dealt with economic history and political economy (which Meier rightly emphasizes have been neglected in "development economics"), provided explanations for the adoption of a Big Push by many countries and why it inevitably led to development disasters, as well as for the indubitable dirigisme in Korea and Taiwan. Could the reason be that its policy conclusions did not support the *Dirigiste Dogma* which both the old and new "development economics" seek to promote?

One of the abiding deficiencies of "development economics" has been its fascination with theoretical curiosa, and the *dirigiste* policy conclusions that can be drawn from them. A second failing is to ignore the heterogeneity of developing countries. But, as the pioneering study by Hollis Chenery and Moises Syrquin (1975) showed, one needs to differentiate between countries in various dimensions. Thus, in the Lal–Myint (1996) study a classification based on relative factor endowments proved particularly fruitful in explaining the different policies and outcomes in the twenty-five developing countries studied.

Meier is right in stressing the growing recognition of the importance of culture and institutions in economic development. But these are very vague concepts which have become black boxes as explanations for differences in economic outcomes. The work of David Landes and Vernon Ruttan (cited by Meier) does not take us very far in understanding how culture effects economic performance. Nor do the cross country regressions which have become so popular with the young. Meier rightly cites the critique made by T. N. Srinivasan and Jagdish Bhagwati (2001) of this enterprise, which Robert M. Solow (1994) has also noted is not "a confidence inspiring project. It seems altogether too vulnerable to bias from omitted variables, to reverse causation, and above all to the recurrent suspicion that the experiences of very different national economies are not to be explained as if they represented 'points' on some well-defined surface" (p. 51).

I did make an attempt to sort out the role of culture on economic performance in my Ohlin lectures (Lal 1998) by distinguishing between two different types of beliefs constituting "culture": material, concerning ways of making a living, and cosmological concerning "how one should live." I argued that, the former, which are the main determinants of economic performance, are malleable, whereas there is greater hysterisis in the latter. This means that, even with persisting differences in cosmological beliefs, different "cultures" can adopt the material beliefs which aid economic development, as Japan, and now India and China have shown. This distinction between cosmological and material beliefs also translates into two types of transactions costs which lie at the heart of institutions: one associated with the efficiency of exchange in finding potential trading partners and determining their supply–demand offers, the other with policing opportunistic behavior by economic agents. A cross civilizational historical story can then be told of these changing material and cosmological beliefs.

In fact the central question about the relative wealth of nations (rightly traced by Meier to the classics) is still the grand theme of the study of development. It centers around the Great Divergence between the West and the Rest. This is where the work of economic historians (which Meier rightly condemns development economists for neglecting) becomes crucial in understanding how capitalism—the distinctive institution initiating the Great Divergence—arose, and how it is spreading or being thwarted in different countries and regions of the world.

The mechanics of development are now well known—and Meier provides a fair summary in his chapter on growth theory. There also seems to be a fair degree of agreement amongst policy makers on the policy package known as the "Washington Consensus" (despite Meier's cavils echoing Stiglitz and Rodrik on this score) to promote development. It is the imperfect spread of capitalism around the world which needs to be explained. No coherent picture emerges of this "big story" from the work Meier cites in his last chapters. The story of the globalization of capitalism, and the impediments to its sway, becomes the central unifying theme of the study of economic development. It is then a part of a study of global economic history. (My own attempt is in Lal 2006). This is very much what the classics, rightly regarded by Meier as the fathers of this broader and grander subject, would have done today, using some of the new theoretical tools which have been forged since they wrote, and the quantitative historical data that has now become available as a result of Angus Maddison's (2001) Herculean labors.

But Meier omits many theories and constructs which have been inspired by and are important in understanding the development process. To mention just a few: the John R. Harris–Michael P. Todaro (1970) model of migration; the concept of effective protection (W. Max Corden 1966); Ester Boserup's (1965) theory of agricultural evolution; Anne O. Krueger's (1977) three factor open economy growth model and its fomalization by Edward E. Leamer (1987); the analytics of the Dutch Disease (Corden and J. Peter Neary 1982); Evsey D. Domar's (1976) model of a land abundant–labour scarce economy; Krueger's (1974) model of rent-seeking (which Meier does mention in passing); the political economy of tariffs (Ronald Findlay and Stanislaw Wellisz 1982; Wolfgang Mayer 1984; Lal 1989); the political economy of the predatory state (Findlay and John Douglas Wilson 1987; Lal 1984, 2005); and Timur Kuran's 1995 model of preference falsification.

The study of the economics of developing countries has been a highly contested field. Gerald Meier has manfully tried to resurrect what many of us on the other side believe is the dead corpse of a special "development economics." I have briefly set out my reasons for doubting that he has succeeded. But in any contested field it is useful to have the positions set out clearly. Gerald Meier does this for his first love—"development economics." But readers of his book should remember that there is another side to this story.

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Patents: Economics, Policy and Measurement. By Frederic M. Scherer. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2005. Pp. xvii, 310. $120.00. ISBN 1–84542–481–6.

JEL 2006–0726

Professor F. M. Scherer has long been a prominent figure in Industrial Organization, especially in the areas of patents and innovation generally. This book is a collection of eighteen of his papers, reprinted from various journals, books, and reports. The selected papers cover several areas: the role of patents in spurring innovation, sources of innovation, public policy regarding patents, and calculating the private and social returns to innovative activity. Taken together, these papers make accessible a wide range of patent-related economic and policy issues, going from ones of broad interest to any economist to very detailed questions of interest mainly to specialists. To give a flavor of what the book contains, I will discuss the main themes that appear in them, rather than cover each paper separately.

The Sources and Effects of Innovation

Since his student days, Scherer has worked to isolate the importance of the patent system and government policy relating to patents in spurring innovation. While a student at Harvard Business School, Scherer and fellow MBA students worked on a class project to examine the effects on innovation of several recent antitrust settlements that had required the forced cross licensing of large numbers of patents on terms that involved modest or zero royalty payments. In particular, the settlements made by IBM and AT&T with the Justice Department involved thousands of patents. Scherer's initial presumption was that forced cross licensing would reduce the incentive to innovate for the companies involved. Based on a number of company interviews undertaken by Scherer and his fellow MBA students, the surprising result was the opposite: the settlements were projected to have little effect on innovation. The main reason turned out to be that the companies interviewed regarded innovation as a necessary function required by competitive forces, and not something done for streams of royalty payments. The main effect of the forced cross licensing appeared to be that firms would protect their competitive advantages from innovation by patenting a little less and relying more on secrecy.

This sort of result has been extended and strengthened by a number of researchers in the last twenty years or so. For example, Levin et al. (1987) elicited the views of corporate R&D managers concerning the relative effectiveness of several means of protecting the competitive advantages of their processes and products. Patenting was viewed as less effective that gaining a first-mover advantage, moving quickly down a learning curve or having a superior sales and service force. This body of the literature has pretty consistently found that patents play an important role in maintaining competitive advantage, but that this role is not nearly as important as other factors.

Scherer notes that this result does not seem to be true for the pharmaceutical industry. He cites three reasons for this. First, for many pharmaceutical products, the patent describes a molecule, which is the exact product resulting from the patent. Second, FDA requirements for getting approval involve long and costly clinical trial and analyses. Thirdly, the imitator need only demonstrate bio-equivalency to get FDA approval, a much less demanding requirement. Given this fact, Scherer is concerned about patent policy for gene sequences. Gene sequences can make up the proteins or treatments that a patent may be used to produce; in that sense, they are upstream from the pharmaceutical product. Because they are upstream in this particular sense, the possibility exists that an innovation involving a gene sequence might have little value, but could make possible downstream innovations that have great commercial value. This gives rise to three issues. First, how do different patent protection policies affect innovation at different stages of this sequential process? Second, how should rents from innovation be allocated between upstream and downstream patent holders? Third, how should public policy deal with these problems of sequentiality? Scherer proposes several sensible schemes, but events have overtaken his concerns. The

Intellectual Property Guidelines published by the Justice Department address these issues, which are certainly not unique to the biotech industry. Indeed, his suggestion that holders of essential patents in a technology that requires all of them be encouraged by policy to form patent pools is spelled out in these guidelines.

Another important issue in this area concerns the relationship between innovation and market structure. Schumpeter famously conjectured that large firms facing little or no competition should be especially innovative. His argument assumed that the only effect of competition on innovation was to dissipate the rewards to innovation through quick imitation. It did not take account of what Scherer had noticed as a student: that competition might force firms to innovate simply for survival. In a classic 1965 *American Economic Review* paper, Scherer examined the empirical relationship between corporate patenting activity and concentration, and found little or no relationship; certainly nothing to suggest that firms in highly concentrated industries generated more patents than other firms. The data and econometric techniques used by Scherer in this paper were primitive by modern standards, but more recent work, both by Scherer and by others, has done nothing to change this result in any important way. More recently, Scherer has examined interfirm and interindustry differences in the propensity to patent, given R&D spending. He finds that there is no consistent pattern of returns to scale in R&D spending, although the majority of industries display constant or decreasing returns. Holding R&D spending constant, he also finds that industry concentration does not seem to have a disproportionate effect on patenting activity.

Inter-industry R&D Flows and the Return to R&D

Based partly on conversations with the late Jacob Schmookler, Scherer became interested in interindustry flows of innovative effort. Scherer collected data on 15,112 patents issued to 443 companies, issued between June 1976 and March 1977. With a team of assistants, Scherer determined the industry in which each patent originated and the industry(ies) expected to benefit from it. The information was then linked to R&D spending data from the FTC's 1974 line of business survey. This data set formed the basis for three of the papers in the present volume. The first result contained in them is that there are large spillover effects from one industry's R&D to productivity increases in other industries are large. In particular, it appears that nonmanufacturing industries benefit greatly from inventions made in manufacturing, though the reverse is not true. The second result is that both the social and private returns to R&D appear to be high. A third use for this data set was to investigate the theory of Schmookler-striking at the time—that inventive activity responded to economic forces and, in particular, to demand forces. Scherer proceeds generally by relating patents in his data set, which were issued between June 1976 and March 1977, to industrial spending in 1974, taken from the FTC line of business survey. He finds a strong and robust relationship between capital spending and patents, but not between material spending and inventions relevant to the use of materials. Finally, Scherer uses this data set to reinvestigate the relationship between R&D spending. He also examined whether patents per dollar of R&D varied according to industry concentration. Once again, he failed to find strong support for any version of the Schumpeterian hypothesis.

Public Policy toward Patents

Four of the articles in the book deal with patent policy regarding pharmaceuticals in the so-called less developed countries (LDCs). Many LDCs consider pharmaceuticals to be so important to their national interests that intellectual property law does not extend to pharmaceuticals. Pharmaceutical companies in such countries often free ride on the innovative efforts undertaken by companies located in countries where patent laws are strong. This policy by LDCs has been attacked on the ground that it removes much of the incentive to innovate, both for companies in advanced economies and for local companies in LDCs.

The latter point is examined in a paper by Scherer and Weisburst that describes an interesting natural experiment. Between 1939 and 1977, Italy had no patent laws that covered pharmaceuticals; in 1997, patent laws were held to cover them. Itlay's experience does not support the notion that extending patent laws to cover pharmaceuticals will stimulate local innovation. Instead, many Italian companies were acquired by multinationals, there has been no noticeable increase in either R&D spending or drug innovations by Italian firms.

The "harmonization" of patent laws between countries came to a head in 1995 in the Trade-Related Aspects of Intellectual Property (TRIPS) treaty, which required that LDCs stiffen patent protection for new drugs as a condition of joining the WTO. For countries such as India and Brazil, which had developed thriving generic drug industries, this change raised the possibility that generic prices of patented drugs might rise. Scherer and Watal investigate licensing and other policies that are permissible under TRIPS and that might result in relatively low pharmaceutical prices in LDCs. In the main, they suggest that international price discrimination by multinationals might result in higher prices in rich economies and lower pricing in LDCs. They support this by a rather unconvincing Ramsey pricing analysis. Whatever the shortcomings of this analysis, however, Scherer notes that, as of 2003, many AIDS-related drug prices to LDCs were indeed cut drastically.

Patent Values and Stable Portfolios

Three of the papers in the book involve analyses of the distribution of patent values. Scherer has long pointed out that, by any rough measure, patent values and, indeed, values of other innovation-related products such songs and record sales, have highly skewed distributions. The same is true of returns to lotteries and horse racing. In this setting, if the distribution is skewed enough, the law of large numbers may fail to hold, in the sense that portfolios of such items may not have averages that converge to stable values. In the present book, Scherer reports two efforts to carefully calculate the values of patents in Germany and in the United States. His analysis takes account of the interesting feature of the German system that a patent holder must pay periodic renewal fees to keep the patent in force. Scherer's main goal is to estimate the distribution of such values and to try to distinguish between several distributions that are skewed, such as the Pareto and the lognormal. In general, he finds that the evidence favors the lognormal, although there is a suggestion that for some cases a Pareto distribution with a parameter value implying instability may be appropriate. Because of this finding, Scherer and his coauthors conclude that from 45 to 61 percent of the value of the total sample of patents comes from the top 5 percent; the variation in results depends on excluding the highest sample value. Due to the skewness, it is wrong to value the value of a company's patent portfolio simply by counting patents. Also, due to the highly skewed nature of patent values, neither businesses nor governments should engage in policies that involve picking a winner. Rather, they should hold contests between individual R&D projects, as venture capital firms tend to do, and not be dismayed by a high rate of project failure, unavoidable in a skewed return distribution.

All in all, the papers in the present book give a very good exposure to significant issues involving innovation. Both specialists in innovation and general Industrial organization economists will find much interesting material in this collection.

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Reviving the Invisible Hand: The Case for Classical Liberalism in the Twenty-First Century. By Deepak Lal. Princeton and Oxford: Princeton University Press, 2006. Pp. xii, 320. $29.95. ISBN 978–0–691–12591–6.
JEL 2006–1498

This book is a wide ranging and spirited defense of classical liberalism as an organizing principle for the economic affairs of the world. The first four chapters are the heart of the book and deal with the liberal international economic order. Chapter 1 considers the liberal international economic order of the nineteenth century—a period of relatively free trade in goods, along with substantial labor and capital mobility between countries—and how it ended in the interwar period. Chapter 2 considers the transition "from laissez faire to the *dirigiste* dogma" in economic policy more broadly. Chapter 3 addresses the changing fortunes of free trade in recent years. Chapter 4 examines the monetary regimes best suited to maintaining stable money and the free flow of capital between countries. Each of these chapters delve into a variety of topics; for example, the chapter on free trade considers the "new" protectionism, preferential trading arrangements, the WTO, the role of adjustment assistance, and the question of globalization backlash. In addition, each chapter contains a great deal of historical perspective and context.

Chapter 5 examines poverty and inequality both within and between nations and contends that economic growth, resulting from policies that embrace economic liberalism rather than dirigiste controls, have reduced both poverty and inequality. Relying on Surjit Bhalla's work, and taking issue with many of the statistical approaches taken by the World Bank, Lal argues that poverty has been mitigated in India, China, and elsewhere and that income inequality across countries has fallen as well. It is not a surprise that the author, who also penned *The Poverty of Development Economics,* concludes that foreign aid is an idea whose time has passed.

Lal then shifts away from economic policy issues to consider some of the broader challenges facing the liberal order. Chapter 6 takes up the issue of morality and capitalism, where he notes that "previous chapters will not convince the Western anti-globalizers" because of their "underlying belief in the immorality of capitalism." This chapter ranges widely, touching on communalism versus individualism in agrarian societies, Hindu and Sinic civilizations, Victorian virtues and the 1960s cultural revolution, and Christian doctrine, and winds up discussing financial repression in China and India.

Chapter 7 examines another form of antiglobalizers—the "new *dirigistes*"—who are not against globalization but are against capitalism itself. Among these groups is Amitai Etzioni's communitarian movement, which calls for a social paternalism to give capitalism a human face. Chapter 8 on the "Greens and Global Disorder" takes a skeptical view of nongovernmental organizations and civil society. The NGOs are dismissed as "the self-serving and the uninformed, with their own special—often ideological—agendas." Indeed, their "left-wing agenda [seeks] to extend the regulatory system of the U.S. New Deal to the international arena." Lal is critical of "ecomoralists" for their environmental scare tactics and throws cold water on the whole notion of sustainable development.

This book provides a nice blend of personal anecdote, literature review, economic argumentation, and broad empirical evidence. One of the attractive features of the book is that the author draws widely on past thinkers, from Saint Augustine of Hippo and Alexis de Tocqueville to John Maynard Keynes and John Stuart Mill, and shows their relevance to current debates. One disadvantage of taking such a broad-minded approach is that the book sometimes seems to wander so widely as to lose track of the specific theme in each chapter. Still, each chapter has many striking and thought-provoking historical insights and the book moves along from topic to topic at a good pace and in a way that makes for pleasurable reading.

However, Lal's book could have benefitted from a more systematic explanation of the meaning of classical liberalism, its basic principles, and its importance for today. The philosophy behind classical liberalism is reviewed briefly on pages 48 to 51, but much more could have been said. Lal also could have improved the book by giving a longer explication of his opponent's views and being more explicit about those who are expressing such views today. Most chapters begin with just a paragraph or two that states the antiglobalizer view and then marches on to show why that view is erroneous or misguided.

Most importantly, *Reviving the Invisible Hand* did not really give a sense for how the invisible hand is to be revived. There is no specific agenda for the future, no specific blueprint for reform (unlike, say, Milton Friedman's *Capitalism and Freedom*). The reader is left thinking that the nineteenth century order was a pretty good one, but whatever its merits that century is a different place than today. Lal is passionately skeptical of the leftist nostrums of the day, and there is often good reason for his skepticism. Unfortunately, he does not offer much of a positive vision except to reject the nonsense that he sees. One gets the sense that this book was not written to convince those with whom he disagrees about their erroneous beliefs (for perhaps they are unpersuadable), but to provide reassurance and ammunition for those who already share his beliefs.

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The Bourgeois Virtues: Ethics for an Age of Commerce. By Deirdre N. McCloskey. Chicago and London: University of Chicago Press,

2006. Pp. xviii, 616. $32.50. ISBN 0–226–55663–8. *JEL 2006–1500*

What human motivations and social-cultural conditions does capitalism depend upon? For many economists the answer to this question is deceptively simple: capitalism rests upon the self-interested activity of economic agents who rationally plan and strive to execute their plans within a world of stable, well-enforced property rights and considerable individual freedom, at least in the economic sphere.

A different tradition of thought argues that capitalism rests upon a set of character traits and moral virtues that motivate and guide individuals engaged in capitalist activities, without which the capitalist system could not be sustained. The most famous argument along these lines is Max Weber's. In his classic work, *The Protestant Ethic and the Spirit of Capitalism,* Weber argues that modern capitalism emerged through the "worldly asceticism" of individuals adhering to the religious principles of certain forms of Protestantism. These individuals engaged in their profession or trade as a "calling" and were driven to work hard ceaselessly, never wasting money or time, thus accumulating wealth, yet living frugally, not consuming excessively, thus saving what they earned and reinvesting it in their business. They did this, not from economic motives, but rather to live in accordance with God's Will, which according to their Protestant theology demanded from men productive activity with no lapses and no squandering of resources or self-indulgence, demonstrating their worthiness to be among the elect few who will be saved after this life. It was these motives, Weber argues, not the drive to make and spend money, which after all has been a common element of human life and economic activity for millenia, that were crucial to the emergence of capitalism.[1] Adam Smith also recognized the importance of virtues. His first book, *The Theory of Moral Sentiments,* is about the basis for moral conduct. Its sixth part is entitled "Of the CHARACTER of VIRTUE" and focuses on the virtues of benevolence and self-command. Most modern scholars take the view that, while Smith championed the importance of self-interest in the market and other social contexts, he believed that this motive is set in a broader social context in which virtues are vital elements in regulating behavior.

Virtues have a rich heritage in Western thought. They are central in the classical philosophy of Socrates, Plato, and Aristotle; were vital for the Romans who were greatly concerned with civic virtues; and were at the heart of middle ages Christianity. After a prolonger period of relative neglect, in recent years there has been a resurgence of interest in virtues in philosophy, sparked especially by Alasdair MacIntyre's *After Virtue.* Meantime, virtues are also an established topic in psychology, in positive and humanistic psychology, and among social and personality psychologists. Virtues are alive and well in all these fields, recognized as normative guides to conduct that are and should be important in guiding individual behavior in many contexts.

To date, this rekindling of interest in virtues as essential elements in human character and conduct has not reached economics. A few specific virtues have been discussed in the economics literature, such as benevolence or altruism. The neoclassical model of rational choice in general is consistent with the virtues of foresight and self-control (temperance). Virtues arise in specific economic contexts, such as the question of what is proper conduct of an agent in relationship with a principal and in the view that the behavior of individuals who voluntarily pay their taxes is rooted in virtues of honesty and civic-mindedness. But, speaking in broad terms, no general theory of the virtues of capitalism or empirical literature on virtues has developed in economics, and discussion of virtues, as well as Weber's thesis, exist

[1] Weber discusses a number of distinctive features of capitalism. A second crucial one is the rational organization of free, paid labor. He also is clear that capitalism no longer depends on the Protestant religious values he describes but rather emerged in part from them. Indeed Weber begins his essay with quotations from Benjamin Franklin in which Franklin describes virtues that are quite similar to (and, Weber believes, rooted in) the values of worldly asceticism Weber later describes as based in a particular branch of Protestant theology, but which Franklin sets forth for utilitarian purposes. More broadly, Weber studied the relationship between religious values and social systems comparatively. In *Ancient Judaism,* he describes the character and moral qualities of the prophet and the prophet's role in the Jewish nation in the first millennium b.c. In *The Religions of India* and *The Religion of China,* he describes the relationship between religious systems of thought and attitudes and social, political, and economic systems in these two civilizations.

relegated to the periphery of modern economics.[2] A focus on virtues, should it emerge, would connect naturally with behavioral economics, for example in the study of strategies for self-control. Virtues lie also in the intersection of economics and culture, an area which, like the virtues themselves, has remained relatively understudied and may blossom in future.[3]

Deirdre N. McCloskey's new book, *The Bourgeois Virtues,* is a clarion call for economists to recognize the importance of virtues in capitalist society and bring them into economic analysis. McCloskey brings together a wealth of material from the Western tradition and modern philosophic currents of thought and links this, albeit in loose and not always convincing ways, with critical commentary about neoclassical economics. The breadth of sources she draws upon is impressive, and many readers will find the way she weaves together ideas and quotations from diverse sources interesting and valuable. Her narrative is rather kaleidoscopic, however, often linking one quote to the next without much interpretation, which makes the book frustrating in many places, and the details don't seem to cohere easily into a tight structure of argument. Thus her work has its greatest value in bringing the notion of virtues into the world of economics but stops short and leaves critical work ahead to integrate virtues into economic thinking.

Beyond arguing generally for the importance of the virtues, McCloskey has two more specific aims. One is to demonstrate that the fundamental virtues in modern capitalist society are, in fact, the seven cardinal virtues that have been viewed as fundamental in Christianity and Western culture for centuries. These seven cardinal virtues are Faith, Hope, Love, Wisdom, Courage, Prudence, and Temperance. Her argument here is thus largely opposed to Weber's and inherently conservative, trying to claim for the seven cardinal virtues a place in the modern world equal to what they historically had, thus that there is a tight line of connection, in terms of virtues, from earlier periods of Western civilization to our own, whereas Weber believed that there was a discontinuity in the rise of Protestantism and the development of modern capitalism. Her other aim is to call to attention to the sterility of the standard economic description of human behavior—what she calls "Max U"—and start us on the road to flushing it out through incorporating virtues as drivers of behavior. This idea interfaces in an interesting way with behavioral economics and the quest to develop richer models of human conduct.

McCloskey's desire to make both points makes her book sometimes confusing and, in the end, I think she is not fully successful with either, especially the former. As determined as she is to forge an identity between the virtues of modern capitalist life and the virtues of the classical and Christian tradition, and as interesting a thesis as this is, it is problematic. Her polemics against current economic modeling of human nature often get lost in ire at the expense of providing clarity about how to go about incorporating virtues into economic models. Nonetheless her work is thought-provoking and in stretches fascinating and will surely spark further debate.

It should be noted that this book is the first of an ambitious planned four-volume set. An outline of the succeeding three volumes is provided at the end of this one and makes for interesting perusing. In succeeding volumes, it appears McCloskey will take up her two aims separately, and it is to be hoped a bit more systematically, and thus provide a more fully developed and grounded argument for each.

In the remainder of this review, I describe some ways of thinking about and defining virtues, discuss and critique McCloskey's focus on the seven cardinal virtues and present an alternative list of virtues, discuss the notion of a practice, and finally discuss psychological channels through which virtues act and how they might be modeled.

Defining Virtues

Aristotle defines virtues to be characteristics that can be voluntarily cultivated and which are enabling of man achieving his ultimate end or aims of life. For Aristotle, the ultimate end or aim (*telos*) involves living as a good citizen actively engaged in the life of the *polis,* and attaining a

[2] A search on the web of science database reveals approximately eighty-five sources citing MacIntyre's book in 2005–06 and not one is an article in the field of economics. There are several cites in the fields of organizational behavior and accounting in the area of ethics.

[3] In contrast to the relative neglect of virtues, ethics, the study of what are good or proper rules of conduct, has had a significant role in modern economics, for example in the study of rules of fair division and principles for redistribution.

state of happiness, which means both to have lived a good life in conformity with virtue and to live in a state of well-being.[4] Thus virtues are both a means to a greater end but also essential to achieving that end—a man cannot be said to be happy in his life if he does not act virtuously, regardless of his success.

Aristotle emphasizes that virtues are the basis for action and that a man is said to possess a virtue only when he acts in conformity with it. It is not enough to learn about a virtue and believe in it abstractly, one must act on it. He also emphasizes that to act virtuously is a volitional act, that a man cannot be said to act virtuously when he takes a virtuous action by accident or through innate predisposition, but only when he acts volitionally guided by a conception of virtue.[5]

We can contrast Aristotle's position with the modern utilitarian tradition which economic thought by and large lies within. In the utilitarian view, a person's overall happiness is evaluated as his lifetime utility, perhaps incorporating some measure of the utility of others he cares about. According to utilitarian logic, a person should strive to act virtuously to the extent doing so enables him to achieve greater ultimate utility. An individual should be honest, courageous, and generous because by acting that way he will maximize his lifetime expected utility—and he should strive to act according to these virtues only to the extent they help him achieve higher utility, for being honest or generous or courageous has no further benefit beyond its contribution to utility. A classic example is an economic agent acting honestly because "honesty is the best policy" in the long run.

The utilitarian view of the virtues is associated with Franklin and his precepts for good living. Franklin recounts in his *Autobiography* how he came to form a list of thirteen virtues and states that his own "constant felicity" of life, including health, wealth, and reputation, was due to striving to live his life in accord with these virtues. Be virtuous, he believed and restated many times, and you will have a happy, successful life.[6] This utilitarian view is probably the one shared by most of those among modern economists who believe that agents behave virtuously. It also fits with the general approach of evolutionary psychology—the idea that cultures have evolved virtues as constructs that help the members of society achieve greater ends, as well as the working hypothesis that there may be an evolved biologic predisposition toward acting in accord with some virtues.

To develop this instrumental, utilitarian view of virtues further requires understanding why adhering to a virtue and striving to act in a manner consistent with it is instrumentally beneficial, and building models to show how this process works in economic and social systems.

One important step in addressing this issue is understanding how cultivating virtues and striving to act according to them influences behavior, and modeling this process. There are in fact two distinct psychological pathways through which virtues can influence our behavior. One is cognitive: virtues provide ideal models that guide us, that we strive to emulate. The other is motivational: we want to act in accordance with virtues,

[4] Aristotle, *Nichomachean Ethics*, translated with introduction and notes by Martin Oswald (Indianapolis: Bobbs-Merrill, 1978), Books 1, 2, and 3. Aristotle paraphrases the common view, which his definition somewhat follows, as (this is Mr. Oswald's translation) "living well" and "doing well" (p. 6).

[5] It is worth noting, though not essential to my discussion, that Aristotle focuses, famously, on virtues that represent the mean between two extremes. For example, generosity or the correct attitude in regards money is the mean between stinginess and extravagance, and courage is the mean between cowardice and excessive confidence or recklessness. He recognizes that some actions are viewed as wrong and the corresponding virtue is not a mean but an absolute prohibition, but does not focus on these.

[6] In fact, this is probably an oversimplification and misstatement of Franklin's own views, at least as a mature adult. Franklin believed in God, and believed that to be virtuous was to follow God's way. In describing how he came to write his list of thirteen virtues and develop a scheme for living in accord with them, he writes that he conceived the project as a way to arrive at "moral perfection." "I wished to live without committing any fault at any time," he writes. This type of reasoning is not marginalist, involving being virtuous only to the degree that and situations in which it is beneficial. Rather it involves a commitment to be virtuous for its own sake. In addition he states that in the notebook in which he traced his progress in acting in accord with the virtues he had this quote from Addison's *Cato:* "'Here will I hold. If there's a power above us // (And that there is all nature cries aloud // Thro' all her works), He must delight in virtue; // And that which he delights in must be happy.'" Franklin (1963), p. 89–98, quote p. 94. This is utilitarian in some sense, but through a religious path of interpretation.

to feel that we are virtuous and act virtuously. The stories of virtuous behavior that are so important to cultivating virtues and transmitting them culturally—courage transmitted through stories of great heroism, exemplified say by John F. Kennedy's *Profiles in Courage,* wisdom in the story of Solomon—provide both ideal images of virtue in action and are highly motivating in providing exemplars that one can associate oneself with by emulating the virtuous conduct depicted. To incorporate the logic through which virtues influence behavior into economic models in a convincing, well-grounded way requires incorporating these psychological pathways into models describing human conduct, a more psychologically based approach than is manifest either in *The Bourgeois Virtues* or in traditional economic modeling. I discuss this further in the last section.

The next step is to build models describing how virtues are transmitted culturally, an issue I touch on below, and the last step is the construction of models of the influence of virtuous conduct on economic and social outcomes. This last step is the one that economics in its current form is most well-suited to, and indeed there are many specific examples of analyses studying the impact of virtuous behavior, such as honesty—for example in models of reputation and models of compliance.[7] A more general framework, in which there are several virtues, each having its own sphere of influence, in a larger general equilibrium environment, has not yet been developed, but, following the logic of McCloskey's work, can be envisioned.

I note to conclude this section that there may be room for Aristotle's view that being virtuous itself contributes to happiness in the utilitarian view, through incorporating virtues into a utility function. His stronger argument that being virtuous is necessary for happiness could perhaps be captured through a Leontief-style *min* utility function defined over a set of virtues and a bundle of all other goods: for example (this is just a sketch of an idea), $U = Min(v_1; v_2; ::: v_q; G(x_1; x_2; ::: x_n))$.[8] Here virtuous conduct gives its own reward, which may be (but does not have to be fully) separate from other components of utility.

Virtues of Modern Capitalism

There are two main issues to address in considering virtues in relation to modern capitalism:

(1) Do virtues have a significant role in capitalism? This has two parts:

  (A) Are virtues crucial in underpinning capitalism, in regulating the behavior of individuals in economic transactions and capitalist enterprises?

  (B) Is modern capitalist life conducive to the flourishing of virtues—does it encourage or lead individuals to be virtuous?

(2) Given an affirmative answer to (1), what specific virtues are most important for modern capitalist society?

McCloskey clearly believes that the answer to (1), both parts, is a resounding yes. *The Bourgeois Virtues* is peppered with examples in which economic agents act virtuously, or at least strive to, thus are guided by a conception of virtue. We read of Joe, who takes pride in being able to fix every piece of equipment in his factory and relishes the challenge of fixing a machine that isn't working, is not guided by any kind of marginalist maximum utility principle but by the virtue of doing his job as well as possible (p. 470). We are treated to a description of the Royal Palace on the Dam Square in Amsterdam, built in the Dutch Golden Age of mercantilist capitalism, with its four statues representing the four pagan virtues of justice, prudence, temperance, and, substituting for courage, vigilance (p. 290–92). We are told that, for the most part, "honesty is the best policy" rules in the business world. For example, Donald Macaulay's findings presented in a 1963 paper about firms doing business in Wisconsin are presented as evidence that generally business depends on mutual trust, not narrowminded legalism (p. 129). We learn about the values of the Dutch middle class—that the same word is used in Dutch to connote both *clean* and *beauty* and

[7] In the classic reputation model of David M. Kreps and Robert Wilson, there are two kinds of agent—honest and not inherently honest. Thus some agents act virtuously. See Kreps and Wilson (1982), "Reputation and imperfect information." This model has had wide application. An example exploring the impact of honesty and social outcomes is my work with Brian Erard (1994), "Honesty and evasion in the tax compliance game."

[8] If a person derives utility from acting in accord with a bundle of virtues, the model can be expanded to define utility over sets of virtues.

that the Dutch take enormous pride in the cleanliness of their homes. To be sure, some counterexamples are provided, such as the ruthless mentality of traders in the pit of the Chicago Mercantile Exchange (p. 29) and the excess greed of CEO's.[9] But these are treated as exceptions that prove the rule, and McCloskey's view seems to be that there will be unvirtuous behavior in all systems and these cases hardly make capitalism less virtuous than other economic systems.

The idea that many economic agents behave in a trustful and trusting manner, take pride in their work, and exhibit a variety of other virtuous behaviors is not news. Many economists are undoubtedly open to the possibility that this is true, that virtues matter, even if they remain not fully convinced. McCloskey's scattershot examples on this point, while often interesting and edifying, don't advance the discussion much, and she falls well short of the kind of systematic empirical inquiry that would be convincing to those who are not already convinced. A more thorough investigation should consider a broad range of different contexts of modern capitalism. Indeed developing a taxonomy of contexts vis-à-vis virtues would be useful. For each context, the relevant virtues need to be identified, then an empirical investigation (and perhaps experiments as well) conducted to investigate whether individuals who participate in this business context articulate virtues for their roles and strive to act according to them.

Believing in the truth of (1) is consistent both with the Aristotelian view that it is intrinsically valuable as well as important for achieving happiness in life to act in accord with the virtues and also with the instrumental, utilitarian view that acting virtuously is the surest, most direct way to achieve high lifetime utility. Many economists who believe economic agents act virtuously probably take the second view, believe that agents act virtuously because it is in their own long-term self-interest to do so. McCloskey believes that economic agents strive to act virtuously not only from the viewpoint of their long-term success in the capitalist world, but also so they can feel they have lived a virtuous life, thus follows Aristotle and a long tradition. She not only puts forth this view directly, but it is also present in the religious undertones in her book—she calls herself one who has recently discovered (or rediscovered) Christianity. However her examples are not sufficiently carefully presented to clearly distinguish between the Aristotelian and utilitarian positions. Surprisingly, many of her examples are fictional—drawn from the books of Jane Austin, Shakespeare, movies, contemporary novels—thus we must believe are at least in part didactic and meant to illustrate the importance of living virtuously. The examples of actual people tend to be relatively brief vignettes without much context, for which we cannot be sure a clever utilitarian can't come up with a rationale for the apparently exemplary virtuous behavior that is described. To make the point, McCloskey really wants to make—that most people live virtuously for its own sake—requires building a much more careful and extensive empirical case, focusing especially on cases in which the virtuous action and narrowly self-interested action do not, as far as can be determined, coincide. Citing more of the scientific evidence on this, for example on altruism, would strengthen her argument, but for the more specific argument that economic agents strive to be virtuous in their roles in the economy I think far more needs to be done.

The more interesting aspect of (1) is (B), the possibility that market activity is conducive to the development of virtues. McCloskey's view here, which resonates with earlier ideas of Albert O. Hirschman and others writing at the intersection of economics and sociology (and often history), is that market activity supports civility and "small acts of virtue every day" (p. 31), in contrast perhaps to grand heroic acts, which virtue theory is sometimes too focused on. People who trade with each other are less likely to go to war against each other, more likely to respect each other. Engaging in economic activity engenders charity, trust, recognition of the other's point-of-view, builds the sense of hope that goes with investing for the future, and helps develop many other positive, virtuous qualities.[10]

[9] Indeed Chicago (not to mention the Chicago School of economics) is given some tough treatment. McCloskey lauds the trusting environment of life in Iowa as compared with the more mercenary, less trusting economic world of the big city (pp. 130–32). This seems to run counter to her main argument.

[10] Hirschman (1982), "Rival Interpretations of Market Society: Civilizing, Destructive, or Feeble?"

McCloskey gives three main reasons why capitalism is conducive to the flourishing of at least many virtues, if not all. One is a wealth effect: greater prosperity offers more opportunities for more people to have a more meaningful life. Poverty is definitely not a source of virtuousness in McCloskey's world, but rather makes people mean, money-grubbing, more pressured, thus less able to develop fully and virtuously. Part of the wealth effect is the greater longevity that economic development brings, which offers a person more time to develop himself or herself in multifarious ways, which McCloskey believes generally leads to the gaining of virtue. A second reason why capitalism is virtuous is that it is linked to the breaking of traditional social and cultural constraints. The opening of markets is associated with the weakening of strong, traditional ties that pin people to very specific roles that straitjacket them, for example working in the family trade and living in a particular place. These forced requirements stunt human development, therefore reduce virtuousness for McCloskey. In capitalist society, people are freer to construct their own ties, especially the many weak ties that are constitutive of so much of modern life, and construct their identities for themselves. While in philosophy there are different views about whether this is or is not conducive to human flourishing and virtuous behavior, McCloskey is firmly of the view that constructing one's own life makes one more able and willing to pursue the development of virtue. The third reason why capitalism is virtuous that McCloskey offers is that day-to-day life in capitalism is conducive to learning to treat others with respect and fairly and, in general, to cultivation of at least some virtues, such as prudence (good forward-looking decision making) and temperance (learning self-control in the face of all those tempting choices) and at least some kinds of benevolence and a sense of fairness (in regards business transactions, wages, and prices). This argument hearkens back to 1(A)—one learns these virtues because it is through them that one can succeed in the capitalist world.

These arguments are all interesting and in a general way they are all familiar. McCloskey's special contribution is to link the general discussion with virtue. Her basic line of argument is that greater opportunity, more freedom to choose one's life, more need to deal with a wide range of others, and the living of longer life of lesser deprivation all lead people to become more virtuous. This is an interesting thesis but one that needs empirical evidence to support it, while McCloskey offers no more than scattered anecdotes. Further, virtue needs to be operationalized in some way in order to pursue this line of inquiry. How can we tell how virtuous a person is? Should we ask others who know him? Should we assess his virtuousness based on his known actions?

Also, there are surely downsides to capitalism from the viewpoint of the development of virtues, which need to be considered. What happens to those who fail in the open, competitive, less role-encumbered world of capitalism, either because they do not have abilities that are rewarded in the capitalist system, or fail in certain key virtues, for example fail to be prudent or temperate or are unable to forge good working relationships? It seems there is a need in capitalism, if the goal is to enhance virtue, for some mechanism in the system to provide for second chances and mercy, itself a virtue which may or may not be cultivated by capitalism. And how virtues are cultivated needs to be spelled out.

The Seven Virtues and Other Virtue Lists

As interesting as this general discussion of virtues in capitalism is, much of the real meat of *The Bourgeois Virtues* concerns issue (2), the question of which specific virtues are fundamental for modern capitalism. McCloskey has a strong opinion about which virtues are important. She believes that the seven cardinal virtues, at the heart of Christianity and the classical Western tradition, are the key virtues for capitalism, as for all human life, and should be the focus of analysis.[11] This is an interesting thesis, and may well be original, and is worthy of consideration and further examination.[12] As I noted at the outset, to the extent virtues have been discussed at all in economics, it is generally piecemeal, with no real system in mind or larger framework to guide the discussion in terms of nomenclature and what are

[11] She writes that she seeks "to enfold" discussion about virtues in modern life "into the seven virtues of the classical and Christian world" (p. 65), and describes her project as "finding how the classical virtues lie down on capitalism" (p. 317).

[12] One author McCloskey cites who has plowed some related fields is Michael Novak.

the fundamental virtues to focus on. McCloskey has taken things to a new level, where systems of virtues can be set forth, explored empirically, and argued about.

McCloskey's theoretical position in terms of virtues is rooted in Western culture and perhaps most closely the thought of Thomas Aquinas. She quotes from Aquinas in defense of the seven virtues as foundational: "The cardinal virtues," Aquinas notes, "are called more principal, not because they are more perfect than all the other virtues, but because human life more principally turns on them and the other virtues are based on them."[13] "The seven virtues of the Western tradition . . . are ethical primary colors," she claims, all other virtues are subsumed under them or combinations of them. Her argument is not just that these virtues are foundational, but also, following a long tradition, that one is truly virtuous when one acts in accord with the full set or a large subset of them, not any single one. Indeed acting in accord with one virtue and not the others is often akin to vice—for example being courageous but not prudent is foolish, and temperance and prudence without love can be miserly. And her argument is not just about making manifest that the virtues are the bedrock of modern life but also about critiquing modern economic thought, which in her view rests on a single virtue, prudence ("Max U"), and thus fails to capture the inherently multi-faceted nature of human conduct.

McCloskey describes the seven virtues in 22 consecutive chapters over 200 pages, providing a rich tour of historical sources, contemporary discussions, and diverse contexts in which the virtues matter. The seven are divided into the four classical virtues: justice, courage, temperance, prudence; and the three Christian virtues: love, faith, and hope. McCloskey takes up each set in turn, beginning with the Christian virtues. Her sources range from philosophy, feminist thought, and sociology to classical works, artists (Vincent van Gogh gets a chapter), novels, and movies (*Jerry Maguire* and Shane get notice, among many others). No one can doubt her expertise in the field of economic history, and she evinces considerable knowledge of feminist ethics. The Netherlands comes in for special treatment, and there is discussion of Japanese culture and society, though McCloskey acknowledges she is not an expert in this field.

Love is the virtue McCloskey discusses first and in greatest detail, and the nature of her approach and its limitations are well-exhibited by her discussion of this virtue. She begins her discussion with this statement: "Love can be thought of as a commitment of the will to the true good of another." By love she means not the love of desire but the love of genuine caring, concern, and appreciation for another. She means the love of domesticity and good society, referring as an example to the concept of love in Jane Austin—a kind of ethical goodness that is central for being a good person.[14]

A major peeve McCloskey has, here and throughout the book, is the reduction of virtue to utility. In her view, love cannot be reduced to an altruistic term included in utility, the way for example Gary Becker and many economists would like to proceed. Doing this, she says, is invalid because this definition makes love inherently self-interested, since one is obtaining utility through loving another and thus through the other's good fortune one obtains utility for oneself. In addition to not being, so she argues, what we mean by love, this violates a basic principle of ethics in making another person a means rather than an end in herself—think back to Immanuel Kant's categorical imperative and *Critique of Practical Reason.* Rather, as McCloskey develops it through citing the work of a string of philosophers, love is disinterested and is inherently manifested as a constraint on one's will in such a way that one respects the will and being of another; and love is an appreciation for another shown as an attentiveness to the other simply for the sheer love of that person, with no further good to be gained. These distinctions have merit. Yet, without disagreeing with the conceptual definitions McCloskey offers, I was not convinced that love thus defined cannot be represented with a mathematical function that guides

[13] Page 361 in *The Bourgeois Virtues.* Original from Thomas Aquinas, Disputed Questions, 1269–72, "The Cardinal Virtues," Art. 1., p. 112, as cited in *The Bourgeois Virtues.*

[14] In her religious worldview, this love is rooted in love of God and comes from God (chapter 5 of *The Bourgeois Virtues*). She also notes that acting on love alone is not virtuous—love must be combined with other virtues, like justice and prudence, for true virtue.

a person in his decisions and actions, regardless of whether or not we call it utility. Perhaps the function guiding decision making and action needs to have two parts, one utility and the other virtue-based. These two parts may be separate or more likely linked through the notion that right conduct requires attending both to one's own needs and those of others. If constraint is central to love, then love should be incorporated as constraints on will or actions. If appreciation and attention are central, these should be incorporated, perhaps in models of limited attention. Thus the discussion offers useful perspectives but leaves the door open for modeling love in a way that can incorporate it into economic thought.

McCloskey's second point is that the kind of love she means—ethical caring, respect, and appreciation of the other—is central to the workings of economic systems of capitalism. Look at the way individuals paired in business transactions trust each other, without any formal legal protection, and sometimes with no long-term relationship—think of the example of leaving a tip in a restaurant in an exotic foreign country one will never visit again. Consider the way that coworkers show genuine concern for one another, so important in sustaining the camaraderie and teamwork needed for an organization to function properly. Consider the not-for-profit world and the commitment individuals make to helping others as part of socially responsible business. These are all, in her view, actions rooted in love, and the failure to recognize the importance of love as integral in the fabric of modern economic life is a glaring deficiency in modern economic thought, one she traces in part to the male bias of the profession.

The point McCloskey is making is surely sensible; and it is useful to push economists to broaden and enrich their conceptual frameworks to explore the relevance of love in economic life. Her work is helpful in sketching how to think about the virtue of love and its potential roles in economic contexts.

Her approach has significant failings, however. Starting from the fixed list of seven traditional virtues creates a very rigid structure into which all the variety of moral states and feelings relevant to modern economic life must be crammed. The result is a good deal of word-stretching and a loss of subtlety of description. The variety of feelings and behaviors grouped as all falling under the single virtue love illustrates this. McCloskey begins with love as in the ethical caring and mutual respect in a good marriage à la Jane Austin. She moves on to love of God, then to love of parents for children. Only after all of this does she finally come round to the economic sphere, at which point she argues that this same principle of love that is the basis for a good marriage, love of God and love of children is the basis for many behaviors we see in the economy, such as the comradeship of coworkers and treating customers as people and not just potential sales. This identification of domestic and transcendent love with relationships in the business world seems forced. None of these business relationships has the same intensity of commitment—which, McCloskey argues, recall, is basic to the definition of love—as is typical between parent and child or in the spiritual commitment to God. The level of self-sacrifice, especially thought of as ongoing and long-term, is just a lot lower in helping out a customer than in meeting the daily needs of children. Further, the feelings that are associated with these different forms of relationship are different—love for my child is qualitatively different than that associated with friendship with a coworker—which for that matter is different than the feeling associated with a close personal friendship. One cannot help feeling that, in equating these different forms of relationship, McCloskey has ended up in a position closer to what many traditional economists with their "Max U" models do than what many philosophers whose ideas McCloskey has otherwise embraced feel comfortable with.

Further, grouping all of these different kinds of relationships and caring for others under the single term love just to emphasize a rather rigid cultural continuity strikes me as counterproductive. McCloskey's position seems to be the very conservative one that, when it comes to virtues, they have already been mapped out, long ago, and we need only follow the ancients. One of the points of *Habits of the Heart,* a book which comes in for some criticism in McCloskey's book, is that our vocabulary for describing our personal values is depleted, so that we are not very good at articulating the meaning we seek in life or our reasons for why we live the way we do, including the virtues that are most important and we seek to cultivate. While such articulation may not be necessary—one of McCloskey's points—it hardly seems useful to develop a theoretical framework that is so limited in the number of distinct virtues, but rather better to open up the vocabulary and give more choices—choice is good here as in other realms of life. Why not recognize that there are certain virtues specific to economic life, indeed to modern capitalism, as Weber believed, such as (Weber did not pursue this line) the special sense of friendship and willingness to help that coworkers in an organization feel who share common goals and office space, while also realizing that their friendship is grounded in this setting and does not, in most cases, transcend it. And rather than calling this love, give it it's own term, such as "working friendship and solidarity." This particular quality is undoubtedly cultivated through particular channels, like work experiences, that are distinct from those through which domestic love is cultivated, and it is surely possible for an individual to be highly virtuous in his treatment of coworkers but not loving at home, or vice-versa. The advance of analysis here requires making more distinctions, not fewer.

This same grouping and word-stretching occurs with much of McCloskey's discussion of the other virtues. Is faith rooted in God and the belief in ultimate salvation the same as the faith an entrepreneur has that he will launch a successful business? Here I think culturally we do more commonly use the same word for both, but perhaps that points more to an impoverished vocabulary than to the idea that these are the same personal qualities.[15] As a considerable literature in modern philosophy has discussed, justice is a word used to describe a wide range of different decision rules and circumstances. Thus again we should think carefully about whether we want to equate the commitment to fair dealing and *quid pro quo* in business with our view about what is just punishment for murder. Other virtues work better. For example, I found McCloskey's discussion of humility (Chapter 14; considered by her as part of temperance) and its relation to activities like listening to the customer quite persuasive—here a single moral quality seems common across a span of activities (but of course this may simply reflect my own bias and a lack of careful empirical studies). Likewise honesty may well be a fundamental virtue common across many activities. But a partial match is not sufficient: McCloskey states that her program is "finding how the classical virtues lie down on capitalism" (p. 317), that is the full set of seven, and they don't seem to lie down all that neatly.

A different, more empirical approach would be to observe individuals closely at work (and play) in the capitalist system, see what virtues they exhibit in their business behavior, and ask them what virtues they view as important for their work life, that their business practices rest on and in turn cultivate. From this empirical base one could develop a list of virtues, which could in turn be subject to further scrutiny and testing.

Leaving this empirical approach for subsequent research, what do other researchers and observers have to say about the virtues important for capitalism? In the Introduction I have listed some of the virtues Weber thought were central. His list is more focused than McCloskey's, and clearly not encompassing. Modern psychology offers a quite rich menu of alternatives that is definitely worthy of consideration, and which I hope McCloskey herself will consider more seriously as she continues her research. The book *Character Strengths and Virtues: A Handbook and Classification,* viewing virtues from the perspective of positive psychology, is organized around this template of virtues:

Wisdom and Knowledge: creativity, curiosity, open-mindedness, love of learning, perspective.

Courage: bravery, persistence, integrity, vitality.

Humanity: love, kindness, social intelligence.

Justice: citizenship, fairness, leadership.

Temperance: forgiveness and mercy, humility and modesty, prudence, self-regulation.

Transcendence: appreciation of beauty and excellence, gratitude, hope, humor, spirituality.[16]

One evident difference from the traditional seven cardinal virtues is in the first category, the

[15] Faith also has a psychological basis. Erik H. Erikson roots it in the basic trust that develops in infancy—see his *Childhood and Society.*

[16] Christopher Peterson and Martin Seligman (2004), *Character Strengths and Virtues: A Handbook and Classification.* McCloskey discusses this book in chapter 27, but the chapter is only six pages and seems an afterthought. In general, a stronger engagement with psychology would be helpful.

cluster of creativity, curiosity, and open-mindedness. These qualities seem especially important in the context of discussing the virtues associated with capitalism, which is distinguished by its extraordinarily dynamic nature—waves of new innovations and enterprises that arise and overwhelm existing businesses and established practices and technologies, with the continual opening of new markets and ceaseless expansion, penetrating into more and more realms of human life. The virtues that accompany this dynamism include openness to change, creativity, tolerance for diversity, and flexibility, which is connected to the notion of being able to take the perspective of others (also important for the negotiating that is so pervasive a part of business), the last term in this category.

The last category, transcendence, is also one that does not show up in the traditional seven virtues, though it can be argued it is part of love and faith. In regards capitalism I think transcendence is most relevant in the notion of personal development in a free society—the intrinsic value we place on an individual being able freely to fashion his or her own life, to become a unique person—in other words, the value of individualism. Transcendent individualism is partly a heritage of Romanticism. McCloskey argues that Romanticism was largely anticapitalist and anti many of the values and virtues she holds dear, but I believe that is wrong. Romanticism is in fact historically linked with the Enlightenment and the rise of modern society, and was crucial to the rise of individualism, which is a cornerstone of the free enterprise system, especially in its American form.[17]

It is worth noting that not only have the more traditional virtues that McCloskey focuses on been neglected in economic thought, which is the point of her book, but there is also a striking lack of tradition in terms of theorizing about creativity and individualism in economics. I am engaged in such an enterprise, which I believe involves constructing different, richer models of human nature and our conceptual worlds.[18]

Surely many other virtue lists could be produced, each somewhat different and having some interesting insight about modern life. But to go further and construct a list specifically focused on capitalist enterprise would be a novel and worthwhile endeavor.

The Notion of a Practice: A Context for Virtues

In *After Virtue,* MacIntyre emphasizes that virtues are defined in the context of practices. A practice is a socially constituted activity that has internally defined standards of excellence that individuals engaged in the practice strive to achieve, and that is an ongoing, open-ended activity that is continually being modified, improved, and extended. The key feature of a practice as MacIntyre defines it is that there are goods *internal* to the practice, e.g., standards of excellence in performance, and at the same time *external* goods like resources and fame are attached to the practice, accruing to individuals engaged in it, and thus impinge upon it. A nice example is baseball: there are standards for what is excellence in baseball, for example the way a pitcher controls his pitches and sets up a specific pitch to strikeout a batter, and to be recognized as an excellent baseball player is to be recognized as having achieved excellence in regards these standards. At the same time, external goods—fame

[17] Some argue that Romanticism was one root of fascism and thus implicated in the atrocities committed by the Nazis. The fact is that early capitalism had barbaric elements within it, notably the plight of the urban poor—the focus of so much of Charles Dickens's writing, roots from which sprang socialism and communism. Thus both ideologies are linked to social movements that had dire social consequences; but both also are the root of our modern life.

[18] Jonathan S. Feinstein (2006), *The Nature of Creative Development,* especially the Epilogue. Individualism seems to have been lost from the tradition, split off from the mathematical approach that became the hallmark of economics from the late nineteenth century to neoclassical economics. An example of this splitting is John Stuart Mill's work—his essay *On Liberty* is one of the great statements of personal freedom of individualism, yet in his *Principles of Political Economy* he never mentions these principles; thus his thought seems split. One can detect the split in the work of Alfred Marshall as well. He is famous for his *Principles of Economics,* which develops the abstract market model in which all individuality is suppressed; but the planned second volume of this work, which was eventually published as *Industry and Trade* has a long section on the chains of innovations made by specific individuals in key industries, showing his recognition of the importance of individual enterprise. In the Twentieth Century the split shows in the divide between Friedrich A. Hayek's approach, exemplified in *The Constitution of Liberty,* and the highly mathematical neoclassical school, which has been dominant.

and money—are clearly part of professional baseball and impinge upon it. A virtue, in MacIntyre's definition, is "*an acquired human quality the possession and exercise of which tends to enable us to achieve those goods which are internal to practices and the lack of which effectively prevents us from achieving any such goods.*"[19] One must act in accord with the virtues to achieve the goods internal to practices. Thus, it is not simply a question of success, for example winning the game, but rather how one plays also matters. If a pitcher strikes out a batter to win a game by scuffing up the ball he has acted against the rules of baseball and regardless of what external goods he thereby gains cannot be viewed as having attained excellence in the playing of baseball.

It would be interesting to consider how this concept of virtues embedded in practices works for capitalist activities. A small literature has developed recently exploring this idea for organizational practices.[20] Of course for capitalism the "external" goods are in a sense internal: making money is in the spirit of capitalism. But for a host of specific practices, including management & business leadership, manufacturing, and transactions ("the art of the deal") there may well be internal goods or standards for which specific virtues are crucial.

Modeling Virtues

There are two basic behavioral processes that need to be modeled in order to incorporate virtues into economic modeling. One is how virtues are learned and internalized. As McCloskey discusses in her book, we learn virtues mainly through stories in which individuals exhibit specific virtues, like courage or love, to the highest degree, thus are exemplars of these virtues—think of Jesus, Solomon, Martin Luther King or, in the world of capitalism, Warren Buffet. Virtues are cultural constructs, and passed on in culture, like memes or broader concepts.[21] In terms of modeling, in an overlapping generations framework, individuals learn virtues through the stories they hear describing virtuous behavior of members of earlier generations. To the extent we learn virtues not just from a few famous exemplars, but also from our parents and other adult models, the virtues and virtuousness of the previous one or two generations may be an important factor in the development of virtue in the current generation.

The other process, at the heart of any social science model of the virtues, is the process through which individuals draw upon their sense of virtue and act virtuously: the behavior virtues lead to. If an individual has internalized a virtue and values acting virtuously, then a sensible model is that he evaluates each possible action or decision in a given situation in terms of how close it comes to the ideal behavior the virtue calls for. He then weighs this distance as one factor in his decision, presumably together with other, more traditional utility factors. When multiple virtues are involved, as is typically the case, he would evaluate the distance of each possibility from these virtues using a metric of some kind.

As simple and natural as this process seems, deeper issues are at stake. The procedure I have outlined is based on a cognitive model in which individuals evaluate the virtuousness of acts by reference to ideal points—for virtues are inherently ideals. Thus we must ask why this approach is preferred to simply evaluating each act in terms of its virtuousness without any ideal point serving as a benchmark. Indeed in economics, rooted, as McCloskey points out often, in the single virtue of prudence, which itself in its modern form at least is viewed as tied to calculation of the benefits and costs of various courses of action, ideals are really not the basis for decisions and actions. The optimal decision or action generally trades off different benefits and costs and is generally not an extreme or ideal point.[22]

Why then are virtues, inherently ideals, so important? It may be that we learn virtues best as ideals, so that the use of the ideal is really rooted in the process of internalization. Alternatively, it may be that cognitively having an ideal available is

[19] MacIntyre (1984), pp. 181–91; the quote is on p. 191 and is in italics in the original.

[20] See, for example, Rosa Chun (2005) and the special issue of *Organization Studies* (2006), Special Issue on *Organizational Virtue and Moral Agency in Organizations.*

[21] Susan Blackmore 1999; Jonathan Feinstein 2006, chapter 17.

[22] A virtue is an absolute standard such that an individual acting truly in accord with a virtue is not adjusting his action to equate some marginal benefit of action with marginal cost. Rather, he is striving for purity of action, for that is what it means to be virtuous. The philosopher who comes to mind is Kierkegaard.

the best way to calibrate virtuousness. Following this logic, it is plausible that an important value virtues have in guiding behavior is rooted in bounded rationality. The virtuous course of action in a given situation may be relatively simpler to determine, assuming one possesses a well-internalized, well-formed understanding of the relevant virtues, whereas trying to balance competing interests may be too difficult. Thus it may be relatively easy to understand what is the honest or courageous action, whereas figuring out the costs and benefits to all interested parties, including oneself, so as to determine the action that maximizes some sense of social welfare may be very difficult. Similarly following a concretely defined principle of fairness in a particular circumstance, such as equal division, is likely to be simpler than trying to weigh all potential costs and benefits, including those far in the future. Virtues are applicable across a wide range of situations—for example, in the world of business, standards of good conduct in business dealings apply across a wide spectrum of business transactions and contexts, thus cultivating virtues is an efficient way for an individual to prepare himself to act appropriately in a diverse range of situations he may confront. Finally, another reason virtues as ideals may be so important is that virtuous ideals are important not only in guiding individuals cognitively, but also in motivating them: inspired by the ideal of virtue and the exemplars we know, we are motivated to behave virtuously. If this third possibility is correct virtues influence behavior through both cognitive and motivational pathways, as I have discussed above in "Defining Virtues," and for this dual role virtues as ideals are an efficient, powerful construct.[23] This last, motivational factor is clearly related to the issue of commitment. In a situation where it is hard to commit to the right attitude or action having a strong sense of relevant virtues may help one commit. Developing formal models based in these ideas of how virtues influence individuals in their behavior is a challenging and potentially quite important task to be undertaken.

These arguments have analogs at the social level. Thus, just as virtues may be an efficient model for an individual to try to follow, virtues and stories of virtuous behavior seem to be a very efficient way to transmit information about socially appropriate behaviors in a wide range of situations. In particular, it is impossible in transmitting culture to imagine or teach individuals about all possible situations they may find themselves in. Virtues are a powerful social-psychological construct that can guide and motivate individuals across a wide range of situations to act in a socially beneficial manner, thus may be culturally very adaptive.

[23] These different possibilities are of course not mutually exclusive—all three may factor into the way virtues influence behavior.

Conclusion

The Bourgeois Virtues is a significant contribution to the study of the moral basis of economic life and thought. McCloskey has woven many sources and a number of traditions together to provide the beginnings of an argument and discussion of the role of virtues in economic life. Her approach intersects with, but also challenges, ongoing streams of research in the areas of behavioral economics and social, cultural, and institutional economics, and her vision is original. Both empirical and theoretical work will be needed to develop her thesis, evaluate its importance, and shape it to fit the contours of modern capitalist life.

I have argued that focusing on the classical seven virtues is possibly not the best way to proceed in bringing virtues into economics, rather that a more tailored set of virtues should be developed, rooted in careful empirical research. I have sketched a few approaches for modeling virtues in ways that may fit with economic methodology, but I am confident that if theoretically oriented researchers take up the challenge a number of approaches may be explored and developed.

Virtues have truly been neglected, and the further development of this topic can only enrich and improve economics. Virtues should be recognized as important constructs for individual behavior and brought into economic models of individual behavior. They are also important as cultural constructs, and exploring their role in modern economic life and economic modeling can contribute towards the development of an interface between economics and cultural studies, important for many fields of economics, including economic development and cultural change.

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R Urban, Rural, and Regional Economics

Entrepreneurship, Geography, and American Economic Growth. By Zoltan J. Acs and Catherine Armington. Cambridge and New York: Cambridge University Press, 2006. Pp. xii, 250. $70.00. ISBN 978–0–521–84322–5.

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This stimulating book analyzes the interplay between entrepreneurship, innovation, and economic growth in the United States of America. The starting premise is that innovation is the driving force of growth in the "knowledge economy." The book draws on insights from theories of endogenous growth and economic geography to propose a unified conceptual framework that places entrepreneurship center stage in the process of innovation and growth. Empirical evidence on entrepreneurship and employment growth is presented using an impressive dataset on more than 14 million establishments that existed at some time between 1989 through 2001 across 394 Labor Market Areas (LMAs) of the United States.

While there are now large theoretical literatures on economic geography, innovation, and growth, there are few formal models of entrepreneurship and little is known about its underlying determinants. Indeed, in most economic models entrepreneurship is firmly encased within the familiar black box. The authors are therefore to be applauded for their theoretical and empirical contributions in this area, but a thorough understanding of the microeconomics of entrepreneurship remains some way off. A central challenge in understanding entrepreneurship is identifying the right institutional and regulatory framework toward entry and employment. Progress in this direction is starting to be made by innovative recent empirical work on entry barriers and labor regulation using detailed information on actual policy measures (see, in particular, Timothy Besley and Robin Burgess 2004; Juan C. Botero et al. 2004; Simeon Djankov et al. 2002; and Thomas J. Holmes 1998).

In their empirical work, the authors consider a restrictive definition of entrepreneurship as *new firm formation.* While this is undoubtedly an important facet of entrepreneurship, concentrating exclusively on new firm formation sits awkwardly with recent empirical evidence on the importance of reallocation within firms. For example, Andrew B. Bernard, Stephen J. Redding, and Peter K. Schott (2006) find that one third of the net increase in real U.S. manufacturing output between 1972 and 1997 is due to the net adding and dropping of products by surviving firms, a contribution that dwarfs that of net firm entry and exit. Clearly new firm formation is only one dimension of innovation and existing firms account for a large share of total research and development (R&D) in many industries such as Pharmaceuticals. As the authors themselves discuss, the spin-off of existing operations and the acquisition of independent start-ups are now important dimensions of new process and product development.

The delineation of the causal connections between entrepreneurship, innovation and growth raises formidable identification challenges that face any study in this area. For example, employment growth across LMAs is strongly positively correlated with firm formation, but this does not necessarily imply that entrepreneurship causes growth. There may be third factors that cause employment growth and firm formation to covary, and it is hard to find instruments that affect firm formation but have no independent effect on employment growth. The interpretation of the correlation between employment growth and firm formation relates to old debates in economic geography about whether workers follow firms, firms follow workers, or there are mutually reinforcing feedbacks between firms' and workers' locations decisions. It is noticeable that many of the high rates of firm formation in the map on the book's cover are observed in the West and Southeast of the United States, consistent with a general reorientation of economic activity toward these regions. Since many areas within these regions enjoy favorable climates, as emphasized in Jordan Rappaport (forthcoming), it is at least possible that the differences in firm formation are partly endogenous to a relocation of population that is occurring for other reasons.

In the final chapter of the book, the authors discuss the foundations of entrepreneurial policy and distinguish four broad actors: (a) individual agents who identify business opportunities and choose to exploit them, (b) newly formed businesses which innovate using new knowledge and other resources, (c) the economy including all institutions that influence economic growth, and (d) society as the collection of all agents who are the ultimate beneficiaries of wealth creation. Within this organizing framework, several policy instruments are proposed to promote entrepreneurship. While several interesting suggestions are made, the market failures that rationalize intervention are not always clear, nor is the way in which these market failures could be quantified. In contrast to some of the specific policy instruments considered, the most important dimension of public policy may well be the regulatory framework that shapes the overall business climate.

One of the most interesting theses in the book is the claim that the success of American capitalism can be understood in terms of a nexus between the creation of wealth (entrepreneurship) and the reconstitution of wealth (philanthropy). Philanthropy is viewed as being important because it creates new opportunities for future generations of entrepreneurs, which is crucial because new firm formation is so closely identified with entrepreneurship. But philanthropy is surely only one of several factors that are important in enabling new profit opportunities to be exploited and wealth to be created. The institutional environment including the protection of property rights, the legal system, entry barriers and labor regulation all surely have roles to play. The quantification of these factors, both within and across countries, is one of the most exciting contributions of the growing literature on the new institutional economics.

Taken together, this book is the source for a wealth of ideas and empirical facts that will be of great interest to researchers concerned with economic growth and spatial inequality in economic development. In emphasizing entrepreneurship, the authors are drawing attention to a woefully neglected area of theoretical and empirical economics and pointing the way to several interesting areas for further research.

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Z Other Special Topics

The Economy of Prestige: Prizes, Awards, and the Circulation of Cultural Value. By James F. English. Cambridge and London: Harvard University Press, 2005. Pp. xii, 409. $29.95. ISBN 0–674–01884–2. *JEL 2006–0403*

In 1993, the British sculptor Rachel Whiteread won the $20,000 Turner Prize, often referred to as the “Booker Prize for artists.” This accomplishment might have only been noticed in very select circles had it not been for the fact that on the same day she accepted, albeit reluctantly, the K Foundation Prize for Worst British Artist, which took the form of £40,000 nailed to a gilt-framed board that would be publicly burned if rejected. Given that so much of the Turner Prize’s legitimacy was derived from the monetary value of the prize, the sarcastic K Foundation Prize could not be ignored, and the media attention associated with simultaneously being named the most and least worthy artist in Britain caused her work to be much more widely noticed than it would otherwise have been.

In this fascinating book, James F. English, Professor of English at the University of Pennsylvania, deftly paints a portrait of the current state of play in the “economy of cultural value.” While describing the history of cultural prizes, he offers a compelling explanation for the economic forces that have led to their extraordinary proliferation over the last several decades—the circumstances that could result in a controversial artist receiving more recognition for winning a prestigious high-purse prize because she was simultaneously humiliated by another.

English notes that, while royal families have awarded cultural prizes for centuries, the modern global cultural prize emerged at the end of the nineteenth century, most notably with the founding of the festival now known as the Venice Biennale in 1895. It is no accident that this coincided with Alfred Nobel’s will establishing the Nobel Prizes and the first modern international Olympics in 1896. Instead, it marked the formalization of a notion of the arts as international sport, in which the cultural accomplishments of some societies could be pitted directly against those of others. And the competition, of course, was not just on the “playing field.” Just as cities competed to host the Olympic Games as a way of cementing their relative global prestige, so too did cities (and benefactors) clamor to establish cultural awards and competitions.

But while sporting competitions (the emergence of international football also occurred at virtually the same time) have well-defined rules and relatively observable outcomes, cultural competitions are by their very nature subjective, and the work of determining the most meritorious artists and writers falls into the hands of judges, each of whom may have their own agendas and none of whom are particularly well-compensated for the task. (Of course, as any overworked reviewer can attest, these circumstances are not limited to the arts and literature.) And like all other subjective decisions, the awarding of these prizes invites controversy. The very first award in 1901 of the Nobel Prize in Literature, to Sully Prudhomme over, for instance, Leo Tolstoy, sparked immediate controversy, and was exacerbated by the Swedish Academy’s subsequent omission of Tolstoy in each of the nine other years in which he was alive—a move potentially intended to thumb its nose at its critics. Tolstoy is not alone: The list of other authors of works of eminent, enduring value (e.g., Hardy, Ibsen, Joyce, Kafka, Proust, and Rilke) who were later snubbed the Nobel Prize is staggering, and Tolstoy’s omission shows that the controversy over the prize’s legitimacy was already firmly ensconced at the prize’s inception.

English argues that these glaring omissions are actually a necessary component of the cultural prize—that contempt for the awards system itself is founded on a belief that culture cannot be commodified. "This threat of scandal," according to English, "is constitutive of the cultural prize." Historically, indeed, some of the most distinguished would-be recipients of cultural prizes refused to accept them. Jean-Paul Sartre, who assiduously refused all awards offered to him, even famously rejected the Nobel Prize for Literature in 1964. Sartre recognized that the value to him, in terms of the perceived importance of his work, of refusing the Nobel Prize far outstripped the benefits that would accompany accepting the prize. The Swedish Academy also recognized this, English intimates, and knew that to proffer a prize to an awardee who had previously publicly refused all other prizes would be "in effect a Trojan horse."

English suggests that the rules of the game appear to be changing, and points to Thomas Pynchon's near-simultaneous acceptance of a National Book Award while snubbing the American Academy of Arts and Letters as a pivotal moment. By 1989, Pynchon was more than happy to accept a MacArthur "Genius" Grant. And around the same time, after she had been passed over for several prestigious prizes, Toni Morrison's supporters openly and actively lobbied for her to win the Pulitzer Prize, which she did, for "Beloved." Shortly thereafter she won the Nobel Prize as well. English suggests that by breaking with the customary public disdain for prizes, Morrison's supporters may actually have helped to usher in the demise of their importance.

The rapid expansion in the number of prizes and awards has placed considerable burden on organizations and individuals. The number of submissions for any given prize award is such that it is nearly impossible for judges to devote enough time to give all submissions sufficient attention to make a decision—if they even see many of the submissions at all. One consequence of the massive proliferation of prizes is, ironically, the concentration of awards. English provides numerous examples of how the major cultural awards frequently arrive at the same winner. Hence, Steven Spielberg has won 90 awards, as compared with 21 by Alfred Hitchcock, and "Lord of the Rings: Return of the King" won (at least) 79 awards, compared with 3 for "Casablanca." Frank Gehry's total at the time of writing was over 130, while Michael Jackson's exceeded 240. The awardees of MacArthur "Genius" grants, intended to provide start-up capital for talented rising stars, tend to be some of the most established (and best-funded) artists, writers, and scholars in the business.

Meanwhile, as the monetary value of the actual prize award is trivial compared with the costs associated with administering, awarding and marketing the prize (even for the very large purses that command instant respect), the "endowment" of prizes by individuals imposes significant externalities on the organizations "gifted" with prize administration—and, of course, the individuals responsible for making this work. Even the Nobel Prize's endowment imposed these externalities: The Swedish Academy's raison d'etre was "to defend the purity, vigor, and majesty of the Swedish language." But the Nobel did more than just increase the Academy's workload. In accepting responsibility for the Nobel Prize, the Academy "transform[ed] its cultural place and purpose, overriding the intentions of its illustrious founder, King Gustav III, with those of the middle-class engineer and munitions manufacturer Alfred Nobel."

The Economy of Prestige deftly describes the rationale for the cultural prize and the important role of controversy in providing currency for the market of cultural production. Missing, however, from the analysis is some sense of a counterfactual: What would have happened in markets of ideas that possessed different circumstances? Are there situations that English would suggest to lead to a greater proliferation of prizes, or a greater or lesser degree either of controversy or of concentration of awards? Awards in the arts and literature are particularly susceptible to being co-opted for political agendas; does this facilitate or counteract the phenomena that English describes? And what about disciplines whose work output is more easily quantifiable—or commodifiable? Such an enterprise would geometrically increase English's task, but it would provide greater insight into the conditions under which award expansions, seemingly counterintuitive behaviors, and "winner-take-all" markets would emerge and flourish.

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