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Book Reviews

Guardians of Finance: Making Regulators Work for Us, by James R. Barth, Gerard Caprio, Jr. and Ross Levine. Cambridge: The MIT Press, 2012. Hardcover: ISBN 978-0-262-01739-8, $27.95, 280 pages.

Financial regulators (the "Guardians of Finance"), through their actions and inactions, greatly increased the fragility of the financial system prior to (what is now known as) the Great Recession of 2008. This book focuses on this specific aspect of the crisis. While not the only cause of the Great Recession, regulatory failure — namely, deregulatory mania and the unwillingness to enforce existing regulations — was central. The authors note that this cause is common to all the countries that had a major crisis, from Iceland and Ireland, to the UK, Spain, and the United States. They argue for a more open regulatory framework that has greater checks and balances in order to make sure that regulators work in the interest of the public. Overall, their argument is persuasive and the book is written in a style that is accessible to a non-academic audience conversant with financial issues.

There are several reasons why regulatory failure occurred. The authors emphasize the role of psychology, incentives, politics, and ideology. In terms of psychology, regulators who spend a lot of time with financial businessmen tend to have, consciously or subconsciously, a favorable view of financial institutions. This is all the more so for regulators who have an office within the financial institutions they regulate. In terms of incentives, there are revolving doors between regulated and regulators. If regulators do their job in a way that pleases the regulated, they can get very lucrative jobs once they leave their regulatory position. In addition, people from private financial institutions tend to populate regulatory agencies and then return to the private financial sector. In terms of politics, financial institutions have among the largest number of lobbyists in Washington, D.C., and they are able to influence legislations in their favor. Finally, in terms of ideology, over the past thirty years, the persons who have been promoted to the top regulatory positions have been free-marketers — Greenspan and Cox are two famous examples. Their goal has been to decrease regulations, wherever and whenever possible, under the adage "the less regulation, the better."

The authors also note that market-regulation failed because market incentives were changed in such a way as to promote financial fragility instead of a sound financial system. The move of financial institutions from partnerships to public companies decreased the incentive of managers to monitor each other. While the growth of volume-based lending and rating decreased the quality of underwriting, securitization removed the incentive for banks to check creditworthiness. CEO compensation was based on short-term goals (rising stock prices, rapid business growth) that gave CEOs the incentive to take big risks on the asset and liability sides of their balance sheets. Risk analysts either used inappropriate models or were ignored if their views conflicted with the CEO's goals.

Given the existence of market imperfections and the possibility that regulators may not act in the interest of the public, there is a need for better governance, accountability, and transparency of the regulatory framework. Regulators should not be given greater powers if that does not occur. Unfortunately, over the past century, the trend has been to give more and more powers to regulators without increasing public oversight. The Dodd-Frank Act continues this trend by increasing the power of the Fed, and by creating the Financial Stability Oversight Council (FSOC) that is allowed to supervise non-bank financial institutions such as hedge funds. The authors do not believe that the Dodd-Frank Act will promote a safer financial system and note that FSOC regroups together regulators who were not able, or willing, to intervene despite growing evidence of financial fragility. The Basel III Accords also will not fare better given existing market failures and incentives in the financial industry.

There is a need for an independent public institution that can influence the regulatory debates in the interest of the public. The authors call this institution the "Sentinel." It should be politically autonomous (independent budget and not housed in Treasury, Fed, or any other exiting government institution), be independent from the financial sector (members are prohibited from receiving fees from financial services or lobbyists for an extended period of time after leaving the Sentinel), have the power to obtain all information it needs from regulators, and pay market-based salaries to be able to attract the best and brightest individuals from all backgrounds (lawyers, economists, and accountants, among others). The Sentinel would write an annual report to Congress and the President about the existing regulatory practices. The goal would be to influence regulation, supervision, and enforcement in the interest of the public. The authors are aware that the Sentinel may not be always successful in influencing regulatory discussions in favor of the public interest, but it should help to do so when today no such institution exists. An institution like the Government Accountability Office (GAO) does play some of this role but it covers a wide range of subjects, does not have an independent budget, and does not pay market-based salaries. The Sentinel, by comparison, would be exclusively focused on financial regulation, among other things.

The Sentinel is an interesting idea that is worth pursuing. I am not as skeptical as the authors about the capacity to find individuals who believe in government regulation and are willing to work in the public interest even if it is a career limiting gesture ("angels" as they are called in the book). There are plenty of examples of such people in the recent crisis (e.g., Froeba at Moody's, Lee at Lehman, some staff at the Fed), although they were all fired, demoted, or ignored during the boom. But William K. Black's book, *The Best Way to Rob a Bank is to Own One,* provides a detailed account of what happens when such individuals persist and prevail, despite the massive opposition to their success and the knowledge that there is no future for them in the financial industry. The point is to put those people in charge and the Sentinel can help accomplish that.

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Understanding Long-Run Economic Growth: Geography, Institutions, and the Knowledge Economy, edited by Dora L. Costa and Naomi R. Lamoreaux. Chicago: University of Chicago Press, 2011. Hardcover: ISBN 978-0-226-11634-1, $110.00, 390 pages.

This edited volume is the result of a National Bureau of Economic Research (NBER) conference honoring the late University of California-Los Angeles economic historian Kenneth Sokoloff. A student of Robert Fogel's at Harvard, Sokoloff devoted his career to studying long-run economic growth through the lens of economic history. He is perhaps best known for his joint work with Stanley Engerman on the role of factor endowments in shaping economic and political institutions in the Americas. However, he also did important work on the patent institutions and innovation. Unlike some honorary volumes, in which the articles only touch upon the analyses of the economists being honored, this volume's contributions, edited by Dora L. Costa and Naomi R. Lamoreaux, directly engage with the central themes of Sokoloff's work.

Although not explicitly organized in this manner, the thirteen articles in the volume may be viewed as pertaining to three general sections. The first seven papers deal directly with the role of geography in economic development, with their focus varying from the frontier's role in determining current institutions, to paths to financial development, to the mortgage loan market in nineteenth-century France. While seemingly treating different topics, all chapters in the volume adopt the approach Kenneth Sokoloff uses throughout his work. They begin with a carefully chosen set of countries or regions, proceed to identify differences in factor endowments, and go on to analyze how these differences influence institutions and long-term development.

For those interested in a direct look at Sokoloff's approach, they need not go any farther than the first chapter, co-authored by Kenneth Sokoloff and Stanley Engerman. They look at the role factor endowments played in land policies, inequality, and institutions in the Americas. Their argument is that the supply of labor was related to the distribution of land. Only in areas where labor was scarce — like British North America — did elites have to make land holdings more broadly available in order to induce migration. Conversely, in the American colonies of Spain, the availability of labor led to the entrenchment of inequality in land holdings and wealth. This inequality influenced the distribution of political power, and thereby the long-term development of political and economic institutions.

The second section comprises three chapters that focus on innovation and human capital, topics at the core of Sokoloff's work. In particular, the article by

Claudia Goldin and Lawrence Katz on the role of compulsory schooling and child labor laws in the U.S. high school movement is an excellent example of the type of empirical work that more economists should do. Empirical work is not just regression analysis. It should also include getting one's hands dirty in measuring and quantifying important features of society. Goldin and Katz clearly do that by updating and refining a data set of state-level compulsory education and child-labor laws for the period 1910–1939. The data appendix they provide (pp. 304-307) could serves as a model of how to be direct about the trade-offs that a scholar must make in coding data.

The third section includes four chapters, three of which present appreciation of and reflection upon Sokoloff's work by Joel Mokyr, Peter Lindert, and Manuel Trajtenberg. The fourth article is a chapter by Robert Fogel titled, "The Impact of the Asian Miracle on the Theory of Economic Growth." While certainly inspired by Sokoloff's work, Fogel's paper is largely a survey of economic growth theories, with some concluding speculations about the prospective growth paths of the European Union, the United States, and China. While Fogel's article is somewhat out of sync with the rest of the volume, it makes for a very provocative and illuminating read.

This volume should be in all research libraries. The authors are experts in their areas and their chapters either shine new light on important issues or succinctly summarize what is already known. More personally, this book reminded me of why I find economic history so exciting. First of all, economic historians tend to focus on really fundamental issues and this entire volume certainly deals with long-run growth, the most important question in economics. Second, while economic historians do not eschew statistical methods, they are more open to alternative types of evidence and this volume is filled with a variety of evidence ranging from historical narratives to data charts. Finally, economic historians relish bringing theory and data to bear in overturning conventional wisdom. The second chapter, which debunks the idea that having a frontier was crucial to the development of U.S. institutions, is an excellent example of this tendency in economic history.

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A Short History of Ethics and Economics – The Greeks, by James E. Alvey. Cheltenham: Edward Elgar, 2011. Hardcover: ISBN 978-1-84720-201-7, £58.50, 184 pages.

The book's cover features the sculptures of the four Greek philosophers, whose work the book discusses: Socrates, Xenophon, Aristotle, and Plato. This picture conveys the main message of the book: namely, that economics is not independent from philosophy, including ethics; not today, just like it was not in the past. More precisely, the book aims to trace two modern approaches of recognizing economics as a moral science back to ancient Greek thought. These two modern approaches are, first, the well-known capability approach, led by Amartya Sen, and, second, the much-less-known critical approach, represented by "Cropsey, Staveley and their Followers" (p. 8). The choice of these two approaches — in particular, with the imbalance in the extent of their development in economics — is not convincing to me. The capability approach has its own association and conferences, a journal, (even a Nobel Prize winner), and a policy dimension through the UN's annual Human Development Report. I am not aware of such an influence on the discipline of the other approach but, of course, that may be attributable to my biases or ignorance. More importantly, a selection of a set of modern approaches that recognize economics as a moral science is not necessary for the book's objective; and neither is the checklist provided at the end of chapter. Also without this, the book succeeds in demonstrating how the ancient Greeks connected ethics to their economic thinking, and how these connections have been lost to much of today's economics.

Generally, the content of the book displays an eloquent choice of relevant ancient texts and reveals important linkages between the ethics and economics in those writings. The summaries, discussions, and comparisons of the texts are well done, and they are enlightening and useful for any reader who is interested in the very early history of economic thought. The author rightly claims an important role for the Socratic view of the good as *eudaimonia* (which Aristotle would later interpret as virtue). *Eudaimonia* is human flourishing and, for Xenophon and Socrates, almost the opposite of pleasure, which is the foundation of neoclassical economics' concept of utility. The author makes this difference very clear: "Addiction to pleasure is a sort of enslavement that rules out virtuous action in many cases" (p. 34). The book also provides a careful contextualization of the political economy of the time period in which the texts were written. This helps the reader to interpret the texts. The book explains key concepts such as *oikos* (household, community) and *polis* (city with surrounding area). And it informs the reader that economic growth in those times was modest, loans were provided without interest, and the *polis* acted as a regulator in the food market with price controls and export bans. The author only very briefly recognizes the exclusion of women from much of political and economic life and decision-making, which I find unfortunate, given the extensive literature on women in ancient Greek society and philosophy, as well as Plato's progressive views on inclusion and equality.

Most of the book's attention is dedicated to Aristotle as compared to the remaining three philosophers. This is one reason, among others, why I will limit the remainder of my review to the three chapters on him. The author admits that he relies heavily on Martha Nussbaum's interpretations of Aristotle and justifies this by stating that Amartya Sen does so, too. Although I highly value Nussbaum's work and interpretation of Aristotle, I would have rather liked to see more pluralism in Aristotelian interpretations of economic thought, in particular, with more views from economists such as, for example, Deirdre McCloskey and Ricardo Crespo. James E. Alvey's discussion of Aristotle covers the key issues about virtue theory: Namely, that virtue is a mean between two extremes; that virtue is an end in itself; that human beings are social and moral beings; and that a good polis should enable the flourishing of its citizens. At the end of the last chapter on Aristotle, Alvey rightly concludes that Aristotle is far less optimistic — as compared to Sen and Nussbaum — that the polis could and should have guaranteed everyone's flourishing. This conclusion is, of course, understandable because in our times we do not exclude women and the working class from basic human and economic rights. Also, our opportunities to generate wellbeing for all are much better today than two millennia ago. Here, the capability approach clearly departs from the patriarchal and elitist perspective that Aristotle took, despite the more inclusive view of society of his teacher Plato.

The book's conclusion is not novel: Namely, that "the value-free rhetoric of modern economics must be abandoned." James E. Alvey has demonstrated that two millennia ago, economics and ethics were understood as intertwined. I agree with the historical view and I find Alvey's overview of ancient Greek thought to be helpful in showing *how* this was done in the past. I doubt, however, that this will help in overturning the current practice of positive economics. A historical overview showing that it *can* be done differently will not convince those who strongly believe in the possibility and the goodness of value-free science, that it also *must* be done.

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Financial Structures and Regulation: A Comparison of Crises in the UK, USA and Italy, by Alessandro Roselli. London: Palgrave Macmillan Studies in Banking and Financial Institutions, 2012. Hardback: ISBN 978-1-403-94872-4, $100.00, 304 pages.

This book is both historical and institutional and readers of this journal will appreciate the numerous references to the work of Hyman Minsky on financial instability. News of the LIBOR scandal in London, coming on the heels of the bailout of large banks and other financial institutions in the US, reminds us that problems remain with the structure of global banking. This book addresses a fundamental question that policy makers should ask: How can we avoid future taxpayer's bailouts of financial firms? Alessandro Roselli's proposed solution looks to history for answers. He asks: Why banks have been considered "special" historically (i.e., deserving of federal deposit insurance and access to the Federal Reserve's credit window)? And if they still are "special," why are banks permitted to engage in activities that go beyond their historical function and that result in colossal bailouts? The author is well-qualified to make pronouncements on this topic because he has served as a liaison of the Bank of Italy to the Federal Reserve and, more recently, to the Bank of England.

The author traces the history of banking in the US, the UK, and Italy beginning in the early twentieth century. He divides the period into three parts: the interwar years (1920–1940), the post-World War II period (1950–1980), and the most recent decades (since 1980). Roselli uses two broad measures of financial depth to analyze the evolution of the financial systems in the three countries. The first measure is the ratio of financial institution assets to total financial assets (FIN) while the second is the ratio of total financial assets to real wealth (FIR). Although these measures are not exact, the author's purpose is to provide a "point of reference and as a means of checking our remarks on interrelations between institutions and financial structures" (p. 5).

The book discusses important legislation, institutional changes, and financial innovations in each of the three countries. The differences existing between the US, the UK, and Italy are important to note. Some of them pertain to insurance, regulatory mechanisms, and banking-sector fluctuations. For example, although the US adopted federal deposit insurance in 1933, in the UK it did not happen until 1979, and in Italy – until 1986. In the UK, there is a single regulator, unlike in the US with its myriad of state and federal regulatory agencies. The three countries have also had different experiences within the context of the recent financial crisis, whereby Italy's banking sector has remained relatively stable. Although Italy is not without its problems, the author notes, the Italians have been more conservative in mortgage lending than their Anglo-Saxon counterparts, which partially explains their relative financial stability.

A straightforward definition of a bank is an institution that plays a role in the payments system by offering the public demand deposits as a convenient form of money, while also providing credit, primarily to informationally opaque business firms. Banks want to be thought of as "special" in order to have access, among other things, to the federal safety net provided by such agencies as the FDIC and the Federal Reserve. The problem is that bankers want it both ways: to be "special" *and* to be treated like other firms. This is precisely the reason why banks get into trouble and have to be bailed out with taxpayer's money: Because they argue — and quite successfully, it seems — that in order to meet the competition of financial nonbank firms in providing payments and lending services, they need to be allowed to engage in a broad range of activities and to innovate in financial instruments as they deem necessary. Consequently, if the reoccurring cycle of staggering bailouts is to ever end, it must be decided if banks are really "special." As Roselli effectively notes, it is the bankers themselves, through their use of financial innovations like securitization, that have brought the banks' "special" status into question. One can view securitization as a way for banks to make liquid their non-liquid loans (home mortgages, among others), but, in so doing, the banks substantially depart from their traditional role of loan monitor.

Is it time for banks to go the way of the Bell land phone monopoly in the USA? The solution though not perfect, that Alessandro Roselli suggests is introducing some form of "narrow banking" restricting banks to their historically traditional "special" function of payments-and-credit service. The author favors a tripartite structure in which "narrow" banks would exist alongside redefined commercial banks and investment banks. Although economists from both the left and right have advocated versions of this proposal, it has not gained political traction in the USA yet. However, in the UK, the Vickers Report, which proposes separating retail and investment banking ("ring-fencing"), has been endorsed by the current Conservative government. Moreover, the Labour Party has pledged to bring a separation of retail and wholesale banking if they return to power with the next elections.

For the reasons discussed above, among others, Roselli's is a thought-provoking book. Those interested in understanding the evolution of the global financial system, and possible ways to reform it, will garner great insights from reading it.

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International Handbook on the Economics of Corruption, Volume Two, edited by Susan Rose-Ackerman and Tina Søreide. Northampton, MA: Edward Elgar, 2011. Hardcover: ISBN 978-1-84980-251-2, £148.50, 624 pages.

Volume Two of the *International Handbook of the Economics of Corruption*, edited by Susan Rose-Ackerman and Tina Søreide, presents a comprehensive, detailed, and in-depth analysis of corruption as well as its economic and policy implications. It is surprising, then, that the volume does not feature any pieces by Rose-Ackerman herself, who is a world-renowned expert on the political economy of corruption. Nevertheless, the book presents a palette rich in topic variety and author diversity. The authors not only represent different countries, academic institutions, and think tanks, but they also offer sound policy-oriented research in the interdisciplinary field of corruption studies. They stress the institutional roots of corruption and include new research on topics that range from corruption in governmental regulation and procurement in large-scale public projects to political power, buying votes of impoverished electorates, and private firm payoffs in the form of bribes and kickbacks to state bureaucrats. In this volume, one finds a fine blend of conceptual framing, regression analysis, survey results, and policy implications. All of these components demonstrate a variety of approaches to studying the problem of corruption.

Another feature of the book that adds to its multidimensional character is that the studies, presented in each chapter, focus on different countries representing diverse cultures. Such heavy-weights as Brazil, India, Nigeria, and the USA are beneficially supplemented with smaller and more exotic nations such as Sao Tome and Principe. This makes the volume truly international. Moreover, the strong focus on variety only underscores the importance of corruption and the context in which it occurs. The editors rightly state that "one must study corruption in context. Understanding the consequences of corrupt transactions requires one to know what is being bought with a bribe and how the behavior of public and private actors has been affected" (p. xiv).

The book poses a number of questions, among the most essential of which are: "What will happen if corruption is reduced? Will other forms of favoritism and self-dealing of marginal legality substitute for outright payoffs and kickbacks, or will the result be an honest and well-functioning system? Is corruption the side effect of a larger problem of monopolistic behavior or entrenched organized crime influence?" (p. xiv). The answers to these questions, found throughout the volume, are rich in approach and scope. Unlike other handbooks, typically general in their approach, the work edited by Rose-Ackerman and Søreide employs a slew of elaborate research methodologies that make it a real scholarly treat. It is good to see a handbook that does not replace the notion of economics with case studies and/or policy recommendations, as usually happens.

Along with such standard topics as corruption in procurement, public projects, and public-versus-private governance and performance, this handbook also addresses such new issues as corruption in education. Thus, Pedro Vicente, in Chapter 12, provides an example of favoritism in the allocation of educational scholarships, linking it to oil and vote-buying in Sao Tome and Principe. He notes that "when asked about the importance of different criteria in deciding on the allocation of the scholarships for studying abroad, households were resolute in stating 'personal connections' as the most important factor" (p. 365). In his study, the author observes "a pattern of time change in corruption perceptions that closely matches that of the objective measurements" (p. 368). Unfortunately, however, he makes no use of (or reference to) the scholarly literature on corruption in higher education that has developed over the last years.

One may find it surprising that Russia is virtually absent from the analyses presented in the handbook. Russia should be of greater interest to researchers, with its high levels of petty as well as large-scale corruption, stemming particularly from the mass privatization undertakings of the 1990s and 2000s. Nevertheless, some authors analyze Russia in this volume. For instance, Emmanuelle Auriol and Stephane Straub, conclude in their chapter (ch.7) on privatization of rent-generating industries and corruption that "[p]rivatization per se does not lead to corruption. Corruption generally exists long before privatizations programs are implemented. An important research and policy question is thus to understand the consequences of pre-existing corruption on privatization outcomes, and symmetrically, the impact of privatizations on corruption levels" (p. 266). Yet, further references to Russia, especially its experience with corruption in the oil and gas industries, would certainly expand the explanation of corruption.

Authors apply the concept of rent-seeking behavior when studying the conduct of corrupted bureaucrats. To address the issue of rent conceptually and theoretically, one may find it beneficial to incorporate the works of Karl Marx, particularly the volumes of *Das Kapital* (i.e., volume IV) which contain the notions of absolute rent and differential rent of two degrees. It would be interesting and fruitful to link different forms of corruption and rent-oriented behavior to the Marxian concept of rent.

Overall, while the volume addresses such standard issues as good governance, trust, public projects, infrastructure, procurement, and corrupt bureaucrats, it leaves out some more interesting and less developed corruption issues, including corruption in healthcare, higher education, the justice system, and law enforcement. The book is written in an accessible language and is well-illustrated with figures, graphs, organizational charts, and tables. It will be a valuable resource not only for experts and students of corruption studies, but also for public officials, NGO employees, and scholars of economic and political development throughout the world.

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Latin American Economic Development, by Javier A. Reyes and W. Charles Sawyer. London: Routledge, 2011. Paperback: 978-0-415-49733-6, $59.95. Hardback: 978-0-415-48613-2, $140.00, 338 pages.

In their book, Javier Reyes and Charles Sawyer stress that Latin America covers a vast area and includes a population of over half a billion people. Therefore, they say, offering a general explanation that accounts for the variables contributing to and/or constraining economic growth proves difficult. While so lamenting, the authors also seek to explain the relatively slow growth rates in per-capita GDP. "Sadly," they conclude, "there is no simple or certain answer" (p. 13). The authors, however, do note common features that render the vast region of Central and South America — including Mexico, north of the Isthmus of Tehuantepec — homogenous enough to be explained by some general principles. This focus provides the foundation for their book.

Integral to Reyes and Sawyer's work is the effort to avoid simple or certain answers. They deploy "new institutional economics" to explain the major constraints limiting Latin America's economic growth and development in the last five centuries. Expectedly, the authors emphasize "property rights" and the "rule of law." Their Table 2.1 reports 2008 data from the World Economic Forum regarding property rights. Undaunted by incomplete data, the authors note that "[w]hile research on the effect of property rights on economic growth in Latin America is limited, it is safe to assume that the greater the extent to which property rights are enforced, the more economic growth would be enhanced" (p. 25).

Table 2.3 offers data supporting the authors' "rule of law" argument. Scores run a wide gamut: from Chile's high of 88 percent to Venezuela's 3.3 percent. This leads to the conclusion that Latin America's average of 33.9 percent is well below that of Canada and the USA, with 96.2 and 91.9 percent respectively. This averaged data ostensibly gives credence to the authors' claim that the "rule of law" is important in affecting economic outcomes, while simultaneously validating their reliance upon explanatory powers associated with new institutional economics.

Reyes and Sawyer introduce theoretical approaches to economic growth in their book. They first consider the basic Solow growth model, stressing the importance of labor and capital that derive from domestic and foreign sources. Later, in Chapter 2, they introduce a new growth theory that relates technological changes to advances in knowledge, and especially germane to research and development.

When measuring per-capita GDP growth in Latin America, the authors note that while growth does indeed take place, it tends to run at lower annual rates than in some other parts of the world. Comparative references relate Latin America's sluggish growth to the United States and Canada, as well as to some East-Asian countries. One of the ongoing problems is that, from 1960 to 1999, one finds negative growth rates in total factor productivity (Table 2.10), which the authors relate to problems with institutions. Their understanding of institutions seems to run parallel to that of Douglass North: Namely, poorly functioning institutions impede economic growth, whereas well-functioning institutions foster economic growth.

I agree with Reyes and Sawyer that for a vast region with an average middle-income status, Latin America exhibits comparatively slow growth rates in per-capita output. However, I remain unconvinced by the explanatory capability of their scientific approach. It seems to me that it has become fashionable in new institutional economics to advocate that clearly defined "property rights" and "rule of law" are essential to economic growth and development. But what about the vigorous growth rates exhibited by China's economy since the end of the 1970s? China serves as a clear example that per-capita output could also grow rapidly in an economy with an ambiguous understanding and implementation of "property rights." In sum, I find the authors' neo-institutional assertions fully in line with what has become acceptable in the literature in recent years, but what, at the same time, fails to gain support in terms of data and examples.

Reyes and Sawyer's rely heavily on key ideas advanced and/or endorsed by Ronald Coase and Douglass North, while neglecting other concepts and thinkers, including David Landes's analysis in *The Wealth and Poverty of Nations* (1998). Landes considers a variable, such as Max Weber's notion of northern Europe's "protestant ethic," as a relevant non-measurable factor for generating measurable economic outcomes. In this light, the authors' *Latin American Economic Development* fails to emphasize that Latin America's immigrants, by and large, derive from Europe's southern and catholic regions. Might the dominant faith and related culture generate measurable economic outcomes that we need to consider?

Furthermore, Reyes and Sawyer fail to consider Cold-War power struggles, generating effects on economic development. They neglect to observe that strategic U.S. interests became a powerful force following World War II, which included the selective opening of U.S. markets to absorb portions of the output coming from a retooled Japan and West Germany. Similarly, the United States' policy clearly enhanced per-capita growth in South Korea and Taiwan through technology-transfer and export-promotion efforts in the 1960s and 1970s. By contrast, during the post-war decades, Latin American states received comparatively limited technology-transfer benefits, or access to U.S. markets. The US-inspired "Operation Condor," running parallel to the U.S. aid programs in other countries, failed to include an economic development program as part of an anti-socialist buyout, relying instead on brute force to suppress progressive social movements in Latin America.

Despite these criticisms, this book is nicely composed. The prose and arguments are easy to follow for they are both straightforward and clearly presented. While the book largely assumes that readers possess a background in the principles of economics, the authors still do a fine job introducing the fundaments of economics. Moreover, the authors recurrently emphasize such conceptions as the economics of commodity extraction, production, and stability in export-prices, which prove important for understanding some of the challenges Latin American economies face.

This book would serve well in any undergraduate or graduate-level course which focuses on Latin America's economic development from an international-studies perspective. The authors offer many take-off points for additional lecturing on a host of topics in economics. These could certainly include the challenging ideas advanced by proponents of new institutional economics.

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What Makes Poor Countries Poor? Institutional Determinants of Development, by Michael J. Trebilcock and Mariana Mota Prado. Northampton, MA: Edward Elgar, 2011. Paper: ISBN 978-0-85793-891-6, $35.00, 286 pages.

Michael J. Trebilcock and Mariana Mota Prado offer an exhaustive overview of research in a number of specific areas concerning the relation between formal rules in and the economic performance of a country. They select seven areas of analysis and dedicate a chapter to each of them. The seven chapters deal with such topical issues as the rule of law, property and contract rights, political regimes and ethnic conflicts, public administration and corruption, state-owned enterprises and privatization, international trade and foreign direct investment, and foreign aid. The common thread running through all of these issues is their relationship to development outcomes. In other words, they explore how formal rules — and their modification — impact the development process.

In their analysis, the authors take a balanced approach to all issues under discussion. They repeatedly point out the fact that, in many studies, integration of the informal aspects of the institutional sphere is missing, and that such integration would have contributed to a more careful assessment of the impact of changes on specific formal rules. The authors likewise stress the context-specific aspect of any dynamics that may have resulted from the changes in formal rules. Based on the findings they present, Trebilcock and Mota Prado assert that extreme positions held by different camps of scholars (e.g., best-practice-has-to-be-the-guide vs. best-practice-never-works) are generally not defensible. Rather, they contend, a middle ground provides the most solid foundation for addressing issues germane to the structuring of a society's institutional framework with a view to furthering development objectives.

As the authors argue, the impact of changes depends on the general environment and circumstances in a given society. Drawing on another country's experiences to generate development can help as long as the host society is not too different from the experience-originating one. However, the authors do not elaborate on what happens when countries are too variegated to serve as examples for each other or on how to make borrowed models effective across institutionally and historically different societies.

What would have enhanced the framework, in which Trebilcock and Mota Prado discuss their findings, is the explicit integration of concepts from the original institutional economics (OIE). Such an approach, unfortunately, is largely missing in the book, with one exception being the discussion of Amartya Sen's ideas in the introduction (under the heading of cultural theories of development). Instead, the authors present an institutional approach to development in the introductory chapter, focusing exclusively on the new institutional economics (NIE). As a result, there is no coherent theoretical framework on which to base the discussion. This perhaps may not be a flaw so long as the authors do not try to force results into one of the frameworks they address — and they avoid so doing. In fact, they take the opportunity to delineate some problems that would have occurred had they tried to do so. Yet, it would have been easier to grasp some of the issues left open in the book if these issues were related to a framework that was generated by a process-based theory and that defined development as a process of continuous change. Such framework is indeed offered by the OIE.

At this point, however, the authors have to be content with pointing to the problems as well as to the importance of integrating additional dimensions and theoretical concepts into the development discourse. The focus they stress as crucial to advancing an institutional "agenda for development" is practically useful, helps identify how dysfunctional institutions develop, and allows for the formulation of a detailed strategy for institutional change. Of course, the OIE already provides a framework for achieving these aims even if the authors do not make use of it. Still, Trebilcock and Mota Prado's exhaustive analytical outline points to the existence of loose ends that other scholars could pursue. Ultimately, the book offers a good overview of a number of important issues currently under investigation, and points to some avenues still open to fruitful research.

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