

## 13. Clarence N. Stone and the Study of Urban Politics

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### URBAN POLITICS AND COMPARATIVE NATIONAL POLITICS

Among political scientists, the study of urban politics, whether within one nation or cross nationally, resembles the comparative study of national politics—with one crucial exception. In both the urban field and the comparative field, scholars typically come to know one or several localities very well. Their knowledge ranges across academic disciplines, encompassing language and culture, history and tradition, economic dynamics and outputs, social structures and processes, political institutions and actors, and perhaps international contexts and events. In the course of acquiring such rich knowledge, scholars frequently develop strong attachments to their location. They also often affirm the view that one needs detailed particularistic understanding of the idiosyncrasies of a given place in order to study it effectively.

Of course, some scholars of urban politics, as of comparative national politics, tend in the opposite direction. They seek out aggregated data that are standardized across locations, and they develop parsimonious theories intended to apply across particular situations. This set of scholars seeks to compare many sites along a specific, small set of dimensions rather than examining one or a few sites across an array of dimensions. They may see particular historical, cultural, geographic, or political details as extraneous complications to be set aside in the quest for explainable regularities.

The parallels continue: no matter what the approach, those who study local communities and those who study nations or regions must engage with similar intellectual and empirical concerns. Where are the boundaries of the location or locations, and should they be defined differently depending on the question at hand? What consequences flow from the fact that a given place lies within a larger political unit—Cleveland within Ohio and the United States; France within the European Union or G8? Do distinctive cultural, ethnic, religious, economic, or linguistic characteristics of the population matter for a given analytic question, or are they irrelevant? How much variation in the quality and construction of data is acceptable if one seeks to compare across locations or time periods? How can one

take full account of the location or locations that one knows less well compared with the places that one knows intimately?

A final parallel is especially pertinent to this chapter. Until recently, scholars have been more inclined to do methodological battle over the right approach than to meld them or seek some alternative framing. The rise and partial fall of area studies within the field of comparative national politics is well known, and one can find traces of similar arcs within the study of urban politics. But in both fields, arguably those scholars with the most profound and valuable impact have transcended this dispute, whether by combining aggregated and particularistic analyses, or by developing broad theories flexible enough to apply in varied situations but incisive enough to frame a distinctive viewpoint.

Clarence Stone is in the latter camp, with disturbingly few peers in political science scholarship on urban politics. He is deeply knowledgeable about Atlanta, Georgia, having studied its political development for decades. He is familiar with a dozen other American cities, having studied their educational reform efforts for years. With coauthors or independently, he has developed broad theoretical frameworks—regime analysis, the “power to” approach, the systemic bias of power and inequality, the centrality of agenda setting and coordination, the urgent need for democratic decision making—that explain actions and outcomes not only in his cities but in many others as well. All of this work is undergirded by a few simple, clear principles about human nature and the conduct of social science that are easy to state and surprisingly fecund.

Before commenting on this body of work, I must point out one way in which the study of urban politics and the study of comparative national politics are emphatically not parallel: the former is largely in abeyance while the latter is thriving. The editor of the *American Political Science Review* (APSR) reported in 2005 that “comparative politics . . . now challenges the numerical dominance of American politics submissions [to the *Review*], a turn of events that not very long ago would have been widely regarded as an extremely remote possibility” (Sigelman 2005, 138). He does not distinguish urban politics within the broader field of American politics (or within comparative politics, for that matter). But in the Introduction to this volume, Marion Orr and Valerie Johnson point out that only a tiny fraction of articles in APSR have ever addressed urban politics, even taking into account its heyday in the 1960s and 1970s.

Even more telling about the relative disciplinary strength of the study of urban politics and comparative national politics is membership in the American Political Science Association’s organized sections. Section membership, unlike publication in journals, is completely voluntary and exhibits no competition for scarce space because every APSA member may join any number of sections for a nominal annual dues payment. In 2006, a total of 346 people belonged to the Urban Pol-

itics section; it was one of the smallest of the 34 sections. The section on Comparative Politics enjoyed 1,594 members—almost double the size of the next largest section. Membership in the Urban Politics section peaked at 467 in 1992 and has been slowly declining since. During the same period, membership in the Comparative Politics section has steadily risen from almost exactly the same starting membership in 1989. Something is working for one field and not for the other, despite considerable methodological and substantive similarities.<sup>1</sup>

An examination of Clarence Stone's research accomplishments shows how unfortunate this abeyance is, and it suggests some ways that scholars might be able to overcome it.<sup>2</sup>

#### OCCAM'S RAZOR AND LANYI'S BALLOON

My undergraduate classmates and I thought it a sophisticated witticism to compare Occam's razor (about which we had just learned) to Lanyi's balloon. George Lanyi was a much loved professor at Oberlin College who responded to almost any observation (in Hungarian-flavored English) that "things are much more complicated than that." He was not just an obstructionist; he could in fact demonstrate complexities and subtleties at the heart of most topics of interest to political scientists. And our labored witticism actually did point to an important issue: in seeking to explain a phenomenon or process, should one try to chip away superfluities to arrive at its essence, or should one recognize and take into account its many interactions, contingencies, and dimensions?

Clarence Stone is a devotee of Lanyi's balloon. All of the essays in this volume show in one way or another his belief that "politics . . . , business . . . , and civil society are woven together in intricate ways. . . . Urban regime analysis is therefore an effort to . . . penetrate how these various sectors are woven together. . . . The sum . . . is greater than the parts" ("Urban Politics Then and Now," this volume). He constantly points to intersections among causal forces, the need to distinguish one policy regime from another (in recent work), variations in historical trajectories, differences in governance dynamics across cities, varying interests and resources of political actors, and other ways of taming complexity without simplifying it.

Conversely, Stone is critical of scholars who adhere to Occam's razor. In his view, Paul Peterson's *City Limits* is valuable as far as it goes, but it makes simplistic distinctions that connect poorly with the messiness of actual policy making. Even more problematically, in his view, *City Limits* is premised on the claim that an economic framework is all one needs to make sense of what cities do or do not do. Stone criticizes Peterson for not attending to the political dynamics of power and choice or the social dynamics of demand and creativity. Even Robert Dahl's *Who*

*Governs?* (1961), with which Stone has been in “a career-long dialogue,” is too simplistic from the vantage point of Lanyi’s balloon.<sup>3</sup> Unlike Peterson, Dahl does attend carefully to politics—but Stone argues that like Peterson, Dahl pays too little attention to systemic interactions among societal, economic, and political forces. Furthermore, according to Stone, Dahl defines politics too narrowly as elections, and their outcomes as visible and discrete decisions, thereby ignoring political actions ranging from agenda control to incipient protest.

A proponent of parsimony might reply that Stone is descriptively correct—Peterson builds his theory on simple distinctions among policy types and their material base, whereas Dahl focuses narrowly on electoral politics and its outcomes—but substantively mistaken. In this view, what the study of urban politics (or any politics?) needs is a few sharply delineated and straightforward theories from which one can derive testable propositions, which one should then test. After all, in this view, recognition that the urban world is complicated, interactive, and somewhat contingent is, judging by the work of too many researchers, an invitation to endless description and idiosyncratic interpretation of particular cases. So, a proponent of parsimony might conclude, Stone and a few other urbanists should perhaps continue to explore the complexity of city governance while most scholars should aim for the narrower but more rigorous goal of identifying analytic tools or models that will point to the few most relevant facts, relationships, and causes.

I do not propose to adjudicate between Occam’s razor and Lanyi’s balloon. This is an old debate, spread across most subfields of political science and perhaps across most social sciences. Its relevance here is twofold. First, it indicates one of Stone’s distinctive and perhaps singular virtues; he is able both to insist on complexity *and* to distill from it a clear-cut, testable set of propositions. Consider urban regime analysis: Stone and his students used its broad earlier incarnation to identify diverse sorts of electoral and governance dynamics in different kinds of cities, and Stone and his coauthors have used its more narrow recent incarnation to delineate governance dynamics in distinct policy arenas within a given city or across cities.<sup>4</sup> Those are major achievements, comparable respectively to the broad delineation of “three worlds of welfare capitalism” (Esping-Andersen 1990) and the more focused comparison among national policies of vocational training (Thelen 2004) in comparative politics.

Second, a robust discussion about the best way to explain city governance—many complex interactions or a few dominant forces—might go a long way toward reviving the subfield of urban politics within political science. It would at any rate be one element of a broad strategy of (re-)connecting research on city governance with research on national governance. Let a hundred flowers bloom; scholars of urban politics should develop and encourage the use of formal models or rational choice methods to study cities, along with more traditional approaches that rely on case

studies, historical trajectories, survey research, or aggregate data analyses. Stone's corpus sets the standard for one kind of methodological framework; I would urge others to adopt his breadth of vision and turn it in other directions.

#### HOW ARE CITIES LIKE AND NOT LIKE OTHER POLITICAL UNITS?

The broad strategy of (re-)connecting research on city governance with research on national governance must surely be substantive as well as methodological. There are plenty of links to be made. Again, Stone set the standard, drawing on Hannah Arendt's theories of power, E. E. Schattschneider's analysis of the mobilization of bias, V. O. Key's commitment to a more equal citizenry, Dahl's depiction of pluralism as a goal if not a practice, scholarship in international relations on regimes, cognitive models of framing and agenda setting, W. E. B. DuBois's recognition of the interactions between race and class, and more. Perhaps few other scholars can realistically aspire to such a broad range, but that need not prevent us from pursuing at least more particular connections.

For example, should researchers focus on the capacity of bureaucrats and other state actors to achieve their stated goals? That question could bring the classic insights of *Who Governs?* into direct contact with the thriving literature on bringing the state back in (Skocpol 1985) and bureaucratic autonomy (Carpenter 2001). Perhaps scholarship could usefully focus on path dependence of a city's development, such that its political dynamics are best explained by the timing of its origin, its early development, and choices at critical junctures. In that case, the excellent historical writing about many urban locations could be connected with theoretical work on "politics in time" and political development (Pierson 2004; Orren and Skowronek 2004). Attention to economic and public choice imperatives would start with *City Limits* but move beyond it into recent research on the role of states in a global economy or the impact of rational self-interest within institutional structures. If city councils are enough like national legislatures, then research on the pivotal voter, principal-agent relations, committee dynamics, or electoral imperatives can be brought to bear. Is racial tension in American cities analogous to ethnic conflict in nations around the world? If so, the rich literature on civil wars and intranational conflict could illuminate the history of American riots, and vice versa. Cities compete with one another, develop formal ties for mutual benefit, and trade goods, people, and information. Thus aspects of scholarly writing on international political economy, or even international security studies, could suggest intriguing analytic frameworks—including, but not limited to, regime theory. If interracial coalitions are important to city governance, as they surely are, urbanists could start

with Browning, Marshall, and Tabb's canonical *Racial Politics in American Cities* (1990) and then link it to the rich array of game theoretic work on coalitional strategies and failures.

Any (re-)connection between the study of politics in cities and mainstream political science writings on politics in nations will also need to attend to what is special about cities. Their geographic scale and physical location are obviously different from that of most national governments. How do size and scope matter to governance and activity? (Dahl and Tufte 1973; Katzenstein 1985; Alesina and Spolaore 2003). Cities are near the bottom of a federal system, while national legislatures and executives are near the top. How does that matter? Surely the literatures on federalism, state and regional politics, and cities have much to say to each other, both about nested relationships and about distinctive locations (Manna 2006; Baimbridge and Whyman 2004). Cities have no court system comparable in importance and visibility to the federal judicial system; how does the relative lack of a judiciary change the study of relations among branches of government? Above all, as Stone's research has shown so powerfully, cities operate through a complex web of face-to-face, neighborhood-to-neighborhood, group-to-group, business-to-business, ward-boss-to-ward-heeler relationships. People in national governments have direct connections with one another, but the nature and role of civil society is arguably dramatically different in cities than in nations (Mansbridge 1980; Berry, Portney, and Thomson 1993; Mutz 2006; Fung 2004).

Thus it makes no more sense to treat the study of urban politics as simply a miniversion of the study of national politics than it does to treat the study of urban politics as *sui generis*. Stone's insistence on the importance of complex interpersonal relationships in cities sometimes seems to suggest the latter, but his grounding in broad normative and analytic theories of politics, and his capacity to develop testable theories that move across locations and arenas, pulls his research back from that unhelpful path. If more scholars of both national and local politics would link propositions about cities to research on the study of nations, the field of urban politics would be stronger and more attractive to young scholars.

#### "POWER TO"

Stone's focus on power both in this volume and throughout his research warrants as much close attention as his research on urban regime analysis. Part of the reason is simply its existence; too much scholarship on cities (as well as other political phenomena) ignores power altogether. That observation holds both within political science and in a comparison between political science and comparable disciplines. As an indicator of the former point, consider a simple word count in JSTOR. Among

10 major peer-reviewed political science journals from 1995 to 2007,<sup>5</sup> the combination of words “political power” and “city” appeared 184 times, compared with “economy” and “city” 455 times, and “culture” and “city” 405 times. Nothing much should be made of these absolute numbers, but the ratios of more than two to one is at least suggestive.

As an indicator of the latter point, consider disciplines similar to political science but without the focus on power at their core. There, the study of urbanism is thriving. With the development of GIS programs and connections to an array of other disciplines, urban geography is making a comeback; it is the second largest specialty group in the Association of American Geographers (note from Elvin Wylly to author, March 14, 2007). In sociology, the section on Community and Urban Sociology has a new journal, and membership rose from “the mid-500 level” several years ago to 725 in 2006 (fall 2006 newsletter, available at <http://www.commur.org/>). (The American Sociological Association is slightly smaller than the APSA.) So it is not that the study of cities is of no interest to scholars; instead, the investigation of power or politics in cities seems to be what lacks appeal.<sup>6</sup>

Instead of focusing on the accretion and exercise of power by human actors, urbanist political scientists typically frame their arguments in terms of structures or institutions so that theoretically disembodied forces do all the work. As Stone puts it in “Urban Politics Then and Now” (this volume), “In many urban analyses complex structures tend to be reduced to an economic imperative,” whether understood as capitalist market forces or as rational self-interest. Into this partial conceptual vacuum, his robust and subtle analysis of the difference between “power over” and “power to” is a welcome insertion. Giving pride of place to power to rather than power over is an innovative move, and accords well with regime analysis. Stone notes, “Without denying that ‘power over’ (control) is an important facet of human relationships, let us think about ‘power to’ (moving from an incapacity to act toward enjoying such a capacity). . . . Interdependence becomes a central fact”—just as it is the crucial feature of regime analysis (“Urban Politics,” this volume). Thinking of power as control might accord best with Occam’s razor—such a definition strips the essentially contested concept down to its bare essentials—but thinking of power as effective interdependence certainly fits with Lanyi’s balloon. Power thus understood is complicated, multifaceted, and hard to measure—according to proponents of regime analysis, necessarily so.

Power understood as interdependence in pursuit of capacity to achieve a goal opens a multitude of analytic possibilities, despite the difficulties in operationalizing and measuring it. It can explain total frustration of effort: the mayor can do little to improve schooling outcomes because the school board is independently elected, the school budget is separate from the city’s budget, the teachers’ union is concerned mainly to protect its members, the superintendent works at the pleasure

of the board, advocacy groups seek an array of particular reforms, courts are overseeing expenditures for special education, the federal government is promulgating unfunded mandates, administrators are focused on developing black social capital throughout the city, and middle-class families are moving to the suburbs. And that sentence does not even mention the enormous difficulties in the best of circumstances involved in getting teenagers to concentrate on chemistry labs and vocabulary tests!

Power understood as interdependence might, however, permit a more optimistic analysis in some circumstances, explaining, for example, the breakup of old systems of domination and subordination. As Stone showed in Atlanta, sometimes the mayor needs the votes of members of previously ignored groups or neighborhoods, or downtown business leaders need the help of an insurgent mayor, or advocacy groups can use their resources to bargain for benefits to their members. "Interdependence is thus not a basis for an unceasing war of all against all. Quite the contrary, it is a foundation for seeking workable alignments. . . . There are many potential partnerships" ("Urban Politics," this volume). Stone's recent work with an array of coauthors to study educational reform politics in 11 American cities not only displays highly productive scholarly interdependence, but also documents the successes possible in the public arena—even with regard to school reform—when interacting partners pull in the same direction with a shared purpose.

I see intriguing affinities between Stone's analysis of "power to" and the concepts of some feminist theorists. Anna Yeatman, for example, describes one feminist understanding of power as "capacity . . . —an active and experimental relationship to established and not-yet-established models of self-governance. . . . A democratic state which respects . . . the rights of women . . . constitutes women as agents in their own right who are entitled to the status of a rights-bearing person" (Yeatman 1997, 153–54, emphasis deleted). That sounds very similar to Stone's analysis of how to incorporate poor neighborhoods into an urban governance regime: "The policy 'game' is . . . a more open-ended matter . . . with appreciation that long-term well-being individually and collectively involves interdependence." The new urban agenda requires "the building of associational bridges between the disconnected populations of cities and mainstream institutions" ("Urban Politics," this volume). Not-yet-established and open-ended; models of self-governance and building of associational bridges; agents in their own right and disconnected populations—the contexts of these two analyses are quite different, but the concepts and aspirations are similar.

Yeatman's description of power as capacity is echoed by other feminist theorists. The idea of

[e]mpowerment . . . center[ed] . . . on how people developed and expressed a political consciousness. . . . [It] shifted attention to communities, in addition to the more con-



ventionally studied individuals and institutions. Indeed, one could even speak of the empowerment, as opposed to the power, of an entire polity. . . . [I]ts powers were bounded by what the community thought was the purpose of the community. (Flammang 1997, 33, describing the ideas of Joan Tronto; see also Hartsock 1983)

Again, one can see links between this concept and Stone and his coauthors' work on urban school reform. Their comparative study of 11 cities showed clearly that a necessary (though insufficient) condition for improving education was that salient community members, ranging from mayor to business leaders to poor parents, were jointly empowered in pursuit of a common "purpose of the community."

To my knowledge, neither Stone nor the feminist theorists have used, or are aware of, the research of the other. That lack of interaction has left the analyses on each side thinner. Flammang usefully reviews various writings on whether—or better, when—women as advocates, voters, bureaucrats, and politicians are more likely than men to focus on "power to" rather than "power over." She also addresses the more interesting question of when agencies or processes focus on empowerment rather than domination. She considers local civic organizations, welfare reform efforts, health care, and even education reform—all issues of central importance to cities—to see how new conceptions of power might affect their practices and outcomes. Stone's analysis of successful urban regimes, especially but not only in schooling, would have enhanced her discussion. Conversely, the feminists' subtle and exciting exploration of alternative ways to conceive of power, new ways to study and promote shared empowerment, and the distinctive role of women would contribute to Stone's critiques of the pluralist and power elite frameworks. It could also deepen the explanations that Stone and his coauthors provide for varying levels of success across cities in efforts to improve schooling.

It is not too late for both sides to start that discussion. What, concretely and materially, is needed to shift an urban regime from being preoccupied with domination to encouraging empowerment? What is required for mayors to learn that their chief concern should be enhancing shared capacity rather than controlling potential adversaries? Does electing women to the mayoralty or city council make a difference; do women business leaders behave differently from their male predecessors? Flammang and others have noted that community organizations are often started and maintained by women; under what circumstances can men develop the same aptitude for bridge building? The crucial move here would be to specify clearly and in a way that would lead to falsifiable propositions just what capacity building looks like, how it develops, and what it can effect. By this criterion, Stone's work is farther along of that of most feminist theorists, because he is more of a social scientist than they typically are. But overall, the study of urban politics would greatly benefit from engagement with gender politics and feminist theory (as well as vice versa).

Other arenas of political science also study “power to,” although they seldom invoke the term. The study of agenda setting from a rational choice framework, and of issue framing from a psychological framework, are both close cousins to the conception of power as the capacity to accomplish a task in a complex environment. Linking Stone’s focus on differentially distributed access to points of leverage for change with formal models of how actors in institutions control the options available for decision making, for example, would bring a particular type of rigor to the former and a whiff of reality to the latter. Both would benefit.

Stone’s innovative analysis of power has a broader implication. A weakness of our discipline is that the central concept, power, has no consensual small set of definitions or measures, analogous to money for economists or class for sociologists. Definitions of power have ranged from Max Weber’s and Robert Dahl’s epigram, “A gets B to do what B would not otherwise do,” to Hannah Arendt’s and Richard Neustadt’s claim that power is the ability to persuade, to Steven Lukes’s and John Gaventa’s “third face” of ideological obfuscation, to Michel Foucault’s vision of power as a discipline in the capillaries. Stone adds another understanding. It is high time that more political scientists focus directly on studying power. No one should indulge the false hope of resolving the question of what it *is*, but our numerous and increasingly sophisticated measurement tools, concepts, and data sources could produce an exciting and illuminating debate, and maybe even a few points of shared insight. Urbanists could play a central role in this discussion; they have manageably sized cases, excellent comparative possibilities, well-honed concepts, a great deal of local knowledge, a passionate commitment to empowerment (in most cases), a rich array of aggregated data, and a wide set of questions that cry out for more systematic evidence and analysis.

#### DEMOCRACY AND INEQUALITY

Stone (like feminist theorists) is not foolish enough to jettison traditional understandings of power as control or domination. As he points out in various writings, “power over” must remain a central concept in any study of structural and interpersonal inequality. And the study of inequality in a purported democracy has been a driving motivator of his research from the earliest publications. In fact, Stone describes his career-long dialogue with *Who Governs?* not at all in methodological terms as I did above, but rather as a substantive debate with Dahl

about the fundamental nature of political inequality in a formally democratic system. . . . A major reason for focusing on regime rather than decision is to focus on capacities to shape governing arrangements, as opposed to influence on discrete decisions. Looking

at the level of governing arrangements makes the threshold issue [that is, how many political resources are needed to change the established order] salient and provides greater opportunity to consider stratification-based bias. (Note to author, January 30, 2007)

I see no reason to dispute Stone on that claim; his commitment to reducing inequality and enhancing democracy shines through in everything that he writes. To choose only one illustration, "The challenge is to find leverage points to lessen the impact of socioeconomic inequalities. Ultimately that is where this essay is heading. Normatively that is what regime analysis is about" ("Urban Politics," this volume).

Two points about those sentences warrant discussion in the context of my broader consideration of the present and future study of urban politics. First, they show clearly how much Stone's research is rooted in moral and programmatic rather than purely empirical or scientific commitments. In this, he resembles most other urbanists, but arguably fewer other political scientists. For example, Peter Dreier, John Mollenkopf, and Todd Swanstrom's book of political geography, *Place Matters*, includes a chapter on "What Cities Can and Cannot Do to Address Poverty," and they describe the book as "not only a synthesis of research findings but also as a roadmap for reform" (2004, xi). J. Phillip Thompson issues a "call for deep democracy" in the subtitle of *Double Trouble* in the hopes that his book will "unveil . . . painful *internal* oppressions and exclusions within black politics for the purpose of increasing the black community's power to compel similar unveiling in broader interracial politics" (2005, ix). Even Douglas Rae, who despairs of the future of urbanism as we have known it, concludes *City* with explicit recommendations for "seek[ing] an urban future . . . that recaptures much of what was desirable on the 'soft' side of urbanism" (2003, 422).

Conversely, a plurality, and perhaps a majority, of research-oriented political scientists aspire to be "real" scientists, or at least do not see themselves to be conducting research in order to promote their normative commitments. Most scholars of comparative national politics, to return to my original point of comparison, eschew exhortations about reform in studies of the political economy of industrialized nations or the nature of welfare state regimes. Similarly, most scholars of congressional lawmaking or political party identification or principal-agent relations in bureaucracies make no effort to suggest how the House of Representatives, the Republican Party, or the Department of Energy should do its work better. The point holds broadly across political science. As the editor of *APSR*, Lee Sigelman, has documented, the purpose of only 1.2% of articles in *APSR* since its inception a century ago has been "policy prescription or criticism." Only another 2.9% of articles in *APSR* have combined "policy prescription or criticism and presentation of empirical results." Virtually all articles in both categories were published before 1956. "If . . . contributing directly to public dialogue about the merits and

demerits of various courses of action were still numbered among the functions of the profession, one would not have known it from leafing through its leading journal" (Sigelman 2006, 467).

Like all generalizations, this one has exceptions on both sides. On the one hand, many articles and some books in the field of urban politics are carefully descriptive or explanatory, without any particular normative purpose in view. On the other hand, some scholarship on national institutions or comparative state politics *is* explicitly prescriptive. Thomas Mann and Norman Ornstein (2006) published a heartfelt plea to Congress to get back on track, and surely the many studies of the causes of ethnic conflict or civil war are written in the hope of helping to reduce their incidence. More broadly, postmodern scholarship has taught us that it is virtually impossible to escape one's own value stances in one's research, no matter how apparently esoteric or ruthlessly empirical it is. As Stone argues, "the alternatives appear not to be science versus reformism, but celebratory analysis versus critical analysis" (note to author, January 30, 2007).

Nevertheless, overt political and policy prescription is different from implicit, or unconscious, revelation of moral values. And urbanists, including Stone, do much more of the former than do most political scientists. I have no quarrel with the commitment to recommend policies or to promote particular values through one's research—I do both myself. Nevertheless, those of us who reject the ideal of a value-free, neutral social science must realize that this taste for reformism, especially because most of it comes from the liberal left, is one more way in which the study of urban politics is separated from most other empirical subfields of political science.<sup>7</sup> And it may have contributed to the field's relative and absolute decline in adherents. That is a price I am willing to pay, as are others, but it is a steep one.

The second point to note about Stone's claim that "the challenge is to find leverage points to lessen the impact of socioeconomic inequalities. . . . Normatively that is what regime analysis is about" is more substantive. Unlike many scholars who study race and ethnicity, he sees class dynamics as being just as important in shaping cities as, if not more important than, the black-white racial divide. Stone never loses sight of the power of race to organize structures, opportunities, and outlooks in the United States. Nevertheless, one comes away from the Atlanta books with the sense that a person's wealth arguably has more impact on his or her life chances than does race. After all, black mayors found that in order to get anything done, they needed to deal with white downtown business owners almost exactly as white mayors had done or would have done in their place. That is where the bite in "power to accomplish" really lies. And as Stone points out at the beginning of this volume's "Urban Politics Then and Now," repeated efforts to help poor neighborhoods in Atlanta have fizzled out, in part because wealthy and powerful black lead-

ers have not given those efforts their highest commitment: "Atlanta's biracial coalition handles some matters well, but not others."

Thus, in Stone's understanding of urban regimes, elite actors almost inevitably ally with one another, and their pursuit of their own interests or their efforts to get things done for the city almost inevitably conflict with the interests of nonelites, especially the poor. Furthermore, Stone's analysis of the threshold problem in supposedly pluralist cities points to a larger structural issue with regard to class: "incumbent leadership/established arrangements enjoy substantial advantages . . . and . . . inertia is on the side of an established order. . . . Substantial resources, allies, skills etc. are needed . . . to displace that order with a new one" (note to author, January 30, 2007). And by definition, poor residents of a city lack substantial resources.

I mostly agree with Stone on this point, perhaps because I learned it from him. But the most important point is yet broader: the field of urban politics needs more newly conceived investigations of whether, when, and how urban governance is genuinely open to an array of interests and commitments, or is so tightly constrained that "democracy is a stone of Sisyphus" (to quote Stone's note once again). Dahl examined how unequal persons can effectively express political agency; Stone focuses on the structural and individual barriers to genuine change; Peterson developed the view that the relationship between classes is mutually enhancing rather than competitive, and in any case close to inevitable; Browning and coauthors sought for ways to develop coalitions across races (and therefore presumably across classes). Presumably there are other lenses through which to examine how cities and their residents contribute to the making or prevention of democracy; scholars who create them could revitalize the study of urban politics.

#### TOWARDS A SPATIAL THEORY OF CITIES

The boundary around a city defines the political and policy work of mayors and other urban public officials. But for many urgent concerns, that may be true more in a formal or superficial sense than in a deeper structural sense. That is, the activities of urban political actors or the trajectory of a city may be largely shaped by the horizontal relationship between a city and its adjacent communities or by the vertical relationship between a city and its state, region, national government, or supranational organization (such as the EU). This observation suggests that another way to invigorate the study of urban politics would be to develop spatial theories of politics analogous to what Paul Pierson and others have been doing for a theory of political time (Pierson 2004; Orren and Skowronek 2004; Skowronek 1993).

I do not have a fully developed argument here; that awaits more sustained

attention.<sup>8</sup> One way to frame the enterprise would be to “direct attention to the ‘politics of scale’—the processes by which scale is constructed” through state laws on taxation or incorporation, the creation of regional governance structures, devolution of decision making to neighborhoods, and so on” (note from Susan Clarke to author, February 7, 2007). Alternatively, one could direct attention to the scale of politics—that is, ways in which face-to-face engagement shapes political interactions compared with ways in which links among very large numbers of voters shape distinctly different political interactions. In lieu of a developed theory, let me offer several illustrations of actual or possible analyses of the politics of space:

- Inequalities within cities are intimately, perhaps causally, connected to inequalities between cities and suburbs. As Rae argues, “resources are most available in [suburban] towns. . . . Needs are fewest in those same places. Resources are fewest on a per capita basis in New Haven [and similar cities], and needs greatest. To a very considerable extent, inequalities reinforce one another. . . . Being able to choose *where* is a more powerful instrument for deciding *what* and *how* one’s family will live than anything else. . . . [E]xit has become even more vital than *voice*” (2003, 421, emphasis in original).
- Conversely, a suburb may be attractive socially and economically (but not politically?) to the degree that it is near a thriving city. Features ranging from the density and variety of cultural and artistic institutions and face-to-face interactions among high-end service industries such as financial firms and research hospitals, to mundane matters such as access to public transportation and airports or availability of a low-wage work force, make living in a community near a city more appealing. How suburbanites engage with the city will affect the inequalities that Rae describes.
- A “city” whose boundaries encompass a large, sparsely populated area has different resources and policy options with regard to such issues as public school desegregation, tax policy, waste treatment, water supply, and land use planning than a city whose boundaries are narrowly drawn. It will also have a different political configuration. The point is longitudinal as well as cross-sectional; how boundaries change over time will be of great importance to a spatial theory of politics.
- External interventions by encompassing political units, such as state takeovers of failing schools or judicial intervention in the creation of voting districts, might undermine local civic capacity by creating an environment in which power seems to be “out of our hands.” Alternatively, activity by a larger government might help to even out disparities in political resources.
- The local business community might respond to frustration with local public bureaucracies by appealing to larger political units, such as state, national, or even supranational legislators, to mandate structural reforms in schools, health care, natural resource management, transportation, or housing policy. That intervention too could enhance or reduce inequalities, and could expand or contract resources and policy options, within cities or between cities and nearby communities.<sup>9</sup>

- Cities that are tightly connected with global markets, in part because of their location or topography, will have different trajectories from cities that are only loosely involved with international trade and migration (Sassen 2006).
- The topography of cities and their surrounding metropolitan areas may have important political implications; areas with a lot of rivers have more school districts than areas without, as Caroline Hoxby has pointed out.

This list is far from complete, and it is even farther from being a coherent analytic framework for studying politics in space. But I hope that it suggests some of the wide array of research endeavors that are or could be underway—and especially that it suggests how unifying these endeavors under a broader theoretical umbrella of spatial politics would enable them to add up to more than the sum of individual investigations.

## CONCLUSION

Sigelman concludes his review of a century of *APSR* articles by arguing that “the period from the late 1950s through 1970 or so . . . [was] the one in which political science research was most ambitious, innovative, stimulating, and important—certainly more than it is today” (2006, 473). That was also the golden age of the study of urban politics; is that a coincidence? Sigelman deplores the growth of subfield specialization, in which “a given paper will be selected for publication because it passes muster among a narrow range of specialists rather than because it is considered to be of potentially great interest and importance to a broad range of readers” (475). My main message in this chapter is similar. Scholarship would be enhanced if students of national politics overcame their assumption that bigger is better, whether in political importance or disciplinary status. By ignoring local arenas, they miss opportunities for careful comparative research, important policy arenas, and the invitation to engage with issues of scale and scope. At the same time, scholars of urban politics need to do more to connect with other subfields of political science, to deepen the theoretical dimension of space in their research, to get more leverage from comparisons among cities within and across nations, and to develop testable, causal theories and careful measurement of concepts such as power, regime, scale, and inequality. I hope that they need not also abandon normative ideals and a commitment to reform—although they might usefully be less committed to the proposition that only the liberal left has legitimate ideas for reform. On the normative and policy dimension, they should lead rather than follow the rest of the discipline.

In all of these arenas, Clarence Stone’s work has indeed led the rest of us. If this book stimulates more research of the quality of his own, it will be a success.

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## NOTES

1. Thanks to Sean Twombly for information on APSA section membership, from the association's database.

Of course, it is possible that specialists in urban politics choose to join other sections, such as Race, Ethnicity, and Politics; Public Policy; or Comparative Politics, thus skewing the numbers and concealing their interest in the field. Market dynamics may have something to do with this: many jobs are defined as Comparative, and thus scholars so classify themselves. If so, these patterns reinforce rather than refute my claim that urban politics is a weak (and weakening?) subfield in political science. After all, one need not leave the Urban section to join another; of the 8,000 or so members of APSA who belong to at least one section, 5,200 (about two-thirds) belong to more than one section. Nevertheless, the section on Race, Ethnicity, and Politics is growing rapidly (from 248 when it was formed in 1996 to 599 in 2006), while Urban Politics is declining. And the fact that political science departments now find it useful to orient hiring and teaching around the field of comparative politics but no longer around the field of urban politics is a further indication of the relative strengths of the two arenas in disciplinary terms.

2. It is perhaps relevant that I do not consider myself an urbanist and am not usually considered one by others. However, I belong to the Urban Politics section of APSA, and my research and teaching cover many of the same topics that urbanists address, so I am a fellow traveler.

3. Stone has other crucial, and more substantive, disagreements with Dahl's *Who Governs?* (1961), which I discuss below.

4. Thanks to Archon Fung for pointing out these changes in the way that Stone has described regime analysis since its initial deployment in *Regime Politics* (1989).

5. Operationally, 2007 means the most recent year that JSTOR includes for any given journal. The journals were: *American Journal of Political Science*, *American Political Science Review*, *British Journal of Political Science*, *Comparative Politics*, *Journal of Conflict Resolution*, *Journal of Politics*, *Perspectives on Politics*, *Political Theory*, *Public Opinion Quarterly*, and *Social Science History*.

6. Another example: in the 2006 conference of the interdisciplinary Urban Affairs Association, *power* appeared in only eight unique uses in the program, which listed about 100 panels and 300 papers (<http://www.udel.edu/uaa/>). (One additional use referred to power stations.) In contrast, variants of *culture* appeared twice as often as *power* in the program, and *economic* appeared about four times as frequently.

7. An alternative to encouraging purportedly value-free social science in the field of urban politics would be to encourage expression of a multiplicity of values and corresponding policy recommendations. Nothing, of course, prevents people of varying ideological commitments from teaching or publishing on urban politics. Nevertheless, except for the arena of education policy



and occasionally racial politics, few political scientists generate socially or economically conservative analyses of cities' problems or opportunities. Despite important exceptions, I am fairly sure that conservatives feel less welcome at conferences or convention panels on urban issues than do scholars who identify with the left. How to change that situation remains unclear.

8. Nor is this point especially new; see, e.g., Lineberry (1975); Williams (1975); Danielson and Lewis (1996); and Hayward (2006).

9. Variants of the latter two ideas were developed by Jeffrey Henig in a note to the author and, with coauthors, in *The Color of School Reform* (1999).

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