on executive responsibilities. Therein was a "political opening for the president to thumb his nose at a Congress (usually . . . dominated by the opposite party) pursuing an executive branch investigation" (p. 87). The author's argument is plausible but insufficiently developed. The assumption is that when presidents were themselves accountable for investigating executive branch misdeeds, they were more likely to act responsibly and unlikely to use the pardon power to make their troubles vanish. Then, Congress undermined presidential responsibility with the judicially appointed special prosecutor provision.

Consequently, presidents discovered the utility of dealing with a special prosecutor's reports and possible prosecutions as merely political acts. In that context, the pardon power became an available countermeasure to those political acts, rather than an instrument to be used only for mercy or the public interest. Also, the incentive for politicizing pardons increases when a president no longer faces reelection, as was the case with G. H. W. Bush, Clinton, and George W. Bush. Finally, Crouch describes the presidential proclivity for self-interested pardons as violating constitutional values and deserving reproof. He writes: "The framers intended pardons to be motivated by mercy . . . or made in the public interest, not used in the service of the president's private interests" (p. 146).

This book provides a rich description of the pardon power's uses, casting light into an overlooked corner of presidential power. That is a substantial contribution. However, the analysis of the effects of the independent counsel law as well as the normative assessment of the pardon's uses contribute less than they otherwise might. Crouch weakens both aspects of his book by overlooking salient work on presidential unilateralism, the contributions of Terry Moe, William Howell, and others. The author draws a bright line between proper uses of the pardon and self-dealing uses. Yet Terry Moe's "politicized presidency" is inherently self-dealing, acting in a competitive system to maximize power and political accomplishment. Nor does Crouch consider that the power-seeking president may find little difficulty in justifying as public interest the decisions that may appear to opponents as self-dealing. Thus, this able contribution to our knowledge of the pardon power might have accomplished more regarding presidential pardoning behavior had he brought a wider intellectual scope to his project.

The Political Influence of Churches. By Paul A. Djupe and Christopher P. Gilbert. New York: Cambridge University Press, 2008. 294p. $88.00 cloth, $23.99 paper.
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— David E. Campbell, *University of Notre Dame*

The study of religion and politics has come a long way. For many years, religion was relegated to the fringes of political science and often treated as a sui generis field of study. The early scholars working on religion often had to justify their choice of subject, as many political scientists questioned whether religion really had political relevance. Those days seem to be gone, as the real world of politics has made it abundantly clear that religion has a profound influence on the American political landscape. As religion has come into its own as a legitimate area of research, however, it has remained an open question whether the study of religion could provide theoretical insight into political phenomena beyond religion. Thus, the publication of *The Political Influence of Churches* comes at a propitious time. Just as more and more scholars see the value in studying religion's role in American politics, this book provides an empirically rich account of political discussion and recruitment within congregations.

While the book is ostensibly about churches, its ambitions for developing and testing theory far exceed the domain of religion; it should not be mistaken for being "just" about churches. This is really a book about the social antecedents of political activity, and thus revisits old debates between proponents of the Columbia and Michigan approaches to studying political behavior. Where should researchers focus their attention: communities or individuals? Where should we draw our theoretical inspiration from: sociology or psychology? The new twist on this old debate is extensive evidence from a particular type of community or social context, the religious congregation (which also happens to be the most common form of association in America).

The book is based on a survey of clergy and parishioners from two mainline (that is, liberal) Protestant denominations, the Episcopal Church and the Evangelical Lutheran Church of America (ELCA). After having surveyed nearly 2,500 clergy, Paul Djupe and Christopher Gilbert randomly selected 50 from each denomination and asked for permission to survey their congregants. They ended up with data from 60 congregations, with an average of 100 surveys per congregation.

I suspect that most scholars, either of religion specifically or political behavior more broadly, will find the basic argument of the book—that congregations deserve more attention as microenvironments—to be noncontroversial. But that basic claim hardly does justice to the many arguments made and conclusions drawn by Djupe and Gilbert.

Some of the book's conclusions build on the existing literature. In a chapter reprinted from a 2006 article in the *American Journal of Political Science,* the authors extend previous research on congregations as venues for civic skill building. While they confirm that congregations provide opportunities for the use of civic skills, they also demonstrate that parishioners are more likely to develop those skills when they belong to a homogeneous group within the congregation. Intriguingly, Djupe and Gilbert find that the more parishioners feel "religiously different" from their neighbors, the greater their degree of congregational involvement and, thus, the more they develop civic skills within their congregation. This finding is extremely important and will hopefully be explored in further research better suited to sorting out the causal mechanism at work. It could be that people with distinctive religious beliefs hunker down within their congregations. Or it could be that greater involvement within a religious community accentuates—perhaps even triggers—a distinctive religious identity.

Other findings will be surprising to many readers. For example, Djupe and Gilbert find that congregations, at least of the two denominations they have surveyed, are more politically diverse than is typically assumed. They also question the impact of clergy on the political opinions of their parishioners. Rather than generals who can muster their troops to political battle, clergy of these two denominations are better characterized as editorialists who introduce issues for discussion among their congregants.

Other claims made by the authors, however, will be downright controversial. In particular, they are highly critical of previous literature that employs "religious commitment" as a predictor of political attitudes. For those unfamiliar with research on religion, religious commitment is generally operationalized with an index of religious behavior and particular beliefs. It is the bread and butter of most empirical analyses of religion and politics, especially in the United States. In contrast to the conventional wisdom, Djupe and Gilbert contend that religious commitment is not a proxy for engagement with one's religion but, rather, an individual-level psychological measure that inures people from social influences within their congregation. They argue vociferously that instead of relying on the psychology of an individual's level of religious commitment, scholars of religion should instead focus on the sociology of relationships formed within congregations. In making their case, they throw down the gauntlet to other scholars of religion and politics: "Most popular measures of religious beliefs are insufficient or their connection to political opinions and behaviors is close to random. We suspect that both claims have merit" (p. 87). Within the typically genteel community of political scientists who study religion, this is strong language.

Nor do Djupe and Gilbert limit their criticism to scholars of religion and politics. They also question Diana Mutz's argument that democratic deliberation and participation are at odds (*Hearing the Other Side: Deliberative versus Participatory Democracy,* 2006). Instead, they use their data to argue that the effects of political disagreement are contingent on a person's social context—not so much a refutation of Mutz as a refinement.

The Political Influence of Churches thus covers a lot of ground, confirming, expanding, and often challenging conventional wisdom. It ought to be read widely. However, it is not the final word on the role of congregations in American politics, nor on the political impact of social contexts more generally. Remember that the book is based on a survey of only two religious denominations, and small ones at that. The Episcopal Church and the ELCA each comprise roughly 2% of the American population. And, as mainline Protestant denominations, they represent only one small part of the diverse religious spectrum within the United States. One need only pick up a newspaper to read of disputes and debates within both denominations, most recently over homosexuality. In the aggregate, these denominations are unusually diverse politically. According to the 2006 Faith Matters survey, mainline Protestants actually ranked highest of all religious traditions in a measure of political heterogeneity within their congregations. In addition, over the last 50 years, mainline Protestants have been hemorrhaging members, suggesting a high degree of self-selection and/or retention into these particular denominations. In other words, it remains an empirical question whether the conclusions drawn about Episcopal and ELCA congregations apply to evangelical megachurches, Catholic parishes, storefront black churches, Mormon wards, Jewish synagogues, Muslim mosques, and the many other forms of religious organization within the United States. Furthermore, it is difficult to generalize from a single study of religion since congregations represent only one type of social context, and an arguably unique one at that.

Djupe and Gilbert are aware of their research design's limitations and, to their credit, address it directly. In anticipating criticism of the unrepresentative nature of their sample, they call for further research that acknowledges the variety within American religion (p. 248). This is good advice. I encourage the growing ranks of political scientists studying the diversity of America's religious ecosphere to take that advice, and to draw on this book as an exemplar.

A Shameful Business: The Case for Human Rights in the American Workplace. By James A. Gross. Ithaca, NY: Cornell University Press, 2010. 264p. $59.95 cloth, $21.95 paper.
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— David Cingranelli, *Binghamton University, SUNY*

James A. Gross argues that worker rights like the right to freedom of association at the workplace, the right to bargain collectively, and the right to a safe and healthy workplace are not typical public policy issues. If they were normal policy issues, reasonable people could disagree about the proper balance between what business owners and managers want and what workers want. Instead, these worker rights and others like them have a special status; they are internationally recognized *human rights.* Because they are human rights, every government of the world has a special obligation to protect them, to give them special preference even when full protection of worker rights is opposed by business owners and managers.