And Now for Something Completely Different...

Public Choice and Redistribution in the Metropolis

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Abstract

The public choice approach to metropolitan governance is examined from the perspective of its critics under two broad themes. First, while public choice theory has in large measure won the paradigmatic revolution over how we should study metropolitan politics, it has been imperfectly engaged in the ensuing normal science controversies engendered by it to the detriment of progressive problem shifts both in public choice theory and in metropolitan studies more generally. Second, how the public choice approach responds to opportunities for further engagement depends in large part on how it wishes to characterize itself: as an approach to scientific inquiry, as a collection of policy and institutional propositions, or as a normative stance toward government. These themes are explored by first discussing the public choice approach to metropolitan governance as a paradigmatic revolution, then assessing the approach as a policy revolution, and last, evaluating the approach's stance toward the possibilities for and appropriateness of redistribution within the metropolis.
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Public Choice and Redistribution in the Metropolis

I feel enabled, camouflaged, and constrained by this session's title: "Metropolitan Governance From Different Perspectives." The title is delightfully enabling in that it implies that public choice students of metropolitan government really wish to know what sorts of work nonpublic choice scholars are conducting in this domain. The answer is a quite a bit, and we have a good deal to say to each other. However, I also feel camouflaged in the sense that, although it may be hard for some of you to credit, I consider myself a public choice scholar. The first book I was assigned in graduate school was Vincent Ostrom's *The Intellectual Crisis in American Public Administration*. It was identified as one of most important books ever written on the topic, a claim I took to heart and still believe to be true. I continued to study public choice theory, albeit an heretical kind as provided by the Michigan State Agricultural Economics Department, which was thoroughly embedded in the public choice tradition of John R. Commons. My dissertation was even directed by an Ostrom, albeit one named Chuck rather than Elinor or Vincent. And last, the panel's title is somewhat constraining in that, while controversies over metropolitan politics will be the subject of scientific exchanges for some time to come, it is no longer useful to view these as a paradigmatic conflict. For reasons noted below, it is high time to view these exchanges as simply energetic normal science. In any case, I will try to follow the charge laid out by the panel's title -- presenting a "different" assessment of theory and research about metropolitan government than one commonly hears under the rubric of public choice, but doing so in the hope that this will lead to more productive exchanges between scholars on both sides of issue.
While imperfectly matching the organizational structure of the paper, two broad themes run throughout, themes which I hope public choice students of metropolitan government will regard as constructive in their intent and implications. First, while public choice theory has in large measure won the paradigmatic battle over how we should study metropolitan politics, it has been imperfectly engaged in the ensuing controversies of normal science engendered by that victory. Simply put, the public choice research program often seems to act as though it was still struggling against scholars working within the reform tradition of Robert C. Wood (1961) and the Committee on Economic Development (1966). While the reformers analysis of metropolitan politics was more complex than is sometimes thought, they clearly lost the paradigmatic battle with public choice theory. The arguments offered today in support of government consolidation are less likely to focus on assumed service efficiencies than on levels of political participation, redistributive policies, and economic development. More importantly, this newer research examines such issues with close attention to the role of institutions in structuring behavior and, thus, outcomes, and does so using models fully predicated on methodological individualism and subject to empirical scrutiny. Public choice theory has not fully engaged this newer literature to the degree that it might, to the loss, I would suggest, of both sides of the argument over the costs and benefits of metropolitan political fragmentation.

The second theme concerns how public choice wishes to characterize itself. From the beginning, the public choice approach has been notable as (1.) a normative approach to collective choice offering, (2.) a set or collection of policy and institutional reform proposals about metropolitan government using (3.), a distinctive approach to constructing and testing theories about urban political behavior. But which of these three characterizations is most important to public choice theory's understanding of itself? The answer to this question -- the ordering of the
three aspects of public choice theory -- is very important to critics of the public choice approach, important in the sense that it will either enhance or degrade the likelihood that the further engagement urged above really will develop in the mode of a lively and productive normal science exchange. If public choice theory is first and foremost an approach to conducting science on propositions about metropolitan governance, which could then separately be evaluated from a particular normative stance, then I believe that the prospects for fuller engagement as a series of normal science exchanges are good. But if the order is reversed, what few exchanges occur will likely continue to be occasional bombs lobbed across paradigm walls.

These themes are explored in three sections. First, the public choice approach to metropolitan governance as a paradigmatic revolution is examined. I suggest that there indeed has been a dramatic and very constructive change in how urban politics is studied as a direct result of public choice research and theory. The second section of the paper then assesses the public choice approach as a policy revolution. While the policy claims associated with the public choice approach to metropolitan governance have received strong support, I suggest that this support should and must be viewed as more contingent, always dependent upon the outcomes of continuing normal science research. The third and longest section of the paper examines in detail what I view as the most contingent of these claims -- those concerning the role of redistribution within the metropolis. The paper concludes by revisiting the two major themes noted above.

**The Public Choice Approach as a Paradigmatic Revolution**

The long-running debate between what Elinor Ostrom (1972) labeled the "two reform traditions" on urban services administration has not fully run its course. As we will see, plenty of empirical puzzles remain, and there remain opportunities for further theoretical development of
the public choice approach to understanding metropolitan governance. Still, the character of the
debate has changed markedly. Quite simply, what began as a debate between two distinct
approaches to *doing science* has at least in part devolved from a paradigmatic dispute to a series
or at least a potential series of normal science controversies. If Vincent Ostrom's claim in *The
Intellectual Crisis in American Public Administration* of paradigmatic status for public choice
theory seemed premature in 1974, it is merited today. There remain no developed, fully coherent
alternative paradigms for approaching the study of metropolitan governance. There remain, of
course, plenty of critics of the public choice model (Rusk, 1995, Downs, 1994, Lyons, Lowery,
and DeHoog, 1992). But these critical analyses, however their policy conclusions may differ from
those of mainstream public choice analyses, are couched neither in an alternative method of
analysis nor in a fundamentally divergent view of social science.

In part, the rapid success of public choice theory inhered in the problem it addressed.
Despite the transformation of the urban landscape by three decades of postwar suburbanization,
we lacked a political theory of *metropolitan* politics. Indeed, urban politics within political
science remained profoundly and resolutely anti-suburban both in outlook and, more importantly
for scientific purposes, in attention. Thus, public choice theory filled an enormous theoretical gap
in the study of subnational governance. But despite such fortuitous timing, public choice theory
was ultimately successful for more fundamental reasons. The transformation of a paradigmatic
battle into a series of normal science conflicts was, in my view, a shortened or more compressed
version of the scientific revolution which transformed political science over the course of several
decades. The success of the public choice approach to metropolitan governance lies both in its
lagging part of this more general transformation and then leading its most recent elements.
The lagging component lies in the public choice model's emphasis on methodological individualism (Dowding and Hindmoor 1997, 452-453). While largely couched in the language of economics, such an epistemological stance is fully compatible with the larger behavioral revolution which occurred over the course of the 1950s and 1960s across many subfields of political science. Despite some differences in language and emphases, both eschew crude ecological inference in favor of close attention to the individual motives underlying political behavior. But despite the early and devastating criticisms of Simon (1945), the behavioral revolution had no notable constructive impact on public administration, and exerted almost no influence -- constructive or destructive -- on the study of urban governance (Knott and Miller 1987). Both fields remained firmly locked in a pre-behavioral revolution mode of analysis.

The leading element, of course, was public choice theory's strong emphasis on the role of institutions. The "new institutionalism" which influenced political science so strongly following the mid-1980s (March and Olson 1989) was a core constituent of the public choice approach to the study of metropolitan governance from its inception. More than motivations matter in understanding political behavior. The incentives and opportunities associated with institutions are just as important for understanding behavior, and understanding their role is critical if we are to influence behavior. Indeed, institutions, broadly construed, remain our most readily available instruments for influencing public policy outcomes. While the rest of discipline took a bit longer to reach this conclusion, the public choice approach to metropolitan governance was founded on the centrality of institutions to understanding political behavior.

Given these leading and lagging theoretical elements of public choice theory, its rapid success after 1970 is readily understandable. As an undergraduate and, especially, as a young graduate student, I was fully taken with the public choice approach; it seemed not only to bring
the study or urban government into the modern era, but placed it at the forefront of subdisciplines within Political Science in terms of both theoretical elegance and explanatory power. For these reasons, the early theoretical criticisms of public choice theory (Golembiewski 1977; Baker 1976; Furniss 1978; and Wade 1979) had little influence. These early criticisms largely focused on what were viewed as alien modes of analysis imported from economics. However, the early criticisms were even more fundamentally rejections of methodological individualism, which had already swept political science in the behavioral revolution. And they provided nothing in the way of an alternative theory of institutions by which to understand metropolitan governance.

The result of this transformation constitutes a \textit{paradigmatic revolution} in the fundamental sense of how the scientific study of metropolitan governance is conducted. While any number of empirical analyses purporting to challenge public choice outcome hypotheses have appeared over the last two decades, (Lowery 1982; Marlowe 1985; Dolan 1990; Frisken 1991; Bunch and Strauss 1992), one is hard pressed to find contemporary analyses of metropolitan politics which are not grounded on methodological individualism or inattentive to the central roles of political institutions and the nature of the good or service being provided in shaping choices and outcomes, the three elements Vincent Ostrom (1997, 102) has identified as essential attributes of public choice analysis. As Richard Nixon might have asserted, we are all public choice analysts now (Lowery 1999a). The lesson, of course, is that you should always be careful when starting a paradigmatic revolution. You never know who you might end up working with.

\textbf{The Public Choice Approach as a Policy Revolution}

This paradigmatic revolution has been accompanied by a \textit{policy revolution} as well. This was perhaps most clearly signaled by the contrast between the 1966 and the 1987 Advisory Commission on Intergovernmental reports, reports which offered such profoundly divergent
perspectives on the design of urban governance institutions. More generally, the institutional and policy recommendations so closely associated with the public choice approach obviously have come to dominate popular accounts of urban politics and administration (Eggers and O'Leary 1995; Osborne and Gaebler 1992). And such recommendations now dominate policy agendas both within the United States (Jennings 1991) and around the world (Haque 1996). More specifically in terms of urban governance structures, the public choice approach provided the underlying rationale for recent efforts by the legislatures of Tennessee and Arizona to provide opportunities conducive to enhancing the political fragmentation of metropolitan areas within their states. The direct influence of the public choice approach to urban public policy was also evident in the abolition of metropolitan counties in Britain (Dowding 1996, 58). The policy recommendations flowing from mainstream public choice analyses no longer constitute an insurgent influence in the field of urban public policy. They are the status quo.

Importantly, however, I believe that the policy revolution is or at least should be far more contingent than the paradigmatic revolution, contingent in the sense that the recommendations associated with the policy revolution may be subject to substantial revision due to empirical and theoretical developments flowing from normal science activities within and fully consistent with the analytic methods of the now dominant public choice paradigm. From a scientific perspective, this is as it should be. But such contingency raises both risks and opportunities for the public choice approach. The risk is that the public choice approach will eschew the uncertainties normal science research potentially poses for the policy recommendations with which it has become so closely associated. In short, is the public choice approach simply advocacy of a fixed set of policy recommendations or a scientific paradigm?
I do not believe that it can be both. While a specific set of policy recommendations may be dominant at any given time within the normal science development of a research paradigm, the set must always be viewed as contingent upon further empirical and theoretical developments if the paradigm is to merit the status of science. In the view of many students of urban politics, many of whose own research fully reflects the importance of institutions and whose analyses are founded on methodological individualism, the public choice approach is too strongly committed to its associated policy recommendations and too little engaged in the extensive normal science activity it has engendered. Having successfully replaced the older traditional reform paradigm as the standard approach for scientifically studying problems of metropolitan governance, the public choice approach has seemed to many to too often act as a static and hegemonic paradigm in the sense of not fully engaging critics who, while sometimes offering policy conclusions similar to those of the displaced traditional reformers, analyze urban political behavior using the concepts, methods, and models developed by public choice theorists.

This is a serious problem because fuller engagement in the normal science activities engendered by public choice theory as a research paradigm offers substantial opportunities as well. The most obvious of which is that such research may more fully substantiate the policy recommendations of public choice theory. But I also believe that this is the least important opportunity associated with stronger engagement with normal science activities arising from the public choice paradigm. That is, the real world all too often proves to be far more interesting than our theories about it. And engagements with the real world usually generate progressive problem shifts for scientific research programs. In my view, the issue posing the most substantial risks and opportunities for the public choice approach are associated with concerns about redistribution within metropolitan areas, the topic I consider in the second half of this paper. However, four
other issues offer notable opportunities for solid scientific research -- standard normal science research -- fully within the public choice research paradigm.

The first of these has so far been a very successful area of normal scientific inquiry, successful in the sense that an empirical challenge generated both theoretical developments in public choice theory and a substantial new research agenda. The issue is the process underlying citizen choice among fragmented institutions. The original Tiebout model (1956) and its variants within political science (Ostrom, Tiebout, and Warren, 1971; Bish and Ostrom 1973) emphasized, under the assumptions of full information and limited transactions costs, citizens voting with their feet to secure desired packages of taxes and services within metropolitan regions. Despite some modest positive support for this mechanism (Dowding and John 1996; Percy, Hawkins, and Maier 1995), its plausibility was severely challenged by findings that citizens have very limited information about their choices of tax and services packages within metropolitan areas (Lyons and Lowery 1989; Lowery, Lyons, and DeHoog 1990). The very positive result of this challenge was further theoretical developments by Teske and his colleagues (1993; Schneider and Teske, 1993, and Schneider, Teske, and Mintrom 1995) and by Stein and Bickers (1995) emphasizing quite different causal processes associated with marginal consumers, civic entrepreneurs, and decision heuristics in understanding citizen location choices. The answers are not yet fully in on the plausibility of these alternatives. Still, they represent significant theoretical developments based upon a normal science empirical challenge, developments which have themselves generated a substantial and still growing body of further empirical work. This is how science should work.

A second and -- at least at present -- less successfully realized opportunity for productive research bearing on the public choice approach to metropolitan governance arises from its characterization of bureaucratic motivations, which lies at the core of its hypotheses about the
consequences of large, consolidated governmental arrangements. From its inception (Ostrom 1974), public choice theory has relied on a very stylized, bare-bones model of bureaucratic motivation centered on the work on Niskanen (1971) to summarily dismiss bureaucratic production. Such rejection occurs even when the nature of a public good in question switches from a "good" to a "bad," such as taxation (Bowman and Mikesell 1978); such a switch implies nearly the opposite conclusion about the prospects of service outcomes (Lowery 1982). Such stiff commitment to the bare bones model seems inconsistent, given that Vincent Ostrom (1997, 102) has argued that attention to the nature of the good in question is critical to generating hypotheses about service outcomes. But despite occasional nods to the very extensive body of work still being developed on the interests, opportunities, and behaviors of public employees, near exclusive reliance on the bare bones model of bureaucratic motivations and behaviors continues to be characteristic of most urban public choice research (e.g., Schneider 1991, 32-35).

More to the point, recent research on bureaucratic motivations raises any number of questions about the plausibility of the bare bones model typically employed by pubic choice analysts. Some of this research is essentially anthropological in nature (DiIuilio 1994), while other examples are couched in economics (March 1992) or behavioral social science (Perry 1996; Crewson 1997). Perhaps the most important of these works is Brehm and Gates' (1997, 2) Working, Shirking, and Sabotage: Bureaucratic Response to a Democratic Public, which supports the public choice conclusion that "bureaucratic accountability depends most of all on the preferences of individual bureaucrats." But based on their extensive secondary analyses of data on bureaucratic behavior, they (1997, 2) also conclude that, "Fortunately for us, those preferences are overwhelmingly consistent with the jobs American democracy sets them to do." Perhaps what is most notable about this work is that much of the secondary analysis supporting this conclusion
was performed on data generated by Ostrom, Park, and Whitaker's (1978) police study, although Brehm and Gates misspell poor Gordon Whitaker's name throughout. My point, however, is not that Niskanen was wrong and Brehm and Gates were correct in their assessment of bureaucratic motivations. Rather, it is that such findings deserve a considered response from public choice scholars. At present, public choice scholars simply have not been engaged as they might be in what on its face seems a straightforward normal science question with significant implications for the policy recommendations offered by the public choice approach. A considered response might be limited to further empirical work on bureaucratic motivations defending Niskanen's stylized model, including further reanalysis of the Ostrom, Parks, and Whitaker data. But, following the example of Stein and Bickers and Teske and his colleagues, there might be considerably more theoretical payoff from assessing the implications of a richer theory of bureaucratic motivations for public choice analyses of bureaucratic outcomes.

If the first two issues point to, respectively, successfully realized and unrealized but ripe opportunities for normal science research with the potential to yield progressive problem shifts in urban studies, the last two have not yet appeared on our active research agenda. But they should. Still, they probably will not be on our collective research agenda until there is a challenge, theoretically or empirically, of our standard approach to addressing issues of metropolitan governance. So, I'll be happy to issue that challenge today. Indeed, I've already done so with respect to the third opportunity for productive normal science research.

This opportunity lies in attention to the conditions under which quasimarket institutional arrangements are successful. As noted elsewhere (Lowery 1998), the public choice approach has been quite successful in convincing us that market failure does not imply nonmarket success. By the same token, nonmarket failure does not imply quasimarket success. Quasimarkets, whether
in the form of Tiebout-like governance arrangements, vouchers, or contracting, will only be successful under certain conditions. Among these are a sufficient number of providers offering alternatives in the quality and quantity of goods and services they provide. Yet all too often, public choice analyses stop at saying one local government is bad, many governments are good. But how many? Metropolitan fragmentation is surely not a categorical variable; outcomes surely vary by level of fragmentation. But are they a linear function of the number of governments within an SMSA or of the number of contiguous neighboring localities (Schneider 1989, 62)? We still know very little about the consequences of variations in institutional fragmentation. This is surprising in light of the comparable debate within market economics about the number of firms required for a market to be termed competitive. Even the recent short-circuiting of this long debate within market economics associated with the notion of contestable markets should not excuse our lack of attention to the issue, since the notion of contestibility is something of a double-edged sword for public choice policy recommendations; if we take the notion of contestable markets too far, it could be argued that what we need is not fragmentation per se, but merely nonrestrictive incorporation laws, to achieve its purported advantages. So there is no simple way to avoid this issue. Rather, I believe that the issue should be of special concern when considering Tiebout quasimarkets given that they are not simple markets. Competition takes place over market baskets of goods and services, not over goods or services per se.

The fourth area of potentially fruitful normal science research and, thus, development of the implications of the public choice approach to metropolitan governance lies in more complete attention to its polycentric character. The Indiana University approach to understanding Tiebout quasimarkets has emphasized the role of competition within metropolitan areas. In contrast, Peterson's City Limits model focuses on the policy and budgetary constraints generated by
competition between cities within a country, with the level of inter-urban competition extended still further to the international level by the "global cities" approach of Sassen (1991). While a few scholars, most notably Schneider (1989), have fruitfully exploited hypotheses developed at one level at another level of analysis, we simply do not know how these various levels of competition interact with each other. We should for the simple reason that such interactions may influence our expectations regarding outcomes of any one type or level of competition. If, for example, Peterson (1981) is correct in arguing that economic development policies have at least some collective good character for a metropolitan area as a whole, then one could construct a face valid story suggesting that fragmentation may actually encourage redistributive spending by local governments, as each tries to free ride on others' emphasis on economic development spending. This would be a tragedy of the commons story with metropolitan consolidation required to deal with the collective action problem. If you don't like this story, try another. Perhaps economic development really is more like a private good with little jointness of consumption for individual cities within a fragmented government metropolitan setting. If true, then we may not need actual fragmentation to limit excessive redistribution since Peterson's inter-urban competition would bear equally on spending decisions whether the local political system was consolidated or fragmented. Or perhaps both levels of competition work in a consistent and reinforcing manner. I don't know which, if any, of these stories is true. And I don't believe that anyone else does either. The point is that we should, within the context of normal science, take polycentricity more seriously than as simply a nice-sounding label.

Finally, before considering the most serious risk and opportunity for the public choice approach to metropolitan government, I should define what I mean by normal science research. I certainly do not mean peaceful exchanges among fully like-minded scholars. Controversies, even
quite nasty ones, are characteristic of normal science (Hull 1988). Indeed, contesting over competing hypotheses within a common paradigm is the natural state of progressive research programs (Lakatos, 1970; Hull 1988). For this reason, and because of the still substantial underdevelopment of the public choice research program, I expect that the long-standing debate between the "two reform traditions" will remain with us for some time to come. But given a now shared emphasis on methodological individualism and the centrality of institutions, the exchanges associated with that controversy take place within rather than between paradigms. The various contestants within these controversies now largely share both a common language and a common analytic framework even as they disagree about more prosaic, albeit important, methodology and measurement issues. What is the most valid and reliable measure of levels of metropolitan fragmentation? Given problems of response bias, do reports from surveys or objective indicators provide more valid measures of motivations for moving? Given that none of us can secure sufficient NSF funding to implement our preferred research designs, how do within metropolitan area research designs compare to between metropolitan area designs in terms of internal validity? These are the issues about which we should argue, but only on rare occasions do we do so. We have and are engaging in such debate over the mechanisms underlying Tiebout sorting. But this, unfortunately, is more the exception than the rule. All too often, we behave as though we were engaged in a paradigmatic dispute rather than a policy dispute, pursuing separate research agendas with little direct contact between them (Lakatos, 1970).

**Polycentric Governance and Redistribution**

In the view of many critics of the public choice rationale for fragmented metropolitan political arrangements, the central issues remain racial segregation and income redistribution. This is not a new argument. The social stratification-government inequality (SSGI) thesis of Hill
(1974) and Neiman (1976) appeared shortly after several landmark papers defining the public choice perspective (Ostrom and Ostrom 1971; E. Ostrom 1972, V. Ostrom 1974) and argued that, "Political incorporation by class and status is an important institutional mechanism for creating and perpetuating inequalities among residents in metropolitan communities" (Hill 1974, 1557; also see Miller 1981). Fragmentation both enhances inequalities via more effective sorting by race and class and limits local government's ability to respond to it by separating fiscal need and capacity. Since its introduction, the SSGI thesis has undergone substantial revision, including greater reliance on individual sorting as a causal mechanism, rather than restrictive zoning and land use regulations, which have rendered it in many respects a mirror image of the Tiebout model (Lowery 1999b). But these changes are less important to the topic at hand than the public choice response to the issues raised by the SSGI thesis, whether in its early or revised form. It is not surprising that the SSGI thesis generated a vigorous response -- Elinor Ostrom's 1983 Urban Affairs Quarterly paper, an analysis which I consider closely throughout much of the remainder of this paper. But to many supporters of public choice institutional arrangements, this response was sufficient. Case made, let's turn our attention to other matters.

This "stand pat" response is not appropriate for two reasons. First, the defense of the public choice position on redistribution in the UAQ paper and in other publications was inconsistent in a number of ways. There were at least five distinct public choice responses to concerns raised by the SSGI thesis, not all of which were consistent internally, with each other, or with the larger perspective of governance provided by public choice theory. Second, and more importantly, even if the individual arguments were sound in 1983, significant empirical research and new theoretical developments on a number of topics bearing on those defenses of metropolitan fragmentation has occurred in the intervening years. Much of this new research and
theory raises serious questions about the implications of fragmented governmental arrangements. Accordingly they merit a new response. More importantly, the exchange that response will initiate has the potential to stimulate new theory and further empirical research. So, let us consider this new research in relation to the several defenses offered by public choice theorists of the redistributive consequences of governmentally fragmented metropolitan settings.

**Argument 1: Fragmentation Does Not Uniquely Enhance Inequalities.**

The first part of this defense is a direct challenge of any association between fragmentation and income inequality across jurisdictions. That is, relying heavily, if somewhat selectively, on Schneider and Logan (1981), Elinor Ostrom (1983, 96-98) used part of her 1983 rebuttal to deny that the enhanced sorting expected from fragmented institutional arrangements generates income inequality meriting redistribution. Rather, she argues that sorting by income across suburban jurisdictions is incomplete in all but the most affluent jurisdictions.

While perhaps plausible at that time, more recent research undermines this argument. First, analysis of more recent census data suggests that income sorting by jurisdiction has increased. Thus, Massey and Eggers (1993, 313) conclude that, "During the 1980s, affluent and poor Americans not only pulled apart from one another in social and economic terms, they separated in spatial terms as well." And this is not a new trend; it was fully evident in earlier decades as reported by other research conducted by Logan and Schneider (1981), but not reported by Ostrom. And second, Ostrom failed to discuss race in relation to income. Since segregation across jurisdictional boundaries by race trumps segregation by income (Stein 1987), even the income mixing claimed for middle class suburbs generally is not available to blacks. Indeed, Weiher's (1991, 106) painstaking analyses of Los Angeles and Cook County, Illinois census data from 1960, 1970, and 1980 suggest that jurisdictional sorting by race has increased
over time. In short, this argument has been weakened by more recent research findings, findings which have not been responded to by proponents of governmental fragmentation.

Equally important, even if sorting associated with fragmentation does not enhance income inequalities across jurisdictions, Schneider's research (1989) strongly implies that the redistributive capacity of local governments within fragmented settings is severely restricted by the competition engendered by the Tiebout quasimarket. This is all well and good if one is willing to either conclude that government should not be in the business of redistribution in the first place or conclude like Schneider (1989, 65) that the drop in redistributive spending associated with fragmentation is solely excessive redistributive expenditures in the form of bureaucratic rents.

The first of these positions is a quite different argument which I consider more fully below. For now, however, the key point to note is that it would not be necessary to argue, as Ostrom does in the 1983 response, that fragmentation is not associated with income segregation if one really believes that income segregation is not an issue meriting public attention. The second position -- that reductions in redistributive spending associated with fragmentation are solely accounted for by bureaucratic rents -- relies completely on Schneider's use of the bare bones model of bureaucracy typical of public choice urban analyses. No direct evidence was supplied by Schneider (1989). At this point, all we can really know based on his findings is that redistributive spending is lower in fragmented governmental arrangements. His findings could also be explained by lower levels of income inequality within fragmented jurisdictions or reductions in spending going well beyond "rents." But even if fragmentation does not enhance income inequalities across jurisdictions, it certainly weakens local government's capacity to respond to it. So, in the absence of a recent public choice response to these arguments, I believe that the current research provides
at least a first face case that fragmentation is associated with both enhanced income inequality across jurisdictions and reduced government capacity to respond to inequalities.

This public choice defense shifts on race, however. Bowing to the obvious reality of racial segregation, Ostrom (1983, 94) does not deny its existence and appropriately regrets it as a "tragic fact." But given its persistence in both types of government settings, Ostrom (1983, 93) then argued that moving from a fragmented to a consolidated government structure would have no net effect on segregative residency patterns. Before considering this argument on its merits, it should first be noted that it addresses only one side of the SSGI thesis, that part dealing with racial and income segregation. Even if intra-city segregation patterns persist within consolidated governments, access by the poor and minorities to resources with which to address critical social problems provided by a stronger tax base would improve relative to the alternative.

And recent research on the other side of the argument undermines even the first portion of the rebuttal. Whereas overall metropolitan segregation by race has not changed much since first studied in the 1960s, even as numbers of suburban blacks have increased, there has been an important change in the distribution of the variance in racial segregation explained by neighborhood and jurisdictional boundaries. In an analysis of this distribution in 1960, 1970, and 1980 in two metropolitan areas, Weiher (1991, 87-115) found that the proportion of variance in racial segregation explained by municipal boundaries increased markedly, from 15.7 to 58.1 percent in Cook County and from 34.4 to 69.1 percent in Los Angeles County. Thus, previously high levels of intrajurisdictional segregation are being replaced by interjurisdictional segregation. Weiher (1991, 93-95) credits this pattern to the judiciary which has vigorously discouraged the former in recent decades, but too often turns a blind eye to the latter. It is also consistent with his notion that, once the courts began restricting intracity discriminatory practices, racial sorting
became easier in the suburbs than in central cities given the reduced ambiguity in neighborhood borders provided in the former by their hard jurisdictional boundaries. These findings are more broadly supported by those of Downs (1994) and Rusk (1995), based on large numbers of metropolitan areas, which indicate that racial segregation is now more severe in governmentally fragmented metropolitan areas than in cities with "elastic borders." In short, while this public choice defense may have had some merit it 1983, it now appears that persistent patterns of racial segregation are becoming ever more uniquely a consequence of governmental fragmentation.

**Argument 2: Redistribution May Be Needed, But It's Someone Else's Job.**

Perhaps the response most often presented in defense of fragmentation is the suggestion that higher levels of government could act to ameliorate any of its unintended distributive consequences. National and regional governments could provide redistributive spending directly to citizens or to communities (E. Ostrom 1983, 101; Hill, Wolman, and Ford 1995, 165-68; Dowding, John, and Biggs 1994, 773; Parks and Oakerson 1989, 22-23; Dowding 1996, 54), or these benefits could be provided through voluntary agreements among the governments comprising a Tiebout quasimarket. An often cited example of the latter is the tax-base sharing plan of Minneapolis-St. Paul, Minnesota, in which the many governments in the metropolitan area agreed to share part of the tax revenue arising from growth at the periphery with the city governments in the center (ACIR 1987; Blair, Staley, and Zhang 1996).

One problem critics of fragmentation find with this argument is its circular nature. It offers a defense of governmental fragmentation by first assuming its existence. It is also possible to argue -- and do so from a public choice position using Oate's *Decentralization Theorem* -- that government goods and services should be provided at the lowest level of government at which there are no spillovers. The lowest level at which redistribution might occur without spillovers is
the metropolitan area, not the state or federal government, which implies that metropolitan governments would, all other things being equal, be the appropriate unit of government by which to provide redistributive goods and services. Selecting the appropriate level for providing redistributive services then depends on a prior assessment about the relative importance of different types of goods and services and, thus, the areal boundaries around which to construct primary units of government. If, as is typical of public choice analyses (Savas 1987, Ostrom and Ostrom 1971), primacy is awarded to basic services such as police and fire protection or sanitation services in determining the areal basis of the primary unit of government, fragmentation would be preferred and redistributive services would have to opt for a second best solution via provision at the state or national level. If redistributive services are seen as sufficiently vital to organize the primary unit of government around their provision, metropolitan government would be preferred, with other services provided through second best options. I do not want to argue here which of these options is to be preferred. Rather, I simply wish to suggest that for this public choice response to the SSGI thesis to be more than a circular argument, a much fuller explication and defense of a series of additional supporting arguments is required.

Turning to more substantive concerns, it is difficult to deny the attractiveness of the argument that higher levels of government could and should provide redistributive goods and services. But as a realistic prescription of a practical solution to the problems raised by the SSGI thesis, it leaves much to be desired. Consider first redistribution via voluntary agreements among governments within a metropolitan area. Public choice proponents are as fond of citing Minnesota's tax-base sharing plan as they are -- to the continuing chagrin of advocates of metropolitan government -- of pointing out the paucity of recent metropolitan consolidations. But it should be equally fair to point out that in addition to evidence that Minnesota's plan never
was quite all that it was purported to be (Gilbert 1979), there is the nagging problem that so few other metropolitan areas have followed its example in the more than quarter century since it was passed. If metropolitan consolidations have not swept the nation, neither have meaningful tax-base sharing plans. This is hardly surprising given that coercive solutions mandating significant redistribution would encounter a unit veto, where any one city government can veto redistribution among the separate governments found within a metropolitan area (Keating 1995, 126). And more voluntary contributions by some local governments to others would falter due to problems of collective action, especially as the number of local governments becomes large. As Downs (1994, 171) observed, when "policies require allocating benefits and costs among jurisdictions, sacrifices on the part of one locality or another, or other controversial decisions, this approach does not work." Or they do not work as currently designed. If public choice proponents of fragmentation wish to make this solution more viable, they have a burden of using the institutional creativity for which they are so well known to make them both stronger vehicles for real redistribution and more marketable. After 25 years, simply citing Minnesota's plan no longer offers a plausible escape from the SSGI thesis.

Redistribution could also be provided via the states and the national government. As Ostrom (1983, 101) correctly noted, the plausibility of this argument "is obviously dependent on the continuance of redistributive policies by overlapping governments." But in the years since Ostrom's UAQ paper, the effects of state tax revolts and fiscal crises, the Reagan cutbacks in social spending and elimination of revenue sharing, and the Clinton era "reform" of welfare have taken us in quite a different direction, a pattern replicated around the world in the last decade (Haque 1996). Redistribution has occurred, but in a direction opposite to the manner suggested by this public choice defense of fragmentation. At best, federal government expenditures have
moved from being spatially redistributive to a spatially neutral allocation pattern (Lowery, Brunn, and Webster 1986). Given recent history, then, it seems utopian to count on higher levels of governments to fill the redistributive gaps left or engendered by governmental fragmentation.

Or worse. I must be frank -- perhaps excessively so, for which I ask your forbearance in the interest of initiating a more productive dialogue -- in noting that to many critics of the public choice approach, the suggestion that reliance be placed on higher levels of government or on COGs seems insincere. This argument is a difficult one to make here. But such an assessment has face plausibility given the deafening silence on the part of public choice proponents of fragmented government accompanying all of the policy changes just noted. I certainly would not expect public choice scholars to oppose all or even most of the many changes in public policy at the state and national levels that have occurred over the last 20 years -- changes which have so substantially reduced the redistributive role of the public sector -- on the grounds that they undermined the viability of fragmented metropolitan institutional arrangements. These policies are an extraordinarily heterogeneous mix, including elimination of revenue sharing, tax limitations, budget cutbacks, and welfare reform, each of which has its own unique merits and liabilities. But of all of these policy changes, are not there any that should have been opposed by prominent scholars in the public choice tradition if they really believed that redistribution by higher levels of government could and should be used to ameliorate some of the unintended consequences of fragmentation? If such opposition existed, I do not know of it. The lack of such opposition is not, however, all that surprising when viewed from the perspective of the public choice model as a whole. Given the bare bones model of bureaucracy favored by public choice analysts, it would be inconsistent to suggest that centralized bureaucracies at the state and national levels would be accountable, responsive, and efficient providers of redistributive goods and services
Argument 3: Redistribution is Required, and We Have the Solution.

This argument suggests that the poor and minorities may especially benefit from the advantages of Tiebout quasimarkets. This argument has at least two parts. One is that the efficiency advantages of such communities will provide the poor even greater access to services than that provided by metropolitan government (Ostrom 1983, 103). Ostrom does not consider, however, how substantial such gains must be to compensate for the disadvantages arising from racial and income sorting in Tiebout settings. Two reasons suggest that such gains need to be quite substantial. First, black suburbanization is largely restricted to the poorest, most densely settled suburbs on the fringe of central cities (Logan and Schneider 1984; Downs 1994; Rusk 1995; Schneider and Logan 1981; 1982; Phelan and Schneider 1996; Schneider and Phelan 1993). And second, research assessing Peterson's (1981) City Limits version of the Tiebout model indicates that competition in governmentally fragmented metropolitan areas is associated with less spending, especially on redistribution (Schneider 1989). Thus, even ignoring research suggesting that satisfaction with local government is positively associated with the number and quality of provided services (DeHoog, Lowery, and Lyons 1990), any efficiency gains resulting from fragmentation must be so great that they can compensate for both the initial poorer fiscal base of black and poor suburbs and the pressures to reduce spending associated with competition.

Against these rather substantial hurdles, there is the remaining and rather large disagreement about whether fragmentation actually produces efficiency savings. I realize that this is a very difficult argument to sell to this audience. But like the literature on sorting, the literature on efficiency savings hypothesized to arise from competition within Tiebout settings is large and remains extremely controversial (Keating 1995). Based on a thorough and rather disinterested review of this literature by scholars who have argued on both sides of the public choice divide,
Dowding, John, and Biggs (1994, 787) conclude that, "It is very difficult to test the proposition that the greater the number of jurisdictions, the greater the competition between them. There is virtually no worthwhile evidence to support this [efficiency] implication." At best, they note, research does support Schneider's (1989) conclusion that greater fragmentation is associated with reduced spending. But they follow Ostrom (1983, 101-102) in arguing that there is no necessary relationship between spending and efficiency.

Moreover, the image of centralized, consolidated, monolithic local government offered by most public choice analysts is a caricature, in part precisely because the public choice approach has been so successful on the policy front. Service provision and production -- in all types of governmental arrangements -- is today a complicated mix of direct and both public and private third-party actors which can only be described as heterogeneous in the extreme (Stein 1990; 1993). To the extent that these public choice inspired reforms are successful in generating savings, there may be few additional savings to accrue from fragmentation. Thus, this argument concerning efficiency advantages is at best unresolved as yet, whereas evidence of fiscal disparities associated with fragmentation remains persuasive. Still, continued normal science research on the efficiency advantages of alternative institutional arrangements will continue.

Another part of this argument suggests that even with segregation, the poor and blacks can still benefit from the consumer sovereignty enhancing advantages of quasimarkets by forming their own Tiebout enclaves. The problem for lower-income minorities, of course, is that middle- and upper-class whites have often cornered the tax base required to make such incorporation viable. Or if the market is not already cornered, business interests opposed to incorporation can use their superior political resources to resist incorporation, thereby erecting a quasilegal barrier to access to the quasimarket (Hoch 1985). Indeed, nearly the only black suburb of Louisville,
Kentucky was forced to incorporate as a donut around the only meaningful business district in the vicinity and, as a consequence, proved so nonviable that disincorporation shortly followed (DeHoog, Lowery, and Lyons 1991). Nor is this solution of much use to wealthier blacks. As Phelan and Schneider (1996, 676) note, "Despite the impression that black middle-class suburbs are the wave of the future of integration in the United States, such suburbs are remarkably few in number." For both rich and poor minorities, it is the Tiebout quasimarket itself which constitutes a semilegal barrier to entry into the quasimarket, thereby ensuring that its formation remains partial and incomplete and its advantages available in only a separate and unequal manner.

This should not be taken to suggest that opportunities for the poor and minorities to benefit from fragmentation could not be strengthened and enhanced by additional institutional reforms. Clearly, if public choice theory has taught us anything, market failure does not imply nonmarket success. I will gladly admit that the failure of Tiebout quasimarkets to provide sufficient opportunities for the poor and minorities to form their own viable governments within a fragmented setting does not, by itself, imply a need for metropolitan government. But it is no great stretch within the public choice framework to suggest that governments can step in to ameliorate cases of market failure via means other than direct public production. In this case, that might entail strengthening regional land-use planning so that economically viable areas for new incorporations are not gobbled-up on the basis of wealth. It might also entail explicit COG, state, or national subsidization of city incorporations by the poor and minorities. The point is that the paucity of such viable incorporations over the last two decades requires that this argument be presented in a stronger form than its current passive construction if it is to provide a real solution to problems of metropolitan racial an income segregation. Again, this is an opportunity for the expression of the institutional creativity for which public choice scholars are so well known.
**Argument 4: Redistribution Cannot Be Done.**

This argument is both the simplest to state and the most clearly subject to ongoing normal science research activities whose outcomes remain unclear. Peterson's *City Limits* analysis suggests that municipal governments face an inevitable tradeoff between redistributive and developmental spending. The reality -- if not the inevitability -- of this tradeoff was given empirical support by Schneider's *The Competitive City: The Political Economy of Suburbia.* This argument has been augmented by research on welfare migration (Peterson and Rom 1990) suggesting that transfer-motivated sorting is the cutting edge driving the tradeoff posited in *City Limits.* This argument, so far, applies only to subnational transfers; redistribution by a national government might still avoid some of the constraints of the tradeoff. But even national-level redistribution may be difficult if the "global cities" hypothesis (Sassen 1991) is correct. The nationalization, and then internationalization, of the economy means that cities and even countries lack the fiscal autonomy needed to blunt the inevitable tradeoff of efficiency and equity.

This argument has significant power. Indeed, it is persuasive even to some scholars, such as Dowding (1996, 61-63), who have not been fully supportive of all elements of the public choice approach. Still, each of the components of this argument remains controversial and the subject of ongoing research. The global cities hypothesis, for example, remains subject to frequent challenge regarding the extent of its plausible application (Goldsmith 1995; White 1998). And recent and sophisticated empirical reassessments of the welfare magnet hypothesis, while thus far applied only at the state level and not yet extended to the assessment of cities, have raised very serious questions about its validity (Schram, Nitz, and Krueger 1998; Allard and Danzinger 1998). Thus, the ultimate import of both adjunct elements of this argument against redistribution remains highly contingent on the outcomes of ongoing scientific inquiry.
Even more fundamentally, a number of scholars have raised questions about the inevitability of the tradeoff between the redistributive and development tasks of local government. Although there undoubtedly are tradeoffs between equity and efficiency at some level, not all improvements in equity necessarily lead to reductions in efficiency. Indeed, in some cases redistribution can be an investment in greater efficiency (Stevens 1992, 42-43). In terms of redistribution within metropolitan areas, this proposition underlies the suburban dependence hypothesis, which suggests that suburbs are not economically independent of central cities. Rather, the economic vitality of the former depends on the economic health of the latter. A number of scholars have suggested that racial and economic segregation leads to systematic mismatches in material and, especially, human resources resulting in serious allocative inefficiencies which are felt across metropolitan areas, not just by central cities (Ledebur and Barnes 1992; 1993; Voith 1992; Savitch, Collins, Sanders, and Markham 1993; Downs 1994; Rusk 1995). Thus, the racial and income segregation promoted by fragmented governmental arrangements imposes costs on wealthy white suburbs as well as on central cities. While the suburban dependence hypothesis remains controversial (Hill, Wolman, and Ford 1995; Savitch 1995; Blair and Zhang 1994), even critical empirical evaluations by advocates of public choice institutions report results indicative of significant metropolitan-wide economic costs associated with excessive segregation and disparities in income (Blair, Staley, and Zhang 1996), albeit costs more modest than those reported by proponents of the suburban dependence hypothesis. From this perspective, metropolitan redistribution may not in all cases be counterproductive in terms of economic development. Rather at least some redistribution should be regarded as a necessary investment. The issue of how much and what types of redistribution can be viewed as investments should, of course, be the subject of an extensive normal science research program.
Argument 5: Redistribution Should Not Be Done.

Many of the original critics of the public choice approach to metropolitan governance remain convinced that its real intent was to minimize opportunities for redistribution. To many critics, the Pareto principle represents the core of public choice theory, and the real goal of its institutional analysis is to raise political transactions costs so high that redistribution is highly restricted (Golembiewski 1977; Baker 1976; Furniss 1978; Wade 1979; and Dowding and Hindmoor 1997). In some respects, this is unfortunate in that the Pareto principle is a normative position that, however principled its defense, is clearly an extra-scientific adjunct of the paradigm's more fundamental elements of methodological individualism and attention to institutions. It is also unfortunate in that many public choice analyses fully recognize the limits of the Pareto principle as a normative guide in situations of collective choice in the face of market failure (Stevens 1993, 42; Schmid 1978). Absent the Pareto principle, public choice theory and methods are politically neutral (Dowding and Hindmoor 1997).

But when included, such neutrality is lost. As Dowding and Hindmoor (1997, 460) have noted, the Pareto principle "is not a normatively weak criterion as public-choice writers maintain, but one which is deeply abhorrent to many people." More to the point for our purposes, insistence on the Pareto principle renders each of the four prior defenses of the public choice approach to metropolitan governance, as well as the normal science conducted on their propositions, moot. As Dowding has noted (1996, 60), "Public choice is [when insistent on the Pareto principle] so deeply conservative precisely because it denies, by its very methods dependent upon searching for Pareto-efficient solutions, that redistribution is an issue with which political discussion should concern itself." This leaves many critics of the public choice approach to metropolitan government in much the same position as Israel in regard to its dealings with the
PLO prior to its renunciation of the section of the PLO Charter calling for the destruction of the Jewish state. Absent the Pareto principle, the public choice research program on metropolitan governance can be as theoretically vibrant, empirically rich, and as contentious as any fan of a good scientific fight might want. With it, the public choice research program becomes a closed and insular ideology will little relationship to urban studies more generally.

To many critics, it is unclear which path will be chosen. This became especially evident with the recent publication of Vincent Ostrom's *The Meaning of Democracy and the Vulnerability of Democracies*. It is difficult to read this work as anything but a defense of only a weakly modified Pareto principle. Ostrom begins by describing the fundamental dilemma governing collective choice:

> Opportunities will always exist for some to prey on others. Under conditions of unanimity, some have incentives to hold out for special advantages. Whenever it becomes expedient to relax the conditions of general agreement for other decisions rules, including majority-vote rules, opportunities are created for some to take advantage of such circumstances to prey on and exploit others. (1997, 292)

He then immediately proceeds to outline the long and slippery slope associated with the latter half of the dilemma, noting that when unanimity rules are relaxed,

> Instead of maintaining standards of consensus that are constitutive of self-governing communities of relationships, it is easy to be lulled into believing that others are responsible for one's fate. One can then find that the structure of covenantal relationships has been replaced by a system of command and control. An overemphasis on legality can transform the conditions to life into a quest for exceptions and favors that eventually lead to the corruption of life, to the abandonment of morality, and to conditions of servitude in which everyone is bound by the tethers of legality and the loss of self-governing ways of life. (1997, 292)

While the treatment of the consequences associated with the two sides of the dilemma is a bit imbalanced, I believe that few will disagree that this is a concise statement of the fundamental conundrum of collective choice. The more difficult issue, of course, concerns our response to it.
Ostrom does not opt for an unrestricted application of unanimity, noting (1997, 272) that, "I cannot imagine that human societies can exist without the exercise of some coercive capabilities." Rather, he offers us a potential if limited escape from the Pareto principle, one that bears directly on our concern here with designing institutions of metropolitan governance. That is, relaxation of the unanimity rule might be possible. Ostrom (1997, 272) argues that, "in the presence of common knowledge, shared communities of understanding, and mutual trust, modest coercive capabilities might be sufficiently diffused through a system of social and political order so that a monopoly of rulership prerogatives can be foreclosed." This in turn depends on how public decision-making structures provide for the kinds of interaction and deliberation needed to develop sympathy and trust (Ostrom 1997, 290). Unfortunately, Ostrom concludes that only those institutions which provide for face-to-face, personal interactions are suitable or capable of doing so (Ostrom 1997, 299). As a result, Ostrom's response to the question 'Are democratic societies viable?' is: Only under limited conditions. 'One person, one vote, majority rule' is an inadequate and superficial formulation for constituting viable democratic societies....Person-to-person, citizen-to-citizen relationships are what life in democratic societies is all about. Democratic ways of life turn on self-organizing and self-governing capabilities rather than presuming that something called 'the Government' governs. (1997, 3-4).

In conjunction with Ostrom's extensive prior work on metropolitan institutions, the clear implication is that only fragmented governmental arrangements -- indeed, very fragmented arrangements -- are capable of non- or only weakly-coercive redistribution.

Interestingly enough, this perspective is not restricted to public choice proponents of fragmented metropolitan political arrangements. As I have noted elsewhere (Lowery, Lyons, and DeHoog, 1992), this insistence on person-to-person deliberations as the necessary prerequisite for establishing the trust required for redistributive policy is shared by the communitarian urban
politics literature associated with the work of Barber (1984) and Elkin (1987). However, for many critics of both the public choice and communitarian diagnoses of metropolitan politics, this escape from the Pareto principle is too limited precisely because it restricts the possibilities of redistribution to within-unit transfers, where there is limited inequity in either initial endowments or contemporary needs. To such critics, this proposal reads like the old joke about the drunk searching for his keys under a streetlight. Because of the difficulties of encouraging deliberation between those with needs and those with resources, those with resources and needs should deliberate only among themselves. Worse, this analysis seems to suggest that in order to encourage more meaningful deliberation within each group, we should erect institutional barriers which eliminate whatever limited communication -- direct or indirect-- that might have occurred between the two groups. Indeed, such separation is expected by some to further undermine what limited trust and sympathy now exists among the different income and racial groups populating our metropolitan communities (Bickford 1999a; 1999b). Thus, Ostrom’s solution to the problem of inequity appears to many critics of metropolitan fragmentation as a quasicommunitarian rationale designed to deny the very possibility of redistribution except as it might occur separately among those with like problems and among those with like solutions.

Obviously, this assessment of The Meaning of Democracy and the Vulnerability of Democracies requires several important caveats. First, I view this work as a principled, tenaciously argued, and very interesting piece of normative political theory. Second, I fully agree that normative criteria must inform our evaluation of scientific research findings and our selection among policy options. And third, I well recognize that in the course of a long and distinguished career, a scholar can make contributions in both scientific and normative domains. Still, it should be easy to see how the juxtaposition of significant contributions in the scientific, policy, and
normative domains bearing on the design of political institutions within metropolitan areas may lead some to wonder what is leading what. To many, it seems plausible that the normative stance guides the policy recommendations, which in turn governs the direction, content, and assessment of the scientific research being conducted on the topic. I do not and cannot know if this plausible sequence is true. But something like the PLO renunciation of the call for Israel's destruction would certainly be helpful in quelling the suspicion of public choice critics.

Beyond rejecting the Pareto principle as a prior normative restraint on evaluating institutional design proposals regarding metropolitan government, it should be clear that the public choice position on deliberation and trust as necessary prerequisites for democratic redistribution merits both greater theoretical and empirical attention. Clearly, not all institutions foster equal levels of trust, attachment, participation, and sympathy. We should explore which work better than others under a variety of conditions and for a variety of purposes. And some current research on such issues poses interesting anomalies meriting the attention of public choice scholars. Among these are Lowery, Lyons, and DeHoog's (1992; Lyons and Lowery 1989) finding that civic attachment is greater in neighborhoods within consolidated cities than in their counterparts in fragmented civic arrangements, along with Oliver's (1999) finding that civic participation is positively related to community income heterogeneity. Attachment and participation are not the same as trust, but they are powerful resources that cities can and often do call upon in making local democracy work. And beyond empirical analysis, we should employ our theoretical creativity to design new institutions for further theoretical and empirical evaluation. This, of course, is the purpose of normal science.

Conclusion
Many public choice analysts will surely regard this review of the theory and empirical research surrounding the controversy over metropolitan governmental structure as one-sided and inattentive to the nuances and complexities that comprise public choice research. Such an assessment is valid, in large part because I have taken seriously the charge embedded in the title of this session. I've provided you an overview of the arguments and evidence leading some scholars to conclude that political fragmentation may not be all that it advertised to be and may carry some severe liabilities. While public choice theory has -- at least for now -- won the policy debate over metropolitan governmental structure, I hope that I have demonstrated that, at least on first face, important scientific controversies remain, controversies whose solution may influence future policy reversals and, thus, deserve the attention of public choice scholars.

More importantly, I strongly believe that public choice scholars should be directly engaged in these controversies. In part, this is a purely self-interested position. I conduct social science research in large part because it is fun. It's a lot more fun when someone is arguing vigorously on the other side. My deeper motivation, however, lies in the prospects for better science. Public choice supporters of metropolitan fragmentation all too often behave as though they were still arguing against the traditional reformers, when the arguments and evidence motivating support for more centralized political arrangements have and are continuing to evolve in significant ways. This is evident in the pattern of citations found in the literature on metropolitan governance. Contemporary critics of the public choice approach cite public choice scholars frequently, while the reverse is rare. But in their attention to the nature of the good or service being provided or produced, reliance on methodological individualism, and emphasis on institutions, the works of the contemporary critics are not discernibly different in method from what is typical of public choice analyses of metropolitan politics. In not taking this literature more seriously, the public
choice approach risks becoming an historical anachronism within the urban politics literature, much as did the traditional reform theory while it ignored the emerging public choice approach.

Even more importantly, more intensive engagement over ideas and findings within a normal science context, as opposed to celebration of past victories, offers the prospect of progressive problem shifts in our theories about urban politics. These may well include shifts that are fully consistent with the public choice approach, but that extend it in new and theoretically interesting ways. Such problem shifts, however, may just as well lead to a significant dimming of enthusiasm for fragmented governmental arrangements. I can well imagine, and I hope you can as well, that it is at least possible that a "public choice" case for metropolitan consolidation might emerge from such exchanges. This possibility -- no matter how slim -- is a necessary risk the public choice approach must bear if it more vigorously engages these normal science controversies and potential controversies. Thus, there are both important risks and opportunities for the public choice approach in pursuing these topics.

How should you balance these risks and opportunities? Answering this brings us back, of course, to the fundamental question of what is central and what is peripheral to the public choice approach. This paper has been organized by topics in terms of my assessment of their centrality to that approach. In my view, the paradigmatic contributions of the public choice approach inhere most fundamentally in its method of doing science. The policy and institutional recommendations of public choice analysts are important, but are secondary because they must always be viewed as contingent on future developments within normal science. The normative stance on government offered by public choice thinking, while it has an appropriate role of informing policy choice and institutional design, must remain a tertiary concern in terms of actually doing science on metropolitan issues. If one's commitment to the several elements of the public choice approach is
so ordered, the opportunities associated with further engagement in normal science controversies will certainly outweigh the risks. If the order is reversed, than the risks will be viewed as paramount. Such a reversal of priorities would be as unfortunate for the public choice approach as it would be for metropolitan studies more generally.
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