

“populist technique” is nowhere specified. A clearer definition is not irrelevant; whether there is something unique to this type of leadership (whatever it exactly is) and its relationship to gender versus that of liberal democrats or other types remains unclear.

In sum, both books are welcome additions and, given the less charted terrain, they pose intriguing questions as much as they provide answers, challenging scholars of the region to use these analyses as points of departure for further research.

Party Competition: An Agent-Based Model. By Michael Laver and Ernest Sergenti. Princeton: Princeton University Press, 2011. 292p. \$65.00 cloth, \$29.95 paper. doi:10.1017/S1537592712001855

— Ian S. Lustick, *University of Pennsylvania*

In the first sentence of the concluding chapter of this pathbreaking book, Michael Laver and Ernest Sergenti speak personally to the reader: “We have come a long way together since the beginning of this book” (p. 258). For the attentive reader, that is a fact. This is a book that strives for a great deal and asks for a great deal. It aspires to solve heretofore unaddressable problems in spatial analysis in a manner whose rigor meets standards of both algebraic deductivists and statistically oriented empiricists, while simultaneously disabusing, informing, or converting political scientists prejudiced against, bewildered by, or intrigued with agent-based modeling (ABM).

The rhetoric in the first chapters is meant to signal analytic modeling practitioners that the authors’ turn toward computer-assisted ABM should not be dismissed as “soft” or unscientific, regardless of the less than rigorous patterns that they suggest are common in extant work of this sort (p. x). “We want,” they write, “our own results to have the same scope and precision as typical results from formal models in [the field of spatial modeling]” (p. 8). The results of their work, they promise, will be “rock solid,” the sort that can be “taken to the bank” (p. x).

In a sense, their stance, at least initially, is apologetic. “An analytic result, if it is general, is a beautiful thing. . . . This is one reason why we never use computational methods when analytical results are available” (p. 7). While observing that simulations run on computers *are* formal models, they justify abandoning elegant algebraic proofs of equilibrium solutions to problems in spatial politics for one reason only: because the really interesting questions that multiparty democratic politics poses are intractable to classical, formal, modeling techniques. Indeed, they do not rule out the possibility that future mathematical techniques might render the problems of interest tractable. They are, simply, “impatient” (p. 12), portraying themselves as forced to adopt new technologies in order to address problems of interest and importance.

Multidimensionality is the first problem identified by the authors as intractable to spatial analysis if confined to a search for algebraic solutions. Laver and Sergenti emphasize that the vast majority of research in the field has been confined to unrealistic unidimensional spaces and that problems that rarely occur in a unidimensional space, such as cycling (A beats B; B beats C; and C beats A), abound in multidimensional spaces. Accordingly, generalizing from analytic results for single-dimensional settings is dangerous: “Tiny departures from unidimensionality can catastrophically undermine key inferences” (p. 19). Although the authors limit their attention in this book to two dimensions, they do add other elements of realism to their models that are not, and mostly cannot, be integrated within analytically tractable models. These factors include alternative, boundedly rational, and realistic party strategies for pursuing votes (singly and in combination); a distinction between leader preferences and party interests; different distributions of ideal points in the voting population; disappearance and appearance of parties; variation in the importance of valence issues in relation to thresholds of support; and variation in electoral rules. Although the analytic approach might be able to generate insights into the effects of any one of these elements, it is the ability to combine these factors in fluid, diverse, and realistic ways, while preserving opportunities for disciplined statistical assessment of their impact on one another and on outcomes, that justifies the authors’ commitment to “bottom-up” ABM simulation.

Party Competition is organized into three parts. Part I offers an introduction to ABM and the techniques of data gathering, presentation, and analysis associated with it. Using Markov chains, Monte Carlo parameter specification techniques, and possible-world language, they explicate their procedures and explain the experimental status of large batches of dynamic computer simulations. Part II includes an account of their “baseline” model and its “benchmark” results. In these chapters, Laver and Sergenti also present extensions of the model to accommodate the appearance of new political parties, as well as variation in the strategies used by parties to align their positions with the mostly hidden distribution of voter preferences. Part III features more extensions, including replicator dynamics for party selection of available strategies, integration of valence issues, and leader preferences distinguished from party interests in short-term vote maximization. Every new extension of the model entails significant increases in complexity. The authors justify each one by noting that without the extension, the model would be appropriately considered “preposterous” (p. 106) as a depiction of the actual world of multiparty competition in a multidimensional policy space. (Note abandonment of the apologetic rhetoric). The final substantive chapter uses a comparison of selected batches of 1,000 of their model specifications per country to demonstrate “good

face validity” (p. 249). This limited validation success is claimed as a result of identifying isomorphisms between patterns across those sets and respective features of multiparty competition in Germany, France, Britain, and Spain between 1989 and 2002.

It would be a mistake to evaluate the book solely on the basis of the ability or inability of their models to retrodict specific outcomes in specific cases of multiparty competition. Their validation work at the end does show that their model’s outputs can pass sanity checks, but in light of some of the more sweeping claims and aspirations outlined at the beginning of the book, some readers will feel disappointed not to be able to see an actual test of the model, or of any part of it, against “real” data. Such a critical reading would not do this impressive book justice. It would miss the huge contributions of the authors to the development of standards, best practices, and terms of art necessary to exploit ABM techniques of formal modeling to overcome the dispiriting trade-off that algebra-based formal modeling makes between solvability and verisimilitude.

For example, visualization of patterns in multidimensional time-series data is an immensely challenging problem. It is a problem that computer-assisted ABM brings to the fore precisely because political worlds *are* multidimensional and dynamic, *and* because the ABM computer simulations can produce the massive amounts of data that correspond to that reality. Laver and Sergenti deploy effective tools to address this problem by using computational geometry and “Voronoi” diagrams for portraying the “tessellation” of a space divided into regions defined by their distance to local centroids. The authors demonstrate the uses of the concepts and techniques associated with Voronoi diagrams to frame the problems they are posing and to help the reader visualize and understand the rippling and sometimes surprising effects of even small patterns of dynamic change on the shapes and areas of all regions.

In some respects, Laver and Sergenti lean toward the simplest, most abstract kind of ABM work by repeatedly professing fealty to Robert Axelrod’s famous “KISS” principle—Keep It Simple Stupid—as an imperative to parsimony that they seek to honor. On the other hand, they lean toward maximally ambitious ABM virtualization techniques when they seek to validate their model output with European data from the early 1990s. But these curtsies toward parsimony and empirical validation are misleading. Their work is firmly situated within a middle type of ABM—an *ensemble* or generic approach. Ensemble models are virtual realizations of particular kinds of social science problems. They simulate generic types of problems of interest to social scientists. They are comprised of integrated sets of routines and mechanisms implemented to permit operationalization of claims by contending substantive theories or hypotheses. The vir-

tual worlds, or landscapes, within which agent behavior is observed are more stylized with respect to agent attributes and networks of communication than are abstract model landscapes, but remain temporal-spatially indistinct, in contrast to virtualization models.

Laver and Sergenti present ensemble models, motivated by theoretical problems and literatures whose logics or findings are too complex, or with respect to which necessary data is unavailable, to be satisfyingly explored or assessed via laboratory or natural experiments. Their findings are highlighted throughout the book, often in italicized passages that describe how their simulation experiments replicate analytic results, corroborate unprovable intuitions among spatial theorists, or correct misimpressions about the dynamics of multiparty competition. Among the experimental findings they emphasize are that satisficing strategies are often optimizing as long-term rules of thumb and that the conditions under which this is so are systematically identifiable; that no rule for party competition is best; that the relative and even absolute value of any one rule is context dependent (especially with respect to the distribution of ideal points among voters); that causal mechanisms are identifiable only by literally tracing the dynamic unfolding of simulations; that party leaders committed to *both* their own ideal points and the maximization of party votes lead, under specifiable conditions, to more successful parties than do leaders who abandon their own preferred policies; that valence issues do tend to overwhelm position issues even as they narrow the range of variation in vote-getting strategies; and that depending on the distribution of ideal points, parties willing to accept what they have will outperform parties that ruthlessly pursue marginal advantages.

In the end, it is not only the reader who travels a long way from the beginning to the end of *Party Competition*. The authors do as well. Early on, the dynamic elements of their simulations are treated as “transient” and uninteresting, as they focus instead on the equilibrium states they achieve after “burn-in” (pp. 64–66). But increasingly throughout the book, they cite their own experiences watching the unfolding simulations as key to achieving an understanding of complex processes. They begin the book complaining about the heroic assumptions made by algebra-based formal modelers in order to render interesting problems tractable, albeit uninteresting. They end by presenting an evaluation of their work that is anchored in their own heroic assumption, namely, that their *interesting* model is correct. At the outset, Laver and Sergenti criticize analytic spatial models for lacking correspondence to the real world of interest to political scientists, but their analysis of patterns of change in party decision strategies entails experimentation with quite unreal, temporally unspecified worlds in which the same political system experiences a sequence of thousands of elections, each organized in exactly the same way.

Finally, it should be noted that the book begins with a declared commitment to replicable results and techniques for the enforcement of methodological rigor associated with quantitatively defined correlation-based research and closed-form algebraic deduction. It ends, however, with the acknowledgment that “systematic and rigorous empirical evaluation of our model [was] much more difficult than might be expected,” and by shifting toward a surprisingly subjectivist measure of success. Their readers, they say, must be the “ultimate judges” of whether their work has provided “useful intuitions.” For, as they write, “[d]espite widespread agreement about the rigorous and scientific methods that should be at the heart of any modeling exercise, the ultimate intuitions derived from even the very best work are ultimately personal” (p. 264).

This is a rich and provocative book. Some of its substantive findings with respect to the interaction of dimensionality, leader preferences, valence, survival threshold, and vote-seeking strategies will stand on their own as important advances in our understanding of how spatial politics really does work. While the authors do not solve all the methodological and substantive problems they address, they do raise the bar considerably for those who will try to do better. For methodologically sophisticated political scientists, and especially those interested in spatial analysis, this is no doubt the best introduction available to the challenging, exciting, and, to many, still mysterious worlds of agent-based modeling.

The Quality of Democracy in Latin America. Edited by Daniel H. Levine and José E. Molina. Boulder, CO: Lynne Rienner, 2011. 299p. \$65.00 cloth, \$26.50 paper.

Democratic Governance in Latin America. Edited by Scott Mainwaring and Timothy R. Scully. Stanford: Stanford University Press, 2009. 440p. \$85.00 cloth, \$29.95 paper.
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— Charles H. Blake, *James Madison University*

Are the durability, legitimacy, and worth of a democracy better reflected by faithfulness to liberal democratic procedures or by societal outcomes that are seen by most citizens as indicators of governing effectiveness? Larry Diamond and Leonardo Morlino blended these contrasting visions into a multidimensional conceptualization of what constitutes a good democracy (“The Quality of Democracy: An Overview,” *Journal of Democracy*, 15 [October 2004]: 20–31) when they wrote: “Such a regime will satisfy citizen expectation regarding governance (quality of results); it will allow citizens, associations and communities to enjoy extensive liberty and political equality (quality of content); and it will provide a context in which the whole citizenry can judge the government’s performance through mechanisms such as elections, while government institutions hold one another legally and constitutionally accountable as well (procedural quality)” (p. 22).

Two recent edited volumes on Latin America tackle different dimensions of Diamond and Morlino’s definition of a good democracy. Daniel Levine and José Molina’s *The Quality of Democracy in Latin America* focuses on what Diamond and Morlino termed “quality of content” and “procedural quality,” while Scott Mainwaring and Timothy Scully’s *Democratic Governance in Latin America* is principally concerned with the “quality of results.” In contrast to most edited volumes, in both of these the editors construct a detailed conceptual framework *and* then proceed to apply that framework by developing cross-national measures for countries across the entire region. As with most edited volumes, these contributors do not consistently adhere to the precise conceptual and empirical framework espoused by the editors.

Levine and Molina posit five criteria for democratic quality: electoral decision, participation, accountability, responsiveness, and sovereignty. The editors then present empirical metrics via indices in which each criterion’s respective dimensions are weighed equally across a standardized 100-point scale. Employing these metrics via 2005 data (p. 33) reveals that Uruguay had the highest level of democratic quality while Guatemala had the lowest score. Costa Rica, Chile, Argentina, Mexico, and Panama had above-average scores while Honduras, Paraguay, El Salvador, Venezuela, Colombia, and Nicaragua had below-average scores.

Rather than focusing on comparative statics, the case studies tend to emphasize patterns of change (and stability) over time in the eight countries examined (Chile, Argentina, Mexico, Brazil, Bolivia, Nicaragua, Colombia, and Venezuela). Several of the country chapters omit one or two of the five criteria emphasized by the editors. In addition, the manner in which these five criteria are discussed varies considerably—both relative to the editors’ treatment and in moving from country to country. Perhaps this divergence of perspectives is driven by the differing meaning and relevance of some of these five dimensions as one moves from country to country.

The most evident area of divergence involves the treatment of responsiveness. In the editors’ framework, responsiveness is tied to political efficacy, as the editors assert “that those who consider that their vote is efficacious are implicitly recognizing that politicians are responsive to the popular will as expressed in elections” (p. 29). In stark contrast, Leticia M. Ruiz Rodríguez discusses responsiveness in Chile through the lenses of what she sees as the major public policy issues of the day—human rights, macroeconomic policy, and social welfare policy (pp. 52–53). Mark P. Jones and Juan Pablo Micozzi discuss responsiveness in Argentina via examination of expressed levels of satisfaction with democracy (p. 76), an approach that the editors had rejected as excessively linked to one’s approval of the government of the day (pp. 29–30). Claudio Holzner details linkages among inequality, participation, and political attitudes as evidence of a still-unresolved debt of democratization in