

Book Reviews

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Martha S. Feldman

The Global Management Revolution: A Report on the Transformation of Governance by Donald F. Kettl. Washington, DC: Brookings Institution Press, 2000, 87 pp., \$14.95 paper.

This book operates at several different levels and provides something for many different audiences. At the first level it provides basic information about what is sometimes called the new public management and what Kettl refers to as the global public management revolution. It summarizes the kinds of reforms in which governments around the world have been engaging, the tools that have been used to bring about the reforms, and the challenges for the future of these reforms. At this level, it is a good book for people learning about the practice of public management.

At the second level, it provides information relevant to some of the ongoing arguments in the literature about public management reform. One of the questions addressed at this level is whether public management reform is making a difference. Another is whether the public management reforms can be thought of as a coherent set of reforms, or whether the differences in the reforms with which we are dealing warrant consideration as a unified phenomenon. These are important questions in the literature and in practice.

At the third level Kettl argues that reform in public management is inevitably linked to governance. This is the most thought-provoking part of the book, requiring the reader to think deeply about what governance is and how management is related to it.

Kettl claims that, in a very short period, governments around the world have adopted six core features of global management reform: productivity, marketization, service orientation, decentralization, policy (the ability to devise and track policy), and accountability. Four forces have contributed to this revolution. Namely, there are political pressures to transform and shrink; social changes that accompany the fall of communism and apartheid, as well as the move from an industrial age to the information age; economic constraints; and institutional forces as global institutions become increasingly important.

Kettl identifies and discusses two strategies for implementing the core features of global management reform: the Westminster style and the American style. The former brought about “fundamental restructuring” while the latter brought about “sweeping administrative changes.” In both cases the focus of the discussion is on the national or federal level of government.

The Westminster style of reform has a theoretical base grounded in transaction cost economics, which emphasizes very clear boundaries between politics and administration (Barzelay, 2000). The Westminster reforms are top-down with elected officials providing a set of incentives designed to “make managers manage” (Kettl, 1997). New Zealand is the quintessential example of Westminster reforms.

The United States provides the quintessential, and perhaps the only, example of the American style of reform. Reinvention in the form of the National Performance Review is experimental rather than theoretical, complete with reinvention laboratories (Ingraham, Thompson, and Sanders, 1998). American-style reform is bottom-up in the sense that there were relatively few big policy changes (the Government Performance and Results Act is a notable exception) and the guidance from upper-level officials tends to be aimed at encouraging public managers or “letting managers manage” rather than at making them manage (Kettl, 1997).

Kettl addresses the question of whether the global management reforms have made a difference. Between 1990 and 2000, countries with more aggressive reforms have decreased the size of government in terms of total outlay and numbers of employees. By contrast, public confidence in institutions has generally gone down over the same period, though as Kettl points out, enough time may not have passed for the reforms to have an effect on trust. Overall, the evidence presented is not strong. Kettl acknowledges this and points out that the effects of management reforms are difficult to gauge. He could also point out that such aggregate indicators often cancel each other out when they are used to assess change (Barley, 1986). He suggests that the strongest indicator of success is often perseverance, which implies political support.

The title of the book tells us where Kettl falls on the issue of whether the global management reform is a reasonably coherent phenomenon. Nonetheless it is useful to see where he thinks the efforts to reform management converge and diverge. The five elements of convergence include: a focus on down-sizing that converts to a focus on doing more with less, a focus on results rather than inputs, devolution of responsibilities to local government, greater use of nonprofit and private sectors, and the search for best practices. The main points of divergence are the focus of the Westminster reforms on creating very firm boundaries, while the American reforms tend to blur these boundaries.

Though Kettl provides information on both of these arguments, the arguments per se are not his major interest. Instead, they play a role in his larger argument about the relationship between management and governance. In dealing with both arguments, he emphasizes the political aspects of reform—and here’s where the going gets interesting. His main argument is that managerial reform is about politics and governance and not simply about administration. His argument rests on the observation that (p. 67):

Management reform is not fundamentally about management. Elected officials do not pursue management reform for its own sake but because they believe it helps them achieve a broader political purpose....

In this book Kettl focuses on two aspects of governance: the relationship between the public and private sectors, and the relationship between what elected officials want, presumably representing what their constituencies want and what management does.

Kettl's argument about the relationship between governance and management reform rests on the distinction between what government does and how it does it. He argues for linkage between these two. An important part of this linkage is the relationship between the public and private sectors. An aspect of public management reforms in many places, for instance, has been to move functions from the public to the private sector. Another aspect requires public managers to learn how to oversee the private sector, often through contracts. A third aspect involves learning how to emulate the private sector to achieve some of the advantages of competition and accountability to a bottom line.

Kettl's argument about the relationship between management and governance rests on linkage between what government does and why it does it. He focuses particularly on the importance of political reasons for reform. He points out that people do not pursue management reform for its own sake. This is unobjectionable. He follows that statement, however, with the following: "Managers have little incentive to pay careful attention to performance measures if elected officials do not indicate that they, too, are paying attention" (p. 67). But motivation is seldom that straightforward. While it is clearly important for elected officials to pay attention to performance measures, managers also have incentives in terms of their relationships with the people their organizations serve, the people who work in these organizations, and the managers and employees in the organizations with whom they coordinate. Moreover, the pattern of influence is not unidirectional. Managers also influence the interests of the elected officials and are often the reason politicians pay attention to such measures (Carpenter, 2000; Selden, 1999).

The points Kettl makes about the role of the private sector and elected officials in connecting management to governance are essential to understanding how to theorize and practice public management. We need to understand these connections and explore them. They are a good start to thinking even more deeply about governance issues. Management is about governance in so many ways (Lynn, Heinrich, and Hill, 2001; March and Olsen, 1995). Management reform not only presents new opportunities for interdependence between the public and private sectors and between elected officials and public managers. It also presents opportunities to redefine relationships between management and employees, between government organizations and the public, and between parts of the government (Feldman and Khademan, 2000, 2001). These potential changes in relationship present challenges and opportunities. Managers will have a significant effect on all of these relationships, and what they do with the challenges and opportunities will influence the various effects of the core features of the global management revolution.

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Kenneth J. Heineman

- What's God Got to Do with the American Experiment?* by E. J. Dionne Jr. and John J. DiIulio Jr. 2000. Washington, DC: Brookings Institution. 84 pp., \$16.95.
- The Diminishing Divide: Religion's Changing Role in American Politics* by Andrew Kohut, John C. Green, Scott Keeter, and Robert C. Toth. 2000, Washington, DC: Brookings Institution. 176 pp., \$15.95.

Columnist E. J. Dionne Jr., and political scientist John J. DiIulio Jr., observe in *What's God Got to Do with the American Experiment?*, that both Democrats and Republicans found religion in the 2000 presidential election. Citing George W. Bush's and Albert Gore Jr.'s endorsement of federal assistance to faith-based social service organizations, Dionne and DiIulio believe that a bipartisan, or "ecumenical," consensus on an important policy issue is at hand. (After the election, DiIulio, a Democrat, accepted President Bush's offer to head the White House Office of Faith-Based and Community Initiatives.) It is in an ecumenical spirit that DiIulio and Dionne collaborated with the Brookings Institution to assemble 21 essays from a diverse group of academics, politicians, and religious activists.

The adoption of the "Charitable Choice" provision in the federal welfare reform legislation of 1996 focused public attention on the evolving relationship between civil government and religion. "Charitable Choice" permitted states to funnel public funds to "faith-based organizations." Currently, 63 percent of faith-based children's service agencies receive at least one-fifth of their budget from public funds. Of the 40,000 positions in Americorps, which President Bill Clinton created in 1993, 15 percent are in faith-based organizations such as Habitat for Humanity.

Critics dedicated to the separation of Church and State are not pleased with recent developments. Interestingly, many religious activists are no more willing to champion government assistance to faith-based organizations. As Richard Parker of Harvard's Kennedy School for Government argues, mainline Protestants, Reform Jews, and black Protestants have a greater propensity to seek federal funds and embrace political activism than conservative white Protestants.

Parker notes that many religious conservatives did not join the Moral Majority in the 1980s or the Christian Coalition in the 1990s. Those who did join were never entirely comfortable with politicking. On the other hand, mainline Protestant denominations, in particular the Episcopalian and Presbyterian churches, have paid a price for their activism. Though Parker mentions in passing the membership woes of progressive churches, readers seeking a fuller account should consult historian Thomas Reeves' book, *The Empty Church: The Suicide of Liberal Christianity* (1996).

Cal Thomas, the one-time vice president of communications for the Moral Majority, and Ed Dobson, a Grand Rapids, Michigan, pastor, explain why religious conservatives must shun politics and government funding. Both warn that politicized churches compromise their moral principles and are “seduced by the siren song of temporal political power” (p. 52). Thomas and Dobson, feeling betrayed by hypocritical Republican congressmen, urge religious people to transform American culture through personal example, rather than the ballot box.

In a similar vein, Melissa Rogers, the general counsel for the Baptist Joint Committee on Public Affairs, expresses the belief that morality and religious liberty are undermined whenever churches receive government funds. Peter Wehner, the executive director for policy for Empower America, Jack Kemp’s conservative organization, likewise insists that Christians who seek political power “don’t transform the world; they are conformed to it” (p. 43).

(Rev. Eugene Rivers of Boston, a friend of DiIulio’s, informed the *New York Times* that white conservatives opposed government support for faith-based programs because they did not expect to be sharing federal grants with blacks. Robert Woodson Sr., the conservative black founder of the National Center for Neighborhood Enterprise, in a *The Wall Street Journal* column, contended that, “Many of the religious leaders [such as Pat Robertson and Jerry Falwell] who say they won’t touch government money with a 10-foot pole probably wouldn’t touch some of the people who need these services with a 10-foot pole either.” Addressing the National Association of Evangelicals in March 2001, DiIulio chastised white conservatives for not reaching out to blacks [Edsall and Milbank, 2001; Niebuhr, 2001].)

Kurt Schmoke, the former Democratic mayor of Baltimore, and Stephen Goldsmith, the one-time Republican mayor of Indianapolis and now a member of President Bush’s Board of the Federal Corporation for National Service, recount how they worked with religious organizations to tackle urban ills. In the 1990s Schmoke tried to govern a city that had lost 58,000 jobs and averaged 300 homicides annually (Siegel and Smith, 2001). Desperate to halt the city’s social meltdown, Schmoke asked clergy to establish project “Safe Haven.” When children felt endangered walking home from school they could find sanctuary inside a participating church. With federal grants and the cooperation of religious activists, Schmoke also constructed 300 “Nehemiah” houses for low-income, predominately black, residents.

While Schmoke eagerly sought federal and state funds, Goldsmith, who presided over a city that still possessed a tax base, took a somewhat different course. Although he gained national fame as a budget hawk who privatized municipal services, Goldsmith did not abandon impoverished neighborhoods and forsake public-sector approaches to urban ills. In 1997, Goldsmith, in partnership with the city’s “value-shaping non-profits,” established the Front Porch Alliance (FPA) (p. 75). The FPA, for instance, leased an abandoned city-owned fire station to the Robinson Community African Methodist Episcopal Church, which wanted to use it as a community center. City agencies also transformed crack alleys into parks. Cities, Goldsmith concluded, “will crumble” if “value-shaping organizations” and political leaders do not work together (pp. 77–78).

One final essay warrants notice. Robert Blendon of Harvard’s Kennedy School of Government and his co-authors draw upon polling data from the 1960s and 1990s to mark shifts in Americans’ moral and political values. For instance, four decades ago the majority of Americans trusted the good intentions and efficiency of government institutions. Just a few believed that youths lacked any sense of right and wrong. Today, two-thirds of Americans express distrust of government and more than three-quarters think that youths are amoral (p. 26). Blendon also notes that Americans are

more tolerant of religious and lifestyle differences than was true 40 years ago. At the same time, however, citizens have a greater sense of dread about the future moral health of the nation. This may well be a paradoxical reaction to the growing tolerance of “diversity” in America.

The Diminishing Divide: Religion’s Changing Role in American Politics is a collaborative work by four students of American politics and religion: Andrew Kohut, John C. Green, Scott Keeter, and Robert C. Toth. For readers not versed in the religious and class underpinnings of the New Deal Democratic electoral coalition, and its fragmentation in the past 40 years, *The Diminishing Divide* provides some of the historical context for the issues raised in the Dionne and DiIulio collection. It is much more statistically driven than *What’s God Got to Do with the American Experiment?*

Drawing upon public opinion surveys, including a series conducted under the auspices of the Pew Research Center in the 1990s, Kohut and his co-authors take into account the degree of religious devotion within religious groups. James Hunter, in *Culture Wars: The Struggle to Define America* (1991), contends that earlier sectarian disputes between different religions had, since the 1960s, been supplanted by controversies within denominations between “progressives” and “the orthodox.” The data provided in *The Diminishing Divide* confirm Hunter’s thesis. For instance, Catholics who attend mass infrequently are just as doubtful that religion provides any useful moral guidelines as agnostics and mainline Protestants (p. 29).

The Diminishing Divide points out several apparent paradoxes, including the fact that while black Protestants are just as socially conservative as their southern white counterparts, they are largely unwilling to vote Republican. Historic divisions over the role government should play in the economy not only separate white from black Baptists, but they also keep at least half of America’s Roman Catholics from voting Republican—no matter how much the GOP denounces abortion.

Since the 1960s, white religious conservatives, primarily blue-collar and lower-middle-class southerners, have become Republicans. The Democrats, meanwhile, have embraced upper-income, secularized voters. In the 2000 election, as journalist Thomas Edsall (2001) reported, Gore carried the 25 most affluent counties in the United States while Bush swept lower-income white precincts. Half of the GOP’s voters, however, do not embrace social conservatism, with many libertarians rejecting federal regulation of the boardroom and the bedroom. (For the roots of the libertarian-religious conservative divide, see Rebecca Klatch, *A Generation Divided: The New Left, the New Right, and the 1960s* [1999], and Gregory Schneider, *Cadres for Conservatism: Young Americans for Freedom and the Rise of the Contemporary Right* [1999].)

Currently, blacks and secular Jews are the most loyal Democratic voters, as well as the most hostile to Republicans. Conversely, southern white Protestants are overwhelmingly conservative and the least sympathetic to Democrats (p. 80). One should keep in mind, though, that even in the 1930s, when northern blacks, southern whites, and Jews voted for Franklin Roosevelt, southerners rejected a number of New Deal policies. It was the crisis of the Great Depression, and the military challenge of the Nazi and Japanese empires, that bound such widely disparate groups together.

If one conclusion can be drawn from *The Diminishing Divide* it is this: The political prospects for federal support of faith-based initiatives hinges upon whether policymakers and religious activists can make contemporary social needs their first priority, rather than being weighed down by the burdens of the past. DiIulio, Dionne, and Goldsmith are hopeful that a new history can be written.

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Rodney E. Hero

Language Policy and Identity Politics in the United States by Ronald Schmidt Sr. Philadelphia: Temple University Press, 2000, 282 pp.

The "American dilemma" of race/ethnicity has been most commonly conceived of in terms of the black-white paradigm, and understandably so. The black-African American experience has been uniquely significant and troubling. Its influence on thinking about racial and ethnic relations has likewise been profound and distinct. But there are and have been other aspects, and other groups to consider in the continuously unfolding "politics of democratic inclusion in America." While widely acknowledged, these other dimensions and other groups have received comparatively less theoretical consideration and substantive attention in the political science and public policy literatures. For these and other reasons Ronald Schmidt's fine book, *Language Policy and Identity Politics in the United States*, is a welcome addition.

The central goal of the book, as the third chapter in particular develops, is to "make sense of language policy conflict." As such, the book addresses a set of policy questions, and target groups—Latinos/Hispanics and Asians—whose numbers and relevance are increasing dramatically but whose social and policy situation are much understudied. After detailing a number of specific policy skirmishes that began in the early 1970s and continue into the present (Introduction), and summarizing several areas of policy conflict, Schmidt specifies several preconditions for linguistic conflict: the sheer fact of language diversity, language contact, and language competition. When those conditions converge, Schmidt suggests, two normative perspectives clash. Understanding these perspectives fully, including their roots and consequences are the heart of his analysis.

One perspective, which Schmidt labels linguistic "pluralism," supports various efforts and forms of bilingualism, based on concerns of equality and social justice, particularly

concerns for historically disadvantaged racial and ethnic groups. The other, “assimilationist,” view is grounded in concerns for “national unity” or the “common good.” Much of the book is devoted to developing the normative and substantive policy “dialogues” and debates the perspectives have with each other. In between, Schmidt provides a useful and thoughtful philosophical exploration of identity, individualism, and sense of self and its linguistic dimensions. This lays the groundwork for subsequent analysis.

Each perspective has its own sense of history, or “historical narrative.” American racial and ethnic history as seen by the pluralists is one of considerable discrimination, including “conquest” and “imperialism.” While Schmidt never directly says so, the pluralist view as he portrays it is essentially the “internal colonial” interpretation advanced by Acuna (1988) and others. Assimilationists, on the other hand, have a decidedly more benign view of history, arguing that discrimination is largely a thing of the distant past and, in any case, was an aberration, not the rule, in American history. While Schmidt seeks to provide a balanced, careful discussion, he tilts toward the pluralist sense of history. At the same time, Schmidt is not clear about the causal directions here: Does the position lead to a particular understanding of history, or does one’s sense of history lead to adoption of the particular position on language politics? Or, are those inclined to certain concerns (e.g., equality/justice) thus attracted to the theoretical positions, and, in turn, a certain take on history? These causal interconnections may be beside the point for Schmidt’s analysis, but are of interest nonetheless.

Schmidt devotes an entire chapter to assessing the “flaws” of the arguments and policies proposed by advocates of the two positions. When it comes to contemporary debates about policies, in Schmidt’s rendering the pluralistic position seems to hold the moral high ground because of language’s intimate philosophical relationship with individualism and the self. But what might be called the broader political economy and political ecology of American society makes the pluralists’ policy goals untenable in practice, he suggests. Schmidt seems to criticize the pluralists on practical grounds implying that their position is normatively more “correct” and preferable, but is unrealistic given the power of various social forces. The flaws noted regarding the assimilationists’ views focus more on philosophical grounds.

As well written, accessible, and engaging as the book is, one might wish certain aspects were more clearly developed. For instance, Schmidt mentions in passing Rogers Smith’s work. Smith (1997) and others argue that the “American political tradition” includes several orientations, including the liberal, civic republican (community or communitarian), and ascriptive hierarchy (or inequality). To a considerable extent, Schmidt’s analysis is simply suggesting that language and identity politics are another dimension and iteration of the interplay and tensions of these traditions, with the latter (inequality issues) pulled and tugged by the former two. A major qualification, however, is that language policy may differ in that language is not necessarily seen as an ascribed characteristic but a choice, particularly from the assimilation perspective.

Similarly, the book might have made greater contributions to the literature on Latino and, perhaps, Asian politics and policy, had it been more directly attentive to those. For instance, Schmidt hopes and calls for, but ultimately appears somewhat pessimistic about the attainment of “pluralistic integration” as a major policy goal. He implies that various social, economic, and political forces undermine those hopes and aspirations. But what does that tell us about American pluralism and democracy? What does his analysis imply for such interpretations as “two-tiered pluralism” (Hero, 1992) or “social construction” (Schneider and Ingram, 1993)? Indeed, are the likely

outcomes he projects affirmations of these interpretations? That is, had Schmidt linked his largely normative approach to existing substantive theoretical literature on minority group politics, an already useful work would have been further enhanced.

Observers might also wonder if different dimensions of language policies have distinct politics. Schmidt distinguishes several dimensions of language policy: education policy, access to political rights, and the movement for English as the official language. And each of these has, in turn, several “sub-dimensions.” As Schmidt discusses, each of these policies and dimensions seem to have somewhat unique dynamics. When such policies are addressed and filtered through the complex American federal system, it is not clear that the pluralist versus assimilationist framework alone can adequately explain the nature of the debates and outcomes that emerge. That is, we would fully expect that language policy is not only a matter of ideas—as important as those are—but is also significantly shaped by interest configurations across the many state and local governments, and by a host of institutional factors.

On the whole, Schmidt succeeds in bringing intellectual clarity and insight, along with normative sensitivity and reasonableness, to an important set of policy issues. As the United States has become yet more multi-ethnic, and is going to become more so in the near future, Schmidt’s book provides a conceptual map for understanding certain policy debates that are likely to (re)emerge. His work provides an important vantage point for making sense of America’s recent past and likely future around the politics of democratic inclusion.

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Michael Jones-Correa

Border Games: Policing the U.S.–Mexico Divide by Peter Andreas. Ithaca, NY: Cornell University Press, 158 pp., \$26.00 hardcover, \$15.95 paper.

During the 1990s, in a period when federal government expenditures were shrinking in most areas, spending for policing the United States–Mexico border more than doubled. Spending also seemed to increase despite evidence that border control policies were failing to regulate the two areas of most concern to U.S. policymakers: drugs and illegal immigration. So why did spending for border policing continue to increase? Peter Andreas’ thoughtful and thought-provoking book on U.S. border enforcement policies suggests that the answer lies not in the analysis of numbers of immigrants detained and drug shipments interceded, but rather in the escalating

competition between law enforcers and smugglers, and policymakers' attempts at image management.

In the 1980s, as it became increasingly evident that drugs—particularly imported cocaine and heroin—were exacting a heavy toll on American society, and that illegal immigrants were becoming increasingly visible throughout the United States, politicians and the media increasingly blamed a border that was “out of control.” This charge, Andreas points out, was ironic, since the border had never really been controlled—the United States–Mexico border has historically been porous—yet by the end of the 20th century the federal government had considerably more resources to control it than ever before.

Nevertheless, as policymakers sought political cover, these charges had the effect of increasing the flow of resources for policing the United States' southern border. As border policing increased, it meant that smugglers—whether of drugs or undocumented people—had to adapt or be driven out of their lucrative markets. Those smugglers who survived became part of increasingly sophisticated (and violent) conglomerates operating in the underground economy. Each advance in technique or strategy by law enforcement was matched by a corresponding counter-maneuver outside the law; both were caught up in an escalating cycle of response and counter-response. As the stakes increased, one consequence was the heightened corruption of law enforcement officials, particularly those engaged in policing the drug trade.

Andreas' key insight is that the relationship between law enforcement and smugglers is symbiotic. Thus, more spending on border control by law enforcement officials does not end smuggling—restrictions simply increase the price of the smuggled product, and this only spurs smugglers to try ever more ingenious ways to side-step any controls. Smugglers' persistence, on the other hand, only makes it easier for law enforcement agencies to persuade legislators to give them additional funding to deal with a continually growing problem. Policymakers are willing to acquiesce in these requests for funding as long as this allows them to report that the border is “under control.”

A second point worth noting in Andreas' study (a point made perhaps less clearly and forcefully than it should be) is that the figures law enforcement agencies use to support their demands—the numbers of migrants apprehended, or the kilos of drugs confiscated—are meaningless. The success of law enforcement in capturing and returning migrants have no meaning, for example, without a sense of the total flow entering and staying in the United States. And of these no one has a clue—there are no solid data. To say the figures are meaningless is not to say, as Andreas sometimes does, that these deterrence policies are a failure (we can't judge their failure without data either). The truth is, we have no standards for judging whether these policies are either successes or failures, and therefore we have no yardstick for measuring them against alternative policies. The tragedy (or farce) of this brand of policymaking is that billions of dollars have been spent on policies which no one is sure work, and which have, as Andreas discusses in detail, considerable negative side-effects.

Andreas believes that at bottom policymakers are more interested in image management than in actually getting to the root of the problem. In the cases at hand here, he suggests that drug and immigration issues would be better dealt with on the demand side than on the supply side. But this suggestion is presented only in the conclusion of the volume, without any real evidence to suggest why we should believe a demand-side policy would work better than the supply-side policy the United States has consistently pursued. Certainly, Andreas is convincing in suggesting that we need to reexamine our approach, but what the alternative approach should be is as yet uncharted.

The one fundamental flaw in the book is that it treats all kinds of smuggling as essentially equivalent. This is not the case. Take the two examples Andreas deals with at length in the book: illegal immigration and drugs. The supply and demand scenario works in the case of drugs: The more law enforcement curtails drug smuggling, the more incentive there is to smuggle drugs. But this works only because the price drug users are willing to pay for drugs is, for all intents and purposes, infinitely elastic. The same is not true of immigration. For employers in the United States, if undocumented migration were curtailed enough to decrease the labor supply so that labor became more expensive, it would most likely act as an incentive for employers to switch to more capital-intensive production processes—not to pay ever-increasing amounts for labor. For migrants, if migration into the United States became more difficult, some people would be willing to pay more to enter—hence the rise of professional smuggling operations. However, most migrants are not going to bear increasing costs indefinitely. At some point the cost of migrating outweighs the benefits they expect to receive from migrating. The possibility of death in the Arizona desert and the certainty of paying a considerable portion of their future wages to smugglers will almost certainly deter some from entering the United States. Andreas admits as much when he notes (p. 109) that one effect of recent U.S. border control policy is that Mexican migrants may now be less inclined to pursue the pattern of cyclical movement between their homes in Mexico and the United States. Once in the United States, they are more likely to choose to stay. A policy of deterrence, therefore, is likely to be more effective in the case of migration than in the case of drug smuggling. The question left open for policymakers is how high a price should the United States exact on would-be migrants to reduce illegal immigration—is a “successful” control policy worth the hundreds of migrant deaths each year? Or is this also an “image management” problem? These are the kinds of questions that Andreas leaves us pondering.

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Steven Kelman

Return to Reason by Stephen Toulmin. Cambridge, MA: Harvard University Press, 2001, 243 pp., \$24.95.

If the aphorism is true that fish will never discover water, then academics who spend their entire professional life amid colleagues in an academic discipline, surrounded by the soothing waters of disciplinary assumptions and methods will, like the fish, not notice problems with the life-sustaining waters they take for granted.

Academics in schools of public policy and management are fortunate to be in an environment that makes it more likely that we will be exposed to others who have different perspectives about how best to learn about the world and about the values that public activities should promote. Partly, this occurs because public policy schools contain faculty from different academic disciplines, particularly people trained in microeconomics as well as those trained in political science, sociology, or organization studies. Partly, this occurs because public policy schools have faculty who are practitioners or because academics at public policy schools are exposed to practitioners, who tend to bring a different way of learning about the world and often different sets of values from the approaches academics in general tend to take (Rynes,

Bartunek, and Daft, 2001). All these different perspectives are a good recipe for creativity and better thinking.

In this book by the philosopher Stephen Toulmin, one almost gets the impression that the author has eavesdropped on methodological and epistemological debates of the public policy and management communities, particularly those about the appropriate role for parsimonious modeling, and for rich, context-laden case studies, as ways of thinking about complex issues. Toulmin sees his goal in this book as being “to steer a middle way” between the argument that abstract deductive logic is the only source of knowledge and the claims of post-modernists or others that rationality “is no more than a by-product of Western or Eurocentric ways of thinking.” He wants to argue a case for greater attention to what he calls “reasonableness” over “rationality,” by which he appears to mean respect for various forms of argument, including conclusions from tacit knowledge or intuition, in addition to logical deduction or parsimonious generalization, as well as greater attention to the peculiarities that differentiate specific situations in addition to the invariant features shared by many situations. And he criticizes what he calls “the bureaucratization of knowledge,” whereby disciplines wall in only certain considerations and approaches as relevant, leaving other considerations or approaches unexamined.

Before the 1600s, Toulmin argues, there was a “balance of reason” that recognized “multiple ways of thinking” rather than giving one pride of place. So, for example, Montaigne, whose essays drew conclusions based on the everyday experience of individual people, was at the time considered a philosopher, not, as he is today, a literary figure. With the religious wars after the Reformation, and taking as its inspiration Newtonian physics, there developed a new domination of “rationality,” which drew conclusions about people and about ethics based on formal logic that abstracted from the situations of particular individuals. (Note that this use of the word “rationality” differs from its use in economic theory to denote behavior that maximizes attainment of one’s values.) The preference for generalizations abstracted from particular individuals occurred, Toulmin suggests, in response to decades of religious wars that produced (for example, in Leibnitz) the hope that one could develop a general religious doctrine to which all could adhere that would be abstracted from the particularities that divided the different doctrines over which people were killing each other.

The logical-deductive approach that became enshrined as rationality partly reflected a view that attention to the general and invariant allowed one to do a better job of knowing and understanding, including understanding the particular. It also, Toulmin argues, reflected a view that the general was more interesting (in the sense of personally engaging) than the particular—a view different from the view in literature, where the features that distinguish individual characters from each other are typically seen as more interesting than those held in common. (“No one could mistake Hamlet for Sancho Panza,” Toulmin notes.) Much of Toulmin’s book is a plea to pay greater attention to the particular.

Toulmin seems to assume that the only way to take particulars into account is to move from mathematics to situated individual narrative. The book thus contrasts formal logical-deductive reasoning that ignores variation across cases, with reasoning drawing conclusions based on rich information about a particular situation. But this of course ignores statistical empirical work based on a large numbers of observations, one of the staples of academic research appearing in this journal, for example. If the objection to a timeless, general deductive model is that it doesn’t take certain specifics of individual situations into account, then there can be ways to deal with that problem other than going to narratives of individual situations—one may use regression analysis

of numerous cases that display and don't display the putatively important specific of an individual situation and see (using separate predictor variables or perhaps interaction terms) what contribution a specific feature makes to explaining the phenomenon of interest. This won't necessarily work, perhaps because it is difficult to operationalize the feature or to gather data on enough observations, but in principle there is surely no necessary contradiction between mathematical modeling and taking specific features of concrete—as opposed to timeless—situations into account.

This distinction comes out in Toulmin's somewhat frustrating discussion of what he sees as the unwillingness of development economists to examine cultural and historical peculiarities that influence economic growth, as opposed to the role of invariant human reactions to generic incentives. It is probably true that the professional training of many economists inclines them to the view that general and invariant reactions to incentives are more important than the idiosyncrasies of history and culture. But it is not true that a scholar who is inclined to believe in the importance of historical or cultural factors must proceed only through rich contextual analysis of individual cases, perhaps drawing conclusions from a case based on some sort of intuition. Surely, that might be one fruitful way to proceed. But the best way might be through statistical examination of a large number of cases, with the cultural or historical factors operationalized as variables that can be matched for predictive power against invariant incentive-related variables. And, indeed, research of this sort exists.

Toulmin notes that doctors and nurses exposed to the theories of moral philosophy in medical ethics courses tend to revolt, finding such instruction frustrating or even useless. Toulmin attributes this to the lack of usefulness of theory. "Despite all the subtlety and depth they display in abstract general terms, the conclusions of a book like John Rawls's *Theory of Justice* provide no effective criteria for settling real-life disputes in actual cases" (p. 129). But surely this is going too far—indeed, it seems like Toulmin is himself here drawing an abstract general conclusion rather than looking at the specifics of individual cases. It is one thing to state that people, such as doctors and nurses, who are not trained in moral philosophy will have trouble using abstract theory to draw conclusions about individual cases. One might even make the broader claim that people other than academics tend to have trouble using abstract academic theories in general to draw specific conclusions. That observation tells us something helpful about differences between academics and practitioners. It is another thing to make the bold, and unjustified, claim that generalities are useless in particular situations. Tell that to a non-academic who nonetheless uses countless generalities in everyday life to draw conclusions about what foods to eat or when to set the alarm clock in the morning. Indeed, it is plausible that the ability to use one's brain to develop generalizations and then apply them to particular situations is one of mankind's most important evolutionary advantages. And I believe that people trained in moral philosophy, or inclined to use abstract academic theories, can indeed use *A Theory of Justice* to help draw conclusions about specific ethical situations.

Finally, I worry about Toulmin's alternative to logical-deductive reasoning, namely conclusions derived from intuition or tacit knowledge. To be sure, there is a good deal of evidence for the intriguing proposition that people do have the ability to draw conclusions based on reasoning processes they are unable to articulate, that "we know more than we can say." (A pioneering treatment of this appears in Polanyi [1983]; an interesting example of scientific experiments suggesting the existence of tacit knowledge is Reber [1993].) However, because tacit or intuitive reasoning lacks transparency, its conclusions cannot be subject to scholarly scrutiny. This is a serious limitation, especially when it is applied to explaining radically over-determined individual cases with many possible explanatory variables. The goal should be to

make tacit knowledge explicit so we can examine its veridicity and so others can use it more broadly. Something analogous takes place in organizations that seek to make tacit knowledge explicit so it can be more widely used within the organization (Nonaka, Takeuchi, and Takeuchi, 1995).

Having said all this, I still enjoyed Toulmin's book. He brings a wide sweep of examples from many disciplines. He makes a case for attention to the particular that, whatever its flaws, is a good reminder of the humanistic bases of the social sciences. And, by having different traditions confront each other, he reminds us that such confrontation is often a path to insight.

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Ending Welfare as We Knew It by R. Kent Weaver. Washington, DC: Brookings Institution, 2000, 482 pp., \$44.95 cloth, \$13.95 paper.

Budgeting Entitlements: The Politics of Food Stamps by Ronald F. King. Washington, DC: Georgetown University Press, 2000, 256 pp., \$65.00.

In 1996 Congress enacted the most significant revision of federal welfare policy since 1935. Why did this legislation pass when so many previous attempts at comprehensive welfare reform had failed? This is the question Kent Weaver asks and successfully answers in *Ending Welfare as We Know It*, a book likely to be recognized as the definitive account of welfare politics in the 1990s.

Weaver's book describes and analyzes virtually every important aspect of contemporary welfare policy. It includes a concise history of reform efforts from Nixon's Family Assistance Plan (FAP) to the Family Support Act of 1988 (ch. 4), a sympathetic look at Clinton's attempt to deliver on his campaign promise to "end welfare as we now know it" (ch. 9), a detailed examination of the odyssey of welfare legislation in the 104th Congress (chs. 10–12), and an evaluation of the initial consequences of the new law (ch. 13). Interspersed with these narratives are sophisticated examinations of the fundamental features of means-tested programs (chs. 2–3), the intellectual trends and policy research of the 1990s (chs. 5–6), the contours of public opinion (ch. 7), and the varieties of interest group activity (ch. 8). Not only does Weaver avoid the narrow partisanship that characterizes much of the writing on this subject, but he skillfully combines first-rate political science analysis with a detailed examination of welfare policy.

Three parts of Weaver's argument deserve special attention. The first is his explanation of the "traps" or dilemmas that confront those who attempt to reform means-tested programs. Most important is what Weaver aptly terms the "dual clientele trap" (p. 45):

Policymakers usually cannot take the politically popular step of helping poor children without the politically unpopular step of helping their custodial parents; they cannot take politically popular steps such as increasing penalties for refusal to work . . . without also risking the politically unpopular result that poor children will be made worse off.

Nearly as important is the “perverse incentives trap” (p. 48):

No policy intended to help poor families can entirely avoid creating incentives for recipients that policymakers and the public are likely to see as undesirable, and no plausible welfare reform initiative is likely to avoid creating some new perverse incentives or making some existing ones worse.

Weaver uses these insights to explain the failure of previous reform efforts (pp. 84–99) and to help us understand why 1996 was different (pp. 359–374). He demonstrates that one cannot hope to explain the twists and turns of welfare politics without appreciating the inherent dilemmas of welfare policy.

Second, Weaver argues that one cannot understand the behavior of politicians without seeing how their public positions are “determined in large part in relation to the positions taken by other participants” (p. 37). A central feature of welfare reform in the 1990s was President Clinton’s famous (or infamous) strategy of “triangulation”—purposely positioning himself midway between liberal Democrats and conservative Republicans. Moderate Democrats in Congress then adopted what Weaver calls a “matching strategy,” appeasing their anti-welfare constituencies by endorsing a position “no further to the left on welfare reform than a Democratic president” (p. 40). Republicans in Congress at first engaged in “strategic disagreement”—insisting on provisions they knew were unacceptable to Democrats in order to keep the issue alive—until they decided they needed to pass legislation to show they were capable of governing.

Third, Weaver offers a balanced and perceptive analysis of the interplay between public opinion and elite opinion on welfare. He emphasizes the public’s remarkably negative view of “welfare” in the mid-1990s (almost 70 percent of the public believed that “the welfare system does more harm than good” [p. 174]), but also shows that the public simultaneously supported programs that provide “assistance to poor children” or are designed to “give people skills to be self-sufficient” (pp. 172–177). Public opinion on specific reforms was both mixed and volatile. The public was united only in its distaste for the status quo. In this context, intellectual trends in universities, think tanks, and the media played a pivotal role. Research on illegitimacy and long-term dependency led some respected liberals to rethink their opposition to time limits and work mandates. Conservative think tanks used their extensive contacts in the 104th Congress to keep welfare reform on the front burner.

Although Weaver’s book is well written, it is not an easy read. He does not offer simple formulae or sonorous aphorisms to explain the passage of welfare reform legislation. Instead he uses welfare reform as a window for understanding the complexities and ironies of American politics. When I used this book in an upper-level undergraduate class last year, the students were at first overwhelmed by the amount of material Weaver presents. But after a while they learned to follow the logic of Weaver’s argument, and many of them later listed it as one of the best books they had read on American politics and policy.

Ronald King’s *Budgeting Entitlements* offers an instructive contrast to Weaver’s *Ending Welfare*. The food stamp program, which King describes in detail, experienced enormous growth in the 1970s and 1980s—a period in which AFDC (Aid to Families with Dependent Children) spending remained level, and real benefits plummeted.

Despite Republican efforts to block-grant food stamps, the program retained its entitlement status. King's interest in the program is not so much to explain the evolution of public policy as to use this example to show how "the form of budget rule affects the conduct and content of welfare policy" (p. 4). As he puts it, "this is a study of the causal impact of rules upon results" (p. 8).

King addresses this abstract political science question by examining the consequences of the food stamp budget cap of 1977–1985. He astutely points out that a budget cap on an entitlement program is an effort to reconcile the logic of individual rights with the demand for legislative control over federal spending (pp. 23–24). For King, experimentation with the cap is also a product of a central trend of the 1980s, the "fiscalization of policy."

Most of *Budgeting Entitlements* is devoted to a detail review of congressional action in the pre-cap (1964–1977), cap (1977–1984), and post-cap (1985–1996) periods. We are treated to round after round of authorizations, appropriations, supplementals, budget resolutions, and reconciliation bills. These narratives are punctuated by efforts to use formal models to explain budget outcomes.

What, in the end, do we learn about the impact of budget rules? At the beginning of the book King implies that the cap would inevitably have a significant effect on budget outcomes because it shifted the budgetary "reversion point," that is to say, "the outcome that will prevail by default if no negotiated agreement is reached" (p. 230). A change in the "reversion" point (p. 27):

affects the kinds of compromise various players would find acceptable. This, in turn, affects the demands they would make and their ability to realize these demands. A shift in the reversion point, through backwards induction, thus alters the logic of game bargaining and the expected solutions from play.

Such reasoning will sound familiar to anyone familiar with current trends in political science.

It comes as something of a surprise, then, to read that the caps proved "cumbersome in operation and irrelevant in effect" (p. 162), that they were "more symbolic than effective" (p. 118), and that "food stamp expenditure caps were permitted to expire as an unnecessary complication that brought neither budgetary nor political relief" (p. 238). Indeed, King announces his agreement with Aaron Wildavsky's assessment that budget gimmicks such as Gramm-Rudman-Hollings are doomed to fail: "The conclusion is that procedural innovation cannot be substituted for tough policy decisionmaking" (p. 165).

The central problem with King's analysis is that his observations do not fit his model, and he never manages to tell us why. A likely explanation is that almost all politicians—even conservative Republicans in Congress and the White House—viewed the "reversion point" under the budget cap as politically unacceptable. They were unwilling to accept blame for cutting "food for hungry children" by 10 percent, 20 percent, or 30 percent in a month. Food stamp advocates—as shrewd a set of political strategists as you will find in Washington—knew this and refused to bargain away hard-fought gains. Ironically, in 1979 they were even able to use a cap-inspired budget crisis to increase benefits.

King's focus on budgetary rules and game-theory reversion points seems to blind him to the most basic features of food stamp politics—most notably the role the program plays as a welfare supplement and equalizer (to use Richard Nathan's terms), the tenuous link between program benefits and nutrition, and the raw demagoguery periodically unleashed by the "hunger lobby." His analysis would have benefited from

more attention to the themes developed by Weaver: the “dual clientele trap,” the politics of “blame avoidance,” the ambiguity of public opinion on welfare, and the strategic interaction of shrewd politicians. Among the many lessons Weaver teaches us is that politics is too important—and too interesting—to be confined to the formal models of political science.

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You Don't Always Get What You Pay for: The Economics of Privatization, by Elliott D. Sclar. Ithaca: Cornell University Press, 2000, 184 pp., \$20.00 cloth.

As we enter the 21st century, public managers face such challenges as recruiting and retaining a diverse workforce, managing technological change, and finding creative ways to deliver public services to citizens. Broadly speaking, Sclar focuses on the service-delivery challenge by addressing the fundamental issue of how government should organize to provide public services. Government can “make” public services internally, “buy” public services from external sources, or use some combination of “make and buy.” The make or buy decision is developed in the context of the contracting approach to privatization. The book focuses on the problems and pitfalls of service contracting, caused partly by the nature of public services, the contracting process, and informational problems associated with any principal-agent relationship.

Sclar begins by making the economic argument for reliance on the private sector. Competition among many private firms should lead to service cost and quality that is equal to or better than what the public sector can provide. The standard market model is the basis for this assertion. The best outcome should be achieved in a market with a large number of unrelated buyers and sellers, each pursuing his own self-interest, with few or no barriers to entry, and with the profit motive providing the discipline for each producer to keep production cost low.

After making the economic case for contracting, Sclar expands on the subject by dealing with several critical questions:

- What type of services should the public buy from the private sector?
- Are private sector organizations more efficient than their public counterparts?
- Is the standard economic model of competition relevant to public contracting?
- Given the nature of public activities, what type of contracting approach is most compatible with public contracting?

What type of services should the public buy from the private sector? Arguing that the decision to contract is complicated by the nature of public services, Sclar places all goods and services in three categories: private, public, and publicly provided. Private goods (shoes, a theater ticket) are clearly the domain of the private sector, and public goods (national defense, a lighthouse) of the public sector. Publicly provided goods are often candidates for contracting out because they are provided in some measure by the private sector (for example, bus transit, refuse collection, vehicle operation and maintenance, mental health services, and emergency medical services). Services in the publicly provided category involve a range of complexity. A key to

successful contracting is the ability for the service to be adequately specified. Specifying work for residential refuse collection, which has a readily observable production process, is much different than specifying work for a mental health contract in which the production process is complex and uncertain, and effect on patients difficult to measure. The type of service contracted influences the contract relationship and the ease with which it can be monitored.

Sclar next investigates the question of whether private sector organizations are more efficient than their public counterparts. Using the case of bus transit, he argues that a fair determination requires that a comparison of private and public modes be conducted to determine which is less expensive. One idea he introduces is the notion that a fair comparison must allow for other potential influences on service cost. For example, consider the relationship between scale of operations and the unit cost of bus transit in terms of operating cost per vehicle hour. Sclar finds a positive relationship between the two variables, which support the finding "...that transit productivity is determined largely by factors external to the organization of the transit operation" (p. 53).

Another issue related to comparative cost is that of transaction cost. Transaction cost is the cost incurred by a public organization to select and monitor a contractor. The true cost of a contract operation is the production cost (normally the annual payment made to the contractor) plus the internal costs incurred by having a contract operation. A fair cost comparison should include these transaction costs as part of the contractor cost. Since the production process is heavily dependent on factors other than organization type (public or private), and since transaction cost is rarely considered, Sclar concludes that cost comparative studies do not yield a clear-cut result one way or the other.

The third question Sclar poses involves the relevance of the standard economic model of competition to public contracting. Sclar argues that it is not relevant for a variety of reasons. The standard economic model assumes there will be robust competition for the services in question, but this is rarely the case. Most contracting actions involve no or minimal competition. Indeed sometimes the public sector is the market. Consequently, rather than competitive markets, public contracting involves interaction with one or a few firms. When competition is removed from the contracting process selecting a cost efficient private provider becomes a managerial challenge because as Sclar puts it, "Competitive market theory provides no guidance" (p.75). The question becomes how best to construct the relationship between the public organization and a private firm in the context of an imperfect market with few sellers and where information available to the public decisionmaker is imperfect and uncertain.

What type of contracting approach is most conducive to public contracting given the complexity of public services, doubt about the cost superiority of private providers, and incompatibility of the standard market model to public contracting? In particular, an approach is needed that better fits the dynamic relationship between the demander and the supplier. Sclar invites us to expand our thinking in terms of how a contract relationship can be formed. One paradigm is the classic or complete contract implied by the standard market model. A complete contract is characterized as such because "...generally the contract terms effectively capture all present and future rights and obligations between the parties" (p. 102). Some characteristics of a complete contract are low frequency of contracting transactions (a one-time or infrequent need), little uncertainty about the work to be undertaken (the process is known and understood), and a specific output within a specified timeframe (repairing potholes or repaving streets).

Another paradigm is the incomplete contract. In contrast to complete contracts, incomplete contracts do not capture all of the present and future rights and responsibilities of both parties. A contract is incomplete because the parties cannot predict and understand the effect of future contingencies. Incomplete contracting also involves the condition of information asymmetry, which leads to two particular problems in the principal-agent relationship, namely moral hazard and adverse selection. Adverse selection involves selection of agents in a manner adverse to the principal (not knowing some information relevant to the contractor selection decision); moral hazard involves situations wherein the agent acts in ways inconsistent with the goals of the principal. Both are caused by the principal's inability to obtain or understand information relevant to selection of an agent and the agent's behavior in carrying out the terms of the contract. Techniques to mitigate the effect of asymmetrical information involve developing contracts with the right performance incentives and penalties to align the interests of principal and agent.

Incomplete contracts generally involve recurring needs over extended periods, and involve problems related to information asymmetry. Contracts for residential refuse collection or public bus transit are examples. Incomplete contracting is also an approach that involves significant cost on the part of the public agency to monitor contractor operations in order to lessen the moral hazard problem. Also, failures in privatization are often associated with the problems of incomplete contracting (p. 122):

...[S]ervice provision occurs under an ever more involved cost-plus-profit reimbursement scheme embedded in a contract laden with penalty clauses for poor performance, incentive clauses for acceptable performance, ever more detailed cost and product definition clauses, and finally arbitration clauses for dispute resolution over the meaning of other clauses.

Since opportunities for complete contracts are rare, and incomplete contracts are costly for government to award and monitor, Sclar concludes that a paradigm shift to relational contracting is necessary. Relational contracting replaces the formal legalistic governance structure associated with incomplete contracting with a bilateral governance structure based on trust and cooperation between organizations. The goal is to replace an adversarial relationship with one of cooperation and mutual benefit.

Situations involving relational contracting have the same characteristics as incomplete contracting. However, the formal legalistic approach to managing the relationship is replaced with bilateral governance focusing on trust and cooperation between the organizations. Cooperation is conceived as the only viable alternative to the problem of how to develop appropriate incentives to reduce the problems of adverse selection and moral hazard associated with incomplete contracting. The managed competition approach, which the city of Indianapolis applied to the Indianapolis Fleet Service, is presented as an example of relational contracting. Public personnel eventually won the right to provide fleet services. Success of the arrangement is due to the cooperation between labor and management.

I found this book to be an interesting read. It contains information relevant to the academic and the practitioner. The central argument of the need for a fresh approach to public contracting is developed logically and the text is filled with examples to illustrate the various points. In my judgment it contributes to the literature by beginning to establish that when considering the efficiency of a particular service, delivery mode (public or private) may not be the most influential factor. Scale of

operation was shown to influence bus transit efficiency; other characteristics of bus transit may also be important predictors of efficiency. It provides a good framework for thinking about the relationship of contracting techniques to the nature of public services. Simple services are compatible with a simple governance structure, while complex services require a more complex governance structure (relational contracting).

My only criticism is that the idea of relational contracting left me hungry for further examples. The managed competition example (the Indianapolis Fleet Service) is a relational contract within a public setting. What would a relational contract look like in a public/private setting? For example, if a jurisdiction wanted to convert a residential refuse-collection contract from an incomplete to a relational contract, what contracting techniques and processes would be employed? More specifically what type of specification of work would be written? What solicitation method would be used? What type of contract would be awarded? What types of incentive and penalty provisions would be included in the contract (if any)? For how many years would it be awarded? Would more than one firm be awarded contracts? Sclar provides a theoretical framework to begin exploring such questions.

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The Initiative and Referendum in California, 1898–1998, by John M. Allswang. Stanford: Stanford University Press, 2000, 352 pp., \$61.00 cloth, \$24.95 paper.

By Popular Demand: Revitalizing Representative Democracy through Deliberative Elections, by John Gastil. Berkeley: University of California Press, 2000, 267 pp., \$48.00 cloth, \$18.95 paper.

These books look at alternatives or improvements to representative democracy in the United States. As such, both books are more concerned with examining or improving the political process than with policy analysis. Both authors believe that American democracy as currently practiced has obvious failings, such as low voter turnout, cynical campaigns, and cynical voters. While Allswang's review of the initiative process is more detached, Gastil's examination of deliberative citizen panels is an advocacy piece.

John Allswang's review of the initiative and referendum process over the last hundred years in California is comprehensive, straightforward, and balanced. A historian who cares about the Progressive roots of this governmental process, Allswang says, "This is a work of history, not policy analysis" (p. 6). He notes how the initiative has served as a vital political force for nearly a century in dealing with some of the most divisive and important issues in California and indeed national politics, from cultural issues (civil rights) to economics (Proposition 13 tax cuts) to trial lawyers versus insurance companies. Appendix A contains a chronological listing of all direct legislation matters on California ballots since 1912, noting the topic covered and whether the proposition was defeated or approved. The text then takes those issues the author considers most important or famous and examines them in more detail. Appendix A contains useful information, and yet it might have been even more helpful if it had contained a column devoted to whether the measure was struck down or modified by the courts, since judicial review has become such an important part of the initiative process.

The book includes a comprehensive bibliographic essay covering the literature on direct democracy, providing the reader with numerous sources for further works on California, other states, and a national perspective. With footnotes throughout the text, the reader is led to sources that cover various initiatives in much more detail than this 100-year review can provide.

The author uses correlation coefficients to measure the relationship of ballot propositions to each other, using the voting results in California's 58 counties (weighted). This produces helpful insights, as in noting the high correlation among the three propositions backed by the insurance industry dealing with limiting attorneys' fees. But he should somewhere note (in Appendix B, if not elsewhere) the limits of making individual-level inferences from aggregate-level (county) data. That is, there is no assurance that the million people voting for a proposition in Los Angeles County are the same million people who voted for another proposition. Only individual polling data can answer that.

Allswang uses Field polls to assess the public's attitude toward the initiative process, and concludes that while voters in general see initiatives as a "positive force," they "are nonetheless aware of some basic weaknesses in the system" (p. 241), including the difficulty of knowing what key interests are backing or opposing initiatives, the need for vast sums of money for campaigns, and the complexity of some initiatives.

Allswang expresses concerns about the process, noting that in low turnout elections, the number of people deciding propositions is quite small, and hardly a representative sample. He further notes that the complexity of measures, when combined with campaigns of simplification, means that voters can easily become confused. Because the bargaining and compromise common in the legislative process are often lacking in the initiative process, Allswang charges that the initiative "tends to create bad legislation" (p. 246). He cites as an example the "3 Strikes" proposition in 1994, which straightjackets judges at a time of an already overcrowded prison system. Yet he recognizes that the initiative process is here to stay.

Like all of us, he is at times better at asking questions than at answering them: "Has direct legislation," he asks, "become more frequent and draconian because state legislatures have avoided basic issues, or is it the opposite—that state legislatures have become more inactive because they have been preempted by direct legislation?" (p. 248). He answers, "It might be either, both, or neither" Still, his book is a useful review on an important topic.

A professor of rhetoric and communication theory, John Gastil is one of a number of scholars who believes that deliberative panels addressing public issues and candidates for office are the best means to counteract the cynicism of the American public and "revitalize" representative democracy. He also supports deliberative panels to consider initiatives on the ballot, as in California. Supported by the Kettering Foundation and active in observing national issues forums during the 1990s, Gastil is trying to develop a structure for a more "representative, deliberative, articulate, and influential public voice" (p. 176). Chosen by random selection and paid to participate, these citizen panels would meet to deliberate over several days, guided by a facilitator, hearing witnesses, and gaining information. The results of these panels would then be reported by the media and in voter guides, hopefully improving media coverage of politics and policy and improving the deliberative process for citizens as a whole. Panels of this type have already been created, such as National Issues Conventions organized with the help of Professor James Fishkin. Gastil is encouraged by these results, but recognizes that for them to be truly successful beyond helping to empower those who attend, the results from the panels must find their way into the mainstream electoral process. That has generally not happened. To move more in that direction,

Gastil hopes that the results of these panels would be used to order candidate names and initiatives on the ballot, with the most popular listed first, and perhaps even include panel recommendations on the ballot. This is a tall order, which would require government to acknowledge the role of these panels.

Gastil notes that no other reform ideas, such as campaign finance reform, directly address the question of the quality of deliberation that is going on among the electorate. It is the need to improve the decisions citizens make that he considers the most important element for revitalizing representative democracy.

To his credit, Gastil addresses criticisms of deliberative citizen panels, such as the fact that insufficient or flawed information might lead to poor group decisions, or that “groupthink” might take over. He does his best to structure the process to avoid these problems. For example, to protect against groupthink, citizen panels would have “a diverse membership, neutral procedures, and a professional moderator” (p. 168). Yet Gastil also notes that he has specified “general properties” of deliberative citizen panels, not everything about them (p. 163). Details may end up being important, especially if the panels develop real clout (influencing the media and the electorate), as he hopes they will.

Gastil believes that economic elites exercise a disproportionate influence on public opinion and the political process, and notes that some fear the same thing will happen in deliberative panels, which will be structured to represent a cross section of people. Gastil does not think that elite thinking will be as powerful, “[i]f panelists consider all views equally...” (p. 191). But on issues as complex as tax policies or health care, it is difficult to imagine how panelists could possibly consider all views equally. Still, such panels could certainly be an improvement over the way current commercial television tends to cover public policies, which is to ignore, sensationalize, or treat them as merely a “game.” Statements such as the above from Gastil remind us that his views are still general, with no guarantees of high-quality deliberation. Yet many, myself included, believe he has provocative ideas worth trying as a way to reduce the cynicism of the electorate and the elected, and to improve American democracy. Few would disagree with Gastil’s goals, as they go to the core of what a healthy democracy embodies. Given our current environment of media ratings pressure, sound bite politics, and attack ads, finding any successful way to get to these goals, including citizen deliberative panels, will be difficult, and controversial.

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Gun Violence—the Real Costs, by Philip J. Cook and Jens Ludwig, New York and London: Oxford University Press, 2000, 224 pp., \$25 cloth.

Gun Violence—the Real Costs is a thoughtful and thought-provoking book, but one that raises harder questions than it answers. The authors’ stated purpose is to show that the costs of gun violence are so high—\$100 billion by their calculations—that remedying gun violence should be a higher priority for society. The strength of the book is the disparate data sources that are brought together and the authors’ analysis of the problems with the current systems for collecting information about gun violence. Its weakness is that the authors overstate the costs to make their case, and underestimate the potential resistance to their remedies.

The book begins with the Columbine High School's shootings, establishing that it is about the impact of intentional third-party gun violence. Yet, of the baseline 32,000 gun deaths used for the cost analysis, 54 percent were suicides. The authors concede that many of the suicides are by the old and sick and thus probably reduce societal costs. More fundamentally, suicides do not contribute to societal fear about gun violence and the subsequent willingness to pay to reduce gun violence.

The authors focus on reducing gun violence, not overall crime, arguing that substituting less lethal weapons for guns will reduce death and injuries at the same crime level. This brings into question their assumption that reducing homicides will reduce police costs because investigating homicides is much more expensive than investigating assaults. The argument ignores the allocation of resources problem: With fixed budgets, police put their resources into solving the most serious crimes. If homicides were turned into assaults because the weapons are less lethal, the police costs would shift to solving assaults, with no net reduction in spending. The authors' assumption that violence prevention costs would fall in proportion to the homicide rate is also questionable because of the unitary risk problem. Police cannot wear 30 percent fewer bulletproof vests because shootings fall 30 percent, nor can schools screen 30 percent fewer students. In most cases, violence prevention costs cannot fall until violence levels fall much more dramatically than the authors postulate.

The authors' data show that determining the costs of gun violence is complicated by race, class, and crime in ways that are not adequately analyzed. Intentional gun deaths, other than suicide, occur at about 1.1 per 100,000 for married adults older than 30 who have four or more years of college; at 133.5 per 100,000 for black men aged 18–29; and 38.5 per 100,000 Hispanic men aged 18–29. A 1996 Philadelphia study showed that 93 percent of homicide victims had a criminal record, and a study from Richmond, Virginia, showed that the risk of gunshot injury is 22 times higher for adolescents involved in crime than for those where are not. These are powerful numbers, forcefully presented, but their implications are not fully developed. The authors recognize that gun deaths of criminals have limited financial effect in traditional economic terms (and may be economically beneficial), with the highest economic costs being secondary to non-fatal injuries. The authors attempt to find non-economic justifications for reducing these deaths and ignore the harsh economic reality that more-lethal guns would reduce overall societal costs when criminals are the victims.

The largest part of the \$100 billion estimate of the cost of gun violence comes from the authors' contingent valuation study of what people will pay to reduce gun violence. As they note in the Appendix, these studies are fragile and subject to many methodological biases. Putting aside general criticisms of the validity of contingent valuation studies, this study is flawed by its failure to include risk differential data. While the authors recognize that asking, "What will you pay to reduce gun violence by 30 percent?" is different from asking, "What will you pay to keep a drug dealer downtown from being shot?" they did not modify their contingent valuation questionnaire to account for this risk differential. They argue that since their subjects showed about the same willingness to pay for reductions in gun violence as for reductions in workplace deaths, the risk differential was not a significant confounding effect. It is equally plausible that the study measured a generic valuation of life and tells us little about the willingness to pay to prevent violence to a population that is distant from the lives of many of the study subjects, or to prevent the death of criminals.

The authors identify three key strategies to reduce gun violence: reduce the availability of illegal guns; increase the penalties for using a gun in a crime; and use intrusive police techniques to find illegal guns in the community. Access to illegal

guns will be limited by eliminating the currently unregulated secondary market (private sales between individuals) and by developing “personalized guns” that could not be used if stolen from their legal owner. This ignores the problem of gun smuggling and the compensatory substitution of an illegal black market for the legal secondary market. It is possible that reducing the secondary market will ultimately increase the availability of illegal guns by stimulating the smuggling of cheaper types of guns than are commonly available in the legal distribution channels.

The authors argue that increasing prison terms for illegal gun use reduces gun violence, and they review studies that indicate that increased police searches and surveillance of potential gun users reduces gun violence. The demographics of gun violence mean that police enforcement would be targeted at poor neighborhoods, and, within those neighborhoods, at young black and Hispanic men. The authors do not explore the implications of this racial targeting, saying that because the affected communities also experience the major benefits of gun violence reduction, they will accept the burden of enforcement. Given the criticism of Mayor Rudolph Guiliani’s efforts at increasing police enforcement in minority communities in New York City, and the national furor over racial profiling, it seems unlikely that the communities would be as accepting as the authors hope.

This book is a good beginning to a more rigorous analysis of the costs of gun violence. The authors deflate some of the previous estimates of very high medical costs and lost productivity costs from gun violence. While their contingent valuation study is flawed, it provides useful background for a study that better incorporates risk differentials. The analysis of existing data sources and their limitations should be required reading for policymakers who deal with demographic information on violent injuries.

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