much more neoliberal in tax policies, less egalitarian in welfare states, and arguably more pro-business.

Monica Prasad's theoretical argument is complex. The author suggests that countries with "adversarial" policies—meaning ones that are likely to split the electorate—and with electoral majorities that shifted between left and right were more vulnerable to neoliberal reform than ones in which the Right dominated throughout, and which produced less divisive or progressive policies. Her argument is that where politicians were able to exploit the kinds of "adversarial" divisions promoted by tax and welfare policies—which resulted in one portion of the electorate paying for the other—and by stringent regulatory policies that pitted government regulators against business, more radical change became possible once the bulk of the electorate's interests shifted to the other side as they became more middle class.

Prasad makes her case in two ways. First, she shows that none of the traditional explanations fully accounts for the neoliberal transformation of national economies—whether those which argue that globalization and capital flight caused a race to the bottom in terms of social protections, those which insist that business groups mobilized to protect their class interests, those which identify the rise of neoliberal ideas as the culprit, or those which explain the differing responses in terms of national cultures. Second, she argues that the best explanation for the success of reform efforts is one that focuses on the politicians who seized on ideas that resonated with the public, which they then used to electoral advantage in favorable institutional contexts.

In the United States, Prasad's story of the triumph of Reaganite neoliberalism is one of political entrepreneurs in fragmented institutions capable of gaining the advantage with supply-side ideas that were not necessarily sound economically but that nevertheless won over the electorate. In the UK, her story of Thatcherite neoliberal transformation is instead one in which government leaders, benefiting from more centralized institutions, developed ideas that served to convert the electorate to policies that radically altered the economy through reduced public spending and privatization of major industries and of council housing. In France and Germany, by contrast, the story is of economic transformation with a minimum of neoliberalism, in the former because Giscardian reform in particular was stymied by state-centered elites, in the latter because neoliberal political entrepreneurs in one part of Helmut Kohl's governing coalition were unable to overcome opposition by more left-leaning politicians in another part or to win over the majority of the electorate to their ideas.

The Politics of Free Markets is rich in empirical detail. Prasad's interpretations of that detail are sometimes controversial, her critiques of rival methodological approaches occasionally excessive. Substantively, the U.S. and UK cases are the most convincing. The discussion of Germany is idiosyncratic but interesting, whereas that of France, while accurate in its conclusions, is flawed, in particular with regard to the discussion of Valéry Giscard d'Estaing's 1970s neoliberalism and his failure to propose privatization—why would he even have imagined it at that time? Methodologically, moreover, Prasad is so intent on dismissing alternative arguments that she tends to caricature them, especially with regard to explanations in terms of national culture or of ideas.

The rejection of arguments in terms of ideas is the most surprising, however, since Prasad's own innovative methodological approach is itself all about ideas constructed through deliberation and debate and carried by political actors to win over electorates within given national policy and institutional structures. Such a rejection may result from her limited reading of the "ideas" literature, in which she assumes (wrongly) that it considers only economists' ideas, which she (rightly) shows were more often than not ignored by politicians unless they fitted their own political ideas. It may also be because she is intent on rejecting a particular approach to the explanation of ideas, which she sees as static, deterministic, and idealistic because it ignores interests and institutions. But in condemning so categorically all approaches to ideas in political science, she fails to recognize that her own methodology fits with the work of a growing group of ideational scholars (e.g., Sheri Berman, Mark Blyth, Craig Parsons, Colin Hay, Len Seabrooke, John L. Campbell) who can be considered part of the newest of the "new institutionalisms" in political science and sociology, which I call "discursive institutionalism" because it takes ideas and discourse seriously as constructive of interests, rather than just reflective of them, and sets these in an institutional context.

Finally, Prasad, by demonstrating the importance of ideas and discourse in an institutional context, also implicitly offers an answer to the problems of democracy that she herself raises in the introduction and elaborates in the conclusion, which comes down to "Beware majorities that shift from poor to rich in countries where the Left occasionally managed to institutionalize more politically liberal and economically progressive policies." The real moral of the story, following from the logic of her own argument, is: "Beware politicians whose ideas and discourse seek to exploit those shifts in wealth, and hope instead for politicians with another set of ideas and a discourse capable of convincing vacillating electorates as to the best interests of their country."

Social Traps and the Problem of Trust. By Bo Rothstein. New York: Cambridge University Press, 2005. 256p. $80.00 cloth, $30.99 paper.
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How do you get from Moscow to Stockholm? That is, how do you create efficient, honest government like that

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of Sweden as opposed to the incompetence and corruption found in Russia? This question drives Bo Rothstein into an analysis of the causes of social traps—situations where actors cannot cooperate due to mutual distrust, even if cooperation is mutually beneficial. Eliminating social traps requires generalized trust, that is, social capital. How does one engender trust? Robert Putnam, both in *Making Democracy Work* (1993) and *Bowling Alone* (2001), argues that social capital is created through civil society, a process that bubbles up through history and culture. Rothstein takes this view to task for not analytically distinguishing between associations (a behavior) and trust (a belief), for it is really the belief that needs to be generated. Trust, in his view, need not evolve organically from below; it can be generated from *above* by establishing efficient political institutions. If public officials are corrupt, citizens will infer that people in general are dishonest and behave dishonestly themselves. Thus, social traps are sprung. The reverse is also true: Honest governance begets social trust.

Yet even with better administrative monitoring, there remains a credibility problem. Why should citizens in low social capital environments believe that governmental institutions will be honest? The solution to the credibility problem—and the solution to social traps, in Rothstein's view—is to create *universal political institutions* where benefits are equally distributed, forcing officials to act impartially. As he puts it, "selective, needs-testing welfare institutions undermine interpersonal trust, while experiences of universal institutions tend to increase it" (p. 126).

How are such institutions formed in the first place? Cooperation is guided by the "mental maps" people develop regarding others' likely behavior. The key is collective memory, "the spawn of deliberate and strategic action by political elites" (p. 161). (Rothstein rejects the more general concept of culture as being overly deterministic.) The "stab-in-the-back" myth of the Nazis was an example of collective memory, but it can also be harnessed for good. Rothstein illustrates this by tracing labor market relations in Sweden in the early twentieth century. Sweden had as much labor strife as any other industrialized nation, punctuated by the military firing on strikers at Ådalen in 1931. Yet by 1938, labor and management had struck the cooperative Saltsjöbad Accords. How did they move from mistrust to trust? Rothstein argues that a battle for collective memory began between radicals seeking to intensify the struggle against capitalism and moderate labor leaders who saw labor relations spinning out of control. The latter interpretation of the collective memory predominated, establishing an imperative for compromise that was mirrored by the bourgeoisie. This was not simply the lucky result of enlightened leaders. Rather, Rothstein notes that corporatist labor market institutions previously established by the Swedish state had developed habits of dialogue and trust between workers and managers well before Saltsjöbad. Social capital in Sweden came from above.

The major flaw in the study of social capital is an inability to guide political leaders in the way to build trust where it is lacking. By developing a viable argument as to how political institutions can create social capital, Rothstein fills the gap and (potentially) greatly increases the utility of social capital as an analytical concept. This in itself is quite an accomplishment. His argument also offers the advantage of simplicity in application: If you want to have a high social capital society like Sweden, adopt Sweden's universal institutions.

The novelty of Rothstein's argument is the causal sequence he proposes—good government (from universal institutions) leading to trust, rather than trust leading to good government. Yet the very history he examines to establish this point raises a question. Policies designed to avoid social traps were implemented by the Swedish state in the late 1800s, which helps to explain cooperative labor relations in the 1930s (pp. 191–97). *Why did the Swedish state adopt such policies in the first place?* Other states faced similar pressures but did not go down the same path. If universal political institutions are the key, we need a better sense of how and why these were selected in the first instance. Without a better explanation of how the causal chain began, Rothstein's evidence is insufficient to discount alternative explanations. The obvious alternative as to why this model was followed in Sweden but not elsewhere in Europe takes us back to Putnam: Perhaps Sweden had already developed a base of social trust that encouraged the adoption of corporatist structures in the first instance.

Rothstein's portrayal of universalism is also problematic, as he defines it in substantive and procedural terms. Mostly he seems to mean universal welfare benefits: "The comparatively high levels of social capital in Scandinavian countries may thus be caused by their extensive policies for economic equality and equality of opportunity" (p. 100). Yet he later defines universal institutions as exhibiting impartiality, objectivity, and equal treatment (p. 129), even saying that "[t]rust does not come from the actual contents [of policy]; it is based on respect for procedures" (p. 145). Government can be fair, impartial, and objective without having universal welfare programs, of course. So which does one need to produce social capital? Lack of clarity muddles potential application.

Potential application is further confused because Rothstein offers minimal comparative empirical data to support his argument. There is a clear and testable hypothesis here: Countries with universal welfare states should have higher social capital. Methodologically, he relies on logical argumentation and a case study of Sweden—with the exception of one set of regressions (without robust results) including other states (p. 113). More systematic, comparative analysis would bolster his conclusions.

That being said, *Social Traps and the Problem of Trust* stands out among a crowded field of books on social

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capital, a work that should not be overlooked by serious scholars of the subject. Rothstein's shortcomings, moreover, are perhaps forgivable. This is an ambitious book exploring a crucial political problem. The sad reality is that deciphering the path from Moscow to Stockholm may be beyond the reach of even his many talents. Still, he deserves credit for his valiant effort to address this large and important issue.

Multiparty Democracy: Elections and Legislative Politics. By Norman Schofield and Itai Sened. New York: Cambridge University Press, 2006. 248p. $70.00 cloth, $24.99 paper.
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— Indridi H. Indridason, *University of Oxford* and *University of Iceland*

The elegant median voter result of the Downsian model of electoral competition—which suggests that in order to maximize votes, parties move towards the ideological center—does not entirely translate into multiparty systems. To begin with, as Charles Plott ("A Notion of Equilibrium and Its Possibility under Majority Rule," *American Economic Review* 57 [no. 4, 1967]: 787–806) shows, an equilibrium generally does not exist in multidimensional spaces. Moreover, Gary Cox ("Centripetal and Centrifugal Incentives in Electoral Systems," *American Journal of Political Science* 34 [no. 4, 1987]: 903–35) demonstrates that if an equilibrium does exist in multicandidate contests, then they must be noncentrist. But multiparty politics are even more complex, as one must consider which coalition will govern following the election and what policy it will implement. These considerations may in turn affect the voters' strategies and the parties' choice of platforms.

Norman Schofield and Itai Sened's contribution is to consider the effect of valence in a multiparty stochastic voting model. When candidates differ in valence, a low-valence candidate generally has an incentive to choose a platform away from candidates with high valence, as any voter will naturally prefer the high-valence candidate if the candidates offer identical platforms. Thus, in a local Nash equilibrium (a pure Nash equilibrium generally does not exist), high-valence candidates tend to locate closer to the center of the voter distribution, while low-valence candidates tend to adopt extremist positions. Candidate convergence can occur, but only under relatively strict conditions. It is interesting that if the conditions fail, even high-valence candidates can improve their vote share by moving slightly away from the center. In Schofield and Sened's model, it is the difference in the candidates' valence that is crucial, along with the dimensionality of the policy space and the variance of the electoral distribution that determines the party system centrifugal tendencies.

One of the nicest aspects of *Multiparty Democracy* is how closely the formal model and its empirical tests are tied together. The authors consider elections in four parliamentary systems and the United States. The voters' utility functions, with the addition of sociodemographic variables, form the basis of the empirical vote models. Once the parameters of the model have been estimated, they are fed back into the formal model to obtain the "convergence coefficient." If the convergence coefficient is large, the parties are predicted to diverge from the electoral mean. This is, indeed, the case for most of the elections considered. The convergence coefficient for Italy in 1996, however, is sufficiently low to sustain a convergent equilibrium. Yet, there is little to suggest policy convergence at the electoral mean, but Schofield and Sened argue that the strong effects of sociodemographic factors (which are not a part of the formal model) on the support of the Northern League explain divergence in this case. However, policy divergence occurs in all of the cases considered in the book. One's confidence in the theory would undeniably be strengthened with the inclusion of a case in which convergence did occur and was predicted by the convergence coefficient.

The fact that parties' policy platforms diverge thus offers limited support for the theory, and so it is important to consider the theory's other main implication, that low-valence parties locate farther away from the center. As analytical solutions do not exist, the (multiple) local Nash equilibria are found computationally, using the empirically estimated coefficients. While there are no formal tests to evaluate the closeness of the predicted and observed locations, the results appear fairly convincing, especially with regard to the larger, higher-valence parties that tend to locate closer to the electoral mean. A minor complaint is that only limited information is provided about the computational methods and only examples of the simulated equilibria are shown.

The authors' central formal result assumes vote-maximizing parties, but they also consider variations of the model that assume parties maximize the probability of winning and coalition membership. Some postelectoral situations are characterized by the presence of a core party, which can be assumed to form either a minority or a coalition government. If the core is empty, coalition instability ensues. The postelectoral game may affect the parties' strategies. Some parties may adopt a platform with the aim of becoming the core party, while other parties may seek to prevent this possibility from happening and to extract maximum gains from the coalition bargaining. The authors consider party positioning in several elections on the basis of this logic, which provides for an interesting, even compelling, account of the parties' calculations, but it falls short of a formal test as the theory does not produce sufficiently precise predictions.

The anticipation of the outcomes of the coalition-bargaining stages is highly intriguing and certainly warrants further consideration by scholars. However, Schofield

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