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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

The Oxford Handbook of Professional Economic Ethics. Edited by George F. DeMartino and Dierdre N. McCloskey. Oxford and New York: Oxford University Press, 2016. Pp. xxii, 777. $150.00. ISBN 978–0–19–976663–5, cloth. *JEL 2016–1097*

The editors of this Handbook asked leading thinkers within economics and beyond to ruminate on those aspects of economic practice which they view as ethically fraught (p. 4). Their stated hope was to "generate the new field of professional economic ethics" (p. 4). The volume is long and contains thirty-seven chapters, with a few authors writing more than one chapter and many coauthored chapters.

As with all books with many chapters, writing a coherent review is challenging. It is perhaps even more difficult than usual for this volume because the authors' themes and examples cover a wide terrain, with many overlaps and some contradictions between chapters. But, given the objective of the volume, focus here is on the extent to which the handbook may lay claim to this new field. The conclusion must be negative. It constitutes a series of attacks on mainstream economics and the practice of economics, but does not offer a serious alternative.

One of the policy prescriptions accepted throughout much of the volume is "first, do no harm." But many of the policy prescriptions violate this mandate. Were an intelligent layman to read it (with little or no prior knowledge of economics), s/he would surely conclude that "mainstream economics" has nothing to offer and is downright harmful. And that is certainly not true, as even Joseph Stiglitz (an author on one of the thirty-seven essays) recognizes. The handbook is almost entirely about what is perceived to be wrong with "mainstream economics" and little about suggestions for improvement, save for calls for economists: (1) to indicate that they are uncertain of their prescriptions; (2) to set forth what alternative viewpoints might be; and (3) for a stronger code of ethics than that called for by the American Economic Association.[1] Many of the suggestions that are made are entirely impractical and would, in my judgment, make things worse. And while it is certainly true that economists' understanding is imperfect and will improve over time, it is not true that all of "mainstream economics" should be discredited nor that all policy prescriptions based on existing knowledge are fatally flawed.

There are a few helpful, constructive chapters. Chief among them are those focusing on the ethics of randomized control trials. Here, issues relating to both the extent to which subjects must be informed as to the purposes of the study and the ways in which information may be gathered and used are considered. There is also a chapter, by Martin Ravallion, urging more emphasis on ex post evaluation of the impact of development projects. Clearly evaluation is desirable, although even he does not indicate the criteria for how much additional effort should be made for evaluations and how it would be financed. Most funds for evaluation come from aid budgets. If some of those funds are to be used for additional evaluations, a question arises as to whether more evaluations will yield a sufficient return to compensate for the foregone projects that might otherwise be funded.

[1] Many of the authors were among those who urged the Executive Committee of the AEA to adopt a code of ethics. It did so. The code calls for authors of articles to disclose their sources of funding. Most of the comments on the code consider it to be too weak, as is discussed later.

Most of the volume, however, is much more negative. The extent of rejection of mainstream economics is indicated to some degree even from chapter titles. These include: "'Econogenic Harm': On the Nature of and Responsibility for the Harm Economists Do as They Try to Do Good," by George DeMartino; "Poisoning the Well: or How Economic Theory Damages Moral Imagination," by Julie Nelson; and "The Complex Ethical Consequences of 'Simple' Theoretical Choices," by Robert Frank, to name just a few.

But the condemnation of the mainstream is even stronger than that! The extent of rejection is almost breathtaking, usually with little or no evidence. David Ellerman, for example, writes of the "right church" of neoclassical economics (p. 531). He asserts that ". . . the prognosis is that the bulk of the mainstream economics profession and the major development institutions will continue to worship at the shrine of social engineering dressed in the garb of modern economics" (also p. 531). He bases his case largely on the contrast between the "gradual" approach of the Chinese and the "Bolshevik" methods of the Russian reformers (p. 523). Astonishingly, he cites treatment of property rights (in his view, gradual in China contrasted with "big bang" in Russia) as a major reason why gradualism in reform is to be preferred.[2] He neither acknowledges that there had been no prior history of dismantling a centrally planned economy, nor that there is still a lively debate as to the merits of gradualism versus speed.

Yet another example of this condemnation comes from Sharon Welch. She asserts that "professional economists have been very willing to design and implement grand policy solutions to pressing social problems without taking adequate account of the limits of their science and control over the world they seek to improve" (p. 56). She cites with approval a social action group that has three principal approaches: engagement, innovation, and impact (p. 56–7). Except that each step entails seeking to support "forgotten populations," it is not at all clear what the approaches are or what policy prescriptions are entailed.

[2] One might instead contrast Poland (whose reform was much more rapid) and Hungary and reach the opposite conclusion.

Julie Nelson launches her essay by listing some assumptions underlying the theory of competitive markets, and asserts that they induce people to behave more selfishly than they otherwise would. Her thesis is that "there is something ethically troubling about a profession that promotes such an economic theory." At no point, however, does she grapple with the proposition that in most circumstances, many people (not necessarily all) will respond to the incentives with which they are confronted, and that efforts to thwart the price mechanism, such as price controls and rationing, usually result in corruption and black markets.

The chapter by Tomas Sedlacek further exemplifies the extent of the distrust. "We economists . . . actually seem to believe in a basic triangle of three myths/beliefs—the mysterious *invisible hand* of the market, the . . . *Homo economicus* and the *animal spirits*" (p .232). Speaking for this economist, I do not think the three are the whole truth, although I would argue that in almost all circumstances: some (and usually enough) will respond to increased or diminished incentives so that incentives matter, and there almost always trade-offs and opportunity costs for activities undertaken or foregone.

A second major concern about most of the chapters is that allegations are seldom substantiated with documentation, and even when they are, the evidence is at best very partial, and usually anecdotal and unconvincing. Daly, for example, believes that growth is harmful: "The net destructive consequence of the current scale and growth of the economy . . . is greatly downplayed, if not totally ignored" (p. 179). He continues by citing the World Bank's 2008 Growth Report, anticipating a four- or five-fold increase in world economic output by 2050, and asserts (without evidence) that "The social and environmental costs of the Tower of Babel are already growing faster than the production benefits, making us poorer not richer" (p. 180).

It is highly unlikely that the populations of most countries in Africa, most of Asia outside Japan and the "four tigers," and other low per capita income countries would support a government committed to suppressing growth, but Daly offers no evidence whatsoever that the negatives outweigh the positives. He continues, "the elite-owned media, the corporate-funded think-tanks, the kept economists of high academia, and the World Bank—not to mention Gold [sic] Sacks [sic] and Wall Street—all sing hymns to growth in harmony with class interest and greed. The public is bamboozled by technical obfuscation, and by the false promise that, thanks to growth, they too will one day be rich" (p. 177). He concludes that people are getting poorer and not richer because of environmental degradation, without providing evidence or citing any source.

No documentation is provided to indicate that this is so. Robert Fogel, twenty years ago, documented that in 1900, 95 percent of Americans were living below the modern poverty line. One wonders how Daly would address that statistic.[3]

One theme, common across many papers, is the conviction that the Great Recession proves that neoclassical economics as a whole is a failure. Robert Wade, for example, cites a paper by Dirk Bezener, who we are told used four stringent criteria to ask whether the Great Recession had been predicted, and found only twelve economists who met them (forecast a severe crisis in more than just housing, described the mechanism for the severity, included a time period, and publicly gave a warning). He does not include those such as Rajan (2005) who, while chief economist at the IMF, issued such a warning at Jackson Hole (which was published). Nor does he note or consider Paul Samuelson's famous commentary on the prediction of downturns: "he predicted ten of the last three recessions." In several chapters, *Inside Job* is cited as proof that economists were paid by financial interests and, by implication, that this was morally wrong (p. xiv). Even when economists were paid, it does not prove that their consulting was in any sense dishonest or bought. But that is recognized only in one paper.

It is even suggested (de Martino, p. xiv) that "it is obvious" that economists supporting a political party have a conflict of interest and are dishonest

[3]The interested reader wanting more evidence of this rejection might consult Julie Nelson's assertion that assumptions of utility and profit maximization are having "hugely negative effects" on society (p. 185) and Boettke (p. 117), stating that the influence of special interests on research resulted in misleading ordinary people into "distorted ideas" that led to the adoption of policies that had "enormous costs to society." The policies he cites (one sentence only with no further elaboration) are "deregulation, privatization, and various free market policies."

in their advocacy in support of that party (when, among those I know, it is the other way around: economists support a party that advocates economic and other policies that make sense). Surely, de Martino should have been able at least to cite some pronouncements of economists prior to their political affiliations that differed from those they made once they were supporting policies within the party. And it is doubtful if there is anyone who agrees with each and every policy of any party.

Moreover, while even if we readily concede that failure to recognize the fault lines leading to the Great Recession was tragic, that does not prove either that "mainstream" economists have nothing to say (in the 1930s, the Great Depression was more severe and lasted longer, in significant part because there was less understanding than there is today; wildcat banking was a major destabilizing feature of the nineteenth century) or that all of "mainstream economics" is deficient. To be sure, economists' understanding of many phenomena is still imperfect (and as economies grow, their structure changes), but in much of the field, understanding has progressed. In light of their insistence that all viewpoints should be considered, one would have expected the authors to contrast the conclusions of mainstream macroeconomics with alternative theories before such a complete rejection.

The authors least critical of "mainstream economics" contend that we simply do not know enough to be confident. Some point out that there will always be those injured by policies advocated by economists. De Martino is among these. He points to a stylized Greek family "in 2015 whose economic security is undermined as a consequence of the severe austerity measures now in place . . . " and he says "many economists endorse" (p. 75) austerity. While many Greeks undoubtedly suffered serious consequences as a result of the Greek crisis, there is not even a hint as to what alternative policies might have been and whether they would have imposed more or less hardship.

Moreover, de Martino asserts that since any policy will generate some losers, economists should always study and know who they are or will be, and the welfare of the losers should always be considered. As I shall discuss later in the context of the treatment of cost–benefit analysis, it is not always true that there will be losers. But there are two more general points. The first is that deciding not to change a policy in itself IS a policy. Hence, at best, de Martino's prescriptions might be taken to mean that gainers and losers from an existing policy and its alternatives should always be compared. But that raises the second issue: among the indisputable insights of economic theory is the proposition that most policies have general-equilibrium effects and it is simply not possible to know the identities of all the winners and all the losers.

International trade is a perfect case in point. An increase in a tariff has multiple effects: it raises the price of the imported good in the domestic market, thereby inducing increased domestic production; it also reduces the quantity demanded of the good and the demand for foreign exchange. As such, the exchange rate appreciates. As that happens, the production of goods for export falls as domestic returns to exporters are reduced. Whether there are small reductions in export production across many goods or whether the production cutbacks are concentrated does not matter for my argument. The point is that the gainers (the additional workers in the import-competing firms and the owners of capital in those firms) can probably be readily identified; it is certain that there will be losers, but they may be widely diffused, not only on the consumption side but also on the production side. Indeed, at the time the tariff is raised it is impossible to identify which exporting firms will lose (and, by extension, which workers in exportable industries may lose their jobs). Few economists would quarrel with the straightforward theory I just set out. But now turn it around: let a tariff be reduced. To a degree, the gainers will be diffused and the losers concentrated. But if the exchange rate depreciates (because of increased demand for imports), exportable firms will expand production and new ones may emerge. To users of imported inputs, lowered costs may enable increased exports, and so on. While those gainers are more concentrated, they cannot be identified. Does that mean they should be considered less in any evaluation? How would one "consider and identify" all the gainers and losers before taking action? And if no action is an action, as it surely is, there is a policy decision not to act while study of the identities of gainers and losers is undertaken.

The rejection of mainstream economics leads to calls, throughout the volume, to consider "all viewpoints." Indeed, we are even told in one essay that economists have an ethical obligation to consider *all* alternative approaches to an economic problem. Sheila Dow, for example, urges a "pluralistic" and "wide range of approaches." In her view, all policy advocates should make clear not only what they advocate, but what would be advocated by those with different viewpoints and what their perspective would be on the policy being advocated.

One can just picture what might happen in a country with a seriously overvalued exchange rate, a large unsustainable current account deficit, high inflation, and rapidly diminishing foreign exchange reserves with no access to capital markets. If an economic adviser to the finance minister followed Dow's prescription, s/he would not only present a case for either floating the exchange rate or devaluation and tighter fiscal and monetary policy; the adviser would also need to present the cases for internal devaluation, dollarization, a currency board, and much more. Thinking of what the executive summary might look like defies the imagination! Moreover, there is strong empirical evidence that some of these policies simply do not work. One suspects the adviser would be dismissed. The finance minister would surely opt for advisers presenting a straightforward case. Moreover, the delay in policy changes would itself significantly increase the costs of reform (fewer foreign exchange reserves, or reduced production levels during uncertainty, for example).

There is also a serious and unanswered question as to how "all viewpoints" would be determined. Would they include Marxism, utopianism, communitarianism, socialism, the gold standard, mercantilism? What about ideas that emerge in letters to the editor that most economists would consider "crackpot"?

A third theme in many of the essays is the importance of noneconomic values and the uselessness of economic analysis when those values matter. Some of these arguments even demonstrate a lack of familiarity with the much-maligned "mainstream" economics. One such chapter (Des Gasper) focuses on cost–benefit analysis and what he regards as its uselessness in the face of environmental concerns. Yet finding the least costly way of attaining a given reduction in pollution is surely the domain of mainstream economics (while most would agree that political consensus should drive determination of the magnitude of the reduction). There is also an attack on cost–benefit analysis as "ethically deficient" for many cases, because it does not take into account the impacts on people with little or no ability to pay (p. 536). Yet Harberger (1978, 1984), among others, long ago presented a framework for weights to different income groups to resolve this problem.

Des Gaspar also focuses on developments in which people are displaced, and insists that the displaced should share in the benefits of the project, which is fair enough (although the criteria by which the share would be decided are not considered). Further, he asserts that adjudication and claims must be undertaken on an "ethically justified basis," and that those who will be displaced must fully participate in the process. No action should be undertaken on any further stage of the process until prior adjudication is completed (p. 542). One wonders how long the processes he advocates would require, and whether any project could ever proceed to completion.

Robert Nelson seems to demonstrate a similar lack of familiarity with "mainstream" economics. In a thoughtful chapter on the clash between environmentalism and economics, he concludes that economics can only help interest groups. The idea that there may be ways of achieving the same environmental objective (be it preservation of a species or reducing some emissions) in more and less costly ways does not seem to enter the analysis. While it is true that society must decide when there are trade-offs between economic and environmental goals, economists can surely contribute to understanding of the costs of different means of attaining these goals, carbon taxes being only one case in point.

Another example of this is the chapter by Irene van Staveren. In discussing the financial crisis, she asserts (among other things) that women are more risk averse than men and therefore, incredibly, that "in order to reduce increasing risk levels and market volatility in financial markets, a better gender balance on trading floors seems meaningful, both physically by replacing some male traders with female traders, and, chemically, by administering oxytocin to male traders when market volatility increases" (p. 262). That there may be distributions of risk adversity among both males and females, and that a meritocracy might take into account other attributes of all potential traders is not even mentioned. Nor is there any consideration of the desirable degree of risk adversity.

Despite my critical discussion to this point, there are thoughtful criticisms of economics and policy advice. However, that said, the manner in which the criticisms are framed (usually, as I noted, without documentation) is very likely to reduce the effectiveness and usefulness even there, and there is little constructive suggestion. De Martino is among those in the volume who agree that a first principle should be to "do no harm," or certainly to do no harm that can be avoided.

Yet on that principle, the volume fails miserably. It provides a weapon for any advocate (including the vested interests of which the authors are so contemptuous) of unconventional policies to fight against the policy positions of economic advisers. And not all advisers are mistaken. Sri Lanka, for example, for years had a program under which free rice was distributed to all. The intent, of course, was to provide sufficient food for the poor. Yet evidence mounted that much more than half the benefits of the program accrued to the upper half of the income distribution, while at the same time there was an unacceptably high rate of rice that spoiled before being distributed. Obviously, economists familiar with the program documented its effects and urged its replacement. Resistance came (couched in the language of helping the poor) from middlemen in the rice trade and others who were benefitting (and who cast their objections, of course, in the same terms as the authors of this volume—harming the poor, etc.). A similar program in Ecuador heavily subsidized the sale of cooking gas to rural residents. Much of that gas was obtained by smugglers before reaching the poor, then transported and sold across borders. There was strong evidence that the program harmed the poor, and yet was continued because of the stated concern of harm to the poor. Other means (including smaller gas canisters) could have achieved more of the objective or lowered the cost, or both.

The volume seems to entirely miss the point that there are positive-sum gains from many policy changes, such as the ones just mentioned. In many cases, economists are advocating policies where the smugglers and rent seekers would lose, while those not privy to government favors (or unable to engage in illegal activity) would gain. Yet, for example, Freeman (p. 658) asserts that "Any systematic error in economics acts to the advantage of one class or part of society; this follows from the zero-sum nature of distribution." Countless counterexamples can be found: in the Republic of Korea (South Korea), for example, the phenomenal rate of growth in real per capita incomes in the decades after 1960 rendered all better off. While one can quarrel that there may have been policies that might have achieved even more, it is hard to argue with the track record of that economy over the next several decades. In addition to lifting the per capita income of the people (with an almost constant Gini coefficient) and the average real wage by several multiples over more than three decades, life expectancies rose sharply, nutritional and health standards improved greatly, educational attainments increased sharply, and much more. It was on the advice of economists that policy changes were brought about in the late 1950s and early 1960s. It was NOT a zero-sum game.

Moreover, in some cases, the same objectives can be achieved with less harm than policies actually chosen. Indeed, sometimes the unintended consequences of policies chosen actually work against their stated objectives. A reader of the handbook might conclude that, since environmental concerns transcend economics (or alternatively, generate an externality); economics has little or nothing to say. Yet in fact it has a lot to say: when there are lower-cost means of achieving the same objective (such as carbon emissions), using the lower cost can enable greater environmental improvement or savings to enable expenditures on other (public or private) ends.

The arguments presented regarding economists' conflicts of interest are also greatly exaggerated or unwarranted. They are couched largely in terms of the financial gains (either directly as hired consultants or indirectly via research grants) accruing to those advocating particular policies. A few authors recognize that an interest group would almost certainly engage a consultant already holding views compatible with its interests, but the overall impression given by authors in the handbook is of economists as spokespeople whose views are determined by the interests of their employers. Not only is that view insulting to most economists, it is also largely wrong. Moreover, even if the authors of this volume could prevail and prevent economists from engaging in these activities, vested interests would likely employ noneconomists even less qualified to speak on the issues. Worse yet, not a single example is given of an economist whose policy conclusions from research were at variance with later policies advocated in a consulting or other paid role.

Most of the authors are sympathetic toward or even advocate three measures that they believe would reduce the conflict of interest problem: (1) require economists to list and give a history all of their nonacademic (including think tanks?) activities; (2) be familiar with all alternative viewpoints and lay out how proponents of those viewpoints would react to their proposals; and (3) "know everything" about who will gain and who will lose from a given policy.

Turning to the first, many economists (myself included) have learned huge amounts over the years in various policy and applied roles that have greatly enriched their research. Nonetheless, listing all of these activities (from some of which not much was learned or ever used) is hopelessly unrealistic. A potential employer (including governments) can and often does ask for a curriculum vitae, which lists all past activities, but that is simply not feasible in all published works. Indeed, I believe that all economists should have spent some time in a "real-world" environment, as a greater appreciation of the constraints and pressures on policy is likely to lead to relevant research.

The implication that all economists are swayed by all activities is certainly overblown. Listing all these activities even in a footnote would require more space than most journals allow in an article. Would refereeing books for possible publication in for-profit publishing houses (for a small fee) need to be listed? What of university presses? What of advisory roles for the National Science Foundation or the National Institutes of Health? Why not also list all books ever read? After all, economists read widely to learn and assess situations and those works may influence their views.

Most economists judge a journal article by its documentation and by the evidence and reasoning underlying the conclusions that are set forth. Depending on the question under discussion, the test can be more or less rigorous. But journal articles that fail with sufficient documentation are normally rejected or, if published, quickly ignored. By the documentation test, this handbook will not have great influence.

There is one other important objection to the first prescription: knowledge does increase over time. We do learn, and as we learn, old views are replaced or qualified. In economics this is especially true both because of the complexity of the subject, but also because economies themselves grow and change. To need to present all viewpoints (there are some who still advocate the gold standard, for example) when advising the finance minister of a country experiencing runaway inflation, or unsustainable sovereign debts, is the height of folly.

The second prescription is that economists should know all alternatives and viewpoints. Freeman, for example, concludes that "every economist . . . should be familiar not only with mainstream ideas but also with their principal critics" (p. 664). He also calls for journals to require a discussion of "the theories in the field that most directly conflict with those . . . in the article" (p. 665). This is simply not feasible for most economists.

Moreover, that prescription and much else in the volume is written as if time were a free good. Yet keeping up with any field is challenging enough, and for an economist to attain sufficient familiarity with "all alternatives" to satisfy most of the *Handbook* authors would reduce efforts on much else.

In many ways, the third prescription is the most troubling of the three. Those objecting to a given policy proposal find calling for further research a very effective bureaucratic technique to slow down, or even prevent, the adoption of a policy. Yet authors in the volume only occasionally note that failing to change a policy IS a policy! In many instances, research and experience with existing policies demonstrate the need for change. Those benefitting from existing policies are often those calling for further research to protect themselves. When I was Vice President for Economics and Research at the World Bank, I found it ironic that many proposals were blocked by calls for further research. There were cases where it was clear that no amount of further research would ever be sufficient, and where a call for more knowledge was a call to retain existing policies.

Overall and with some exceptions, the volume makes for frustrating reading. The frustration arises for many of the reasons already stated, but also because the papers are full of criticisms that may have some merit, but where the authors do not propose an alternative, or at least not an alternative that would improve the situation. The call for a code of ethics for economists was met with the adoption of one by the American Economic Association. It has long been the case that authors receiving funding to support their research acknowledge both financial supporters and those from whose comments they benefitted. To go beyond that to the requirements advocated in this volume would generally be ineffective and could often do harm. There is a strong case to be made that existing procedures and incentives provide much of the discipline that authors of this volume seek. The incentives for publication are, in effect, somewhat adversarial in nature.[4]

The rewards for an economist who proves that existing doctrine is either false or in need of qualification are large. Consider the case for free trade: economists have largely accepted the consensus for open trade. An article submitted to a journal providing yet another proof of the Pareto superiority of free trade would have little chance of acceptance or citation if published. An article finding another theoretical or practical objection to free trade would find its way into textbooks and citations in short order.

[4]There is an article, by Robert J. Thornton and John O. Wade (pp. 671–93) on forensic economics, in which the authors are highly critical of the adversarial approach to court determinations of values of losses and seem to believe that prohibiting economists, and asking laymen, to testify would produce better results. There is no convincing argument made as to why that process would be superior. Nor is it clear how laymen would be chosen: if not randomly, there could be equal if not greater conflicts there. If repeatedly, issues arise. And so on. But none of this is discussed.

The rewards for overturning received wisdom are sufficiently strong to provide incentives that, if anything, are biased against doing research supporting received knowledge. On reflection, despite the authors' claims in the handbook, the incentive system already provides a discipline to produce the results the authors seek. In addition, when policies fail to achieve their objectives, the incentives for investigating the causes of failure are strong. In the process, there is learning, and knowledge advances.

Perhaps the Code of Ethics adopted by the American Economic Association will change something. But whether the adoption of that Code, even a stronger one, would change the application of economics in desirable ways is questionable. If it were changed in ways that some of the authors in this volume propose, it would almost certainly entail costs higher than the benefits, from a societal viewpoint.

For many years, those influenced by Max Weber have recognized his distinction between "ethicists of conviction" and "ethicists of responsibility." The former are idealists and may advocate entirely impractical or unattainable actions. The latter act on what is possible and recognize that second-best may be preferable to no action. Weber called the former "windbags" in nine out of ten cases (*Economist*, October 1, 2016, p. 54. For a reprint of the original, see Gerth and Mills, 2010). In the case of this volume, the authors are clearly the ethicists of conviction, while mainstream economists are usually ethicists of responsibility.

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C Mathematical and Quantitative Methods

The Theory of Extensive Form Games. By Carlos Alós-Ferrer and Klaus Ritzberger. Springer Series in Game Theory. New York: Springer Nature, Springer, 2016. Pp. xii, 239. ISBN 978–3–662–49942–9, cloth; 978–3–662–49944–3, e-book. *JEL 2016–1699*

The traditional models for extensive-form games, as developed by John von Neumann, Oskar Morgenstern, and Harold W. Kuhn were specifically designed for the *finite* case. That is, they restrict to situations where there are only finitely many histories in the game, and where there is a finite number of available choices at every history. At the same time, game theory and economics are full of examples where these finiteness assumptions are violated, such as infinitely repeated games, stochastic games, differential games, infinite-horizon bargaining games, and dynamic models of price and quantity competition, to name just a few. In that light, it seems important to have a general model of extensive-form games that captures all these infinite cases as well.

The purpose of this book is to fill that gap, by offering a unified way of modeling and analyzing extensive-form games without imposing any restrictions on the number of histories, choices, players, and time periods in the game. At first sight, this seems a formidable and almost impossible task which, if successful, must necessarily lead to intractable models. The good news is that this book proves this hypothesis wrong, for its model of extensive-form games is rather sparse on notation and the number of objects introduced, rendering it very attractive to work with. Indeed, the degree of generality the authors were after forced them to look for the minimum number of objects and assumptions needed to represent an extensive-form game, and this resulted in a sparse yet flexible model that is able to cover each of the infinite cases above.

As a first step, the book offers a general way to represent the histories and the outcomes in a game, where the histories represent those situations where one or more players make a choice, and the outcomes represent the possible ways in which the game can be played. The proposed representation may be viewed as a combination of Kuhn's graph-based approach and von Neumann and Morgenstern's outcome-based approach, appropriately extended to the infinite case, thereby exploiting the advantages of both representations.

In the graph-based approach, the histories are represented by nodes in a directed and rooted tree, ordered by precedence, and the outcomes can be derived from this object as the maximal chains of successive nodes. The outcome-based approach, in turn, starts from a given set of outcomes and identifies each history with an event in the space of outcomes, to be interpreted as the set of outcomes that are still possible after this history. The advantage of the graph-based approach is that it yields a rather intuitive graphical representation of the game, whereas the outcome-based approach turns out to be more convenient from a mathematical point of view.

In chapter 2 of the book, it is shown that, under certain regularity conditions, both representations are equivalent and can thus be used interchangeably. Indeed, there is a natural way to go from a graph-based representation to an outcome-based representation, by choosing the set of outcomes as the maximal chains of successive nodes and identifying each node in the tree with the set of outcomes that pass through that node. Conversely, if we start from an outcome-based representation with a given set of outcomes and collection of outcome events, then we can naturally construct a tree by identifying each node with an outcome event and ordering the nodes by set inclusion of the associated events. Chapter 2 provides precise conditions under which we can safely move from one representation to the other without "changing the game."

After a discussion of pseudotrees and order theory in chapter 3, chapters 4 and 5 enrich the above representation by adding players, choices and chance moves to the game, from which we can then derive information sets and strategies. Some interesting conceptual issues arise here, which do not appear in the case of finite games. For instance, if the game at hand is infinite, then players do not necessarily have "available choices" at some histories in the game, and hence we are not sure that strategies can be defined at all. If such strategies can be defined, then it is not guaranteed that any combination of strategies induces a unique outcome in the game—something that is needed for a meaningful analysis of the game. Chapters 4 and 5 identify precisely those conditions under which the problems outlined above disappear, and that thus open the door to a strategic investigation of the game.

Chapters 6 and 7, finally, focus on the important class of discrete extensive-form games. In particular, chapter 7 characterizes those discrete games with perfect information that always admit a subgame perfect equilibrium for all continuous preferences, and provides sufficient conditions under which subgame perfect equilibrium is equivalent to backward induction.

In my view, this book is a true milestone for the theory of infinite extensive-form games, both in terms of content and presentation. Throughout the book, the authors always look for the simplest possible models and the weakest possible assumptions needed to represent and analyze an extensive-form game in a meaningful way, thus positioning the book at the frontier of extensive-form game theory. This has resulted in a beautifully crafted book that reveals how even the most exotic of extensive-form games can be represented rather compactly within the unified approach that it offers. As such, I believe that PhD students and researchers with an interest in infinite extensive-form games will benefit tremendously from this book, and that the book will contribute to a better understanding and appreciation of this fascinating area.

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D Microeconomics

Finding Time: The Economics of Work–Life Conflict. By Heather Boushey. Cambridge and London: Harvard University Press, 2016. Pp. xi, 243. $29.95. ISBN 978–0–674–66016–8, cloth. *JEL 2016–1157*

This book by the economist Heather Boushey, is a thought-provoking piece about the economics of work–life conflict. Against the background of sluggish economic growth and increased inequality in the United States since the 1980s, the author uses solid economic arguments to provide support for a set of policies and interventions aimed at giving families control of how much and when they supply time to the economy. The book uses nontechnical language and is written in a rigorous but highly accessible way, appealing to a general audience. Given the author's background as the Executive Director of a Washington think tank, the book takes a pragmatic approach throughout, which would satisfy readers interested in policy making. The book's scientific rigor and multidisciplinary approach to work–life issues will also appeal to other social scientists, particularly sociologists in employment relations and human-resource management and demographers working on family issues. For economists, the deep economic theoretical foundations underlying the main line of argument in the book, a thoughtful analysis of economic data, and a solid understanding of the rapidly growing evidence in the economics literature on work–life conflict, make this book a delight for labor and public-policy economists, and a good read for an economics student at the undergraduate or graduate levels studying these subjects.

The first chapter after the introduction, "Our Roots," provides an insightful historical perspective on the issue of work–life policies. Here, the author sets the stage for a new social contract by explaining why the policies from the 1930's New Deal are outdated in a world where businesses have lost their "silent partner" (i.e., the American housewife). The next three chapters put together a comprehensive wealth of data to provide a historical overview of how middle, low, and high-income families overcome the new challenges of work and family life. The author convincingly shows that families across all income groups have not only lost the *amount* of time at home as the silent partner goes off to work, but also the ability to decide *when* to spend that time as new technologies such as "just-in-time scheduling" algorithms increasingly shift the bargaining power towards firms.

Chapter 5 constitutes a key chapter in the book as it neatly sets out the theoretical foundations that support the notion that social equity and economic efficiency can go hand in hand. A simple but neat economic-flow model is presented to highlight two key points not considered enough by previous economic literature. First, businesses do not necessarily choose what is best for the economy, as their focus is shortsighted. As a result, the positive externalities associated with healthier work–life conditions (for example, more and better parental time investments, and more stable couples) are not internalized in business decisions. Second, a partial-equilibrium approach based on the supply side of the market ignores the importance of the aggregate demand. Thus, in the same way that families increased expenditure as women entered the labor market, so will demand increase as the constraints workers face when supplying labor to the market are lifted.

Using a wealth of evidence including personal examples, case studies, survey data, and quasi-experimental evidence, chapters 6 and 7 take an efficiency wage approach to discuss the policies that can be implemented to increase the time that talented individuals (in families) supply to the market, and chapter 8 engages with the question of care, by highlighting the positive externalities derived from quality care. Chapter 6 focuses on paid sick and maternity leave, and chapter 7 builds a similar case for how a greater control over when employees supply time leads to increases in workers' productivity, lower absenteeism, and higher employee retention rates. In chapter 8, the author rightly ponders why a fully funded educational system cannot also include a fully funded high-quality prekindergarten.

In the final chapter before the conclusion, "Fair: Finding the Right Path," the argument that equity and efficiency are not at odds follows naturally from the previous chapters. Changing the institutional context so that families provide time to the market in more efficient ways seems at this point not much different from lifting barriers to trade or subsidizing R&D to boost technological progress. What is then stopping intervention? The author points the accusing finger at American policy makers' "care-giver bias," exemplified by the long-standing efforts to pit "what's good for families against what's good for the economy."

The author needs to be applauded for bringing together many of the efficiency arguments that float in the economics literature in relation to family-life conflicts. Yet issues surrounding the asymmetric information problems in the care market, as well as the inefficiencies associated to the allocation of time resources within families, are barely discussed. I also remained skeptical about the external validity of some of the research. For example, understanding how different rules on how time is sold and bought in the economy may depend on the nature of job tasks and workers with different skills. External validity poses an important challenge for the design of policies, and raises questions about how new technology such as "just-in-time scheduling" could be put to use to move beyond one-for-all policies such as paid sick leave and universal childcare. These policies were adopted by most European countries in a context of growing female labor participation, but well before the rapid technological advances that are increasingly blurring the boundaries between work and home. Could US policy makers leapfrog some of these policies, in the same way that developing countries leapfrogged the use of telephone lines in favor of mobile phones?

For a long time, the analysis of work–life conflict lingered around issues of fairness. As a result, the academic and policy debates moved at snail speed, because the implication was that what was good for families was bad for the economy. Given different views on fairness, this resulted in the ideal deadlock in academic and political circles. The present work turns this idea upside down, and convincingly takes the reader through an intellectually stimulating journey where efficiency and equity go hand in hand. By taking the debate to a higher level, the book provides the basis for a meaningful discussion on the topic among economists and provides the foundations for new policies that reach beyond partisan differences.

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The Economics of Voting: Studies of Self-Interest, Bargaining, Duty and Rights. By Dan Usher. Routledge Frontiers of Political Economy, vol. 203. London and New York: Taylor and

Francis, Routledge, 2016. Pp. 333. ISBN 978–1–13–893255–5, cloth; 978–1–315–67919–8, e-book. *JEL 2016–1190*

The very first words of the book will certainly raise high expectations in any reader interested in political economy and the study of democracy:

> The economics of voting is about when, and subject to what qualifications, electoral markets are like ideal commercial markets where universally self-interested behaviour yields outcomes that are in some sense best for society as a whole (Front matter, no page number).

The book brings together many interesting elements, but in my view, perhaps inevitably so given its ambitious goal, may leave the reader with some frustrations and possibly some disagreements.

Let me start with a short summary of the book. After a short introduction (chapter 1) and a quick review of some well-known theoretical patterns of majority voting (chapter 2), the book studies in detail the most prominent example where, according to the author, "electoral markets are like ideal commercial markets." This is majority voting over some basic income flat-tax schedules (chapters 3–4). Citizens, heterogeneous in their pretax income, vote over linear redistribution schemes in a world where redistribution is costly because of tax evasion. Voters are assumed to be purely self-interested, meaning that they maximize their post-tax income without any regard to the well-being of their fellow citizens. The celebrated *median voter theorem* applies: there is a unique tax rate defeating any other in a pairwise vote (a Condorcet winner). This unique equilibrium outcome has some properties described as desirable, in that it induces some "moderate" amount of redistribution, leaving no individual starving at one end on the income distribution, while at the same time not expropriating the most productive individuals at the other end of the spectrum.

But as soon as one leaves this unidimensional world, democracy and the ruling of the majority, combined with unrestrained self-interest, lead to chaos and potentially disastrous outcomes.

First, *the majority rule is intransitive*: there is no Condorcet winner in general, so that the outcome of majority-rule voting becomes unpredictable. Bargaining is needed as a solution to this fundamental instability of the majority rule, be it bargaining in political parties when deciding about their platforms, or among legislators to allocate places in the cabinet or to agree on a set of laws to be passed (chapters 5–6).

A particularly striking example of intransitivity arises in the case of *the exploitation problem*, extensively used in the book. Consider a society of greedy, purely self-interested individuals using majority voting to decide how to divide among themselves a fixed-size pie, in any way they want. Majority voting is unpredictable, in the sense that any division of the pie can be defeated by another allocation, a new majority dispossessing the corresponding minority. In such a setting, individuals will have the incentives to form majority coalitions based on any badge they can find (race, religion, ethnicity, region, etc.). This powerful example shows that without any restriction on what can be voted upon, democracy is conducive to chaos and potentially violence. Indeed, when so much is at stake, a group which for some reason has managed to form a majority at one point in time, anticipating the devastating consequences it might face if a new coalition forms to defeat it, will have the incentive to change the democratic institutions and abandon democracy altogether. Civil rights and property rights, and a general consensus that things have to be left out of the realm of collective decision, are the only solution to escape the tragedy of the exploitation problem (chapters 9–10).

Chapters 7 and 8 address the issue of the participation of citizens in elections. Since the chance of one's vote being pivotal is extremely small in any mass election with tens of thousands of voters, it is hard to justify voting from self-interest alone, as soon as one considers that voting entails some costs (transportation or opportunity costs). A sense of duty is needed to explain the observed participation rates, and to ensure the good functioning of democratic institutions.

The book concludes by challenging and criticizing two models that have attempted to explain/predict democratic outcomes on the basis of self-interest alone: the citizen–candidate model (chapter 11) and the probabilistic-voting model (chapter 12). As such, it reasserts the main message of the book: that democracy cannot be sustained by self-interest alone.

The book is interesting reading, and contains a lot of information. Each chapter can be read in isolation, since many chapters in the book are articles that have previously been published as stand-alone pieces.[1]

In my view, two questions might have deserved a bit more discussion and clarification. The first one is about the exact nature and relevance of the political markets-commercial markets analogy. The second one is about the role of self-interest.

The analogy between political markets and commercial markets is the building stone of the book; there are multiple references to the first welfare theorem in the book. Yet, the fact that this is a relevant analogy is somehow taken as granted, without further justification or discussion. To me at least, it is not that straightforward why such an analogy should be relevant in the first place. First, we know that the first welfare theorem fails to hold in the presence of externalities. Yet, such externalities are likely to be prevalent in politics.[2] Second, even in cases where the author perceives the political markets as similar to commercial markets, I am not sure that I fully agree with the analogy. When the author writes, "There is a unique electoral equilibrium, comparable to the equilibrium in a competitive market, in the world of the *median voter theorem* . . ." (p. 2), it is unclear to me how exactly it is comparable. The two situations look quite different, in both their structure and the underlying mechanisms. Indeed, in commercial markets, prices efficiently coordinate private decisions made by individuals willingly engaging in market exchanges, whereas in the median voter theorem, there are no such exchanges and an equilibrium only exists because of the very particular single-dimensional structure. I am not sure that the analogy in that case goes much beyond the reliance on pure self-interest and the existence of a unique equilibrium.[3] Finally, it was not always clear to me what the author's purpose was, when using this analogy. Are economic markets seen as a good benchmark for how we, as economists and social scientists, should try and model political institutions? Or is it a normative statement about how politics should work? In different parts of the book, the author seems to have either, or both, in mind, without always making it very explicit.

Regarding the role of self-interest, the main message of the book is that unrestricted self-interest would be destructive of democracy, and that bargaining, duty, and rights are needed. The interesting question then becomes how such rights, norms, or conventions emerge and can be sustained democratically. An answer seems to be provided in the introduction:

> . . the defense of property rights under majority rule voting must rest upon voters' unwillingness to vote away property rights for fear of what might happen not just to the economy, but to the institution of majority rule voting itself. Formal constitutional constraints surely help, but would be a poor defense of property rights if not bolstered by the long-term interest of the great majority of voters (p. 2).

[1] The chosen format of a collection of essays has some advantages. Results are presented in a colorful and easy-to-read manner. For example, chapter 3 entitled "Patterns of Voting" is a nice introductory overview of the main properties of majority voting. A broad set of approaches are used: some chapters use formal modeling, some are critiques of existing models, some are discussions closer to political philosophy or personal thoughts. But this format obviously also has some less desirable features. There is no clear hierarchy between what is central to the author's thesis (e.g. chapter 10), and what is perhaps more peripheral. (Some chapters can appear somewhat as digressions, in particular chapter 4 on the Laffer curve or chapter 8 about how best to model the probability of casting a pivotal vote in a mass election.)

[2] For example if, as defended by the author, a large participation in elections is necessary for democratic decisions to be representative and perceived as legitimate, then a large participation is a public good. No wonder then that self-interest alone might lead to inefficient outcomes, and that something else like duty is needed.

[3] This seems to me quite different from the attempt by Buchanan and Tullock (1962) to explore a similar analogy between political and economic markets (a seminal reference cited on several occasions in the book). Indeed, Buchanan and Tullock consider political choices much more broadly (than allowed in the setting of the median voter theorem), encompassing situations where, similarly to economics exchanges, political cooperation could yield mutual gains (which is not the case in the median voter theorem).

Should we interpret this as meaning that, in the end, the author thinks that property rights can also be explained by self-interest, and are thus reintegrated into its realm? This blurs the central conceptual distinction that is made elsewhere in the book between self-interest and rights. It would have been interesting to have some discussion in the book about the definition and scope of self-interest, to avoid this kind of apparent contradiction.

Who should be interested in reading this book? Dan Usher, now an emeritus professor at the Economic Department of Queen's University, has been working on the topics covered in this book for over forty years.[4] His thorough expertise is reflected in this book. Students and researchers in public choice, or scholars who want to learn more about Dan Usher's work, should definitely be interested. Scholars in other subfields looking for a broad overview of the merits and limits of the self-interest assumption in the realm of politics might also find it of interest. However, those who are not experts in the field may want to start with a shorter introduction to the author's views on the topic; for example, Usher (2012) offers a good summary of some of the main messages of the book.

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[4] For example, chapter 9 was initially published as a working paper in 1975. Some elements of the central thesis in this book were already present in some of his previous books (including *The Economic Prerequisites to Democracy* (1981) and *The Welfare Economics of Markets, Voting and Predation* (1993)).

E Macroeconomics and Monetary Economics

Monetary Analysis at Central Banks. Edited by David Cobham. New York: Springer Nature, Palgrave Pivot, 2016. Pp. xi, 139. $54.00, cloth. ISBN 978–1–137–59334–4, cloth; 978–1–137–59335–1, e-book. *JEL 2016–1759*

1. Introduction

As any major crisis with far-reaching consequences, the Great Recession (GR) period highlighted the need for institutional reforms and marked significant policy switches. The aftermath portrayed mostly policy changes rather than reforms, however. The US and EU experiences demonstrate that, upon quick realization of the limits of conventional monetary policy to deal with the implications of the mortgage market collapse, central banks resorted to unconventional monetary policies. Specifically, in contrast to policies based on the liability side of the central-bank balance sheets, asset side balance-sheet-based policies were put into practice. The zero lower bound (ZLB) and nonincreasing inflation rates were hit quickly, in spite of the significant expansion of the central-bank balance sheets, and appeared to have rendered the quantity theory of money an obsolete tool of policy analysis. By covering the monetary-policy frameworks and implications in three countries, partly with a comparative perspective, this book contributes to the understanding of the changes in monetary policy analysis in the post-GR period. As such, it is of considerable interest to those in the central-banking practice, as well monetary theorists around the world.

Quantitative easing policies (QE), the extent and timing of which varied across the countries, became a central monetary-policy instrument in countries that were impacted directly by the GR. Coupled with currency wars, however, QE pushed the short-term policy rates to the ZLB. Facing a liquidity trap, central banks resorted to large-scale asset purchase (LSAP) and forward-guidance (FG) policies to revive the monetary transmission mechanism. Friedman (2015) predicts that, of the unconventional policy tools of the post-GR period, LSAP and the use of central-bank balance sheets will be established as conventional policy tools, whereas FG will not, as its credibility and effectiveness in the real sector has been debated. With the policy switch from the conventional open-market operations towards LSAP, the central banks' balance sheets have since increased several-fold in the United States, United Kingdom, and the European Central Bank. While according to traditional quantity theory an expansion of such dimensions would have resulted in inflationary pressures, this has not materialized, at least thus far. Friedman (2015) argues that central banks can utilize both the interest rate and quantity (level and composition of its assets) instruments independently and simultaneously in this framework.

Given this background, Cukierman (2016) argues possible overestimation of the implications of the ZLB, which implies rethinking of the justification of unconventional polices. He argues that ignoring the impact of credit rationing and omitting financial stability objectives from the monetary rule pose a negative bias in the estimation of the natural rate of interest. The observed effectiveness of capital injections supports this view and that "ZLB . . . may be of lesser significance than currently believed." According to Cukierman, because long-term rates have a sizable effect on the transition mechanism, the natural rate needs to incorporate the long-term risk premium so as to ensure the position of the central banks as the lender of last resort for risky assets. Linking the natural rate to the health of financial institutions highlights the importance of financial regulation and supervision that appears to have been somewhat neglected in the process.

2. *The Content*

The volume, edited by David Cobham, starts with a review of conventional monetary analysis adopted in high-income economies since the 1970s. It provides a discussion of the transition from monetary targets towards a period of "little monetary analysis" and "too much emphasis on inflation targeting" by the 2000s, and back to "serious" monetary analysis again in the aftermath of the GR. References to the monetary policy experiences of the United States, France, and Egypt complement the studies in the rest of the volume.

The novel contribution of the second chapter, by Jon Bridges, James Cloyne, Ryland Thomas, and Alex Tuckett, is a thorough account of the empirical analysis of the impact of the crisis, the pursuant QE, and the credit shocks in the UK economy at both aggregate and sectoral levels. In view of the poor forecasting performance of the DSGE models (see, for example, Smets and Wouters 2007) and the absence of a reliable DSGE model that incorporates essential credit and financial frictions, Bridges et al. note that the Bank of England (BoE) has resorted to structural VAR modeling of the real and the financial sector for its policy analysis. The model is argued to account for the sectoral level of credit contraction and consumer funding gap in the post-GR period.

The third chapter, by Philippine Cour-Thimann and Bernhard Winkler, is on the ECB's unconventional monetary policies that are in the form of central-bank balance-sheet operations and their transmission mechanism. The authors argue that the contingent easing instruments of the ECB have reduced the risk perception and systemic risks. Because the multiplier effect of these policies on the real economy was limited, however, the ECB also resorted to QE in 2015, combined with FG. The main beneficiaries of these policies were the financial sector and the large corporations, while cash hoarding by large corporations left small- and medium-sized enterprises starving for funds. The authors argue that pre-euro period monetary-policy instruments that targeted the real sector, such as discounting commercial bills, would have reduced the risks accumulated in the ECB's balance sheet effectively and have also served macroprudentials better than the unconventional policies.

Taking stock of the US, UK, and ECB experiences, it is fair to note that the lender of last resort role of the central bank in the post-GR period targeted mainly large-scale private enterprises and their shareholders, and thus deepened wealth differentials across the economy. An analysis of the BoE (Bank of England 2012) shows that LSAP by BoE has saved large corporations from defaulting and benefitted the top 5 percent of income holders, who hold 40 percent of the financial assets outside pension funds, whereas it possibly depressed the purchasing power of those with bank deposits. Similarly, Piketty and Saez (2013) observe that the US recovery after 2008 was mainly observed in the top percentiles of the income distribution, where corporate profits and financial bonuses were concentrated. Hence, the unconventional monetary policies of the post-GR era appear to have significant redistributive implications; as large-scale bailouts may increase the moral-hazard risk in large corporations, lack of sufficient spillovers to small-scale enterprises and wage earners may lead to a reduction in their productive efficiency. Time will provide a test of sustainability for LSAP policies, but if the resulting dissatisfaction of the bottom 90 percent income groups with their relative incomes and indebtedness keeps on growing, it is predictable that the system will eventually be forced to bail out these groups as well.

The last chapter, by Christopher Adam, Pantaleo Kessy, and Ben Langford, is on Tanzania, the only developing-country case study in the book. After independence in 1966, the country managed to reduce inflation successfully by using monetary targeting until the mid-1990s, and adopted reserve-money programming afterwards. As financial deepening started to weaken the transmission mechanism, the Bank of Tanzania (BoT) switched more recently to interest-rate targeting. The change from a quantity target to a price target meant switching from rule to discretion. The authors argue that, having been vested by operational independence, BoT anchored expectations well and gained credibility that helped its capacity to manage inflationary pressures after 2010. As exemplified by the Tanzanian experience, monetary-policy evolution in large low-income economies in Africa, specifically Kenya and Uganda, is thus argued to show a path that replicates that in developed countries prior to the GR episode.

3. The Critique

Overall, the reviews of monetary-policy analysis in the United Kingdom, European Union, and Tanzania in the volume are highly useful to understand the evolution and state of monetary-policy analysis in the post-GR era. Comparative discussions of a few other countries also complement the volume nicely. The choice of Tanzania as the only developing-country example in the book leaves the volume's coverage short of being satisfactory, however. While the introduction states that lack of consistent high-frequency data constrains rigorous monetary analysis in a sizable portion of developing economies, this does not moderate the fact that the chapter on Tanzania stands alone in the volume. A general discussion of how the repercussions of GR-affected monetary-policy analysis in other developing countries would, I think, have improved the contribution of the volume.

Another weakness of the volume is the lack of elaboration of the notion of macroprudentials. While the impact of the GR seems to be currently averted by unconventional monetary policies, many argue that the potential of systemic crises is probably not sufficiently dealt with. The "qualititative easing" (in the terms of Buiter, 2008) aspect of unconventional monetary policies highlights the importance of improved systems of financial regulation and supervision (RS) for avoiding moral-hazard risks and achieving long-term financial-sector stability. While independent central banks and developed monetary-policy analyses are necessary, they are not sufficient to eliminate these risks unless supported by independent institutions of RS. Incorporating some discussion of relevant institutional and structural positions of the economies examined in the volume would have therefore enriched the understanding of the long-term success of these policies in terms of their potential to eliminate the risk of future financial crises and resulting distributional problems.

About two decades after settling with the best-practice institutions for monetary policy to effectively deal with the ill effects of high inflation, developed countries now face the challenge of complementing them with the institutions of financial stability. Posen (1996) argued that financial opposition to inflation had a crucial role in withstanding inflationary pressures that arise from fiscal dominance. As the world has moved into a low-inflation plateau, it is interesting that policy makers now face the need for institutional measures to prevent the risks of financial dominance. This volume leaves me looking forward to further studies that analyze the state of macroprudential reforms that aim to prevent potential future systemic crises.

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F International Economics

Economic Aspects of Genocides, Other Mass Atrocities, and Their Prevention. Edited by Charles H. Anderton and Jurgen Brauer. Oxford and New York: Oxford University Press, 2016. Pp. xvi, 709. $99.00. ISBN 978–0–19–937829–6, cloth. *JEL 2016–1780*

The large and growing literature on the economics of conflict has so far examined mostly small numbers and/or protracted events, including insurgencies, civil wars, terrorism, and more. Strangely, the far more rare but far more deadly large-numbers events, where noncombatants are butchered in scores, have been largely underresearched, despite the obvious importance of the topic for purposes of prevention and intervention. This collective volume takes a first, substantial step toward filling the gap. As the editors, Charles H. Anderton and Jurgen Brauer—two leading scholars in the field—point out in the introductory chapter, the gap is actually twofold. There is a *genocide gap* in the field of defense and peace economics, as well as an *economics gap* in the field of genocide studies, which has been dominated by historians and other social scientists who have noted the economic issues in their subject matter, but generally looked askance at the economist's toolbox. The book endeavors to address this twofold gap by means of a two-pronged strategy: a number of chapters designed for beginners, which introduce noneconomists to the basic tools that are or can be of use in the field and provide examples of application, and a number of chapters designed for economists, which summarize what there is and add new contributions. The editors define their subject broadly as encompassing genocides proper (as defined by the United Nations Genocide Convention of 1948) as well as war crimes, crimes against humanity, ethnic cleansing, and other mass atrocities, all of which are captured by the umbrella term "genocide and other mass atrocities" (GMAs).

The twenty-eight chapters, whose authors are drawn mostly, but not exclusively, from economics and include many of the leading scholars in the field, range from theoretical papers to surveys of empirical research to case studies, some of which are put to the test of formal models; the emphasis on policy implications looms large throughout the book and is the special focus of a few chapters in the last part. The collection has secured the blessing of Nobel laureate Roger B. Myerson, whose chapter (28) summarizes his own work on the political economy of democratic transitions, which can be of use for a society struggling to recover in the aftermath of conflict. The economics used throughout the book belongs for the most part in the mainstream, optimizing approach, but also includes two insightful applications of prospect theory to the "locking in" of repression (chapter 6, by Anderton and Brauer) and to the "psychic numbing" that dampens the public's reaction to mass killings (chapter 26, by Paul Slovic, Daniel Västfjäll, Robin Gregory, and

Kimberly G. Olson), as well as a chapter on the law and economics of the "too little, too late" syndrome that afflicts international intervention in atrocity crimes (chapter 27, by Brauer, Anderton, and David Schap). Besides economics, a variety of other fields and methodologies provide complementary approaches, including demography (chapter 4, by Tadeusz Kugler), genocide studies (chapter 17, by Elisa von Joeden-Forgey), logistics (chapter 18, by Yuri M. Zhukov), machine learning applied to found datasets (chapter 23, by Rex W. Douglass), forecasting models (chapter 24, by Charles R. Butcher and Benjamin E. Goldsmith), and management science applied to business involvement in genocides (chapter 25, by Nora M. Stel and Wim Naudé). Frequent cross-referencing between chapters helps the reader navigate the book.

Ensuring a reasonably high, uniform quality in a collected papers volume is well-known to be a daunting task—all the more so when the subject is so broad and the number and range of contributions so wide as in the case under review. If as a rough measure of success we count the number of chapters that fall below the modal standard of the book, this one passes the test with flying colors. This did not just happen: besides writing, jointly or separately, several keystone chapters (1, 3, 6, 13, and 27) that provide perspective and coherence to the whole collection, the editors clearly worked hard on planning the overall design and reviewing the individual papers to minimize straying (as evidenced by the acknowledgment notes in most of the chapters). That said, a few chapters do fall below the standard. Chapter 8 (by Néstor Duch-Brown and Antonio Fonfría) talks about a complex industrial-organization model of violent conflict, incorporating social structure and political competition and possibly leading to genocide, but the model is then not provided. Talking models that do not yet exist is not helpful. Chapter 20 (by Neil T. N. Ferguson, Maren M. Michaelsen, and Topher L. McDougal) addresses the mass violence against civilians perpetrated by the drug cartels in the "drug war" in Mexico and frames it as the outcome of a three-sides game of strategic interactions among national government, subnational governments, and drug cartels; though it is a very interesting modeling setup, the model is then not formally solved and the results are stated, not derived. Finally, chapter 21 (by S. Mansoob Murshed and Mohammad Zulfan Tadjoeddin) has a disconnect between an empirical-historical part about the politicide in Indonesia in 1965–66, its genesis and its consequences for economic development, and a model of individual participation and collective engagement in the mass killings; the model does not explain the economic and social history and the latter is not a test of the model.

Discussing the individual chapters in detail is ruled out by limits of space, but a couple of general comments can be offered. First, the feature of GMAs that has most baffled researchers so far is the extremity of the events, which almost by definition is difficult to account for from a rational-choice perspective. There seem to be only two ways to meet this challenge. A straightforward way is to argue for the likelihood of near-corner solutions in standard optimization models, which is theoretically parsimonious and should definitely be pursued (as Anderton and Brauer do in chapter 6), but inevitably has some ad hoc flavor. A richer, more promising way is to introduce social interactions between groups, especially in the framework of the economics of identity à la George Akerlof and Rachel Kranton, which can function as magnifier of reactions in a theoretically grounded way. A number of chapters make a start in this direction, including an economic discussion of the role of identity in the build-up to the Holocaust (chapter 14, by Raul Caruso), a discussion of the gendered, reproduction-centered dimension of identity in the ideology of the perpetrator group (chapter 17 by von Joeden-Forgey—very interesting food for economists' thought), as well as some initial attempts at modeling (chapter 21 by Murshed and Tadjoeddin and chapter 22 by Partha Gangopadhyay—the latter an interesting evolutionary dynamic game designed to explain the mass killing of a nonthreatening minority such as the Shiite Muslims in Pakistan, otherwise hard to understand). As the editors themselves note (p. 23), it seems fair to say that more hard, imaginative work is needed for this direction of research to deliver on its promises.

My final comment addresses the overall purpose of the book—establishing the economic rationality of GMAs. As the book testifies, this is a complex task, as it involves choices by multiple actors. Some interesting research covers secondary, though important, actors, including populations that choose between staying and leaving the conflict areas (chapter 11, by Ana María Ibáñez and Andrés Moya), the western public and governments (chapter 26), and international organizations (chapter 27). The principal actors, however, are the individuals who join the killing and the governments or organized groups that almost invariably start and direct the GMAs. A number of micro-level studies of participation, including chapters 9 (by Patricia Justino), 12 (by Maria Petrova and David Yanagizawa-Drott), and 15 (by Willa Friedman), as well as the social-interaction chapters mentioned above, speak to the first issue and make some substantial progress. The second issue, however, is more elusive. The easier part is the rationality of *means*, addressed in chapter 6—once the government has decided to destroy a group, why choose killing rather than alternative means (such as deportation, starvation, or enslavement)? The more difficult part is the rationality of the *end* itself—why would the government choose destruction of the group in the first place? The two theoretical chapters that analyze this issue model mass killing as a strategic choice of a government facing a rebel group in a context of insurgency or civil war (chapter 7, by Joan Esteban, Massimo Morelli, and Dominic Rohner) or as a strategic weapon in the contest between two armed groups for the control of a territory, as in Colombia (chapter 19, by Juan F. Vargas). Large-number datasets, mostly covering the post–World War II period and extensively discussed in chapter 3 (by Anderton) and used for empirical assessment in chapter 10 (by Anke Hoeffler), confirm that the overwhelming majority of cases fall within these coordinates. Here as elsewhere, the current hunt for large datasets is all well and good. However, there are extremes even among extreme episodes, and one such is surely the Holocaust—the paragon of genocides—which targeted a nonthreatening, non-territorial group uninvolved in the war. Clearly the rationality of the Holocaust itself—as distinct from the rationality of the means used and the incentives for participating in it (examined in chapter 14) —still eludes this book's effort. Without it, traditional genocide scholars, clinging to a "primordialist" view (of the type "men kill because it's in man's nature"), may still hold the high ground, claiming that we economists are staging a play in which everyone falls into place, except that the main character is missing. It may well be that in the end we will have to yield and concede that the Holocaust is indeed beyond economics, but not before making a determined, concerted effort at it.

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Europe Isn't Working. By Larry Elliott and Dan Atkinson. New Haven and London: Yale University Press, 2016. Pp. viii, 312. $30.00. ISBN 978–0–300–22192–3, cloth.
JEL 2016–1777

Larry Elliott and Dan Atkinson are two men with a mission. In the preface to their book *Europe Isn't Working*, the two British journalists are upfront about what that mission is:

> There are those on the left who feel uneasy about voicing their concerns about the euro, in the main because of the company they have to keep. This book explains why those misgivings are unnecessary. The single currency was not, is not, and never will be a progressive project (p. viii).

This sets the tone for a self-confident attack on the European single currency—part left-wing take-down of the euro, part take-down of the europhile left.

Elliott and Atkinson are keen to remind the reader that their first collaborative effort, *The Age of Insecurity* (1998), anticipated that the European Monetary Union (EMU) would stumble. They are acutely aware that if a version of *Europe Isn't Working* had been published in the late 1990s, predicting some of the subsequent events instead of describing them in hindsight, it would now be hailed as prophetic. Appearing in 2016, however, it is hard to escape the impression that the book is fighting yesterday's war. As the authors themselves note about their main target audience, the British left, it has become "quite cool . . . to be not just against the single currency but to voice doubts about the European Union itself" (p. 157). Given this, the misleading reference to "Europe," which has somehow crept into the title of a book preoccupied with the euro, whiffs of a sales ploy for the Brexit era.

Many of the book's key themes will be familiar to anyone who has followed the birth and life of the euro through the financial and opinion pages of UK newspapers: the EMU was conceived as a political project, with too little regard for the economics of a single European currency; its membership criteria and design reflect predominantly political considerations and compromises; the resulting heterogeneity of its members and institutional flaws left the euro vulnerable to crises; when such a crisis hit, the Eurozone's emergency response was botched and consistently behind the curve. The consequences have been high unemployment in Southern Europe, economic stagnation for the Eurozone as a whole, and political strife among its members.

Elliott and Atkinson take aim at those parts of the left that had cheered the euro on, expecting the EMU to deliver economic growth, convergence, and cooperation among its members in a pan-European social-market economy. In practice, they argue, its effects have been the exact opposite. They illustrate their point by dedicating a chapter each to the euro experience of several different EMU countries: France, Ireland, Greece, Italy, and a counterfactual euroized United Kingdom ("a bullet dodged").

While their account of the euro's failings is opinionated, the flaws of the single currency, which Elliott and Atkinson highlight, are real. In the face of the asymmetric economic developments triggered by the global financial crisis across the Eurozone, the worst-affected countries were unable to cushion the blow through national monetary policies or exchange-rate devaluations. Together with limited labor mobility and insufficient fiscal coordination across European borders, this set the stage for large losses in output and employment. This aspect of the euro crisis vindicates the theory of optimal currency areas (OCA), which harks back to Robert Mundell's (1961) pioneering article.

Some of the euro's early critics, whom the authors cite approvingly—such as Paul Krugman, Joseph Stiglitz, the late Rudiger Dornbusch, and the former UK Shadow Chancellor Ed Balls—were guided by OCA theory. Elliott and Atkinson acknowledge this intellectual debt as somewhat of an afterthought. From an economist's perspective, the theory would have merited greater prominence in an endeavor such as theirs. It could have provided a framework around which to organize some of the themes of the book. Moreover, it would have allowed the authors to explore which of the euro's troubles were foreseeable, and which have only become apparent ex post.

According to the emerging academic consensus (see Baldwin and Giavazzi, 2015), the inability of EMU members to conduct their own interest- and exchange-rate policy is responsible only for part of the Eurozone's woes since 2010. The unchecked buildup of large internal imbalances during the boom years, fragile banking sectors, and the absence of national lenders of last resort created the conditions for sudden stops in capital flows, which amplified the crisis. Yet, as Frankel (2015) recalls, economists' initial appraisals of the EMU paid little attention to the issues of debtor moral hazard and the appropriate bank supervision framework in currency unions. On a pessimistic reading, the euro proved more crisis-prone than economists anticipated in the 1990s. More optimistically, the crisis has laid bare underappreciated weaknesses of the single currency, which initiatives such as the fledgling European banking union now seek to address.

Elliott and Atkinson are skeptical about the ability and willingness of the Eurozone to take the steps towards further integration that are required to complete the euro. They strongly believe the time has come to ditch the single currency. To them, the seeming unwillingness to abandon a failed project is yet further evidence of the European Union's frustrating inflexibility. At no point do they take a stance on what a "progressive" alternative to the euro should look like—or, at least, a more workable one.

Devising the former might be too tall an order, but even the setup of the latter is far from clear: a fixed exchange-rate regime, floating currencies, or some hybrid thereof all present their own challenges to a group of countries seeking closely integrated goods and factor markets. Furthermore, as the authors readily admit:

> All of these options would be costly, especially the demise of the euro. But all breakups are painful and expensive (p. 256).

Since this is so, perhaps the EMU partners can be forgiven for trying to "work on it" first.

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I Health, Education, and Welfare

The Oxford Handbook of Economics and Human Biology. Edited by John Komlos and Inas R. Kelly. Oxford and New York: Oxford University Press, 2016. Pp. xiv, 831. $150.00. ISBN 978–0–19–938929–2, cloth. *JEL 2016–1850*

The *Oxford Handbook of Economics and Human Biology* explores, and to some extent defines, the field of economics and human biology. This field has expanded massively over the past decades, and focuses on how economic conditions affect human biological outcomes and how biological outcomes affect economic processes. The research questions overlap with the field of health economics; however, this book is less concerned with health and health systems, and more focused on measurable aspects of the organism, e.g., height, weight, blood pressure, and birth weight.

The book is a collected-papers volume of thirty-eight papers, where each paper provides an overview of a topic of interest. Although the authors of these chapters are mainly economists, the handbook is interdisciplinary and seeks to be relevant also for anthropologists, historians, biologists, biochemists, physicians, environmentalists, and researchers in public health. Hence, it includes contributions from authors across different disciplines.

The book is divided into four parts. The first part aims to introduce the reader to the topic of economics and human biology. This part contains an introductory chapter followed by four chapters that provide a background on anthropometrics. The final chapter of the first part is by Gregory Coleman and Dhaval Dave and explores econometric methods in economics and human biology. The focus of the chapter is on identifying causal impacts of anthropometrics on economic outcomes, and vice versa. This chapter is a nice introduction to the many challenges faced by applied economists in this field of research, as it summarizes the main challenges and potential solutions. It is well-written and is therefore recommended for researchers that want to conduct empirical studies in economics and human biology.

The second part of the handbook is on biological measures as outcome variables. This part describes how variables such as height, body mass index (BMI), and biological well-being vary with changing economic conditions. The chapter also, however, explores the development of anthropometric measures, like obesity and height, over time and within subpopulations. This part of the book illustrates the *interdisciplinary nature* of this field of research and its wide-reaching consequences. For example, one chapter looks at slave heights, while another addresses cross-country variation in income inequality and children's health. This section contains well-written papers, though it could have benefitted from a theoretical model to link the different topics and simplify a complex area of research. An additional challenge in this field is to identify causal relationships, as discussed in detail by Chad D. Meyerhoefer and Muzhe Yang in their chapter on poverty and obesity.

The third part introduces the concept of using biological measures as determinants of monetary outcomes, productivity, and welfare. Many of the chapters discuss both how biological measures may influence economic outcomes, and how economic outcomes may influence biological measures. These are not new questions; however, a growing number of datasets now contain various measures of genetic, functional, and hormonal biomarkers, which economists are starting to integrate into their analyses. Stephen F. Lehrer opens this part by clarifying the distinction between biological time-varying measures such as hormones and biological time-invariant measures such as DNA. The chapter addresses how such properties of biomarkers can be used to answer specific research questions. The following two chapters by George L. Wehby and Jere R. Behrman discuss twin studies and the use of genes as inputs, and in doing so, they introduce central econometric methods in the field of economics and human biology.

The next series of chapters in this part discuss investment in health. For example, Harold Alderman and David E. Sahn describe the impact of nutrition on productivity. The book then turns to a much researched topic, namely, monetary outcomes of biological attributes. Jane Greve discusses the classical question of whether or not there is an impact of obesity on income and wealth. This is followed by two chapters that discuss the effect of obesity on income inequality across countries, and the impact of height on wages.

The book then discusses early childhood influences on later life outcomes. This topic has received attention in economic research in the past decades, and some of this research is discussed in the book. However, this section could have been improved with more references to conceptual frameworks, like the ones discussed by Heckman (2007) and Currie and Almond (2011). The final chapter in the third part is concerned with neuroeconomics, where Jason A. Aimone and Daniel Houser discuss how neuroeconomics can shed new light on a number of economic theories.

Up until now, this book reflects the fact that most of the research on this topic has been funded by high-income countries and is consequently conducted from the perspective of high-income countries. The final and fourth part of the book widens this perspective by including regional studies in economics and human biology. In particular, the chapter by Alexander Moradi and Kalle Hirvonen underlines country-specific effects by exploring the phenomenon of tall African adults despite low national income. Then the focus shifts to countries that are not necessarily low income, by the inclusion of chapters with different clusters of countries in Europe, Asia, and Latin America. The discussions in these chapters focus not only on how economic factors shape human biology, but also the interaction between human biology with genetics and cultural factors. The different chapters serve to underline factors other than gross domestic product as indicators of human development.

Overall, the book effectively discusses and illustrates some of the main challenges in economics and human biology. It is difficult to establish causality and there is a need for theoretical models. When these parts are further developed, the field will become more transparent. As for now, identifying the direction of causality between the different economic and biological factors remains a central challenge.

The interdisciplinary nature of this book has the effect that it introduces readers from any field to new ways of thinking about the interaction between economics and human biology. When reading the content list, this book might come across as a collection of papers that are more-or-less connected. However, when reading the chapters, the reader will most likely have a different experience. The chapters seem to go well together and are well-written, which makes the book enjoyable. In addition, the book summarizes many of the main research topics and illustrates the challenges faced by this field. Hence, I would recommend this book to any reader who is interested in economics and human biology, although, this book is especially interesting for researchers who plan to conduct research in this field.

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O Economic Development, Innovation, Technological Change, and Growth

Managing the Macroeconomy: Monetary and Exchange Rate Issues in India. By Ramkishen S. Rajan and Venkataramana Yanamandra. New York: St. Martin's Press, Palgrave Macmillan, 2015. Pp. xvii, 210. $105.00. ISBN 978–1–137–53413–2, cloth. *JEL 2016–0374*

Managing the Macroeconomy: Monetary and Exchange Rate Issues in India is a book written with passion and commitment. The authors are well known in the field of exchange-rate and monetary policy. They have painstakingly incorporated their decade-long research efforts, which is clearly evident in the references the authors have attached at the end of each chapter.

In a nutshell, the book in its six chapters unfolds the macroeconomic management of the Indian economy, highlighting some of the key and critical issues pertaining to monetary policy and exchange-rate management in India. The issues include: (1) the effectiveness of monetary policy, (2) interest-rate pass-through and its impact on inflation, (3) exchange-rate and reserve management, (4) J-curve analysis, and (5) sources and stability in external financing and foreign-direct investment. The arguments put forth by the authors have been well designed and sequenced and also have been supported by and vindicated with strong empirical analysis. In order to arouse interest among the readers in the preface itself, the authors have provided a chapter-by-chapter synopsis.

The design of chapter one is well documented. It starts with the key macroeconomic concepts relevant to India, viz; growth, inflation, and balance of payments, followed by management of these by monetary and exchange-rate policies. The analysis of fiscal sustainability (Annex 1.1) has suggested "regaining fiscal discipline." The authors opine that with fiscal policy relatively handicapped, "the monetary policy has become the important stabilization tool for India via its impact on exchange rate and interest rate." The authors therefore have examined the effects of monetary and exchange-rate changes and their impact on Indian Economy.

While discussing the effectiveness of monetary policy, the authors have presented the transmission mechanism of monetary policy through an important channel, i.e., interest rates. It is pertinent to note that this topic is also currently being debated in India in many fora. The authors have carried out empirical research to quantify the extent of pass-through to the lending rate. In this context, the suggestions given by the authors are: (1) development of a stable and liquid yield curve and (2) development of a debt market.

The discussion on exchange-rate and reserve accumulation led the authors to comment that India has managed the impossible trinity, not by moving to corner solutions, but by adopting an intermediate approach. Following the conventional wisdom, the authors have concluded that, "The RBI [Reserve Bank of India] has been managing the trinity by intervening asymmetrically to generally prevent appreciations of INR [Indian rupee], but not necessarily depreciations, hence accumulating international reserves in the process."

The chapter on impact of exchange-rate pass-through on inflation in India has both conceptual and empirical rigor. The literature survey on the subject is well narrated. The authors also discuss the J-curve effect. The research question the authors have addressed in this regard is: Does the J-curve phenomenon hold for India at the aggregated and disaggregated country as well as sectoral levels? The empirical results suggest that INR depreciation has helped in improvement in trade balance at the aggregate level. However, it varies from country to country. More interestingly, both at aggregate level and at sectoral level, the authors do not find any J-curve effect. Nevertheless, the authors opine that real exchange-rate changes could be used to facilitate an improvement in India's trade balance.

The authors also devote a chapter to external financing in India. This chapter is comprehensive but does not truly reflect the monetary-policy impact. The objective of this chapter has been to highlight the Make In India initiative of the government. Some reflection on capital flows and exchange-rate movement would have been contextually befitting to the theme of the book, monetary policy and exchange-rate management. Nevertheless, even as a stand-alone piece, the discussion is rich in content, particularly in the note on India's outward foreign direct (OFD) investment flows (Annex 6.2).

The subject of monetary policy in India has undergone a metamorphic transformation in terms of its objective, operating procedures, and operating target. The transmission mechanism of monetary policy, which remained as a black box in the past, is now highly transparent with policy repo rate becoming the short-term signaling rate and the overnight weighted average call rate being aligned to the policy repo rate through the liquidity management by the RBI. Furthermore, the flexible inflation targeting (FIT) with the objective of a floor (2 percent Consumer Price Index inflation) and ceiling (6 percent of Consumer Price Index inflation), coupled with the Monetary Policy Committee (MPC) taking a view on policy repo rate and monetary management, have added a new challenge and also an opportunity to monetary-policy making. In addition, the introduction of a term segment of repo as a monetary policy instrument has provided a new avenue to monetary-policy management. These are issues the authors may consider in the revised version.

Notwithstanding the observations in the preceding paragraph, overall, the book has covered the important macroeconomic policy issues with a strong conceptual framework, comprehensive literature survey, and robust empirical analysis. The book is strongly recommended for researchers, students, policy makers, practitioners, and analysts interested in India's macroeconomic management—particularly monetary and external-sector management.

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The Politics of Innovation: Why Some Countries Are Better Than Others at Science and Technology. By Mark Zachary Taylor. Oxford and New York: Oxford University Press, 2016. Pp. xiii, 427. ISBN 978–0–19–046412–7, cloth; 978–0-19–046413–4, pbk.; 978–0–19–046414–1, e-book; 978–0–19–046415–8, e-book; 978–0–19–060925–2, online component. *JEL 2016–1498*

During the last two to three decades, innovation has become a more central issue on policymakers' agendas, and the interest in innovation policy has increased a lot. The agendas of policy makers and researchers have also been broadened, from focusing mainly on technological advances in "high tech," to also include innovation in services, organizational innovation, social innovation, and innovation in poorer environments/countries, which—although less spectacular technologically—may be very important economically. To improve the knowledge base for policy making, many countries, also developing, regularly conduct surveys in which firms are asked to identify factors that support or hamper their innovation activities. Dedicated public-sector organizations focusing on innovation support have been established in many countries. Innovation policy has also attracted the interest of the OECD, which during the last decade has produced numerous surveys of how innovation policies evolve in different nations, with particular emphasis on the challenges for policy and governance in this area.

One might have expected a book on *The Politics of Innovation* to engage with these recent trends, but this is only the case to a quite limited extent. As explained in part one of the book (and discussed in more detail in the appendices), the author operates with a rather narrow perspective on innovation, focusing mainly on (radical) technological product and process innovations, i.e., "high-tech" innovation, which he prefers to measure through patents (adjusted for quality differences as reflected in citations). Armed with this methodology, he then goes on to explore the innovation (or science and technology) capability of various countries, which he finds to vary a lot, and much more than their levels of income (or productivity) would indicate. However, it is common knowledge that patents are awarded for invention (new ideas for how to do things) and not for innovation (implementation of new ideas in practice), are much more common in some technological and industrial fields than in others, and tend to concentrate in a limited number of rich countries. Thus, it is possible that the very skew distribution that he observes may have less to do with innovation as such than with the specific measure he chooses to employ.

The remainder of the book is devoted to the explanation of the observed cross-country differences in patenting (and to some extent related measures) for a sample of (mostly) developed countries. The starting point for the discussion of this topic in the second part of the book is the classical market-failure theory of the 1960s, on the basis of which five so-called "pillars of innovation"—property rights, research and developement subsidies, education, research universities, and trade policy—are identified. The method employed is to collect statistics for these pillars and explore the correlation with patenting through scatter plots. Many will probably find this method a bit simplistic (and deficient when it comes to discussing causality, etc.). Nevertheless, it is shown that with the exception of trade policy, the correlation is high in all cases, and it is claimed (but not documented) that the pillars collectively explain some 90 percent of the variation in innovation as measured by patenting. However, despite the high correlation, there is also some variation in how countries perform on the various pillars, leading the author to conclude that there is no unique combination of science and technology investments that guarantee success in innovation.

The author then goes on to explore in the same manner as before the possible impact of other factors that have been central in recent research on economic growth and development, such as degree of democracy, political decentralization, etc., and concludes that there is little evidence suggesting that these other factors matter much. He also expands the discussion of possible explanatory factors by extending the "market failure" perspective to also include possible "network failures," which he argues may be important, and he illustrates the relevance of the argument using historical evidence from a limited number of countries (mainly Israel, Taiwan, Ireland, and Mexico).

The third part of the book is perhaps the most original. Here the author presents, aided by a blend of statistical research and historical case studies, his so-called "creative insecurity" theory of innovation. The argument is that the distribution of resources toward different ends in a country depends on the economic interests of powerful domestic actors, and that in such a setting public investments in science and technology are not likely to get high priority, unless powered by substantial external threats of a military or economic nature. This is an interesting idea. It is easy, from US history for example, to find examples of investments in cutting edge science and technology financed by the military. On the other hand, it hardly explains, as the author concedes, why peaceful Switzerland is a top performer in "science and technology," so there probably is more to it than that. However, the author is undoubtedly right in pointing out that the political economy of science, technology, and innovation policies requires more scholarly attention, not only from economists, but also from political scientists, historians, etc.

The title of this book may be a bit misleading, and readers interested in the evolution of innovation policy as a new field of politics may have to look elsewhere. However, it is a highly readable, well-documented, and well-argued contribution to the literature on comparative economic development, which many readers may find interesting and thought provoking.

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