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Permalink

<https://escholarship.org/uc/item/3q85q7d9>

Journal

The Journal of Politics, 81(1)

ISSN

0022-3816

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Publication Date

2019

DOI

10.1086/700726

Peer reviewed

**Looking for Influence in All the Wrong Places:
How Studying Subnational Policy Can Revive Research on Interest Groups**

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Abstract: The American politics literature on representation focuses on voters and elected officials, but a growing group of political scientists is arguing that more should be done to study interest groups. Yet there already *is* a large literature on interest groups—and it has struggled to show evidence of interest group influence. I argue here that the interest group literature’s near-exclusive focus on the federal government has hindered its progress: basic questions have gone unasked, important interest groups have gone underappreciated, and the amount of influence has been underestimated. By studying U.S. subnational policymaking, scholars would discover different constellations of interest groups, and they would find that the variation in subnational governments allows for empirical designs that are better able to detect interest group influence when it exists. The payoffs of a subnational focus would be substantial—both for our understanding of interest groups and for the study of political representation.

Keywords: interest group, influence, representation, state, local

Running header: Looking for Influence in All the Wrong Places

Interest groups used to be at the heart of the study of American politics (e.g., Schattschneider 1960; Dahl 1961), but they aren't today. For the last few decades, the broad theories that have motivated most of the research in the American politics subfield have neglected interest groups, and have instead put the emphasis on voters, elected officials, and their linkages through elections (Downs 1957; see Hacker and Pierson 2014). As a result, the American politics literature on representation has become one largely about how well the positions of political elites align with citizens' preferences. The representation of other political actors, such as organized groups, has been treated as a peripheral matter, as though questions about interest group influence are somehow separate from studies of political representation.

Very recently that has started to change. Hacker and Pierson (2014) have argued that the American politics subfield's reliance on the Downsian theoretical framework has come at the expense of its "substantive bite": key political and policy phenomena of interest, such as asymmetric party polarization and rising economic inequality, are difficult or impossible to explain with theoretical models centered on elections and voter turnout. The corrective, say Hacker and Pierson, is an alternative approach—one that conceives of politics as a struggle to shape public policy, and one that brings organized groups back to their rightful place as central actors in American politics. And it is not only Hacker and Pierson who favor such a corrective. Today, growing group of political scientists is pushing American politics scholars to once again make interest groups a sustained subject of research, asserting that interest group influence needs to be studied if we want to fully understand how well American government represents its citizens (e.g., Bawn et al. 2012; Gilens and Page 2014; Anzia and Moe 2015; Moe 2015).

This is an exciting juncture in American politics research, but also an awkward one. It is awkward because there *is* an existing literature on interest groups—quite a large one—much of

which has been devoted to the very question of whether interest groups have influence. Its conclusions, moreover, are decidedly mixed. Some studies do uncover evidence of influence, but just as many turn up little or none (Smith 1995; Baumgartner and Leech 1998; Burnstein and Linton 2002; Hojnacki et al. 2012). Tellingly, in the most comprehensive interest group study of the past decade, Baumgartner et al. (2009) study over 1,000 groups on 98 national policy issues and find that interest groups' resources have little to no relationship with whether the groups get what they want. Interest group scholar Beth Leech (2010, 553) sums up the situation well:

“For those who try to quantify and systematically measure [interest group] influence...it has proved illusive...Almost everyone believes that interest groups are influential, and yet systematic studies have as often pointed to the limits on interest group influence as have concluded that strong influence exists.”

This is no minor stumbling block. Effectively, what we currently have is a growing call for American politics scholars to do more to study interest group influence and a large existing literature on that very subject that finds few signs that interest groups have any influence at all. So how do we proceed? For scholars hoping to heed the call for more research on interest groups, how should we go about doing it? And how do we make sense of and learn from the vast body of research on interest groups that already exists?

I argue here that there is a particular feature of the interest group literature that has hindered its progress and prevented it from finding clearer evidence of interest group influence: its near-exclusive focus on the federal government. The vast majority of research on interest groups has centered on national politics, and, within that, mostly on the U.S. Congress (see Hojnacki et al. 2012). In large part because of this national focus, basic questions have gone unasked, important types of interest groups have gone underappreciated, and the overall amount

of interest group influence in the country has been underestimated. Even more importantly, because the national context requires scholars to leverage variation within a single government, the whole enterprise of studying interest groups has developed into one that is not well set up to detect interest group influence, even when it exists.

The study of interest group influence would therefore benefit greatly from a shift to studying U.S. subnational policymaking. By turning from a single government that is awash in interest groups to sets of governments with considerable variation in interest group activity, scholars would recognize the need for a theoretical logic to explain that variation. They would discover different constellations of interest groups—constellations in which groups like unions often have a much larger presence than they do at the national level. They would discover contexts with less inter-group competition and weaker partisan and ideological constraints—both of which stand to enhance interest group influence. And with 50 states and almost 90,000 local governments, there is no need to limit analysis to variation within a single government. The subnational context offers thousands of cases, each with their own interest groups, and each with their own policies. The main challenge, as with any work on U.S. subnational politics, is data availability. But that is both a challenge and an opportunity. Given the potential for subnational research to revive the study of interest group influence, it is a challenge well worth taking on.

Interest Group Activity

The place to start is with interest group activity, broadly defined.¹ After all, before we can get to questions of whether interest groups affect outcomes, we first need to know the lay of the land: Are interest groups active? Which ones? How active are they? On what issues, and under what conditions?² The interest group literature only addresses some of these questions, and even for those it does address, it doesn't do so in a way that establishes a solid foundation for the study of interest group influence.

Consider this very basic question: Under what conditions are interest groups active in a government? Or, alternatively, what explains the overall amount of interest group activity in a government? A scholar looking only at the federal government wouldn't bother to ask such questions, because it is well known that there are thousands of active interest groups in Washington, DC. At the subnational level, however, the answers to these questions are less obvious. We know interest groups are active in state politics, but what explains variation in the overall amount of interest group activity from state to state? Local governments are even more

¹ Most research on interest groups examines campaign contributions and lobbying, but interest group activity can also mean mobilizing voters, providing publicity for campaigns, nominating and endorsing candidates, and more.

² Answering these questions does not necessarily require scholars to “solve” the related puzzle of why individuals join groups. That puzzle has been a hurdle since Olson (1965), but the fact of the matter is that groups do become politically active—many of them—and so we should go about explaining patterns of that activity. See helpful discussions of this in Wilson (1995) and Gray and Lowery (1996).

of a mystery. They have less in common with the federal government than the states do (see Berry 2010), and we can't even say for sure that interest groups are always active in local politics (see Peterson 1981; Oliver 2012). Not only are there no readily available datasets on local interest groups, but also the interest group literature doesn't offer a general theory to tell us what to expect. Aside from Moe's (2005, 2006) studies of teacher unions in school districts and a few studies of mostly large cities (e.g., Cooper et al. 2005; Feiock et al. 2014), we have little sense of how interest group activity varies across local governments.

The near-total lack of data is a disadvantage to studying interest groups at the local level, but it's also an opportunity, because it frees scholars from the temptation of crafting studies based on what can be answered with available data. In local politics, we are forced to start with theory. And we are free to begin with what interest groups care about and proceed from there. On this, there is near-consensus: interest groups care about public policy.³ Public policy should therefore serve as an anchor in the study of interest groups (Hacker and Pierson 2014).

To explain the overall amount of interest group activity in a government, for example, we should start by thinking about what policies are at stake in that government. Oliver (2012) points out that governments vary dramatically in their scope—the number of issues on which they make policy decisions—and so we should expect the overall amount of interest group activity to be

³ Interest groups also need to survive (Gray and Lowery 1996), and they need resources and access to policymakers to influence policy, but their main goal—the reason they need resources and access—is to influence policy. As Moe explains, we should not just be focused on the *resources* groups have to influence politics, but also their *incentives* to influence politics (see Moe 2011, 2015).

positively related to that. The policy stakes also stand to be larger in governments of 1 million people than in governments of 1,000 people, which suggests that larger governments should feature more interest group activity than smaller governments. These are just two basic hypotheses, and many more are possible. But they illustrate how the local context has the potential to inspire new thinking about interest groups, and also how what Hacker and Pierson call a “policy-focused approach” is a productive one for studying interest group activity.

The policy-focused approach can also be used to develop expectations about *which* interest groups will be active, *how* active they will be, and *when* they will be active (under what conditions and on what issues). These, too, are simple questions—and important stepping stones to understanding interest group influence. However, the existing literature has not produced a theory to answer them, in part because it has been focused on the federal government.

For one thing, studies of the interest group system in Washington, DC, have been mostly descriptive. Using lists of lobbying groups with Washington offices, for example, scholars categorize the groups on several dimensions (such as whether they are membership groups or institutions) and then use counts of the groups in different categories to draw conclusions about whether the interest group system is biased in favor of certain interests (e.g. Scholzman and Tierney 1986; Scholzman et al. 2012). This work has been informative, but it has not led to a theory that could explain which interest groups will be active in other governments.

These studies are also not designed with an eye toward testing interest group influence. The group categories in this research are not based on what groups are trying to achieve policy-wise, and counts of groups do not tell us how large or active the groups are—even though surely that matters for influence. If the ultimate goal *were* to study influence, it would be more useful to categorize interest groups according to their policy goals—and to measure how actively they

pursue those goals and with what resources. But when the jumping-off point is a list of over 11,000 groups in Washington, DC, that isn't really possible. There are too many groups and too many issues for such an approach to be realistic.

Using a policy-focused approach to study interest group activity in the states would be more manageable, but so far no one has pursued the opportunity. There is a sizeable literature on state interest group systems pioneered and developed by Gray and Lowery (1996),⁴ but their work isn't set up to be a launching pad for studying interest group influence. Like the research on the national interest group system, Gray and Lowery's research relies heavily on databases of registered lobbyists, categorizations that aren't based on groups' policy goals, and counts of groups as a key metric. While Gray and Lowery do advance a theory, its starting premise is that the main goal of interest groups is to *survive*. Gray and Lowery therefore set out to answer questions such as how many environmental groups or gay rights groups can be sustained in an environment with limited space and resources. Their theoretical account of interest group populations is not a policy-focused theory. It does not start with policy and work backward and forward. And it is not a midway point toward studying interest group influence.

The study by Baumgartner et al. (2009), in contrast, *is* designed to study interest group influence, and it starts with *policies* and works outward to document the interest groups active on each one, including each group's policy position. This is a better set-up for studying influence, because the active groups are categorized according to their policy goals. But it still does not answer the question of which interest groups will be active and under what conditions. For while Baumgartner et al. meticulously document which interest groups are active on 98 different issues, they do not present a theory to explain the patterns of activity they find.

⁴ See also Lowery and Gray (2007) for references to their many articles on the subject.

This means that if we turn to state and local politics, there isn't an existing theory we can draw on to explain the kinds of interest groups that will be active. Fortunately, the subnational context is an excellent one within which to develop (and test) such a theory, and we can continue to use public policy as an anchor. For questions about which interest groups will be active in a government, and how active they will be, we should start by considering what policies a government makes and then work outward to think about which groups have a stake in those policies. This would be difficult to do at the national level because there are so many issues. But most state governments make policy in fewer areas than the national government, and local governments are even more constrained. As Oliver (2012) explains, municipal government is largely concerned with land and money: deciding what can be built and where, and spending on public goods such as streets and sanitation and public safety. By definition, the nation's special-purpose governments make policy on a single issue (Berry 2009). For many local governments, then, it is possible to enumerate the most important issues, develop expectations about the interest groups that would be active on those issues, and then aggregate up to make predictions about the overall composition of the interest group system. The relative simplicity of local government makes this feasible.

But it is not just the numbers of issues that are different at the state and local level. The issues themselves are also different. And so if this general approach has merit—if the kinds of policies at stake shape the kinds of interest groups that are active—then the composition of state and local interest group systems should look quite different than that of the federal government. What this suggests, then, is that by focusing almost entirely on national politics, the interest group literature is providing an incomplete—and in some ways inaccurate—picture of interest group activity in the United States.

Unions are a major case of this. Both the interest group literature and the political science literature on labor in the United States have focused mainly on national politics, and for reasons that are likely related, they also tend to talk of private-sector unions as though they are the sum total of the union movement. A core theme throughout this literature is that unions are weak and in decline (e.g., Dark 1999; Schlozman et al. 2012), which, for private-sector unions, is true: fewer than 7% of private-sector workers today are members of unions (Hirsch and Macpherson 2018). But in the public sector, union membership rates are much higher—34% across the country—and they have barely budged since the 1980s, even with Republicans’ recent moves to roll back collective bargaining rights and ban agency fees. Moreover, that 34% national average masks substantial variation in union membership across states: in New York, for example, 67% of all government employees are in unions, whereas in South Carolina, only 7% are. Not only are public-sector employees today more organized than private-sector employees, but they also now make up a majority of all union members in the United States. It is a big mistake, then, to think of private-sector unions as the sum total of the union movement.

Why, then, does the American politics literature neglect the importance of public-sector unions, both to the union movement and as interest groups in American politics? Almost certainly because public-sector unions are much more active in state and local politics than in national politics. Unlike private-sector unions, whose collective bargaining rights are sanctioned by national law (most notably the National Labor Relations Act), public-sector unions are granted collective bargaining rights by *state* laws (see Saltzman 1985; Anzia and Moe 2016). These state laws are crucial to the strength of public-sector unions: by simply looking at whether states require, allow, or prohibit collective bargaining, and also whether they previously allowed agency fees, it is possible to explain a great deal of the variation in government employee union

membership across states (Moe 2011). It makes sense, then, that public-sector unions would be more active in state politics than national politics, because up to this point it has been the states that have made the policies that most directly affect their membership rates.

There is another consideration that is even more important, which is that compared to private-sector unions, public-sector unions actually have even greater incentive to be active in politics—especially in subnational politics. The reason, quite simply, is that public-sector union members are employed by the government. By getting involved in politics, public-sector unions are in the unique position of being able to influence the elections of their employers—the very government officials who make key decisions about their members’ jobs, salaries, benefits, and working conditions (Moe 2005, 2006; DiSalvo 2015). In states that require collective bargaining (as most do), this means that public-sector unions are helping to select the people who sit on the opposite side of the bargaining table. This is a powerful motivator of political action—one that private-sector unions do not share. And most of the resulting political activity is targeted at state and local government, because that’s where most of the government jobs are: out of the roughly 17 million full-time government jobs in the United States, well over 14 million are state and local government jobs (U.S. Census 2012). By focusing on national politics, then, scholars have neglected the governments where most public-sector union activity takes place.

Anyone who *has* studied U.S. public-sector unions knows that the place to look for most of their activity is state and local politics, and anyone taking a serious look at interest groups in state and local politics will be confronted with the reality that public-sector unions are usually major players. Consider the very small body of work on these matters. Thomas and Hrebenar (2004) regularly rank the top 40 most influential interests in the 50 states, and they consistently find that state affiliates of the National Education Association vie for the top spot with state

chambers of commerce. Moe (2011) finds that teacher unions spend considerable sums in national elections (about \$5.4 million in 2008) but that they spend far more in state elections: \$61.8 million in the 2008 cycle (see also Hartney and Flavin 2011). In local school board elections, teacher unions are typically the most active interest groups, and they are often successful in electing their favored candidates (Moe 2005, 2006). And the activity of public-sector unions is by no means limited to teachers or to the electoral arena. Police unions, firefighter unions, and correctional officers are often highly active in state and local politics (Page 2011; Anzia and Moe 2015); and public-sector unions are active not just in elections but also in lobbying, through collective bargaining (Moe 2011; DiSalvo 2015; Anzia and Moe 2015), and even as decision-makers in state government agencies (Anzia and Moe forthcoming). Much more work needs to be done on these matters. But from what has been done so far, it is clear that public-sector unions are among the most active groups in state and local politics.

A sustained focus on subnational policymaking would therefore broaden and deepen our understanding of interest group activity in the United States. It would make it clear that certain groups have been underestimated and understudied as a result of the literature's preoccupation with national politics. It would require scholars to answer questions that remain unasked, and it would motivate them to answer existing questions from a policy-focused angle. Given the centrality of policy to what interest groups do, the payoffs of such a focus would be substantial.

Interest Group Influence

The literature's focus on national politics can also explain some of the difficulties researchers have had in their attempts to demonstrate interest group influence. The first reason is that interest groups probably do have more influence at the subnational level than the national level. But second, and more generally, studying interest group influence within a single

government brings with it considerable empirical design challenges, and those challenges have made it difficult for scholars to detect influence even where it likely exists. It is in the study of interest group influence, then, where the shortcomings of the existing literature are especially large, and where the promise of studying subnational policymaking is especially great.

Why might interest groups have more influence in state and local politics than in national politics? For starters, there stand to be big differences in the level of interest group competition. National politics is intensely competitive, and political battles are often highly salient and well publicized. As Baumgartner et al. (2009) explain, on almost all of their 98 national issues, interest groups are mobilized on both sides, clashing with one another. And when Smith (2000) finds little evidence of influence by the U.S. Chamber of Commerce, it is largely because the Chamber only takes positions on issues that unite the business community—issues on which the business community faces intense competition (see Pierson 2015). By comparison, state and especially local governments are smaller in size and scope, and for reasons I discussed above, they probably feature fewer active interest groups—making it less likely that any given group will face direct group competition over the policies it cares about (see Anzia 2014). That tendency—together with lower levels of citizen knowledge and participation at the subnational level—creates conditions that are probably more favorable to the interest groups that *are* active.

Interest group scholars also point to partisanship and ideology as constraints on interest group influence (e.g., Leech 2010), and at the subnational level, these forces either vary across contexts or are weaker across the board. States, for example, vary in the extent of their elite polarization (Shor and McCarty 2011), and there are at least some important state issues that are not intensely partisan, such as public pensions and criminal justice (Anzia and Moe 2017; Grumbach 2018). Local politics tends to be even less partisan and ideological: most local

elections are non-partisan, and the issues that define local politics are custodial rather than ideological in nature (Oliver 2012). Thus, if forces of partisanship and ideology limit interest group influence, then we should find more influence overall in subnational politics.

But these substantive differences are not the only reason scholars should turn their sights to subnational politics to understand interest group influence. Such a step would also come with significant methodological advantages, because the variation in subnational governments allows for empirical designs that are much better able to detect interest group influence when it exists.

To explain why, it helps to first step back and consider why a focus on public policy is so important to the study of interest group influence—and also why public policy should be central to the study of representation more generally. The first claim is simpler: if interest groups care about public policy, then to assess whether interest groups have influence, naturally we should be focused on the extent to which they get policy to reflect their interests. Did large banks succeed in inserting favorable provisions into the Volcker Rule? Did teacher unions successfully limit the number of charter schools? Are automakers happy with emissions standards? In the study of interest group influence, the obvious dependent variable to be studied is public policy.

But public policy and interest group influence should also be central to the broader study of representation. As I described earlier, mainstream research on political representation has proceeded as though one only needs to study the alignment between political elites and the mass citizenry to assess whether representation is working well. Perhaps this might be justified if policy outcomes always aligned perfectly with citizen preferences, but what about when they don't? Surely we should consider interest group influence as a possible contributor. And that's just the beginning. Mainstream studies of representation typically rely on public opinion as the benchmark against which representation is to be evaluated, and yet a substantial body of research

raises serious questions about whether alignment between elite and public opinion is really a sign of well-functioning representation (e.g., Lenz 2012; Achen and Bartels 2016). There are also issues on which the public does not *have* meaningful opinions (e.g., Zaller 1992; Anzia and Moe 2017), and situations in which citizens have opinions on outcomes but not on the specific policies that produce those outcomes (e.g., Arnold 1992). The absence of a public opinion benchmark does not make these issues irrelevant to the study of representation. Actually, these are the kinds of issues on which interest groups are in the best position to push policy in their favor (Bawn et al. 2012). My point here is that just like interest group scholars, representation scholars should be studying the *content of public policy*. They should be thinking about whether and when policy is in the public interest, even if the public isn't paying close attention.

That brings us to one of the major problems with using the national government to study interest group influence: it is a single government, and so at any point in time, the overall content of public policy is fixed. This is a problem because researchers studying causal questions are in the business of making comparisons. To make comparisons, we need variation. And to find variation in the federal government context, where overall public policy is fixed at any given point in time, scholars have either turned to variation in policymakers' *positions* on issues, or they have made comparisons *across issues*. Both maneuvers have the effect of removing the content of public policy from the dependent variable, and both are a set-up for underestimating interest group influence.

Consider studies that look to the roll call votes of members of Congress for evidence of influence. Such studies can certainly be productive and informative, but it is not always clear how legislators' votes and positions are connected to actual policy outcomes (e.g., Mayhew 1974). Also, policy is ultimately shaped by both decisions and non-decisions (Hacker and

Pierson 2010; Moe 2015), and interest groups can exercise influence by *blocking* issues from reaching the agenda. The special sets of issues that actually come to a roll call vote are probably the most salient, contentious ones—the ones on which interest group influence tends to be limited (e.g., Pierson 2015). Thus, while stated positions and roll call votes do offer a solution to the design problem of limited policy variation in the federal government, it is a solution that leads scholars to look for influence in places where they are less likely to find it.

Another way of creating outcome variation in a single government is to examine variation across issue areas (e.g., Baumgartner et al. 2009; Gilens and Page 2014). The problem here is that policy outcomes are not comparable across issue areas: there is no way to say, for example, whether auto emissions standards are friendlier to interest groups than abortion policy. To resolve this dilemma, scholars shift from studying policy to *policy change*, asking—for each issue—whether the policy changed or the status quo persisted. Then, by coding whether interest groups prefer policy change or the status quo, scholars can test whether interest groups that are more active, or that have more resources, are more likely to get the outcome they prefer—in a way that is comparable across issues.

The main problem with this maneuver is one that Baumgartner et al. (2009)—who use this approach—identify: if an interest group is influential, it is quite likely that the status quo policy already embeds that influence. In fact, perhaps the interest group is so influential that it has already succeeded in getting most of what it wants, such that further changes in its direction—even if the group prefers it—are unlikely. In this scenario, we would certainly not want to conclude that the interest group is weak just because the policy did not change. Instead, we would want to somehow measure the friendliness of the status quo policy to the interest group. But with this approach, we can't do that, because again, we cannot compare the interest-

group-friendliness of auto emissions policy and abortion policy. And so in an effort to find variation in a single government, scholars have to treat all status quo policies as equal, regardless of how much interest groups may have influenced the status quo.

Even beyond the tendency of these studies to underestimate interest group influence, there is a certain irony to all of this. Scholars care about interest group influence out of a concern about the content of public policy and whether it is in the public interest, and yet to make possible studies of interest group influence in the national context, they have removed the substance of policy from their outcome variables. The result is a body of work that does not actually say much about how public policy is different (or not) because of interest group influence—even though presumably that was the motivation behind the inquiry.

Yet none of these contortions are necessary. Instead of thinking about what is possible within a study of the federal government, we should instead think about how best to study interest group influence and then find an environment with suitable conditions. And if we want to understand interest group influence because we care about how it might impact public policy, then we should be doing more to explain policy—full stop—and recognize some of the limitations of analyzing policy change, legislators' positions on policy, or roll call votes. When possible, we should focus on the substance of public policy, explore variation in the substance of that policy, and then see whether that variation can be explained by variation in interest group activity, resources, or strength.

By studying subnational policymaking, we do not have to compare across issues, nor do we have to look to roll call votes for evidence of interest group influence. The U.S. states offer 50 units that make policy on many of the same issues, including 50 policies on auto emissions and 50 policies on abortion. Across local governments, there are thousands of versions of land-

use policy, thousands of approaches to law enforcement and policing, and thousands of local education policies. In principle, then, we can measure policies of the same type across many units, and once we have done a proper job of explaining variation in interest group activity across those units—which groups are active, and how active they are—we can begin to test whether that interest group activity shapes the public policies governments produce. Studies of this kind would be less likely to understate interest group influence, because the outcomes being studied would reflect the totality of interest group success on the issue. Importantly, they would also yield insights into how public policy is different (or not) as a result of interest groups.

There has already been some productive work that adopts this approach, including work studying the policy effects of government employee unions (e.g., Moe 2011; Hartney and Flavin 2011; Anzia and Moe 2015, forthcoming; DiSalvo and Kucik 2017), state business associations and conservative networks (Hertel-Fernandez et al. 2016; Hertel-Fernandez forthcoming), and even the lobbying efforts of governments themselves (Payson 2018). Perhaps tellingly, each one finds some evidence that groups are shaping the policy outcomes they care about. In addition, there is existing work that leverages rich variation in subnational political institutions to better understand the conditions under which interest groups have influence (Gerber 1999; Matsusaka 2004; Anzia 2014). But these studies are just the beginning. Much more can and should be done.

Caveats and Conclusion

If subnational politics has such potential for the study of interest group influence, then why have so few interest group scholars ventured out into this territory? One possibility is that political scientists often consider state and local politics to be inherently less interesting or less important than national politics. But there is no good justification for this. State and local governments account for half of all public spending in the U.S.; employ more than 14 million

government workers; and make major policy decisions about education, law enforcement, health policy, criminal justice policy, and much more. If this is a reason why interest group scholars have focused almost exclusively on national government, it certainly isn't a good one.

A second possibility is that interest group scholars actually view studies on many issues (in the federal government) as superior to those that only study one or two issues across states or cities, perhaps out of concern that the findings of single-issue studies are not generalizable. I would agree that we ultimately want to work toward theories of interest groups that carry across issues and contexts. But studies of single issues across government units can contribute a great deal to such an effort, and as I have explained here, studies that compare across multiple issues often build in design problems that significantly impede progress—actually preventing us from getting closer to meaningful and generalizable findings about interest group influence.

Almost certainly the main reason for the dearth of research on state and local interest groups is the shortage of easily accessible data. At the state level, there are datasets of registered lobbyists and campaign contributions, but anyone wishing to research interest groups' activity beyond those forms has to collect new data (e.g., Anzia and Moe forthcoming; Hertel-Fernandez forthcoming). Scholars are starting to do more to assemble and analyze data on state policies (e.g., Caughey et al. 2017; Grumbach 2018), but so far little has been done to use data on state policies to study interest group influence. The situation is worse at the local level, where not only are there no existing datasets of interest group activity, but there also aren't many readily available datasets of local policies beyond public finance outcomes. This means that scholars wanting to take advantage of the richness of the subnational context discover that doing so would require the collection of entirely new data—both on interest group activity and on public policy.

As I said earlier, however, this is both an obstacle and an opportunity. Interest group scholar Allan Cigler (1994, 29) has written that “data availability has been the major determinant of the interest group politics research agenda, framing both the questions we explore, and the topics we avoid.” Because of this, much of the interest group literature has been constructed around existing datasets of interest group activity in national politics—and has resulted in a large body of work that is not well set up to detect interest group influence even when it exists. So we should not let the shortage of data and the necessity of collecting new data stop us from studying subnational politics. For even if it is practically a desert when it comes to data, it is practically a promise land when it comes to the variation in policy and interest group activity that we need to make progress in the study of interest group influence and representation.

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