BOOK REVIEWS

Bowling Alone
Robert D. Putnam
New York: Simon & Schuster, 2000, 541 pp.

This important book has already sparked conversation about community and civic engagement. Robert Putnam has analyzed prodigious information on various measures of community, and, although this review highlights my perceptions of its shortcomings, his work contributes substantially to our understanding of issues of community. He interprets his measurements of dwindling civic engagement as a reversal of many decades of growing civic activity. Putnam does express optimism for creating more engaged communities, which makes sense given the irrepressibly social nature of humans. But his argument does not couple that optimism with a dynamic view of community.

Putnam indicts a multiheaded Hydra for that trend, emphasizing a generational shift over the century; by Putnam's measures, baby boomers and generation-Xers are much less civic than those born before 1940. At a more explicit causal level, Putnam's main culprit is the growth of television and the associated isolation and one-way communication. He masterfully evokes the image of friends bowling together, watching TV between turns instead of talking.

Putnam carefully avoids overgeneralizing by seeking corroboration from multiple sources. Unfortunately, he falls prey to a curse of social science in general: He uses available data on individual and social phenomena, but those phenomena are largely undocumented. Putnam is looking under the lamppost for the key. Unfortunately, the lamplight does not suffice to illuminate our increasingly dynamic, informal social and civic associations, and he chooses not to recognize this fact fully in his argument. Putnam endeavors to quantify a nation that no longer exists. While he was bowling, we have become more specialized in jobs and education, more culturally and politically heterogeneous, and more aligned with others according to shared activities and interests.

He also holds us to a very high engagement standard. The generation born before 1940 was exceedingly civic by Putnam's measures, and his analysis of the Progressive Era shows that mid-20th century civic involve-

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ment was unique, and far from representative. Putnam would like us to revive that unusually high civicness, arguing that this change would also benefit us individually. He urges us to overcome the collective action problem he sees in civic engagement.

Although he does address the recent growth of small groups, online communities, and more informal forms of engagement, Putnam's definition of community is premised on participation in particular groups or activities, such as Kiwanis, Parent-Teacher Associations, and local political activity (*e.g.*, petition signing). That definition of community is static in the extreme, particularly in the context of the dramatic social and technological changes of the past three decades. His analysis trivializes the connectedness we currently experience through more informal channels. He also downplays the connectedness and personal gratification that we experience (and that he found in the data) at work. Yes, work has crowded out community activity in some ways, but as his data indicate, work also creates opportunities for friendship and volunteering in environments where many feel as if their efforts can affect social outcomes, unlike increasingly specialized and professionalized government, charities, and civic associations.

Putnam may also have found many symptoms of a common cause, the growth of federal government involvement in individuals' lives. He does not address that causal connection in any detail, even to falsify it as an alternate hypothesis. He mentions government growth as a potential explanatory variable, but he discredits it based on federal nondefense spending as a share of gross domestic product (p. 282, Figure 78). That share increased through the 1960s and early 1970s, and this period saw Vietnam and political turmoil (culminating in the massive trust destroyer of Watergate) in addition to "Great Society" spending. In the face of those data, Putnam still asserts that "it is much harder to see which government policies might be responsible for the decline in bowling leagues, family dinners and literary clubs" (p. 281). That is asking a lot of your data. I offer an alternate hypothesis: Increased federal spending on social programs, coupled with decreasing trust in the actions and institutions of government, gave us less incentive to participate in our own mutual support and governance. That change evolved into our widely acknowledged modern cynicism. Thus we instead focus our limited time and energy on activities and institutions where we make a difference and feel trust and personal growth. The discussion also does not make clear whether or not he tested the crowding out hypothesis in his regression analyses in addition to the "ocular regression" of looking at the spending graph.

When he performs a state-by-state comparison of civic measures, Putnam also fails to make an explicit causal connection between cultural heterogeneity and measures of civic disengagement. Putnam does mention the heterogeneity of American urban culture at the turn of the century in his discussion of the Progressive Era, but he does not layer that observation on the increased complexity and specialization of modern life. Not surprisingly, he finds that Scandinavian countries, the northern plains states, and Utah have high civic engagement by various measures. Civic engagement is easier when society is culturally homogeneous, as has been the case in those locations. Creating cultural homogeneity should be a long evolutionary process in a vibrant, dynamic society, but we are certain to experience frictions in that process.

Community is where you find it, in the eye of the participant, and the data on which Putnam relied would have been unlikely to capture the more choice-driven, less physically defined manifestations of community that have grown in the past decade. His data also point to an alternative explanation that he did not explore—as government became less trustworthy, more federal, and more professional, individuals saw their personal efficacy, and therefore their gratification from civic engagement, diminish.

Even if we believe that there is a collective action problem, and that individuals do not capture all of the benefits of their actions, we must explore why more people have chosen behavior so different from what we typically associate with community and civic engagement. *Bowling Alone* certainly furthers that important conversation.

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Law's Order: What Economics Has to Do with Law and Why It Matters
David D. Friedman
Princeton, N.J.: Princeton University Press, 2000, 329 pp.

Law and economics appears to be the most lasting and important of all the analytical approaches to law that arose in the 20th century. Today most law schools include on their faculty economists trained in law or lawyers trained in economics. Several law schools specialize in the economic approach to law and a number of judges explicitly use that approach to help them to decide cases. Economically minded legal scholars teach and write about all areas of law, not just antitrust law or corporate law, as was true a generation ago. *Law's Order* is an introduction to this exciting field that is suitable for the interested layman, students, and also, in several chapters, law and economics scholars.

Law's Order opens by asking why the standard of proof in criminal cases ("beyond a reasonable doubt") is higher than in civil cases ("preponderance of the evidence"). Lawyers are not good at answering questions like that (often because they do not ask such questions), even though such questions strike to the heart of what the law is. The typical law school explanation is that more is at stake in criminal cases and thus the standard is higher. Yet, as Friedman notes, a one-million-dollar civil case would be for most people a more serious threat than a potential day or two in jail. Moreover, there is no legal doctrine that requires that the standard of proof be raised when the stakes in a civil case increase, nor is there a requirement that the standard of proof be reduced when the stakes in a criminal case decrease (the standards of proof for murder and assault are the same, for example). Friedman's explanation for the different standards is that in a civil case when the defendant loses, the plaintiff gains; but in a criminal case the defendant's loss of liberty is not matched by a gain to anyone else. Criminal punishments are therefore more costly, from a social point of view, than are civil punishments, and, all else being equal, we should be more careful about imposing them.

Having suggested the usefulness of economics for an understanding of law, Friedman turns to a more in-depth examination of economic ideas. He has good discussions with legal applications of incentives, public goods, insurance, moral hazard, and issues of strategy and bargaining. Chapter 4 presents the Coase theorem while the following chapter has an extended discussion of how it can be applied to the problem of constructing efficient legal rules. The heart of chapter 5 is a "spaghetti" diagram that illustrates under what conditions various legal rules will lead to various outcomes. The diagram and accompanying discussion summarizes a large literature and alone is worth the price of admission.

The second half of *Law's Order* examines the law and economics of property, intellectual property, contracts, the family, torts, crime, and antitrust. The economic approach to law shows its virtues here as the same issues arise repeatedly in all these contexts. All of these chapters have interesting material, although I thought that the chapter on crime focused too much on puzzles of optimal punishment, such as whether the rich should pay higher fines than the poor. I would have preferred, especially for student use, more discussion on crime deterrence. The calculation of expected punishments, the impact of shall-issue gun laws on crime, and appropriate public policy towards negative and positive externality crime deterrence (e.g., LoJack versus The Club) are but a few potential topics that come to mind.

One of the things I especially liked about *Law's Order* is that it is more about how to think about law than it is about legal doctrine or case history. Friedman presents arguments rather than statements and often he follows an argument with a counterargument (and sometimes a counter-counterargument). Thus one gains a sense of how economic tools are consistently applied to complex issues. I also liked Friedman's willingness to address big issues, such as why we have both criminal law and tort law, and whether we could do entirely without criminal law, as did saga period Iceland. Discussions of alternative legal systems help us to understand the virtues and, more important, the defects of our own system of law. Law and economics scholars could learn much from these chapters.

Some of *Law's Order* strengths may be counted as weaknesses, depending on your point of view. The focus of the book is on thinking tools and consequently Friedman cites few cases (although there are more on the Web site accompanying the book), and he gives little sense of legal history, legal evolution, or contemporary issues, such as mass toxic tort cases. The single chapter on the American legal system is short and perfunctory, saying little about federalism, constitutional law, or the replacement of common law with regulatory and statutory law, a prominent factor in 20th century American law. *Law's Order* surveys only the broad features of the law; it does not delve into details.

Increasingly, the World Wide Web is being used to provide supplementary material to books. *Law's Order* takes this approach further than most by placing *all* the notes and supplementary material on the Web. Instead of footnotes or endnotes, the margins of *Law's Order* contain icons indicating when supplementary material is available. To access that material you have to log on to the book's Web site, click to jump to the chapter you are interested in, then click again to jump to the page number that contains the icons you wish to read, and at this point you are presented with an electronic page identical to that in the book. You must then click on the relevant icon to—at last!—discover the supplementary material. Since the icons are not numbered, there is no way to jump straight to the material of interest, nor can you access in one page all of the notes to a particular chapter. The reader cannot create either a page of links, as a professor might want to do to draw the attention of students to important cases or other material. Some of those problems can and hopefully will be fixed. Once found, the supplementary material can be quite useful, as it includes links to online cases, papers, and references. It is unfortunate, however, that that material is not easier to access.

In short, David Friedman has written a superb introduction to law and economics. *Law's Order* would make an excellent textbook for undergraduates; it is fun reading for an economist not specializing in law; and it has enough interesting questions and advanced material to be of interest to scholars.

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Money, Exchange and Production: Further Essays in the History of Economic Thought
Thomas M. Humphrey
Northampton, Mass.: Edward Elgar, 1998, 178 pp.

The subtitle "Further Essays" alludes to an earlier collection, *Money, Banking and Inflation: Essays in the History of Monetary Thought* (Edward Elgar, 1993). Thomas Humphrey, vice president and research economist at the Federal Reserve Bank of Richmond, specializes in applying the perspectives of doctrinal history to current policy problems. His new book reproduces six articles published in the bank's *Economic Quarterly* from 1993 to 1997.

The earlier book also consists of reproduced articles, 35 on monetary topics and 3 on geometrical tools. Dating from 1971 to 1992, all but 5 of them come from the Richmond Fed's *Economic Review* (as the bank then called its journal). Drawing on a longer period and being much larger (38 against 6 articles, 455 against 188 pages), the 1993 book is a richer collection, ranging more widely over monetary issues, than its 1998 update.

The new book's first article traces the understanding of the velocity of money not as a constant but as a function of several variables back to Sir William Petty in 1662 and 1664. The second article shows that Knut Wicksell, despite his discussing price-level changes and price-level control in terms of interest rates, was as much a quantity theorist as Irving Fisher. It illustrates how illuminating what I call the "translation test" can be—the rephrasing of ideas or theories from one conceptual or terminological framework into another. The third article reveals John Wheatley, active during the Bank Restriction period 1797–1821, as a hypermonetarist who believed that money-stock changes exert no influence on real variables even in the short run, thanks to perfect wage-price flexibility.

The three remaining articles deal with techniques and their histories—reciprocal-demand (offer) curves, the Edgeworth box and its elaborations, and mathematical production functions before Cobb-Douglas. Humphrey illustrates how analytical problems call forth techniques and improved techniques find application to further problems. Humphrey sets an admirable example by titling his diagrams clearly and stating in English, not just in symbols, what is measured along each axis. Furthermore, most diagrams have substantial captions summarizing what they are supposed to show, and how.

Since the diagrammatics and mathematics do not readily lend themselves to a reviewer's comments, I'll simply recount three stories from those articles. One tells why Abba Lerner's 1933 paper on "Factor Prices and International Trade," which made pioneering use of the production box diagram to prove the factor-price-equalization theorem, did not see print until February 1952. A fellow student to whom Lerner had given his only corrected copy to be retyped for submission to a journal lost that copy on a London bus. Not until after reading Paul Samuelson's 1948 and 1949 papers on the same topic did Lionel Robbins recall Lerner's old term paper, find another copy still in his files, and urge Lerner to publish it unaltered in *Economica*.

Humphrey quotes Jacob Viner's diagnosis of Alfred Marshall's puritanical sense of guilt over mathematics, especially geometry. It yielded him "so much intellectual and aesthetic delight that it for that reason alone become somewhat suspect to him" as a tool of economics (quoted on p. 81).

Around 1877 Leon Walras sought the help of a fellow Lausanne professor, the mathematician Hermann Amstein, in formulating the leastcost conditions of factor hire. Responding in a letter, Amstein "worked out virtually the entire theory of marginal productivity in modern algebraic dress," complete with partial derivatives and Lagrange multipliers. Unfortunately, Walras had not yet taught himself enough mathematics to understand Amstein's formulation, while Amstein knew too little economics to appreciate its significance. Thus it languished in the Lausanne archives until rediscovered and published in 1964 (pp. 152–54).

In a fresh preface Humphrey points out some of the lessons illustrated by his reprinted articles. He correctly (if unfashionably) defends the importance of the history of ideas, and he demonstrates the advantage of organizing it by analytical techniques rather than only by dates, leading personalities, or schools of thought. History gives insights into how science progresses. Ideas from different specializations interact, as do problems and techniques.

In his preface, Humphrey advocates presenting economic reasoning in a variety of ways—words, diagrams, and math. Irving Fisher comes to my mind not only as a creative economist but also as a painstaking expositor who did not think it beneath himself to make repeated stabs at getting his ideas across to the reader. Besides using words, diagrams, and math, he sometimes also devised mechanical models. Humphrey regrets the apparent trend toward abandoning diagrams. (Again I can make a pitch for the "translation test" while also regretting the blanket condemnation of mathematics in economics by some segments of the Austrian school.)

I have long been an admirer of Humphrey's work and his attitude toward economics as a cumulative science, a serious intellectual endeavor, not just an academic game. His preface pays tribute to the professors who had the greatest influence on him—Howard Dye at the University of Tennessee and Erskine McKinley, Jacques Melitz, and Herbert Geyer at Tulane University. Humphrey's 1993 and 1998 volumes together (though scarcely the latter alone) provide a fine introduction to his work. They would serve well as primary or supplementary texts in advanced courses in monetary theory or the history of economic thought.

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Power and Prosperity: Outgrowing Communist and Capitalist Dictatorships
Mancur Olson
New York: Basic Books, 2000, 199 pp.

Shortly before his untimely death in 1998, Mancur Olson had nearly completed a manuscript that he first titled *Capitalism, Socialism, and Dictatorship*, a title reflecting the inspiration of Joseph Schumpeter's classic 1942 book on *Capitalism, Socialism and Democracy*. For better than any other economist since Schumpeter, Olson addressed the most important issues of political economy and explained them in language accessible to a broader audience. In effect, the book just published is Olson's elegant but unfinished last symphony—his not-quite-completed manuscript plus a foreword by his Maryland colleague Charles Cadwell and a new title.

As the new title suggests, the book addresses two loosely related themes. In chapters 1 through 5 Olson summarizes his general theory of the relation between "power and prosperity"—between the structure of the political system and economic outcomes—based on his own prior analyses and the other major contributions of modern public choice. The most important new insights in those chapters build on his 1991 article on "Autocracy, Democracy, and Prosperity" and a 1996 article (with Martin McGuire) on "The Economics of Autocracy and Majority Rule: The Invisible Hand and the Use of Force." Other sources of material for those chapters include his 1982 book on *The Rise and Decline of Nations* and his delightful put-down of the Panglossian political economy of some Chicago economists in a 1996 article on "Big Bills Left on the Sidewalk: Why Some Countries are Rich and Others Poor." These chapters reflect Olson's special ability to explain and illustrate subtle analytic concepts in lively English and with historical examples.

The power and scope of the insights from this perspective are impressive. Government is fundamentally the exchange of tribute for protection—against both local and foreign predators. And the primary and continuing political challenge has been to design institutions that prevent a government that is effective in its protective role from itself becoming the most oppressive local predator. During the long prehistory of hunter-gatherer bands there was almost no government, primarily because there was little property to protect or to steal. Government first became an important institution to protect and support agriculture and other time-dependent and site-specific forms of production.

Olson attributes the origin of autocratic government to the acts by a roving predator to become the dominant local predator in a specific area. This autocrat has a purely self-interested incentive to set a revenue-maximizing tax rate and use part of the tax receipts to provide a level of government services for which the value of the last unit is equal to the inverse of that tax rate. The local population is better served by an autocrat than by competitive predators in the same area, primarily because a monopoly predator has a more "encompassing interest" than one of several predators. Moreover, the population is better served by an autocrat with a long horizon and has ample reason to wish that "long live the king" if the only alternative is another autocrat with a shorter horizon.

Autocrats appear powerful but are often vulnerable. Their power and wealth attract attempted coups and foreign aggressors. And the death of the autocrat often provokes a succession crisis. Olson attributes the origin of democracy to accidents of history that lead to four conditions: a broadly equal dispersion of power among those groups that overthrow the autocrat so that no one leader can declare himself the new autocrat; no group is sufficiently strong in some region to create a miniautocracy; the number of groups is small enough to permit voluntary collective decisions; and the area over which the new democracy is established is spared conquest by some foreign aggressor. The same conditions that lead to a democracy also lead to checks on the power of the executive. Olson expects a democracy to have both a lower tax rate and a higher level of government services than an autocracy because the controlling group in a democracy has a stake in the level of the general economy as well as the benefits of government that accrue only to that group.

Over time, Olson expects democracies to be subject to two contrary tendencies: On the one hand, Olson finds that property and contract rights are more secure the longer the span of democratic systems or the tenure of individual autocrats. On the other hand, Olson expects democracies to become increasingly controlled by coalitions of special interests over time. That leads him to conclude that some "hard" autocracies that suppress special interest groups and have a long horizon may have higher economic growth than older democracies, at least for a generation or two. That is an awkward conclusion because he does not explain why only a few such autocracies adopt good economic policies.

Olson introduces his second theme with a brief transition chapter on "The Sources of Law Enforcement and Corruption." What Olson finds is that the most influential private interests support market-augmenting government policies, such as the enforcement of property rights and contracts. On the other hand, the most directly affected private parties have an incentive to evade market-contrary policies, such as price controls. That simple lesson deserves repetition.

Chapters 7 through 9 summarize Olson's special theory of Soviet-type autocracies and some conjectures about why the post-Soviet Russian economy, to date, has contracted. This material is largely based on research by the Center on Institutional Reform and the Informal Sector (IRIS) that Olson founded at the University of Maryland. Olson offers a unique and convincing explanation of why the Soviet economy generated a much higher output for the state during the Stalin period than is suggested by his general theory of autocratic governments: the combination of forced saving and *declining* marginal tax rates. His explanation of why the post-Soviet Russian economy has contracted is plausible but less convincing. In contrast with Germany and Japan after World War II and China after the Cultural Revolution, Olson contends, Russia did not go through *enough* of a crisis to strip away the layers of special interests accumulated during the Stalin period. In that case, the prospects for a successful transition to a market economy and liberal democracy seem quite pessimistic in the absence of another crisis or a new autocrat.

The final chapter, appropriately, summarizes Olson's normative and positive perspectives on political economy. He favored laissez-faire economic policies that permit individual firms to make extraordinary profits without government interference or to fail without subsidy or protection. On the other hand, he favored social insurance for individuals because many fortunes are due to luck. His economic judgment led him to observe that, to be successful, a market system requires two essential conditions: (1) "Secure and well-defined individual rights" and (2) "the absence of predation of any kind." And his political judgment led him to conclude that "These two conditions are most likely to be satisfied, I think, in secure, rights-respecting democracies where the institutions are structured in ways that give authoritative decision making as much as possible to encompassing interests."

Mancur Olson was a friend, we shared a similar research agenda, and we argued about many, mostly little, issues. I will miss his important insights, his humor, and his humanity.

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