How the States Shaped the Nation will be of great value to scholars of voter turnout, state politics, and American political development. The careful data collection required, along with the rich historical details of state electoral-reform adoption, are important contributions. This feature alone makes this book a must-read and must-cite in the study of electoral laws and voter turnout in the United States. The breadth of the analysis will surely raise questions by some as to particular modeling and measurement details, but these concerns will be resolved elsewhere. The more important question is whether and how the work advances what we know about the effects of state electoral reforms and what we know about voter turnout.

One of Springer's ambitions in this work is to contribute to our understanding of individual-level turnout studies. On this point, her contribution is perhaps more subtle, but also important. The assumption in current studies, she notes, is that the effects of election reforms are the same regardless of political context and history. Her state-level analyses indeed demonstrate the importance of political context and history, often ignored in previous research. Just as important, however, they also underscore the distinctive impact of expansive, compared to restrictive, election laws—a distinction not emphasized or explored sufficiently in existing studies. And this is a key insight in the study of election reforms: Limiting engagement is not the same—in origin or impact—as allowing engagement. The same state politics and political culture that emerge to pass expansive versus restrictive laws are those that must provide the additional incentives to vote, once expansive structures are in place, while the administration of restrictive policies is assured to some extent by virtue of the policy enactment.

Where does this leave scholars of voter turnout? The adoption of these laws is now part of the information environment of elections in the United States, and so assessing what determines individuals' responses to the adoption of more restrictive or expansive election laws is now a critical question. As Springer suggests, states that pass expansive laws are likely to also support their administration, and parties, candidates, and others likely react to them in their campaign strategies. Understanding the effects of these (other) electoral institutions is thus critical to a better understanding of electoral reform and voter turnout in the United States.

The Hidden Agenda of the Political Mind: How Self-Interest Shapes Our Opinions and Why We Won't Admit It. By Jason Weeden and Robert Kurzban. Princeton: Princeton University Press, 2014. 363p. $29.95.
doi:10.1017/S1537592715003850

— Elizabeth Suhay, *American University*

At first glance, this book's big-picture focus and accessible writing style suggest that it is written for a general, educated audience. Perhaps it is, at least in part. However, the authors' main audience appears to be *us*—political scientists. Jason Weeden and Robert Kurzban, who are evolutionary psychologists, want to redirect the field of political science. Toward what? Ostensibly toward recognizing the power of interests—broadly conceived—in shaping democratic politics. The vast majority of the book's 300+ pages is spent trying to persuade the reader that people's everyday practical concerns play an outsized role in shaping their attitudes toward all manner of politically relevant topics, from religion to affirmative action. While the authors commit a number of unforced errors that diminish their persuasiveness, I admit to being at least slightly redirected.

There's more to *The Hidden Agenda of the Political Mind* than just this argument, however. I would wager that the authors have a second, more controversial, goal: orienting political science in such a way that theoretical frameworks from evolutionary psychology are a sensible next step. Perhaps we should call this "the (relatively) hidden agenda of Weeden and Kurzban," as the relevant text occupies only about 10 pages and is somewhat vague. I'll return to this topic at the end of the review.

The authors begin by explaining why people are relatively blind to the influence of interests on their own political (and related) opinions. They argue that individuals' preferences stem largely from unconscious, emotion-driven processes, processes that tend to reflect a person's "inclusive interests" (i.e., those practical, everyday goals that involve the well-being and success of oneself, one's family, and one's allies). Yet because the processes that generate our preferences are subconscious, we are unaware of why we prefer what we do. Rather than simply shrug when someone asks us why we believe as we do, we engage in "spin," explaining our preferences in moral, value-laden terms. This helps us recruit others to our side while also improving our public image. This is not a completely new idea to the study of politics—think about scholarship in the rational choice tradition, or, from a different perspective, Marxist notions of capitalist ideology. However, mainstream political science does seem to have moved away from the idea that ideologies often cloak interests, more often (as the authors argue) reversing the causal arrow to argue that ideologies drive all manner of preferences.

The authors' next move is to explain why contemporary political scientists downplay the role of interests in shaping political preferences. They argue that we have made two main errors.

First, we define self-interest too narrowly—as a short-term concern for material goods. We ought to broaden our concept to encompass concern for family and social allies and for "practical" goals that may not have much to do with material goods directly, such as personal freedom, societal respect, and stable families. Should we do so, we will find more interest. I suspect that most of us would agree with this, and, thus, this is largely an unfair criticism. Public-opinion scholars who have employed the narrow version of self-interest often have done so in an effort to debunk overly simple notions of human motivation that used to be popular among economists and game theorists. And many political scientists have explicitly argued for an expanded notion of "interest" (e.g., Dennis Chong, *Rational Lives*, 2000; Michael Dawson, *Behind the Mule*, 1995; Jane Mansbridge, ed., *Beyond Self-Interest*, 1990). This said, if there is some truth in Weeden and Kurzban's definitional criticism it is this: We know what narrow self-interest is, but we have failed to create a common language for talking about the varied interests that stretch beyond it.

Second, according to the authors, political scientists have been careless in their model specifications. Our worst offense is engaging in something that the authors call "DERP-ish" behavior. To DERP is to insert into a statistical model an independent variable that is a *d*irect *e*xplanation of the dependent variable *r*enamed as something else. (The "P" is for *p*sychology, where all this DERP-ishness started, I gather.) Such variables—ranging from symbolic racism to party identification—are problematic because, in their close conceptual and empirical resemblance to the dependent variable of political preferences, they crowd out the influence of other independent variables (such as interests). On a related note, we often "overcontrol," which can lead to statistically insignificant coefficients on causally important variables—such as interests—that enter a causal chain early but have indirect effects. In these criticisms, the authors overlook the fact that each of these issues has been discussed by prominent political scientists, including Paul Sniderman and Chris Achen. However, these behaviors persist to an extent. In my view, a reminder of the costs of unthinkingly DERP-ing or overcontrolling is useful.

Weeden and Kurzban do much more than just critique political scientists. Three core chapters discuss how individuals' inclusive interests shape their views on morality politics, politics related to group identities, and social welfare policy. For each, the authors explain what interests are present in an intuitive (although sometimes surprising) way, link those interests theoretically to political preferences, and then back this up with public opinion data.

In Chapter 4, "Fighting over Sex: Lifestyle Issues and Religion," the authors contrast two groups of people: "Freewheelers" (people who are sexually promiscuous, do not have many children, and like to party) and "Ring-Bearers" (people who have sex only in committed relationships, have more children, and—you guessed it—party less). Why are Freewheelers less likely than Ring-Bearers to be religious and to support various socially conservative policies, such as making it difficult to obtain contraception and abortions? In a nutshell, the former wish to maintain their freedom and the latter want to keep their marriages intact (and the best way to do that is to find ways to reduce other people's promiscuity). Given the obvious counter-argument that socialized religious belief is, in fact, the most important causal force in shaping social conservatism, this first argument is the weakest of the three; however, it succeeds simply because it makes a plausible case for interests in the arena where we least expect to find them.

In Chapter 5, the authors tackle attitudes toward policies that, by design, help some groups at the expense of others (e.g., compare meritocratic, discriminatory, and affirmative action policies). They have a less challenging task in this chapter—as well as in Chapter 6, on attitudes toward social welfare and economic redistribution—but their clear-sighted analysis of the ways in which various groups' interests likely play out politically is illuminating. Some may find the arguments obvious. Others may see that they have missed the forest for the trees.

The public opinion data provided are supportive of the authors' framework that people's interests—broadly conceived—shape their political perspectives. This said, the data offer more of a promising beginning than an airtight case. Throughout, the authors make causal assertions with cross-sectional observational data, and psychological arguments with standard survey questions. It is also difficult to parse their data presentation, which contrasts various subgroups. These contrasts—nonparallel, sometimes inconsistent, and not explained well—raise (hopefully unwarranted) suspicions that Weeden and Kurzban have selected comparisons that best make their case.

Although it only occupies a handful of pages (pp. 34–40, esp. p. 38; also pp. 207–10), the authors' evolutionary psychology framework clearly drives their perspective on—and interest in—"interest." Most of the relevant text is in Chapter 10. While earlier chapters of the book suggest that the authors take a cautious and nuanced view of the relationship between human evolution and contemporary politics, this impression is erased here. They seem to argue that our "inclusive interests" are little more than mechanisms of survival and procreation at the end of the day (p. 207). They also dismiss socialization as having any independent causal influence on political views, calling this perspective "scientifically implausible" (p. 208) because twin studies find little evidence of the influence of "shared environment." In my view, these assertions are implausible. The average fertility rate in the United States is less than two children per woman; we are obviously not all maximizing our reproductive capacity (as any Freewheeler will also tell you). And the dismissal of socialization based on one empirical method and one statistic—they ignore the effect of "unshared environment"—is scientifically problematic.

Another disappointment is the authors' discussion of individual and group differences. Evolutionary frameworks often downplay individual and group differences—our DNA is 99.9% the same, after all. One could work within an evolutionary framework and still argue that

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many of the diverging interests in Chapters 4, 5, and 6 stem from a person's place in the social structure. To put it too simply: We all seek resources and respect; some of us are born into groups (race, gender, class) that have relatively more or less of those things; our political views reflect this. Chapter 10 makes clear that this is not Weeden and Kurzban's argument. The only possible source mentioned of the on-average differences in political views between men and women, gay and straight, smart and less smart, Freewheeler and Ring-Bearer is genetics (p. 209).

The authors back away somewhat from these heavy-handed assertions a moment later when they say that there is a "range of interesting factors that influence diverse political opinions" (p. 210); however, given that they have dismissed socialization and offered no other causal factors, this strikes me as insincere. Weeden and Kurzban appear to have replaced one narrow view of human motivation with another: our genes.

It is important to add that to criticize this narrow view is not to reject all evolutionary psychology. There simply is more to it, and that "more" is untidy, is not well understood, and intersects with culture. For example, moral feelings (e.g., sympathy) may have arisen to motivate care for family but now can extend to people on the other side of the planet. Human culture influences what we perceive our interests to be, what we perceive to be good or moral, and the extent to which we pursue one or the other. Further, such cultural influence is *enabled* by evolution. In a 2015 *Political Behavior* article ("Explaining Group Influence"), I wrote about the role that the emotions pride and embarrassment play in group conformity. As with most emotions, these psychological mechanisms probably have helped humans thrive. Weeden and Kurzban would be on firmer ground if they embraced a broader view of the ways that evolution influences behavior.

Thankfully, one can remain agnostic about the ultimate source of people's interests, and most of the arguments in *The Hidden Agenda of the Political Mind* still work. I agree with Weeden and Kurzban that people, especially partisan ones, often believe themselves to be virtuous defenders of an ideology when they are, in fact, pragmatic defenders of themselves, their family, and their friends. Wider recognition that *all* sides—to an extent—engage in this fallacious thinking might calm some of the righteous emotions that often get in the way of democratic deliberation.

COMPARATIVE POLITICS

Bankers, Bureaucrats, and Central Bank Politics: The Myth of Neutrality. By Christopher Adolph. New York: Cambridge University Press, 2013. 390p. $109.99.
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— David A. Steinberg, *John Hopkins University*

Central bankers are key decision makers in all modern economies. Their decisions regarding interest rates and financial market regulations have profound effects on inflation, unemployment, economic growth, and financial stability. Many believe that the recent global financial crisis occurred in large part because of the failures of central bankers. Likewise, many fear that the Federal Reserve in the United States will raise interest rates too aggressively in the near future, and doing so will harm the country's economic recovery. Given the high stakes involved, it seems imperative to understand what drives central bankers' decisions.

The conventional wisdom holds that central bankers are apolitical technocrats who seek to fulfill their mandated objectives to the best of their abilities. In *Bankers, Bureaucrats, and Central Bank Politics*, Christopher Adolph shows convincingly that this established view of central banking is mostly wrong. The actions of central bankers, Adolph argues, depend heavily on their personal beliefs and interests.

The book introduces a novel "career theory" of central bank behavior. The starting point of the theory is that central bankers are agents with their own independent preferences and beliefs about monetary policy. Those preferences influence how they set interest rates and help explain variation in inflation and unemployment rates. A key contribution of Adolph's career theory is to shed light on the origins of central bankers' monetary policy preferences. The theory posits that the preferences of central bankers are systematically related to individuals' previous career experiences. Past careers, according to the author, matter for two main reasons. First, previous careers shape individuals' ideas and beliefs through processes of socialization. For example, individuals with experience working in a private bank are typically more hostile to inflation than individuals whose previous careers were in the public sector. Second, prior careers shape individuals' interests. Since central bankers often want to return to their old career after they leave their post in the central bank, they have an incentive to adopt policies that will please their old bosses, who are also their potential future bosses. The book's core argument is that the past careers of central bankers shapes monetary policies and economic outcomes.

To test the career theory of central banking, Adolph constructs an original data set of the career experiences of central bankers. Most of the analyses focus on a data set that covers 598 central bankers from 20 developed countries between 1950 and 2000. The book also examines an original data set on the careers of central bankers in a large number of developing countries. This data set alone is