Unlike most other areas of the social sciences, the study of urban politics has been slow in developing a comparative research agenda. This article explores the potential in comparative urban governance research. Urban regime theory does not travel very well, partly because it is an undertheorized framework and partly because it is in many ways an abstraction of U.S. urban political economy. To escape these obstacles to comparative research, this article argues that regimes should be conceived of as a culturally and historically specific model of urban governance. Comparative urban governance holds tremendous potential in assisting scholars in uncovering causal mechanisms and drivers of political, economic, and social change at the urban level.

**Keywords:** urban governance; local government; comparative research; political institutions; urban politics

Comparative analysis has become an integrated component of research in most subfields of political science. Whether cross-national or cross-sectorial, it is clear that research areas such as political economy, institutional analysis, or administrative reform have benefited tremendously from the growing number of comparative analyses.

Whether by default or design, however, the study of urban politics and governance has been conspicuously immune to these changes and developments; urbanists have been surprisingly slow in using comparison as a research strategy. Notwithstanding a number of significant exceptions to this rule (Clark and Hoffmann-Martinot 1998; DiGaetano and Klemanski 1993a, 1993b, 1999; DiGaetano and Lawless 1999; DiGaetano and Strom 2003;
Savitch and Kantor 2002; Sellers 2002; see also Pierre 1999; Stoker 1995), urban politics has largely remained dominated by single-case studies supplemented with the odd two-case comparison. This becomes all the more frustrating, because the comparative approach offers excellent possibilities to systematically test hypotheses about causal relationships between different variables.

There are probably several reasons for this apparent reluctance among urbanists to adopt a comparative research agenda. One conceivable explanation is that many urbanists tend to define their research area as an interdisciplinary field rather than as a subfield of political science. Therefore, changes and developments in mainstream political science will only have a limited influence on the urban politics research agenda.

Furthermore, urbanists typically emphasize the importance of adopting a holistic and context-embracing approach to understand the contemporary city or the historical trajectory of urban development. Embarking on a comparative research agenda entails by necessity some degree of reductionism as a step in preparing empirical observations for comparative assessment. Comparison requires a robust analytical framework defining the variables to be compared, leaving out as much contextual “noise” as possible. Comparative political science is, thus, by definition more parsimonious than the in-depth, context-embracing, idiographic studies that one sees in much of urban politics (cf. Hay 2002).

An additional explanation to why one sees so little comparative research in urban research relates to the role of theory in urban politics and the ontological foundation of urban social and political theory. Most dominant theories in urban politics draw on—or, more correctly, are abstractions of—political, economic, and social aspects of the American city. Whether these theories relate to urban political economy (Peterson 1981; Hill 1984; Vogel 1992), urban democratic theory (Hill 1974, 1994), the role of civil society, or patterns of political and social cleavages, the theories that dominate the urban political discourse are, for the most part, derived from the case of the American city. Arguably, this applies most directly to urban regime theory, and that is the reason why that theory has been said to travel extremely poorly, as Keating (1991) and others have pointed out. Urban regime theory is to a large extent an abstraction of U.S. urban political economy, and therefore it cannot help conceptualize or explain different aspects of urban politics in other parts of the world (see, e.g., DiGaetano and Klemanski 1993b, 57-58; DiGaetano and Lawless 1999, 547; Harding 1994, 1995).

Although this is a valid argument, it does not give a full account of the wider problematic it seeks to address. *Strictu sensu*, regime theory explains the linkages between private capital and political power and the potential
synergies that can be exploited between these spheres of urban society. Furthermore, it highlights the differences between urban government (the reliance on political structures in governing the local state) on one hand and governance (the process of coordinating and steering the urban society toward collectively defined goals) on the other hand. Thus, urban regime theory offers one a theoretical model of American urban governance and the role of government in such governance. This model is different from the Continental European, Scandinavian, or Asian models owing to differences in the size of the public domain in urban society (Keating 1991), the predominant mode of economic and social production, and the role of urban government in urban governance more generally. The point here is that if one theorizes the underpinnings of urban regime theory somewhat more than Stone does to increase its ability to offer a relevant conceptual and analytical framework for urban politics in other national or regional contexts, one needs to see regime theory through the lens of urban governance and also needs to acknowledge that U.S.-style urban regime analysis is heavily ingrained with values, norms, and practices that are typical to the American city. The article will return to these issues later.

All of that having been said, however, there is some truth to the critique levied against urban regime analysis that it is undertheorized and more ethnocentric than it purports to be. For theories to be able to travel, they must be either sufficiently abstract not to become associated with only one (type of) case, in which case they become Grand Theory and of little help in guiding empirical analysis, or sufficiently reduced to isolate the dependent variable from contextual factors, in which case the theory becomes simplistic bordering on the banal. Urban regime theory is not the only case of undertheorized theories that fail to escape the empirical milieu from which they evolved, and here lies another explanation to the idiographic nature of much work in urban political and social theory; the implicit ethnocentrism in much of urban politics theory constitutes a significant obstacle to comparative research. This problem may help explain what Alan Harding (1995, 46-47) describes as a “powerful tradition of skepticism among UK political scientists about imported US theories and methods[,] US approaches are generally dismissed as insensitive to fundamental differences between the two countries.” Jonathan Davies makes a similar point, arguing that regime theory is not a useful analytical model in Britain largely because urban regimes are not likely to emerge in the UK political, economic, and institutional context (Davies 2003).

There may well be other reasons why idiographic studies have come to dominate urban politics and the study of urban governance, but the points
raised here probably capture the key explanations. Urban politics seems to have embraced complexity and richness in context at the expense of parsimony, a research strategy that has been instrumental in fostering a deep understanding of many key aspects of urban governance but that also has to some extent obstructed a systematic comparative analysis of urban governance.

Against this backdrop, the overall argument advanced in this article is rather straightforward. Urban governance—the pursuit of collective goals through an inclusive strategy of resource mobilization—is as old as local government, yet most theories of urban politics fail to properly conceptualize or explain patterns of urban governance. On rereading some of the classical works in U.S. urban politics, to give an example, one is struck by the predominance of single-case studies—New Haven (Dahl 1961), Atlanta (Hunter 1953; Stone 1989), San Francisco (deLeon 1992), Cleveland (Swanstrom 1985), Detroit (Jones and Bachelor 1986), or Chicago and Pittsburgh (Ferman 1996), just to name a few of the many books of this character—as well as the predominance of in-case explanation, albeit embedded in the larger urban theory literature. Without the slightest denigration of this impressive body of work, it could be argued that structured and focused comparative work might have generated more analytical mileage than the large number of single-case studies have to offer (cf. George 1979). Comparison is the most common and most rewarding research strategy of controlling for contextual variables and for uncovering causal patterns of explanation. For all their brilliance, single-case studies generate at best hypotheses about such causal patterns; comparative research enables one to take the analysis one step further toward scientific explanation.

The dominant analytical framework of urban political research in the United States for the past two decades has been urban regime theory. Although this theory holds some potential in generating comparative research, very little has been done in this respect (but see John and Cole 1998; Stoker 1995; Stone 1997). It is ironic in some ways to observe that despite the close kinship between urban regime theory and theories of urban governance discussed earlier, regime theory to some extent misses the general point in urban governance: the close linkages among norms and values, institutions, political objectives, and policy outcomes. Urban governance has normative as well as institutional dimensions that are interconnected; hence, one needs to uncover what these dimensions are and how and why they differ between different urban and national contexts.

The case studies mentioned earlier all testify to the powerful collusion of concerted political and corporate action, but not very many of those studies
attempt to uncover the extent to which the norms that sustain such cooperation are institutionalized. To some extent, policy outcomes become the conventional proof of shared values between downtown players and city hall. True, outcomes could be valid indicators of shared values and objectives, but they could also be manifestations of extreme pragmatic behavior from both sets of actors. This critique against U.S. regime analyses is part of the undertheorizing argument frequently levied against urban regime theory. To learn more about urban regimes, one needs to approach those regimes from a more developed theoretical perspective—a perspective that I suggest should be the urban governance perspective—and to embark on comparative research. Such research would be a powerful instrument in uncovering the significance of social and economic structures in shaping urban governance. In the absence of such comparisons, there is very little that one can do in terms of developing a deeper and more theoretical understanding of urban regimes or urban governance.

Put slightly differently, there is much to suggest that one has come to the end of the line in terms of what urban regime theory can help uncover or explain. The next set of issues on the research agenda revolves around institutional factors such as what difference do different structural configurations of the local political system make in terms of how that system relates to its external environment, as well as around normative or value-based issues, like how do we explain the significant differences in urban policy style and urban policy objectives (see Sellers 2002)? Perhaps the most obvious cluster of research questions includes the relations between institutions and norms in the urban political setting; to what extent urban policy objectives are shaped by those at higher institutional levels and by local political, social, and economic interests; and to what extent these institutional and normative variables explain differences in the urban governance process (cf. Lewis 1996; Pierre 1999).

Urban regime theory has provided a useful base for understanding many of these issues, but, coming back to an earlier point that will be elaborated below, if one wants to be able to formulate more general propositions about these issues and relationships, one needs to approach them in a comparative research strategy. For one to be able to do that, one should conceptualize objects of study less in terms of urban regimes and more in terms of urban governance, because this framework is much less tied to a particular national context than is urban regime theory. Furthermore, urban governance as an analytical model embraces both the institutions and the actors in urban politics; to be sure, urban governance is probably better geared than most other theories to conceptualize the heterogeneity of actors on the urban political scene.
The step is not as big as it might appear. Clarence Stone, the “grand old man” of urban regime theory, argues that “regime analysis is centrally about governance, not land-use practices” (Stone 1997, 1). Thus, the focus of urban regime theory is on the fusion of political, economic, and social interests toward some jointly defined objectives and not so much on specific issues. This distinction is crucial to the development of comparative analysis. As DiGaetano and Klemanski (1993b, 58) suggest, in a perspective quite similar to Stone’s, “What is needed for comparative study of urban governance . . . is a broader definition of regime”. The point made in this article is that regimes as portrayed by urban regime theory describe one type—one value on the dependent variable—of urban governance typical to the U.S. political, institutional, and economic urban context. This approach allows one to look at how different such contexts tend to produce different types of urban governance and the consequences of those arrangements.

Furthermore, the article argues that to uncover what explains the development of different models of urban governance—and, indeed, to introduce a genuine explanatory dimension into the study of urban politics and urban governance—theoretically anchored and empirically systematic analysis are required. The good news is that urbanists often tend to do more comparative work than they tend to be aware of or at least seem to argue theoretically in ways that suggest a comparative thinking. By highlighting unique or non-path-dependent findings, the scholar conducts, in fact, comparative assessments of his or her empirical material. Similarly, observing developments throughout time is also, if properly designed, a type of comparative study. The bad news is that urbanists frequently underestimate the significance of designing their comparison in such a way that it allows for the formulation of theoretical propositions, something that prevents them from using the comparative approach to its full capacity.

The article first outlines the urban governance research field and the most salient research questions raised there. Following that, I bring the comparative dimension into the analysis by briefly looking at the basic logic of comparison and how it might be applied to urban politics. A concluding section summarizes the article.

**URBAN GOVERNANCE: WHAT IT IS AND IS NOT**

The concepts of governance in general and urban governance in particular are frequently misunderstood. To some extent, that is probably attributable to the multiple meanings that are accorded these concepts. To be able to assess
the potential for comparative urban governance research, I should first define what this refers to.

GOVERNANCE AS THEORY

One definition in the current literature is governance as a theory (see, e.g., Pierre and Peters 2000, 2005; Pierre 2000). It is fair to say that there is not as of yet a full-fledged theory of governance. Instead, governance offers an analytical framework or at least a set of criteria defining what are the “objects worthy of study” (Stoker 1998a). A governance perspective on urban politics directs the observer to look beyond the institutions of the local state and to search for processes and mechanisms through which significant and resource-full actors coordinate their actions and resources in the pursuit of collectively defined objectives. Obviously, urban governance for the most part displays a significant degree of centrality for political institutions in these processes. The governance perspective, however, allows one to move beyond the textbook assumption that these institutions are in full and exclusive control of urban governance and instead to think of the role of those institutions as a variable.

Anyone even remotely familiar with urban regime theory recognizes the kinship between that theory and urban governance as a theory. What sets the two theories apart is that urban governance makes no prejudgment about the cast of actors involved in shaping the urban political agenda, nor does it make any assumptions about the normative direction and objectives of the “governing coalition” (Stone 1989). In fact, urban regime theory describes one of several different models of urban governance (cf. Stone 1997). In the literature, models (or modes) of urban governance have been defined with regard to the functional aspects of such governance (DiGaetano and Lawless 1999) or with respect to the orientation of the dominant participants and their objectives (DiGaetano and Strom 2003; Pierre 1999). Thus, the urban governance framework is more open and parsimonious toward alternative “regimes” and alternative urban policy agendas pursued by that “regime.”

GOVERNANCE AS A NORMATIVE MODEL

Another meaning of urban governance that emerged primarily in the United Kingdom during the 1980s and 1990s was that of a desired model of public-private interaction and cooperation at the local level (Leach and Percy-Smith 2000; Stoker 1998b). Space will not allow a detailed discussion of the driving forces of these changes. Suffice it to say that central government aggressively promoted an inclusive model of urban governance partic-
ularly with regard to the role of the local business community (Harding 1998). The normative, political focus was to de-emphasize local government as the sole source of service provision and instead to advance a territorially defined strategy of resource mobilization from the public sector as well as the corporate sector, organized interests, and other significant actors at the local scene. This shift originated as part of the larger Thatcherite project and has been pursued also by the Labor government since 1997.

The normative model of governance, thus, portrays the analytical framework of urban governance as a political end in itself and as such has been a key feature of UK national urban policy for the past couple of decades (Leach and Percy-Smith 2000).

GOVERNANCE AS AN EMPIRICAL OBJECT OF STUDY

The third meaning of urban governance is, quite simply, urban governance as an empirical phenomenon. The theory, or framework, of urban governance discussed earlier identifies the process of creating and sustaining governance as the chief focus of inquiry. Different academic disciplines will obviously direct the scholar’s attention. For political scientists, the significance of political structures—the role of government in governance—is perhaps the most intriguing aspect of urban governance. For all its pedagogical appeal, however, Rhodes’s notion of “the new governance” as “governing without government” is slightly misleading, because governance without any role of governmental presence is not very likely to occur. Furthermore, Rhodes’s slogan misses the point; the role of government in governance is not a dichotomous but a continuous variable. Political institutions can play a great many different roles in governance. Clarifying what shapes that role in different institutional contexts, how it plays out in different national settings, how it relates to the configuration of the local business community, and several such issues comprise a set of key issues in comparative urban governance research.

We argued earlier that urban regimes should be conceived of as one type of governance in terms of the configuration of actors, the types of goals and objectives pursued, and so on. Understanding urban governance more broadly, that is, to investigate to what extent different social, political, and economic forces tend to produce different models of urban governance, requires a comparative approach. The absence of comparative work is arguably the main obstacle to theoretical development in this field. The article now looks at what the comparative approach has to offer as a heuristic process.
There is a disturbing element in the U.S. academic jargon, according to which an expert in, say, French politics “does comparative politics.” Comparative politics, in this understanding, is an umbrella label for political science focused on other national contexts than the American case. Apart from the confusion this tends to entail, it also obscures what comparative analysis is all about (Dogan and Pelassy 1984; Peters 1998). Equally frustrating are edited volumes presenting a series of case studies that is not held together by a common comparative framework. Although these publications certainly tend to be rich in context, they scarcely allow the editor to make any more general comparative observations.

Comparison, in its simplest form, is the process through which the observer assesses the defining features and significance of an object under study. Almost any observation, whether one makes it in academic research or real life, implies some form of comparative assessment, such as big-small or tall-short. Without comparison, one cannot tell whether an object is big or small. Case studies have their heuristic value, but it is not possible to tell whether the findings from a case study are unique or common unless one places his or her findings next to those of other case studies. The social sciences cannot rely to any greater extent on experimental research; hence, one is left with developing methods that can be applied to social actors and institutions in their natural environment. For all its simplicity and limitations, the comparative method offers more analytical mileage than is often understood, not least among urbanists.

From these rather banal observations on comparison, I should quickly move toward outlining the basic elements of a comparative analytical framework. A comparative framework, in turn, must be based in some form of theory or causal model, some stipulative statement about what causes variation in the dependent variable. These causal models translate theory into variables that, in turn, are linked to the empirical cases, or what Przeworski (1987, 39) aptly refers to as a process of “reducing proper names to explanatory variables.” Observed differences between cases A and B are explained by theoretically defined explanatory features of the two cases.

A comparative analysis can obviously be designed in almost any number of ways. With regard to the anticipated properties of the cases one wishes to study—given that cases are selected in a purposeful and not random fashion—it is customary to make a distinction between comparing cases that are similar on one hand and cases that are as different as possible on the other hand (Peters 1998; Przeworski and Teune 1970). The most similar systems design draws on identifying and selecting cases that are as similar as possible
in all independent aspects but in which there is variation in the dependent variable. This design allows the researcher to control for a larger number of contextual factors while at the same time conducting empirical study in close proximity to theory; most similar systems design has been referred to as a “parallel demonstration of theory” (Wickham-Cowley 1991, 11, quoted in Peters 1998, 38). Most similar systems design that is the preferred approach in mainstream political science should have plenty to offer for urban politics as well. By comparing similar systems with few but significant differences while controlling for other variables, one is in a good position to uncover a causal relationship between two variables; the differences in the dependent variable are caused by different values on one of the independent variables.

Both the most similar systems design and the most different systems design are to some extent idealized models of comparative inquiry. Where in the real world does one find systems that differ in all respects but two, or where does one find that systems are similar in all important respects but two? Adam Przeworski, one of the leading scholars in comparative politics, raises these very questions and concludes dismally, “It may be that the ‘most similar systems design’ is simply a bad idea” (Przeworski 1987, 39-40). The interesting point here is that urban research is a more promising field of comparative research than comparing nation-states. First of all, for the comparative urbanist, the embeddedness of cities in national institutional contexts offers good possibilities of intranational comparison among cities, an analysis that allows for full control of national policies or factors related to political culture (cf. Sellers 2002). Furthermore, by using countries as cases and cities as units of analysis, the scholar can conduct intranational as well as international comparisons. Finally, there is the simple observation that there are more cities than countries in the world and subsequently a greater universe available for case selection. All other things being equal, that should make it easier to find cases that allow the observer to use the most similar systems design or, alternatively, the most different systems design.

Comparative analysis of cities within the same national context is a strategy that conveniently allows the observer to control for a number of political and institutional variables. A comparison between, say, Santa Monica and Buffalo, however, is not very likely to yield any intriguing results because the key variable defining the main differences between the two cities is not political or institutional but economic. As Colin Hay (2002) points out, one frequently must look beyond the political to find explanations of the political, an argument that is far from novel to any urbanist. For the comparativist, this obviously means that the analytical framework must be designed in such a way that it incorporates what are believed to the main explanatory variables. Coming back to the comparison between Santa Monica and Buffalo, one can
identify any number of differences between the two cities, but without a causal theory one cannot decide which of these variables should be included in the analytical framework. Climate is significantly different between the two cities, although one should not expect it to offer much in terms of an explanation of political behavior in the two cities. To reiterate a comment made earlier, comparison must depart from a robust causal model. The quality of the answers—the research findings—is a direct reflection of the quality of the questions guiding the research. Therefore, comparing two cases of a particular phenomenon without departing from a conceptual or theoretical framework will not yield findings that enable one to say very much about the explanatory capacity of different independent variables or about causal mechanisms more broadly.

COMPARISON AS HEURISTIC DEVICE

The social sciences, as Guy Peters reminds us, are an inherently comparative process (Peters 1998). The full heuristic value is rarely exploited, however, mainly because the comparative framework is not sufficiently elaborated. Simply placing two cases next to each other will not yield very much insight into the causal mechanisms of change in the two cases. That said, comparative research does not have to become a research process devoid of context. By describing the cases in a holistic fashion, highlighting the internal logics of each of the cases while at the same time teasing out changes in the variables identified by the analytical framework, the final analysis will both set the stage for meaningful comparison as well as tell a good story. An important element of comparative research is not just to isolate causal processes but also to present the cases as a set of interrelated economic, political, and social processes embedded in an institutional system. These contextual accounts—some would say narratives—have very much to offer in terms of the overall Verstehen of the individual cases. Peter Evans (1995, 20) describes this type of process of developing scientific explanation nicely:

The overall result is a mosaic of concrete evidence melded by an argument that is abstract and general. If the combination convinces, it is not because each piece of evidence or each link in the argument is irrefutable. It is because the overall gestalt makes sense.

In comparative urban governance research, the scholar is faced with the challenge of striking the right balance between reducing complexity and uncovering the causal mechanisms on one hand, and allowing for contextual richness—what Evans calls the “overall gestalt” of the case—on the other.
Much of the convincing argument and analysis in the single-case studies in urban politics are derived from the richness in detail that helps the reader understand the logics and dynamics of that particular case. To allow for fruitful comparison, however, cases must be treated in a more structured and standardized fashion, something that almost by definition forces the scholar to leave out some of the contextual background. This is not an impossible dilemma; the mainstream comparative politics literature offers several excellent examples of comparative research that elegantly combines methodological stringency with theoretical clarity and contextual richness. Peter Hall’s (1986) seminal institutional analysis of macroeconomic policy in Britain and France is one of the best examples of successful comparative research. In the urban politics literature, recent work by Savitch and Kantor (2002) and Sellers (2002) demonstrates both that structured comparison does not prevent contextual richness and also that such comparative research generates insights and findings that would not have been possible to extract from single-case research.

A CASE OF WHAT?

Defining the case is arguably the most important step in comparative research. The answer to the seemingly banal question A case of what? defines the broader theory that could (and, normatively speaking, should) serve as the more general and abstract reference to the case study. This is also a strategy of defining the broader universe of cases to which the case under study belongs. Thus, by asking what the case under investigation is a case of, one identifies both appropriate theories as well as the range of comparable cases. This also means that cases and units of analysis need not be identical; in fact, they rarely are (Yin 1984). If a research project is concerned with local economic development as a case of urban governance or urban regime behavior, units of analysis could be defined as individual development projects or decision-making processes.

Furthermore, defining what the case is a case of also helps one avoid comparing apples and oranges. One could, on a theoretical level, compare apples and oranges under the heading of comparing different kinds of fruit. With comparative scholars, however, interest often tends to lie in comparing functional equivalents, that is, not to compare by institutional names or labels but rather by function. For example, comparing “regional governance” in the United States and European countries will effectively mean comparing different phenomena; regional governance tends to refer to the same phenomenon as metropolitan governance, that is, the coordination of cities and their surrounding suburbs. To complicate this further, Norris (2001, 532) talks of
“metropolitan governance (aka ‘regionalism’).” Regional governance and regionalism in the European context, however, refer to something rather different; here, the concept denotes patterns of regional social, political, and cultural awareness and assertiveness (see, e.g., Keating 1996; Keating and Loughlin 1997). The point I wish to make here should be obvious: to facilitate comparative analysis, nominal or conceptual similarities may mask important empirical differences, and so one needs to ascertain that what are being compared are functional equivalents among different national contexts.

COMPARATIVE URBAN GOVERNANCE

Given the strength of central-local command links in some national context, a pertinent question might be exactly what is being compared in cross-national analyses of urban governance; is one highlighting how nation-state directives play out in different urban and regional contexts, or looking at more genuinely urban phenomena? The answer to this question is probably both. Comparative research on urban governance is a good example of how one needs to balance the contextual richness of each individual case against the need for parsimony and simplicity on the other. One realizes at some level that much of the explanation to how the governance of a particular city has evolved lies in the political, social, cultural, and historical context of that city. In its extreme version, this argument suggests that all cities are more or less unique and therefore do not lend themselves to any meaningful comparison with other cities; indeed, comparison should be a pointless exercise altogether. The opposite position suggests that—although acknowledging that cities do differ in some or even several respects—cities display significant institutional similarities and a similar embeddedness in the regional and local economy, and are, on the whole, equally exposed to the thrust of globalization, and therefore, comparison is both possible and indeed critical to any scientific explanation of their patterns of behavior or trajectory of change.

The basic argument of this article is that comparison is critical to take the study of urban governance to a theoretical level and also to the development of scientific explanation in the field of urban politics. Understanding the formation and reproduction of urban governance is one of the most crucial aspects of urban politics. The huge interest in urban regime analysis in the United States throughout the past decade is proof of the centrality of these issues. The next step in this research is to raise more analytical and theoretically derived questions, such as: What is the role of institutions in urban governance? How does urban governance differ in different economic envi-
ronments? How sensitive is urban governance to changes in the institutional arrangement of the state or the region? Is policy choice a sufficient explanation to the economic development trajectory of cities, or can a stronger explanation be derived from an analysis of how those choices reflect the policy preferences of actors on higher institutional levels or the preferences of the city-regional business community?

Providing answers to these issues requires comparative analysis. Such analysis can be cross-national, but it can also be between two or several cities in the same country or even in one city studied throughout an extensive time period with changes in the independent factors such as institutional arrangements or central-government policies vis-à-vis the cities. The recent work by scholars like Savitch and Kantor (2002) and Sellers (2002) shows very clearly how much is to be gained by employing comparative framework which combines sensitivity in the analysis of the individual cases with a comparative analysis which uncovers drivers of change and causal relationships between key variables in the analysis. The basic argument advanced in this article is that one’s understanding of urban regimes or urban governance cannot go much further without systematic and structured comparative research. This applies not only to urban governance but also to the social sciences more broadly; as Guy Peters (1998, 25) points out, “To be effective in developing theory, and in being able to make statements about structures larger than an individual or the small group, the social sciences must be comparative.” Taking the study of urban politics and governance to that level is a truly exciting academic enterprise.

NOTES

1. An issue that Rhodes does not address is to what extent “governance without government” is an attractive arrangement from a democratic point of view. Governance without any electoral input or democratic accountability cannot be sustained in the longer term in any Western democratic society.

2. It should be emphasized that Stone and Mossberger and Stoker all have developed the regime concept to allow it to take into account regimes that are driven by other objectives than economic growth (see Stone 1997, 2004a, 2004b; Mossberger and Stoker 2001). I see this work as a promising way of opening up the regime concept and indirectly making it more susceptible to comparative work.

3. This conceptualization of urban governance is also helpful in resolving the issue of the significance of economic factors in urban regimes. The recent debate on this issue in Journal of Urban Affairs to some extent testifies to the problems in properly conceptualizing the role of private capital in urban regimes, or in urban governance more broadly (Davies 2004; Imbroscio 2003, 2004; Stone 2004a, 2004b). No one disputes the significance of corporate resources and the ramifications of corporate action—or inaction (Crenson 1971; Offe 1985)—on the urban
economy. What is at issue is rather how best to understand the interplay between political and economic factors and the range of policy choice in different economic settings and circumstances. Jones and Bachelor, in their seminal study on the urban political economy in Detroit—a case that should offer powerful support to those who seek to argue the primacy of economic factors—make an interesting concluding observation on these issues:

If it is silly in urban politics to talk of the free will of political leaders, it seems equally silly to talk about determinism. It is particularly hard to conceive of having underlying structures cause surface behavior in these cases. (Jones and Bachelor 1993, 248)

Thus, although urban regime theory is open to both the significance of the downtown elite as well as that of city hall, it is particularly susceptible to study various forms of synergy between these forces. The urban governance model is in some ways even more open to incorporating nonpolitical actors, because it makes no conceptual prejudgment about who is an important actor in any given context.

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Jon Pierre is professor of political science at the University of Gothenburg, Sweden, and adjunct professor of political science at the University of Pittsburgh. In 1996-1998, he held a chair in politics at the University of Strathclyde. He has held visiting professor appointments in Canberra, Hong Kong, Montevideo, and Nuffield College, Oxford. His research interests include urban governance, public administration, and public policy. His latest publications include Handbook of Public Administration (Sage, 2003), coedited with B. Guy Peters; Challenges to Policy Capacity (Palgrave, 2004), coedited with Martin Painter; and Politicization of the Civil Service in Comparative Perspective: The Quest for Control (Routledge, 2004), coedited with B. Guy Peters.