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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.

B History of Economic Thought, Methodology, and Heterodox Approaches

The Provocative Joan Robinson: The Making of a Cambridge Economist. By Nahid Aslanbeigui and Guy Oakes. Science and Cultural Theory series. Durham and London: Duke University Press, 2009. Pp. x, 302. $84.95, cloth; $23.95, paper. ISBN 978–0–8223–4521–3, cloth; 978–0–8223–4538–1, pbk. *JEL 2009–1173*

Who was Joan Robinson?

When Joan Maurice went up to Cambridge to study economics at Girton College in 1922, women had just been allowed degree titles at the University but only the following year were they admitted to the University Library and University Lectures. They also became eligible for University teaching posts and membership of Faculties and Boards of Faculties but had to wait until 1948 to be granted full membership to the University of Cambridge.

No more than 500 women were accepted as students at any one time; the class lists of candidates to the Tripos—the Cambridge examination ending the three-year honors degree course—were to be given separately. A separate room was provided for women students to sit their exams but, in each honors class, the same standard was set for men and women.

It was through Alfred Marshall—who engineered the establishment of a separate Tripos in 1903—that economics became an autonomous and respectable academic discipline in Cambridge. Marshall *was* economics and the "*Principles* were the Bible" (Joan Robinson 1951, p. vii). When Joan Maurice came to study economics, however, it was in the form taught by Arthur C. Pigou, who had worked the hard core of Marshall's analysis into a system of static theory.

After two years in India, where she had followed her husband Austin Robinson to a job as tutor of the Maharajah of Gwalior, Joan, now Robinson [henceforth JR], came back in 1928 and settled in Cambridge. There she made acquaintance with two people who were to become pivotal in her intellectual and emotional life—Richard Kahn and Piero Sraffa. The occasion arose when she was attending Sraffa's lectures, which the Italian economist, rescued by John Maynard Keynes from Mussolini, had started giving that year. Sraffa's course was also followed by Keynes's favorite pupil Kahn, who was preparing his fellowship dissertation on the *Economics of the Short Period* (Kahn 1989). The work remained unpublished for many years but it has been recognized as a seminal contribution to the theory of imperfect competition and what eventually became Keynes's "short-period method."

Many years later, JR gave a vivid account of the impact of Sraffa's lectures: "[they] were penetrating our insularity. He was calmly committing the sacrilege of pointing out inconsistencies in Marshall" (Robinson 1951, p. vii). At the core of Sraffa's critique of the Marshall–Pigou apparatus was the assumed symmetry of demand and supply in the determination of relative prices of individual commodities produced in competitive conditions.

The economic debate of the late 1920s revolved around these issues in Cambridge. It started with an opening round of controversies over increasing–decreasing returns definitions of industries and was followed by a second round of contributions over the more general issue of the consistency and realism of Marshallian supply and demand analysis. In 1930, a symposium in the *Economic Journal* on the representative firm added fuel to the controversy.

JR's appearance as contender in this arena was greeted with wonder. "Who is Joan Robinson?" Gottrieff Harbeler asked Kahn, after reading an article signed with that name in the *Economic Journal* of December 1932 on rising supply price and added: "The Christian name sounds like a woman's, but the article seems to me much too clever for a woman."

Defender of Marshall's Box of Tools

JR's first publication was a contribution to the debate on Marshallian tools of analysis. *Economics is a Serious Subject,* anonymously dedicated to Sraffa, dealt with the questions raised in those discussions with a more general aim in mind, i.e., to defend the methodology of making unrealistic assumptions against the charge of the mathematician, who would defend logic against realism, and the charge of the plain man, who would do exactly the opposite (Robinson 1932). In this pamphlet, while Sraffa was cast as a "fundamental pessimist," she would prefer to label herself, together with Kahn and Austin Robinson, as an "analytical optimistic"—one who will make hypotheses acknowledged to be heroic in order to be able to give formal treatment to an economic problem.

When *Economics is a Serious Subject* was published, the book destined to bring her fame and academic respectability, *The Economics of Imperfect Competition*, had already been completed. As in the case of Kahn's dissertation, the starting point of her book was Sraffa's proposal "to re-write the theory of value, starting from the conception of the firm as a monopolist" (Robinson 1969, p. 6); its aim was to extend the marginal technique to all market forms and respond to the challenge posed by Sraffa.

JR sought to demonstrate that, if either factor heterogeneity or factor specialization were allowed for, the supply curve for a single industry could—contrariwise to Sraffa's claim—in fact be rising. In his 1926 article, Sraffa's point was that factor supply, although fixed in the system as a whole, may be considered infinitely elastic for an industry since increasing costs are confined to the rare case of an industry in which there is a specialized factor employed exclusively by that industry (Sraffa 1926). She set out to find cases in which an industry uses a specialized factor and provided a classification of such cases, her argument being that, since a priori intermediate cases between perfectly elastic and perfectly inelastic supply of a factor cannot be ruled out and since they can effectively be found in reality, there was no reason why they should be dismissed as irrelevant. Her approach is neatly summed up in her letter to Sraffa of 1931: "I am not trying to defend Marshall and his knife handles. I do not mind how few the cases of I[ncreasing] R[eturns] there are as long there are some on which I can use our ingenious analysis of monopoly under I[ncreasing] R[eturns]."

Sraffa had questioned the Marshallian assumption of perfect competition in the presence of increasing returns and the asserted independence of demand and supply schedules. Robinson did not take these points on board as implicating the abandonment of Marshallian theory; rather, she was looking for a form of apparatus which could be made consistent with ad hoc assumptions.

Apostle of the Keynesian Revolution

Although JR openly disowned her *Economics of Imperfect Competition* only in 1953, as from 1934 she sidelined these issues, throwing herself wholeheartedly into the Keynesian Revolution. This she began by assisting Keynes in the transition from the *Treatise on Money* to the *General Theory* and then set about elaborating, popularizing, and defending its message.

Her involvement started when she helped Kahn to finish the index of the *Treatise on Money* in September 1930 and became deeply involved in the activities of the Circus—the group of young economists who met between January and June 1931 to discuss Keynes's book. She wrote a paper, "A Parable on Saving and Investment," where she attacked Keynes's main argument in the *Treatise* as "tacitly assuming that output was unchanged" (Robinson 1933, p. 82).

In the Lent term of 1932, while attending Keynes's lectures, some of the Circus members—notably Kahn and Joan and Austin Robinson—signed a Manifesto in which they challenged certain propositions asserted by Keynes in his lectures. They argued that Keynes's "new" argument that an increase in investment leads to an increase in output requires that output of consumption goods be increased. This was a generalization of the mechanism presented in Kahn's multiplier article of the previous year (Kahn 1931), which studied the effects of increased investment on the demand for consumption goods and their prices, where the latter depend on the elasticity of the supply curve of the consumption goods (Maria Cristina Marcuzzo 2003). They went on to state that the appropriate method to tackle the problem was "Supply and Demand." Keynes was persuaded. Perusing his Autumn 1932 lectures—whose title was changed from "The Pure Theory of Money" to "A Monetary Theory of Production"—we begin to come across the expression "demand as a whole relatively to supply as a whole" (Keynes 1979, p. 53).

Kahn and JR influenced the introduction of the method of supply and demand in the argument of *The General Theory* and, in particular, the use of the short-period supply curve derived in conditions of a given degree of competition. This was the result of their common belief in the validity of the Marshallian apparatus (supply and demand plus marginal analysis), generalized in the work they had done in *The Economics of the Short Period* and *The Economics of Imperfect Competition* and extended to deal with the effects on prices and output of consumption goods following an increase in investment.

In "The Theory of Money and the Analysis of Output," published in October 1933, JR set out her criticism of the *Treatise* ("This argument is valid upon the assumption that an increase in demand for consumption goods leads to no increase in their supply"; see Robinson 1951, p. 55), urging Keynes to take the analysis to its conclusions, i.e., that "output may be in equilibrium at any number of different levels" (Robinson 1951, p. 56).

In the following months, she became more closely involved in the development of Keynes's work and was among the recipients of the first proofs of the *General Theory*, which she commented on in June 1935. Shortly afterwards, she wrote some essays drawing riders from the *General Theory* (which were published as *Essays in the Theory of Employment* in 1936), subsequently embarking on the project of writing a version of the *General Theory* suitable for teaching to first-year students; this was to be her *Introduction to the Theory of Employment* (1937).

By that time, she had become an apostle and a proselytizer of the Keynesian revolution, eager to teach and propagate it. She was accused of being fiercely opinionated, one-sided, and bad-mannered and, as a result, her path to acceptance in the Faculty of Economics of Cambridge was littered with obstacles. Despite an impressive record of publications, she was made Full Professor only in 1965.

Heroes, Apprentices, and Innovative Thinkers

A segment of this story, from the time when JR was invited to give eight lectures at the Faculty of Economics (1931), through the two years as Probationary Faculty Lecturer until she was at last made Faculty Lecturer (1938), constitutes this book's timeframe and object of inquiry. Its scope is summarized by the authors as follows: "This book is an investigation of the circumstances under which [JR] achieved recognition as an innovative thinker and became a leading figure in the most exciting theoretical movement of the time" (p. 50).

This passage captures the letter rather than the spirit of the book—its contents rather than the motivations behind it, which are best revealed in an observation the authors make in the Introduction: "In the received historiography of interwar economics at Cambridge the favoured genre is the epic . . . Homeric adventures of ideas" (p. 15). The authors aim to counteract the "mythical" epic with a less glamorous and more realistic narrative, recounting the saga of master–apprentice relationships, admissions rituals and membership criteria, the dominance of patronage and social network: the small world of a guild of metiers.

JR's activities in the 1930s are seen as a process of career production and professional identity construction, to be examined in terms of strategies and tactics to achieve the objective. In understanding the Cambridge economics of the time, they say, the unit of analysis is not the heroic single theoretician, but "an epistemic community: the Marshallian guild."

Is our knowledge enriched, our understanding of the times enhanced, by this switch in approach? A distinction has to be made here between the material that the book offers the reader—the primary sources—and the interpretation the authors make of it. There is no doubt that their archival research is impressive. My only qualm is that they give no information as to when they are quoting a hitherto unpublished source. In any case, they dwell on JR's correspondence up to 1938 more extensively than anyone before. They have managed to reconstruct episodes and circumstances in her life (such as the fateful weekend of October 1938 that led to her being hospitalized as a psychiatric patient for six months) and make sense of a number of scrawls in the letters of this period over which many have labored (see Marcuzzo and Annalisa Rosselli 2005).

The chronicle of events is lucidly narrated with a wealth of footnotes and background references, meeting a high standard of historical investigation. Similarly, their reconstruction of Cambridge academia—the "research as dialogue" style—is full of informed and illuminating details. Their prose is refined, clear, and enthralling. Still, I cannot find myself in accord with their picture of JR and the Cambridge group of economists with whom she was mostly associated. They leave too much out by insisting on the guild analogy and, as is often the case with analogies, the resemblance is carried too far. Let me give two examples to explain why I find their account wanting in some respects.

Firstly, the reader is not provided with a full understanding of the *intellectual* divide between Sraffa and Robinson, which may help to explain why Sraffa rapidly abandoned imperfect competition and why, twenty years later, JR disowned it. In the 1930s, far from rejecting the postulates of Marshallian theory she defended them; in the early 1950s, when she became aware that Sraffa's scientific project meant a return to classical political economy as a radical alternative to the Marshallian method and theory, she was one of few among the Cambridge economists to endorse it, although she was never able to convince Sraffa that she understood it (Marcuzzo 2005).

Secondly, the authors seem unconcerned about which aspects of Keynesian economics were high in JR's agenda in the aftermath of the *General Theory*; this is a pity, for they may help to explain why she is considered the midwife to post-Keynesian economics. She was the first to recognize that, in some respects, Kalecki's framework was superior to Keynes's and made an early attempt to enlarge the scope of analysis to incorporate technical change, innovations, and changes in income distribution. Rather than following Hicks's reconciliation of Keynes with "classical theory" with his restatement of the *General Theory* in terms of IS–LM, she attempted to bridge Keynes, via Kalecki and Sraffa, to Marx and the classical authors.

Cambridge economics was not a unified body of thought and Cambridge economists, while valuing intellectual partnership, dialogue, and criticism, had each their own agenda, reflecting their political, social, and individual concerns. The bond was not a guild but a community sharing values and interpersonal commitments.

While Schumpeter's view of them as accustomed to "throw their ideas into a common pool" is too rosy a picture, the authors of this book come close to parody when they claim that "Cambridge economists were operators of small scale intelligence networks that exercised surveillance over colleagues and extracted intellectual resources that could be employed to advantage" (p. 148).

The network of personal, emotional, and intellectual bonds interwoven in the texture of Cambridge economics, which fascinated and attracted many scholars well into the 1970s, is downsized to a world of strategic moves that need to be scrutinized and explained in terms of payoffs—repetitive and zero-sum games.

Are we any the wiser by being told that JR played the academic game according to the rules of Cambridge (different but certainly not barren from the Chicago, Vienna, Harvard, or the LSE of yesterday or of today for that matter)? Is the Cambridge tradition of economics made less inspiring by the discovery of its embedment in the social construction of doing economics of its time? Are those whom Roy Weintraub epitomizes in the blurb to the book as "individuals engaged in defending the Cambridge tradition" to be unmasked as worshippers of false idols?

I expect this book will raise dust but I fear it hardly suffices to do justice to the images of Joan Robinson, and of Keynes, Kahn, and Sraffa, and their standing in the history of economic thought—neither masters and apprentices nor Homeric figures and heroes but innovative thinkers and human beings, more gifted than most.

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C Mathematical and Quantitative Methods

Experiments in Economics: Playing Fair with Money. By Ananish Chaudhuri. London and New York: Taylor and Francis, Routledge, 2009. Pp. xviii, 249. Paper. ISBN 978–0–415–47630–0, cloth; 978–0–415–47631–7, pbk.
JEL 2009–1193

Early in his new book, *Experiments in Economics: Playing Fair with Money*, Ananish Chaudhuri states that ". . . this book is written for people with no background in economics" (p. 1). Instead, it is intended for individuals who,

". . . have an inquisitive mind and are open to new ideas and thoughts" (p. 1). These sentences are critical to understanding the purposes of this book as well as its usefulness. In the past quarter century, experimental economics (and more broadly behavioral economics) has gone from being an oddity on the fringes of mainstream economics to a major component of the field. Insights from experimental economics have fundamentally changed how economists think about diverse topics such as the provision of public goods, efficiency wages in labor markets, and equilibrium analysis in game theory. Not surprisingly, the rising importance of experimental economics has led to a number of attempts to survey part or all of the existing literature (e.g., John H. Kagel and Alvin E. Roth 1995, forthcoming; Colin F. Camerer 2003; Charles R. Plott and Vernon L. Smith 2008). Most of these surveys are aimed at professional economists or, in the case of Charles A. Holt (2007), are intended as textbooks for undergraduate classes.

As the opening quote indicates, Chaudhuri is not trying to write a book for professional economists. Critically, he is also not trying to write a textbook on experimental economics. The book does not describe the nuts and bolts of how an experiment is run, skips many areas that are of central interest to experimenters (e.g., there is no material on auctions, markets, or pure game theory), and does not provide an exhaustive survey of the areas he does cover. Instead, Chaudhuri picks a small number of topics (the ultimatum game, trust games, public goods game, social dilemmas, and coordination games) that largely share a common theme—other-regarding behavior. In these games (other than the coordination games), subjects regularly eschew monetary gains to impose greater fairness, punish those who have harmed them, and reward those who have been kind. The robust occurrence of other-regarding behavior is arguably the single most important finding from experimental economics, making it a natural focus for Chaudhuri. Focusing on other-regarding behavior is also a smart choice because the everyday nature of the dilemmas and emotions evoked by these games makes the material easily accessible for a general audience. Focusing on this subset of experimental economics, Chaudhuri writes a book that anybody could read. His goal is not only to explain what experimental economists have learned about other-regarding behavior in various settings but also to make it clear to readers who are not professional economists why these insights are important and why experimental methodology is a good tool for gaining these insights.

For each topic, Chaudhuri follows the same basic approach. A few real-world examples (or at least nonlaboratory, as many are drawn from books and movies) are offered to illustrate the main issues. For example, trust games are introduced with a story about how Chaudhuri asked a cab driver to wait outside *without having been paid* while he ran inside to check the location of a job interview at the ASSA meetings. Having laid out the main issues, Chaudhuri then introduces the bones of the relevant economic theory. He takes pains to not make these explanations overly abstract. The battle of the sexes, for example, is introduced in terms of a short story by O'Henry (The Gift of the Magi) with specific payoffs used to capture the emotional states of the main characters. Chaudhuri carefully explains why the theory captures something essential about the nonlaboratory examples and also why there are elements of the real world situation that are missing from the theory. This is typical of the book's general approach. Chaudhuri assumes that the reader genuinely wants to understand how economists think about the matter at hand but doesn't assume a large base of preexisting knowledge about economic theory or experiments. The bulk of each chapter is then devoted to discussing the experimental literature. Instead of providing a laundry list of papers and results, Chaudhuri tells a story. Starting from one or two seminal papers, he shows how the initial papers raised new questions that led to new papers (and yet more questions). The goal is not just to explain the topic at hand but to provide the reader with a general understanding of how the research process works. A nice feature of the literature reviews is Chaudhuri's willingness to include papers that weren't published in A-list journals. In one subsection, discussing the relationship between trust and expectations, the primary papers discussed come from the *Journal of Economic Psychology*, *Experimental Economics*, and the *Southern Economic Journal*. These may not be journals that make tenure committees jump for joy but all of the papers feature carefully designed experiments that made an important contribution to clarifying the relationship between trust and expectations. Even readers who are intimately familiar with the research topic being discussed may find an important paper or two they had previously not been aware of. Each chapter concludes with a short section stressing the links between laboratory experiments and concerns drawn from real world examples.

Chaudhuri has produced an extremely readable book that not only explains what experimental have done in studying fairness but also helps readers who aren't insiders understand why they should care. Ultimately, the main question a reviewer has to ask about Chaudhuri's book is who should read it? It isn't the right book to introduce graduate students to experimental economics, is not appropriate to be used as a textbook for an undergraduate class, and is unlikely to knock the latest iteration of *Freakonomics* out of the best-seller lists. What this book is perfect for is giving interested readers who are not professional economists a flavor of what experimental economics does and why it is important. It is the book I gave my mother when she wanted to understand what I was always babbling about (she loved it!) and the book I use as the source of an additional reading for my intermediate microeconomics class when we play an ultimatum game in class. Economists who want a nontechnical treatment of experimental economics to give them a flavor of the field will find this book useful as well.

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F International Economics

Tournament Approaches to Policy Reform: Making Development Assistance More Effective. By Clifford F. Zinnes. Washington, D.C.: Brookings Institution Press, 2009. Pp. xxii, 369. $26.95, paper. ISBN 978–0–8157–9719–7. *JEL 2009–1266*

Development assistance looms large in the public discourse and foreign aid remains squarely on most policy agendas concerned with growth, poverty, and inequality in the developing world. Nevertheless, the role of foreign aid remains highly controversial. In my assessment, aid has had, on balance, a positive and statistically significant causal effect on growth over the long run with point estimates at levels suggested by growth theory (Channing Arndt, Sam Jones, and Finn Tarp 2009). But this in no way precludes that concerted efforts must be made by all involved (donors and recipients) to make international development assistance more effective—and this is even more so in the wake of the global financial and economic crisis, which is bound to make development assistance an even scarcer resource.

Much of the literature on foreign aid is couched in macroeconomic terms. Yet, aid and lending relationships involve complex contractual and agency relationships that are essentially microeconomic in nature. In a paper for the World Bank ABCDE conference a few years ago, I therefore argued that: "conceptual innovations in modern microeconomic theory should be enlisted to improve aid effectiveness" (David Roland-Holst and Tarp 2004). In addition, the closely associated literature on decentralized development (Pranab Bardhan 2002; Jean-Philippe Platteau 2003) is both challenging and indispensable reading for anyone interested in the necessity and intricacies of local participation in development. It was therefore with great enthusiasm that I embarked on reading the volume under review, eager to understand the current literature and guidance on tournament approaches and their role in improving aid effectiveness.

The overall aim of the book—as stated by the author—is to present and assess a newly emerging class of foreign aid delivery, called prospective interjurisdictional competitions (PIJCs), designed to overcome obstacles related to local ownership and insufficient funding. Essentially, this amounts to approaching intervention design as a "game" with prospective rules and payoffs, strategies and beliefs in which a predefined group of players must compete to achieve the best implementation. Put more simply, the author is concerned about how to introduce more competition in the allocation of scarce resources used to promote policy reform. Several types of PIJCs are identified and the book is organized around three chapters reviewing, respectively, certification, tournament, and other relevant experiences (chapters 3, 4, and 5). Chapter 3 covers simple and pecuniary certifications, chapter 4 looks at what is referred to as pure and mixed tournaments, and chapter 5 gives some additional examples of related mechanisms in a greater variety of sectors. A total of twelve case applications are carefully discussed, ranging from "simple and fast" deregulation in Romania, to public services report cards in Jharkhand in India, the Kecamatan development program in Indonesia, and the fiscal federalism and regional fiscal reform project in Russia. A foreword, an introduction (chapter 1), and an overview of the PJIC approach (chapter 2) provide the reader with background before the case material, which is followed by synthesis and analysis (chapter 6) and conclusions and scope for future applications (chapter 7)—plus a variety of appendixes with supporting material.

On the face of it, all this would seem to provide both a conceptual framework to think about the issues involved and practical examples that enable a deeper analysis of how to make development assistance more effective based on microeconomic theory and reasoning. The book is well written. It contains a lot of stimulating insights into the intricacies of policy reforms in a wide range of country and sector contexts. There is also lots of food for thought that will be useful for both academic economists and development practitioners.

Upon closer examination, however, there are a number of disappointments and the present reviewer must admit that my original enthusiasm became tempered in the review process. First, the volume is not really focused on "tournaments" and "the effectiveness of foreign aid." Instead, the definition of the class of measures and the range of policy reforms covered are so broad that the whole exercise becomes, yes, fluffy. The essential message would seem to boil down to the observation that incentive compatibility matters and that the preferred choice among the different implementation approaches depends on initial conditions, number of recipients, and objectives involved. This is neither new nor surprising. It may be a useful change of language and a different way of putting the problem but not really a change in underlying insight. And throughout, actual practice in international development assistance is glossed over in a way that is bound to provoke the practitioner. For example, much is made of the fact that aid agencies in some cases use input-based performance rewards as if this were a new observation and a fatal shortcoming. Frankly, aid agencies will of course in some cases use this modality but this does not *prove* that aid agencies are not concerned about ultimate impact. If a tighter, focused set of new measures could be identified, then maybe the reader could be convinced that there is something new here that could help make foreign aid more effective than is presently the case. But as it stands, what is included is really little more than a recount of existing challenges and a careful and interesting review of the existing toolbox of potential reform modalities.

A second disappointment—and a somehow paradoxical one given the broad range of modalities considered—is that the author tends to conceptualize development and policy reform as a project design/implementation problem. This applies mostly to the tournaments, but the dividing line between these approaches and other forms is often blurry. This means that what is being proposed is only likely to be relevant to a small subset of development and reform situations. It is often implicitly assumed that the sponsor (donor) knows exactly what is to be achieved as well as how and why development interventions work. Not so in real life, where donors often do not have well-defined objective functions and where much remains to be learned about what works. Yet, this is not the analytical point of departure of the author. Instead, focus is on design problems assuming away many of the fundamental challenges in development and development aid.

A third and final critique is a lack of attention to the downsides of competitive solutions.

I believe in competition, but the lessons from developed countries about the introduction of market-like mechanisms in public service provision are far from rosy. I believe the author is fundamentally on a problematic path when it is noted that "the tide has definitely turned toward the view that donors must only supply funding to cost-effective projects and that this priority takes precedence over project opportunities from less capable countries, which unfortunately tend to be the more needy ones. On the other hand, this tournament approach does achieve the goal of identifying the more capable target countries" (p. 139). In making these statements—which refer to the "good policy" criteria used by the Millennium Challenge Corporation (characterized on p. 46 as a "farsighted" sponsor)—the author has decided to ignore a very large skeptical academic literature on these issues. Arguably, "good policy" can be dangerously misleading as the fundamental criterion for aid allocation. Simplistic macro rules-of-thumb compromise more rigorous credit and need standards, and increase the risk that aid becomes politicized and allocated inefficiently. It is regrettable that many of the world's poorest people live in conditions of substandard national, regional, and/or local governance and lack any tenable means of changing these institutions. It would be gravely ironic for aid agencies to compound the misfortunes of these people with discriminatory aid allocation—especially since we do not at present have the necessary understanding of the complex links in particular country circumstances between aid, growth, and development objectives, such as poverty reduction, to justify selectivity as the guiding principle of aid allocation. Moreover, it would run counter to the well established experience that the marginal impact of aid may often be very high in difficult and needy circumstances.

In sum, the topic addressed in this book is most interesting but I fail to share the point of departure that seems to be the combination that donors/sponsors do know what needs to be done and that little is working in actual practice. This is neither correct nor an effective starting point for trying to make development assistance more effective. I do not know whether this is what the author is sensing when making the caveat that "I must end on a cautionary note by underscoring that any 'conclusions' reached will need to be taken as only suggestive, awaiting further study" (p. 21). I was hoping for a more definitive set of conclusions and some firm guidance.

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I Health, Education, and Welfare

Who Has the Cure? Hamilton Project Ideas on Health Care. Edited by Jason Furman. Washington, D.C.: Brookings Institution Press, 2008. Pp. vii, 277. $49.95, cloth; $22.95, paper. ISBN 978–0–8157–3008–8, cloth; 978–0–8157–3009–5, pbk. *JEL 2009–0892*

The history of health care reform in this country over the past twenty (or even sixty) years might be characterized as the absence of comprehensive reform in spite of a steady stream of proposals from both politicians and academics. "*Who Has the Cure?*" continues this tradition with an interesting grab bag of ideas about changing the health care system. The variety of topics covered by the book's chapters illustrates the fact that "health reform" has many different possible meanings, including but not limited to covering the uninsured, reducing inefficient spending, and improving the design of existing public programs.

The chapters in the book fall into two categories: covering the uninsured and "other." Four chapters present different proposals for achieving universal health insurance coverage. A standard menu of such proposals might include one single-payer plan, one built around employer mandates, and one relying on tax credits and individual mandates. The editors here seem to have taken a different approach by confining themselves to what is remotely feasible politically. Thus, there is no proposal for a single payer plan. A chapter by Gerard F. Anderson and Hugh R. Waters on "Medicare Part E(veryone)" describes not a single payer plan, as the title seems to suggest, but rather a regime in which anyone may buy into Medicare—or they may have private insurance. A chapter by Stuart Butler outlines a plan that replaces the current tax treatment of employer-sponsored health insurance with a refundable tax credit for the purchase of health insurance. Ezekiel J. Emanuel and Victor R. Fuchs outline a voucher-based proposal for expanding private coverage. Jonathan Gruber describes the recent expansions of coverage in Massachusetts and considers how that approach might be implemented at the national level.

The result of this focus on feasibility is four chapters that are quite similar in some ways. It is both a strength and a weakness of this collection that you have to be paying quite close attention to keep the proposals straight. For example, economists might refer to the refundable tax credit described in the chapter by Stuart Butler as a "voucher," while the chapter by Emanuel and Fuchs uses this term to mean, literally, a voucher, as in "this slip entitles bearer to health insurance coverage." The difference—I think—is that the value of Butler's tax credit is a fixed dollar amount while the value of Emanuel and Fuchs's voucher varies with the bearer's expected medical spending—but you see we are getting into quite detailed territory precisely because the editors have, sensibly, narrowed the scope for discussion (without actually lining the proposals up next to each other in a way that would make it easier to understand their similarities and differences). There is, for example, general agreement that employers must continue to play some role in a reformed system, although the proposals differ somewhat about what that role should be and, in particular, what one ought to do about the current exclusion of employer payments for health insurance from both income and payroll taxes. The chapter by Stuart Butler makes a compelling case on both efficiency and equity grounds for ending this exclusion—indeed, it is hard to think of any reason (except, perhaps, a campaign promise not to raise taxes on the middle class?) why one would want to keep it. The chapters by Gruber and by Emanuel and Fuchs also advocate getting rid of it. Anderson and Waters, in contrast, preserve it in the name of political feasibility and it is a sad commentary that the termination of such a patently regressive bit of tax policy may, indeed, be less feasible than throwing Medicare open to everyone.

Ultimately, the similarity between these four of the book's chapters reflects the political reality that we are not, and never will be, Canada (single payer) or the United Kingdom (National Health Service). Rather, we are grappling with the thorny question of whether we want to be Switzerland (mandatory private health insurance with high patient cost-sharing) or the Netherlands (mandatory private health insurance with low patient cost-sharing).

The remaining three chapters cover diverse topics. A chapter by Joseph Newhouse and Richard Frank proposing fixes for Medicare Part D is thorough, balanced, and constructive, acknowledging the program's success in providing prescription drug coverage to millions of Medicare beneficiaries before turning to ideas for fixing its flaws. Some of these ideas seem relatively uncontroversial, such as bringing drugs that by an accident of history are covered by Part B under the Part D umbrella. Others, such as standardizing the menu of plan offerings so as to reduce the potential for confusion among beneficiaries, are likely to provoke a stronger reaction. Regardless of whether one agrees with their proposed fixes, this chapter provides an excellent starting point for any discussion of changes to Part D.

A chapter by Jason Furman argues that income-based cost-sharing should be a critical element of health reform. The idea that more cost sharing might be welfare-enhancing is not new. Martin Feldstein made this point quite succinctly in 1973: "American families are in general overinsured against health expenses" (Feldstein 1973, p. 251). At the time, household out-of-pocket spending constituted about one-third of total health services and supplies; today that figure is closer to one-tenth (figure 7-1, p. 185), so the need for more cost-sharing can only have increased. Furman argues that cost-sharing ought to be tied progressively to consumers' income and that Health Savings Accounts, which couple very high-deductible insurance plans with pretax spending accounts, are undesirable because of ways in which they disadvantage lower-income consumers. This chapter raises some interesting questions. How much out-of-pocket spending *should* households bear? And how should this spending be distributed with respect to the household's income, health status, or other factors? I don't think we have good answers to these questions or to the closely related question of how subsidies for the purchase of insurance ought to be designed. Furman's answers to these questions seemed to be a somewhat ad-hoc jumble of efficiency and equity arguments, which boil down to the idea that people should face some risk for the sake of efficiency, but not too much, especially not if they are poor, in the name of equity. But how much is too much and what are the positive and normative criteria used to evaluate different proposals? This chapter would have benefitted from a tighter link to recent theoretical work on this question, which also incorporates information on the effectiveness of different treatments (e.g., Mark V. Pauly and Fredric E. Blavin 2008).

Finally, a chapter by Jeanne Lambrew proposes a strategy to encourage the use of preventive services. This chapter relies heavily on the premise that new bureaucracies would function more effectively than existing ones do. For example, Lambrew proposes the creation of a "Wellness Trust" as an agency within the Department of Health and Human Services, with "a sufficient number [of trustees] to ensure balanced decision-making, but not so many as to deter the development of a consensus" (pp. 240–41). Among other things, the Trust would "release periodic reports and updates to ensure that the relevant information needed to guide the system is available" (pp. 241–42). This may actually be rather difficult to do for many reasons, not least of which is that the relevant information may not be available. For example, Lambrew proposes using the recommendations of the U.S. Preventive Services Task Force to guide policy—but between the publication of her chapter and the publication of this review, the Task Force backed away from a previous recommendation urging routine mammograms for women in their 40s, without actually advising against mammograms for this group, thus creating significant public confusion. Perhaps even the best scientific evidence is currently insufficient to inform public policy on cost-effectiveness.

The contrast between the very clear and detailed descriptions of policies to expand insurance coverage and the relatively vague descriptions about how to improve cost-effectiveness (in addition to Lambrew's chapter, there is Emanuel and Fuchs' proposal for a new Institute for Technology and Outcomes Assessment) illustrates a point made by Jonathan Gruber at the end of his chapter: "[T]here are principally two problems with the health care system in America: a lack of coverage and poor cost-effectiveness. The health industry knows how to solve the first problem but not how to solve the second" (p. 140). I would modify this slightly by saying that the main obstacle to expanding coverage is political, but the obstacles to improving cost-effectiveness are technological—we need to figure out how to identify high-value care before we can design systems that steer people toward these treatments and away from low-value ones.

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J Labor and Demographic Economics

The Nature of Demography. By Hervé Le Bras. Princeton and Oxford: Princeton University Press, 2008. Pp. xii, 361. $95.00, cloth; $39.50, paper. ISBN 978–0–691–13943–2, cloth; 978–0–691–12823–8, pbk. *JEL 2009–0149*

The Nature of Demography is a well-written survey of what is often called "formal demography" or "mathematical demography." The author, Hervé Le Bras, doesn't use the modifiers "formal" or "mathematical," calling the book simply "a general introduction to demography."

This reflects the French tradition from which he writes—he is director of research at the National Institute of Demographic Studies (INED) in Paris, an institution that has played a pioneering role for decades in the style of demographic analysis presented in the book.

The book provides an excellent and up-to-date presentation of the kinds of things one would expect to see in a demography textbook. Chapter 1, "Mortality," introduces the basics of life tables. The chapter includes a brief discussion of the use of life tables for life insurance and annuities, applications important to economists. Chapter 2, "Fertility," discusses measures of fertility, models of reproduction, and model fertility schedules such as the Coale–Trussell model. Chapter 3 is simply called "Censoring," covering an important set of issues affecting measures of both fertility and mortality.

Chapters 4, 5, and 6 discuss period versus cohort measures of fertility and mortality. Most commonly used demographic measures such as life expectancy and the total fertility rate are "period" (cross-sectional) measures based on age-specific rates for some given period (typically one year). These "synthetic cohort" estimates do not reflect the experience of any actual cohort, an obvious limitation, but are used because they provide convenient summary measures based on data from a single point in time. The distinction between period and cohort measures is fundamental in understanding demographic rates, and Le Bras provides a useful overview of this distinction in chapter 4.

Chapter 5 focuses on the important issue of the ways in which period measures of fertility can give misleading signals about fertility behavior when there are changes in the timing of fertility. The potential for misleading inferences based on period measures of fertility has been an important topic of debate by demographers studying the large decline in period fertility rates in many European countries. As Le Bras points out, "the period index can fall because childbearing is occurring later, without any reduction in the number of children when the cohort fertility is complete" (p. 93). Le Bras illustrates the problem using fertility data from France, pointing out that period fertility measures indicated that France had below replacement fertility in the 1980s, even though the cohorts having births in that period ended up with fertility above the replacement level of 2.1 births. The problem was that there was a significant shift toward later births among these cohorts, a pattern observed in many high-income countries in recent decades. Le Bras uses microsimulation techniques to analyze how postponement of births could have affected period measures of fertility in France, even in the absence of a change in completed cohort fertility.

Chapter 6 discusses the issue of whether similar "tempo" versus "quantum" effects apply to the case of period estimates of life expectancy. This has been a contentious topic of debate in mathematical demography, with work by John Bongaarts and Griffith Feeney (2002) arguing that just as changes in the timing of fertility affect period fertility rates, changes in the timing of mortality can affect period estimates of life expectancy. Le Bras has been a contributor to this debate and this chapter draws on a previously published paper on the topic (Le Bras 2005). Le Bras argues against the interpretation of Bongaarts and Feeney, concluding that "the notion of delay does not apply to mortality in the same way as to fertility or marriage" (p. 117). Whether or not you find his argument persuasive, this is a useful chapter for understanding this particular point of debate about period measures of mortality.

Having laid out the fundamentals of measuring fertility and mortality in chapters 1–6, Le Bras brings fertility and mortality together to model population dynamics in chapters 7, 8, and 9. This section of the book begins with an interesting intellectual history of attempts to formally model population growth as a function of birth rates and death rates. These chapters include some of the core essentials of mathematical demography, such as Lexis diagrams and stable population theory. While most economists never learn stable population theory, it is a powerful analytical tool that can have high returns in any economic model that includes age structure. Beginning with a model that assumes constant age-specific fertility and mortality rates, it is easy to show that the population converges to a constant proportional age distribution and a constant population growth rate. Le Bras provides a proof of weak ergodicity in chapter 8 and works through the key elements of the renewal equation that lies at the heart of stable population theory. Chapter 9 shows how the same models can be used to model fluctuations in population size and population age structure, including a nice example of oscillating cycles in the number of secondary school teachers. Stable population models have been used very effectively to analyze the economics of public and private intergenerational transfers. (e.g., Ronald D. Lee and Shelley Lapkoff 1988; Lee 1994). While LeBras does not discuss these economic applications, he provides an excellent overview of this type of demographic modeling.

Economics appears in the book in a fairly limited way. The most central is chapter 10, "Economics and Population," and chapter 11, "Life Cycles and Old-Age Pensions." The first of these is focused mainly on demographic cycles and their possible links to economic variables. The large literature on economics of fertility gets relatively little mention in the book, although there is a brief and quite clear discussion of quality–quantity models of fertility. This discussion is probably too brief to be of much value for economics students or researchers but it will hopefully help introduce many noneconomists to this very important dimension of the economics of fertility. The chapter on life cycles and old-age pensions provides clear analysis of pay-as-you-go pension systems, including the role of population growth in "golden rule" equilibria.

Part 3 of the book (chapters 12–15) is called "Space and Networks," including chapters on marriage markets, migration, and population density. The chapter on marriage markets includes models that will look somewhat familiar to economists who have worked on marriage markets, although the chapter does not refer to any of the large literature on economics of marriage. The chapters on migration include a nice historical overview of the "laws of migration," although, as in the case of marriage, there is no mention of economic approaches to studying migration.

For economists, the value of this book is not as a place to learn about economic demography. The economics topics that are in the book are generally well done but the treatment is too terse to provide a foundation in economic demography. On topics such as marriage and migration there is no treatment of economic approaches at all, even though many economists have worked in these areas. The real value of the book for economists is as a place to learn the essentials of demographic measures and mathematical demography. Economists without demographic training who find themselves studying topics like fertility, mortality, or age structure would do well to consult this book. Many economists can benefit from understanding the basics of life tables, the distinction between period and cohort measures of demographic rates, and the power of stable population theory. This book is a very good resource for acquiring that understanding.

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Networks in Telecommunications: Economics and Law. By Daniel F. Spulber and Christopher S. Yoo. Cambridge and New York: Cambridge University Press, 2009. Pp. xvi, 493. $120.00, cloth; $50.00, paper. ISBN 978–0–521–85710–9, cloth; 978–0–521–67386–0, pbk.

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The last twenty-five years have seen a dramatic change in telecommunications. Before AT&T was broken up under a 1982 antitrust decree, the United States had essentially one national network—or set of interconnected networks owned by AT&T—a few other local carriers, and a fringe of nascent, long-distance competitors. These companies offered only traditional voice/data services over copper wires. Mobile "cellular" telephony had just begun and its potential was so underestimated that AT&T did not even fight to keep its mobile operations when it was divested of its local companies. AT&T and its divested progeny did not offer Internet connections or video services; cable companies did not offer voice services. Telecom operated in a "plain vanilla" world, dominated by a single, heavily regulated carrier.

As simple as this world may have been, it was still much too complicated for the regulators to control efficiently. The antitrust case that resulted in AT&T's breakup, *U.S. v. AT&T*, was brought because the Federal Communications Commission (FCC) had failed miserably in its principal task—the regulation of interstate telecommunications. By setting long-distance rates far too high in an effort to cross-subsidize local telephone services, the FCC had invited entry into long distance as transmission costs fell due to the development of microwave. But the FCC had not figured out how to establish the rules governing interconnection between these new entrants and AT&T, leading inevitably to a series of lawsuits, including the federal antitrust case. The end result was the breakup of AT&T and twenty years of legal and regulatory conflict over the regulation of voice services in a vertically fragmented environment.

If the ancien régime of telecom regulation was a dismal failure, the current one is far more complicated and finds regulators on much more treacherous terrain, according Daniel Spulber and Christopher Yoo. Their new book, *Networks in Telecommunications: Economics and Law,* is principally a warning to regulators. Competition has replaced regulated monopoly, and the need for retail price regulation has all but evaporated. As networks change and proliferate, new carriers and content providers lobby to gain access at regulated rates to all or portions of the established telecom companies' networks, which the entrants then use as inputs for delivering a variety of services. Thus, regulators must now decide how and under what terms these competitors should be provided such access. Spulber and Yoo caution that this shift to *access* regulation creates enormous problems because ". . . [Access regulation] has created the need for more comprehensive understanding of how network components interact within the context of a complex system, as well as some basis for determining the impact of access regulation on network design. Absent some greater insight into these considerations, regulatory authorities will be hard pressed to shape policy in ways that are both coherent and constructive" (Introduction, p. 3).

The authors clearly are pessimistic that the regulators who failed in the simpler, pre-1982 era will succeed in this more complex endeavor, for they tell us that: "We demonstrate that the complexity of networks implies the need for additional regulatory forbearance" (p. 3). The chapters that follow are in essence a lengthy warning to regulators that they should abstain from regulating access wherever possible.

The book begins with a primer on network structure based on graph theory. It then turns to a list of mechanisms by which users access the network, whether through voluntary transactions or regulatory mandates. These include (1) retail access—used by consumers, (2) wholesale access—used by resellers of a network's services, (3) interconnection—the mechanism used by other similar networks to connect with a network's customers, (4) platform access—used by sellers of complementary services, and (5) "unbundled" access—the use of only certain parts of the network by rivals that offer competitive services. Spulber and Yoo show how granting various types of access to a network can affect the capacity of the network and, therefore, its design.

Spulber and Yoo's major contribution is to demonstrate quite simply, but clearly, how network efficiency and capacity can be disrupted if the network owner loses control over traffic flows. Because regulated access necessarily forces the operator to cede such control to third parties, such regulation carries with it the danger of reducing network efficiency in the short run and dulling the incentive to invest in network upgrades or expansion in the longer run. This is particularly true of regulatory mandates requiring operators to lease individual facilities, such as subscriber lines or transmission paths, at regulated rates. Because the network is an interconnected set of links and nodes designed to provide the planned capacity at minimum cost, the incremental cost of building any one network element may be much less than the opportunity cost of removing it from the network operator's control. Thus, such an element cannot be leased to a competitor at incremental cost without potentially reducing the value of the network, perhaps substantially (p. 49).

Moreover, because telecommunications networks are characterized by economies of scale and scope with substantial common and joint costs, it is very difficult for regulators to assign costs to any set of components or network services. This was always a problem, even in the simpler era in which a single, integrated network provided largely voice services. The problem has become much more complicated as networks are modified to deliver multiple services, including high-speed Internet connections and video services. In the modern era in which competitors are allowed by regulators to access "common carrier" networks, the regulator must set a price for such access. But networks have been built and adapted over many years to changes in technology, services, and demand, making it virtually impossible to base this access price on the embedded cost of the network. For this reason, regulators attempt to build complex engineering/economics models of an idealized network and to base the access prices on the Total Element Long Run Incremental Cost of each network element. This a fool's errand to Spulber and Yoo, not only because such an exercise requires the arbitrary allocation of joint and common costs but because each competitor's leasing of such an element affects the network's configuration and, therefore, the relationship between inputs and outputs. Quite simply, they conclude that "It is therefore not possible to divide a network's costs among its components in a meaningful way" (p. 71).

After the opening section's straightforward theoretical analysis (and admonition to regulators), Spulber and Yoo turn to a discussion of the mechanics of regulation, particularly the regulation of network access. This second section is based principally on U.S. legal institutions and precedents but its analysis is certainly relevant to other regulatory jurisdictions. In this section, Spulber and Yoo dismiss or question many of the conventional views that because of certain characteristics of telecommunications networks—such as scale economies, sunk costs, and joint and common costs—telecom markets are particularly susceptible to monopoly abuses. With changing technology, the authors argue, entry into telecom markets can and will occur if incumbents attempt to appropriate monopoly rents. Much of the analysis in this section borrows on the first, theoretical section of the book and is therefore rather repetitive. The authors reiterate their concerns over regulators' mandating network access, concluding that "Markets rather than regulators should determine access to networks" (p. 189).

The last section of the book is designed to bring the economic and policy frameworks of the first two sections to bear on current policy issues in the United States. This section includes a long chapter on the application of antitrust to network industries, including a lengthy critique of the essential facilities doctrine and the changing approach of antitrust to vertical market exclusion. Compelled access to a network owner's allegedly essential facilities is supposed to be an antidote to vertical exclusion, but once again the authors warn us that the remedy is likely to discourage network investment without correcting the underlying problem of the monopoly bottleneck. Equally important, the essential facilities "doctrine" of antitrust is rarely invoked precisely because courts cannot easily find an efficient mechanism to mandate access to such facilities.

The last two chapters use the authors' analytical framework to address two major issues that are at the forefront of current "broadband" policy discussions: unbundled access to last-mile broadband networks and "network neutrality." The latter policy proposal involves restrictions on network operators' pricing flexibility and their ability to charge internet content suppliers access charges, policies that the authors criticize because of their likely effects on network investment.

There are two areas of inquiry that one might have hoped that Spulber and Yoo would have addressed more extensively in this otherwise exhaustive inquiry: the effect of access regulation on the network owner's ability to respond to technical progress and the regulation of wireless networks. Surprisingly, Spulber and Yoo do not focus much on the effects of mandated unbundled access regulation on the network owners' ability to adjust to new technologies or changing market forces. But once a network owner is forced to share its facilities with competitors, the latter have the ability to lobby the regulators to block changes in network design or operation which might be welfare enhancing but damaging to the competitors. These concerns have created delays in the deployment of new fiber-based networks in recent years and could further postpone the deployment of new technologies if regulators were to impose unbundled-access requirements on new networks. The FCC is currently being lobbied to impose such rules on new fiber-optic networks once again after forswearing such regulation in 2003–05.

Nor do the authors provide much discussion of the regulation of wireless networks because, unlike other countries, the United States has largely deregulated wireless services and has only limited regulation of wireless interconnection rates. It would have been useful to describe the reasons for this difference and the vulnerability of wireless network operators to new cries for regulation under the "network neutrality" mantra.

The principal audience of *Networks in Telecommunications* should be regulators, but they are unlikely to welcome its central message. Much of its contents are already well known to economists who study telecommunications but even they will benefit from reading Spulber and Yoo's approach to the problems of network regulation because it explains so clearly the dependence of network design and consequent efficiency of operation on regulatory decisions, particularly those involving access. Unfortunately, the book is so detailed and narrowly focused that it would be an unlikely choice for a textbook for any course other than a law/economics course in telecommunications. Nevertheless, it will prove to be a useful resource for regulatory economists and for lawyers bent on challenging ambitious regulators.

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