

# Policy Attention in State and Nation: Is Anyone Listening to the Laboratories of Democracy?

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Do patterns of policy attention at the state level influence agenda setting in Washington over the short term? We examine this question by first developing a series of hypotheses about such linkages. We test these conjectures with a data set pooling measures of policy attention at the national and state levels for several years and several policy areas. We find little evidence that changes in state policy agendas in the aggregate influence national patterns of policy attention.

Federal systems provide ample opportunities for their policy systems to interact. Indeed, perhaps Morton Grodzins' (1966) most significant contribution to the study of federalism was his observation that federal systems behave as a structure with many cracks. When policy is impeded or opposed at one level, it readily flows to another. These policy systems can support each other as well, providing examples of policy successes to be emulated and policy failures to be avoided, a process that has led to extensive literature on vertical policy diffusion and policy learning in federal systems (Karch 2007; Shipan and Volden 2006; Daley and Garand 2005; Tews, Busch, and Jorgens 2003; Hecht 2001; Mossberger 1999; Boeckelman 1992; Weissert and Sheller 2008; Thompson and Burke 2007). While we have learned much from this literature, especially in terms of policy learning about and the vertical diffusion of specific policies, there remains as yet considerable confusion given quite mixed empirical results. Some of the studies noted above find considerable evidence of diffusion, but many others do not.

In examining this problem, we take a broader view of the process in two respects. First, while much of the literature over the last decade focused on quite specific policies, ranging from Mossberger's (1999) analysis of enterprise zones to, most recently, Weissert and Scheller's (2008) analysis of state influences on national health policy, we opt for a macrolevel perspective so as to better set the research on specific cases of vertical interactions in context. Simply put, whatever case analyses

tell us about the vertical diffusion of a *specific* policy (and, indeed, they can tell us a lot!), they—in the end—cannot tell us how these specific cases fit into more general patterns of policy learning and diffusion. Is a specific case of learning or diffusion an exception to the more general pattern or part of it? Indeed, we know little—most likely because of the difficulties of doing such research—about policy diffusion and learning at the macrolevel by which to set the more specific policy literature in context.

Second, some of the null results in the policy literature might result from the use of too rigorous a criterion for learning or diffusion. That is, it may not be so much the specific *content* of a policy that diffuses as “*attention*” to the issue it is designed to address. In recent years, of course, considerable attention has been accorded to the politics of attention (Baumgartner and Jones 1993; Jones and Baumgartner 2005). The rationale for this research is clear. Before policy solutions are developed, attention must focus on the policy problem the solutions are intended to address. Further, much of the research in the policy agenda’s literature employs macrolevel analyses suitable for addressing our first argument above. Still, despite the extensive and still growing body of research in the politics of attention literature, scholars have only recently begun examining cross-jurisdiction influences on policy attention. And it is here that this literature can usefully connect to the literature on policy diffusion. That is, before ‘*policy solutions*’ can diffuse, ‘*policy attention*’ at one level of government must be synchronized with that of another. Further, the politics of attention literature examines a much broader range of potential interactions across levels of government going beyond only positive emulation of one government by another. Especially important here are vertical interactions entailing negative response on the part of one level of government to policy actions of the another, actions that might include policy push-back or even, in the case of the national government, preemption.

Approaching the problem of understanding vertical interactions within federal systems from a macrolevel perspective by focusing on the politics of attention rather than diffusion or policy learning may help us to reconcile or at least place in context some of the mixed findings in the literature. But as yet, we know little that is systematic about such processes of shared attention across levels of the federal system, especially in terms of how their separate agenda processes influence each other over the short term. Recently, however, Baumgartner, Gray, and Lowery (in press 2009, available at: <http://prq.sagepub.com/cgi/rapidpdf/1065912908322407v1>) examined how policy attention across a range of issues as measured by Congressional hearings influences attention in the policy agenda process in the states as captured by patterns of bill introductions across those same policy areas. They found strong if complex and varied links over the very short term, suggesting that attention in the national agenda-setting process has considerable influence on policy attention in the states. Indeed, they found evidence of emulation and

substitution as well as of contemporaneous response—in the attention given to different policy topics running from the American national government to the states. This finding raises the obvious question of whether this direction of influence is reciprocated. That is, do patterns of policy attention at the state level then, in the aggregate, influence agenda setting in Washington over the short term? We examine this question by first developing—based on Baumgartner, Gray, and Lowery's (2009) analysis—a series of hypotheses about such linkages. We then test these conjectures with a data set pooling measures of policy attention at the national and state level for several years and several policy areas.

### State Influence on the National Agenda

Policy attention at one level of government might be linked to attention at another level in several different ways. At the heart of all of these mechanisms is the democratic process linking politicians and their constituents. That is, at the most simple, artifactual level, national (Erikson, MacKuen, and Stimson 2002), and state (Erikson, Wright, and McIver 1993) legislators are politicians who have strong incentives to respond to constituent needs. Constituents determine their tenure in office and, as a result, public policy is linked, if in a sometimes complex manner, to public policy. More to the point, state and national legislators share the same constituents. Thus, national and state agendas might well be coupled or at least appear to be coupled as the politicians at different levels of government independently pursue their electoral interests. More substantively, state and national attention to specific policies is hardly segmented in a classic layer cake fashion (Grodzins 1966). Many presumptively state issues—including regulation of health maintenance organizations (HMOs), the death penalty, abortion, and even the fate of Terry Schiavo—have been the focus of Congressional attention. Federal actions or inactions on such issues take place alongside independent state activity. Thus, separate actions at one level of the federal system may well influence the other. The Defense of Marriage Act of 1996, for example, was both plausibly influenced by prior state activity and then DOMA influenced subsequent state legislation. And, at least in terms of the impact of federal activity on state policy agendas, these influences can occur rather quickly. Baumgartner, Gray, and Lowery (2009) report that variations in federal policy attention as measured by numbers of Congressional hearings on a topic had a measurable impact on state policy attention as captured by bill introductions in state legislatures on those topics both contemporaneously and with a one-year lag. Thus, it seems reasonable to expect that a similar pattern of influence—but operating in the other direction—might operate. Several such patterns are possible.

The first is the simple '*contemporaneous effect*' alluded to above with both levels of government struggling simultaneously with a common policy problem or issue.

In this view, legislative agendas at all levels reflect less each other than real policy issues facing society and the political demands of the public for their solution. Several mechanisms might plausibly insure such a common pattern of response to policy problems. Truman (1951, 511), for example, identified the locus of interest mobilization as disturbances in society. Organized interests engage in political activity to secure redress for these disturbances. More to the point, it is not obvious that organized interests seek such redress at different levels of government in a purely sequential fashion. Moreover, legislative entrepreneurs at all levels of government have powerful electoral incentives to monitor their constituents' concerns (Wawro 2000; Weissert 1991; Mintrom 1997). Political parties at all levels win elections by finding issues on which to campaign (Rabinowitz and Macdonald 1989). 'If legislators, parties, and organized interests at all governmental levels respond swiftly to the same disturbances in society, then we should see the content of legislative agendas at both the national and the state level changing in a contemporaneous and non-causal manner reflecting the public's concerns'.

A second possible form of linkage is a *substitution effect* where attention to policies is pursued in different venues provided by our federal structure of government in a sequential fashion. This idea was noted by Truman (1951, 323) and Grodzins (1966), who argued that the federal system can be viewed as a structure with many cracks through which influence may be exercised. Patterns of influence impeded at one level may find opportunities for influence at another. Indeed, state officials often frame their attention to problems as a response to federal inaction or what Krane (2007, 462) has called in the context of environmental policy during the administration of George W. Bush "filling a policy void." Thus, in justifying his state's more rigorous than average environmental laws, former California Governor Gray Davis (2002) noted that, "The federal government and Congress, by failing to ratify the Kyoto treaty on global warming, have missed their opportunity to do the right thing. So it is left to California, the nation's most populous state and the world's fifth largest economy, to take the lead." Similar responses by the states have been noted in regard to state attention to health policy following the failure of President's Clinton's comprehensive health care proposal in 1994 (Gray, Lowery, and Godwin 2007a, 2007b; Gray, Lowery, Monogan, and Godwin 2010). Martha Derthick (2002) has chronicled the successful legal actions of the attorneys general of forty-six states against the tobacco industry after Congress failed to regulate. Perhaps the best current example is immigration where, in reaction to Congress's failure to act in 2007, 1,562 immigration-related bills were introduced in the fifty state legislatures in 2007, triple the number in 2006; 240 of them were enacted into law (National Conference of State Legislatures 2009). Bill introductions and enactments continued at roughly the same pace in 2008, but picked up in the first half of 2009. Such responses on the part of the states may not be entirely a function of neglectful inaction on the

part of the national government. Indeed, in what they call “one of the classic virtues of federalism,” Pickerill and Chen (2007) argue that state experimentation—as with the regulation of medical marijuana—might be encouraged by deliberate and thoughtful national policy inaction when a policy issue remains politically unresolved and controversial.

The inverse—the national government stepping in to fill a void left by state inactivity or incapacity—may be true as well, although very recent examples are perhaps more difficult to identify in the modern era of expansive national government. One classic example is civil rights where the national government addressed the issue only after the states, especially southern states, failed to do so. Nathan (1990) argues that there is an ideological direction to the swing of the federalism pendulum: during liberal periods when society as a whole favors governmental action in a new field or of new kind, proponents will find it more efficient to concentrate their energy on achieving meaningful policy change at the center. And lest the current generation of scholars forget a less recent period of federalism, this was certainly one of the underlying rationales—the incapacity and unwillingness of the states to address critical national issues—offered to justify the expansion of national powers in the period from the depression era through the Johnson administration (Reeves 1990; Bowling and Wright 1998).

A third and we think more typical relationship between Congressional and state legislative activity is a *stimulation effect* reflecting many of the examples we noted earlier. Importantly, such stimulation may be negative, a form of critical response, or positive, a form of emulation.<sup>1</sup> Activity in Washington will necessarily stimulate state law-making in those situations, such as the “No Child Left Behind Act,” where federal acts have significant negative consequences for state laws and regulations. Indeed, state actions may be in the form of a push-back or resistance, as was certainly seen in the case of the “No Child Left Behind Act” (Shelly 2008). Such push-back may be common on policies entailing significant national funding and mandates (Gormley 2006; Dinan 2008) or involving policy changes that represent sharp departures from the status quo of prior federal policy, as was the case with state reactions to a number of national policies during the administration of George W. Bush (Krane 2007). The inverse, of course, may be true as well. When liberal policy activity at the state level—for example, on regulating fire arms, legalizing gay marriage, or allowing assisted suicide—during the administration of George W. Bush threatened conservative mores, the GOP was not adverse to efforts to preempt or at least constrain state policy activity. Critically, such stimulative links in policy attention in the federal systems are rarely considered in the policy diffusion and learning literatures precisely because they are essentially negative or reactant rather than positive or emulative. Still, it seems more than reasonable given these examples to expect that policy attention might well diffuse even if the policy lessons or policy instruments do not.

Stimulation resulting in emulation represents the more classic view of the states as policy laboratories and is often the focus of the diffusion and policy learning literatures. From adoption of the income tax to regulatory policies, states have often taken the lead in attending to emerging policy issues. Such a linkage may better reflect a diffusion of legislative entrepreneurship, where national legislators see that there is electoral hay to be made in following a path already trail-blazed by the states. National interest organizations and party entrepreneurs may mobilize for similar reasons, learning from watching their colleagues in the states. Other scholars place the federal government's receptivity to state policy ideas in the socialization experiences members of Congress had early in their careers. Representatives often served earlier in state legislatures where they internalized state-level policy agendas; once in Congress, they bring these ideas to the table, and thus the Congressional agenda comes to look more like the states' (Berkman 1993). A good example of policy diffusion/learning is the Personal Responsibility and Work Opportunity Act of 1996 which contained the TANF welfare reforms that had percolated up from the states. In the 1980s, many states had experimented with "workfare," "learn fare," and so on within the AFDC program, and their experiences provided some of the substance for the new federal program and, even more important, the political legitimation for stricter policies overall (Hecht 2001, 23). Hecht's analysis showed that nearly 60 percent of states' welfare experiments between 1978 and 1996 were later picked up by the national government (2001, 102).

Other case analyses have been far less supportive of this expectation, however. As noted earlier, Weissert and Scheller (2008) found little evidence that state experimentation on health care led to identifiable action at the national level between 1993 and 2006. Similarly, Boekelman (1992), Mossberger (1999), and Thompson and Burke (2007) found little or no evidence that federal policy was subsequently influenced by state activity on the policies they studied. Still, we do not know if these negative cases are exceptions—albeit important ones—to a more general pattern of policy learning across levels of government or if a broader focus on policy attention—which would encompass both the negative and positive stimulation—would tell us a different story. Even in the absence of policy learning, *attention* to a policy topic by state governments, through either emulation or critical reaction might lead to changes in the attention to an issue at the national level of government.

Still, we must keep in mind a strong *null hypothesis* that state policy activity may have little impact on the national policy agenda. This null hypothesis has a stronger theoretical foundation than merely the negative case results of Weissert and Scheller (2008), Boekelman (1992), Mossberger (1999), and Thompson and Burke (2007). Peterson (1995) argued for a functional theory of federalism in which each level of government focused on policy areas in which it was most competent. This theory explained why the federal government focused on redistributive policies, while state

and local governments concentrated on developmental policies. And certainly, despite ever more rapid diffusion of policy innovations, not all states focus on the same issues at the same time (Gray et al. 2005). Thus, it is not obvious that the states as a whole might influence the national policy agenda in a systematic manner. And even if state agendas moved together in lockstep, much of what attracts the attention of state legislators may well not be what concerns their national counterparts. This would be especially true for a number of issues that are mainly influenced by national policy or on the other hand mainly by state policy. State attention to corrections policy, for example, may be partly related to federal concerns. Still, the states are the primary force behind corrections policy whereas the federal role is very limited. The reverse is true with regard to other policies of broader national concern, such as nuclear proliferation. Thus, different policy areas of concern to the states feature more or less involvement of the federal government. This might well suggest that, at best, we should expect a more limited impact of state policy activities on the distribution of federal attention to issues in those areas where the states play relatively little role. And last, given Baumgartner and Jones' (1993) punctuated equilibrium model, legislative agendas are quite sticky, changing only periodically and with some difficulty. If so, then it is not clear that the national policy agenda would respond in anything close to a contemporaneous manner to activity at the state level. For example, the collapse of President Clinton's national health care proposal in 1994 arguably did lead to greater state attention to health care, but this developed rather slowly over several years as the realization that the national government was, at least for a time, out of the health care game (Gray, Lowery, and Godwin 2007a, 2007b). "In short, there are also plenty of good reasons to not expect to find a strong short-term relationship between aggregate levels of state policy attention and such attention at the national level."

## Exploring the State-National Agenda Connection

### Design and Data

Examining these hypotheses at the macrolevel is difficult given our need to broadly measure both federal policy agendas and the policy agendas of the fifty states over time. Quite simply, generating time series data on the latter is an extremely time-intensive activity. To address this problem, we employ two existing data sets on, respectively, Congressional and state agenda setting in a pooled cross-sectional, time series research design, thereby producing a data set on policy attention to a smaller number of policy topics over a relatively short set of years.

The *dependent variable* in the analysis is change in national policy attention, which we measure by numbers of Congressional hearings on a topic from the Policy Agendas Project ([www.policyagendas.org](http://www.policyagendas.org)), a now standard measure of



Washington policy attention (Baumgartner and Jones 1993; Jones and Baumgartner 2005). The Policy Agendas Project categorizes hearings into 226 distinct subtopics. To match these data to the measure of state policy attention, these data were reorganized into twelve policy areas with clear matches with the state data on policy attention.<sup>2</sup> There were a number of policy areas coded by the Policy Agendas Project with no clear match to the categories in our measure of state policy attention. These were necessarily excluded from the analysis. In still other cases, the level of analysis in the state data was not as deep or refined as those in the federal data. And finally, the state policy codes simply grouped policies somewhat differently. The latter two situations necessitated combining the national hearings data in new ways. In the end, there were twelve matches between the reorganized Congressional hearings data and the measure of state policy attention.<sup>3</sup> Our Congressional hearings data include, then, information on numbers of hearings on agriculture, banking and finance, communications, education, local government, law, health, insurance, natural resources, transportation, utilities and energy, and welfare. As seen in figure 1, the twelve policy areas vary markedly in terms of number of hearings held on the topics, from only a handful on insurance to more than a hundred in some years on health care.

More specifically, we examine percentage changes in the number of Congressional hearings on these twelve issues over several years for which comparable state data were available. The years vary somewhat across our analysis given the availability of data on both independent and dependent variables. That is, the two series do not fully overlap. But on the dependent variables, we examine changes in number of hearings in 1995–1996, 1996–1997, 1997–1998, 1998–1999, and 1999–2000. Most of our data, then, cover the period of President Clinton's second term. This period has both advantages and disadvantages. In terms of the former, this was clearly a period of active state policy agendas in a number of important areas. For example, the states actively struggled with large and complex health policy agendas. In part, the states were *reacting* to a lack of national policy activity following the collapse of the Clinton national health care proposal earlier in the decade (Gray et al. 2010). On the other hand, states were also actively experimenting with policy innovations ranging from welfare reform to enterprise zones that *attracted the attention* of national policy makers. Thus, this could have been a time period in which several different forms of linkage across the policy agendas of the two levels of government could be expected to have been observed. But there are also two limitations of the period examined here. First, anecdotally at least, many federal domestic policy initiatives occur early in the first term of a new administration, not during the second term. And second, there was only a limited variation in party control in Washington over the period, with Democrats controlling the White House and Republicans the Congress. Still, within these



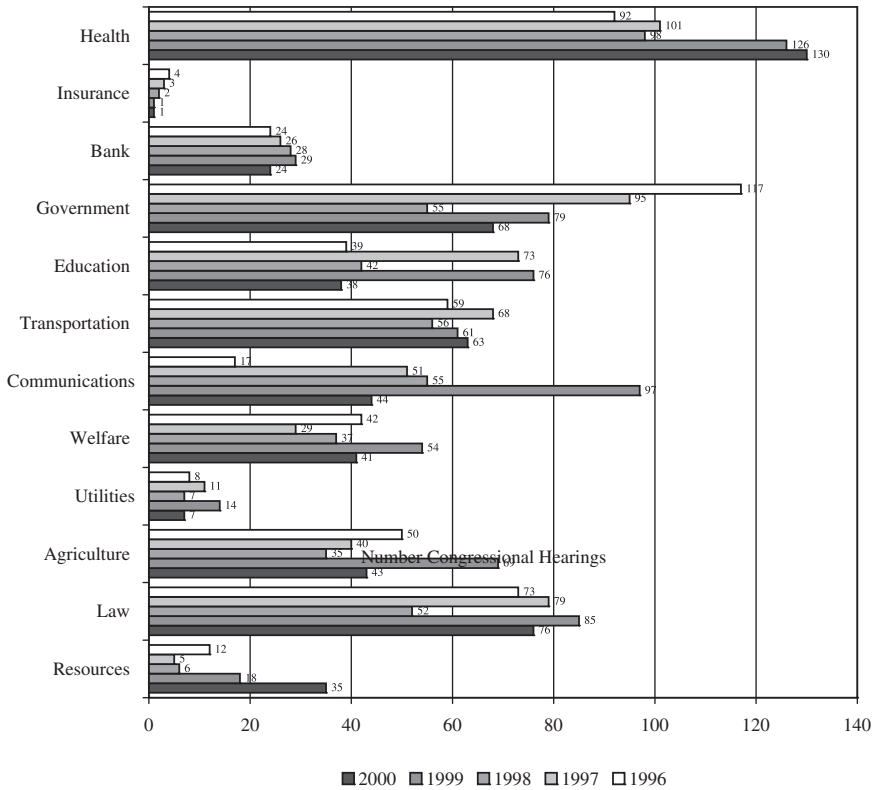


Figure 1 Congressional hearings by topic, 1996–2000.

limitations, these are the best data available to test our hypotheses about the linkages across aggregate policy agendas within the federal system.

Given the use of lagged values in the full model, only three sets of change scores are used in our most complete specifications, giving a pooled *N* of thirty-six (twelve policy areas over three years). In other, more partial specifications excluding one or another of the several independent variables, fuller forty-eight sets of change scores are used (twelve policy areas over four years). We have already noted the significant variation in hearings across the twelve topic areas in figure 1. Even more importantly, given our attention to percentage changes in number of hearings from one year to the next, figure 1 also suggests that there is more than sufficient variation from year to year within the policy areas to evaluate policy change. That is, the twelve policy topics do not show a common pattern of change across even the few years of hearings we examine, with perhaps the exception of fewer hearings in election years. Indeed, the simple correlation of the pooled 1996–1997, 1997–1998, 1998–1999, and 1999–2000 change scores and their lagged

values is only  $-0.254$ , with the negative value reflecting the cycle of Congressional hearings over the election cycle, with fewer hearings on most topics in election years.

The key *independent variable* is change in level of attention to the twelve policy topics across all fifty states. Lacking comparable and reliable hearings data on the states, our measure of policy attention is provided by Gray et al.'s (2005) use of bill counts, which builds on the bill count strategy pioneered by Bowling and Ferguson (2001).<sup>4</sup> In doing so, we rely on the general findings of the policy agendas literature to validate our indicator. This literature suggests that, despite having their own dynamics, there are persistent and strong relationships across multiple policy agendas running from media attention, legislative activity, and budgetary outcomes (Baumgartner and Jones 1993; Jones and Baumgartner 2005). More specifically, the validity of comparing agendas on these two measures is appropriate for three reasons. First, across all of the policy agendas studied in the policy agendas literature, these two—bill introductions and committee hearings—are quite proximate to each other in the policy process. We are not making a broader or longer leap of faith by, for example, comparing the media agenda with budgetary outcomes. Second, prior work looking at the relation between Congressional committee hearings and state bill introduction, albeit in the other direction, (Baumgartner, Gray, and Lowery 2009) has found systematic associations between the two indicators. And third, prior work on the national Policy Agendas project (Wilkerson et al. 2002) suggests that the levels of policy attention reflected in bills and hearings as policy agendas are closely related to each other and that both bills and hearings are equally capable of identifying temporal patterns of changes in policy agendas. Thus, we believe our comparisons to be valid. We measure policy attention by either the total number of bills on the twelve topics introduced in the state legislatures in each year from 1995 through 1999 or the number of bill introductions weighted by state size. In either case, change in state attention is measured by the percentage change in the total number of bills from 1995 to 1996, 1996 to 1997, 1997 to 1998, and 1998 to 1999.<sup>5</sup>

As seen in figure 2, there is considerable variation in policy activity across the twelve topics and across years within topics. In terms of the former, it should be noted that the patterns of high and low policy attention in figure 2 for the states only partially map those reported for Congressional hearings in figure 1. Health policy, for example, has drawn a great deal of attention at both the national and state level over this period. In contrast, insurance generated very few Congressional hearings, but it has been the second most active area of bill introductions in the states. And while law has attracted considerable attention at the national level, it was one of the quieter areas in the states in terms of bill introductions. More relevant for our purpose, there seem to be few secular trends in the data reported in figure 2 with the possible exception being the somewhat lower bill counts in the

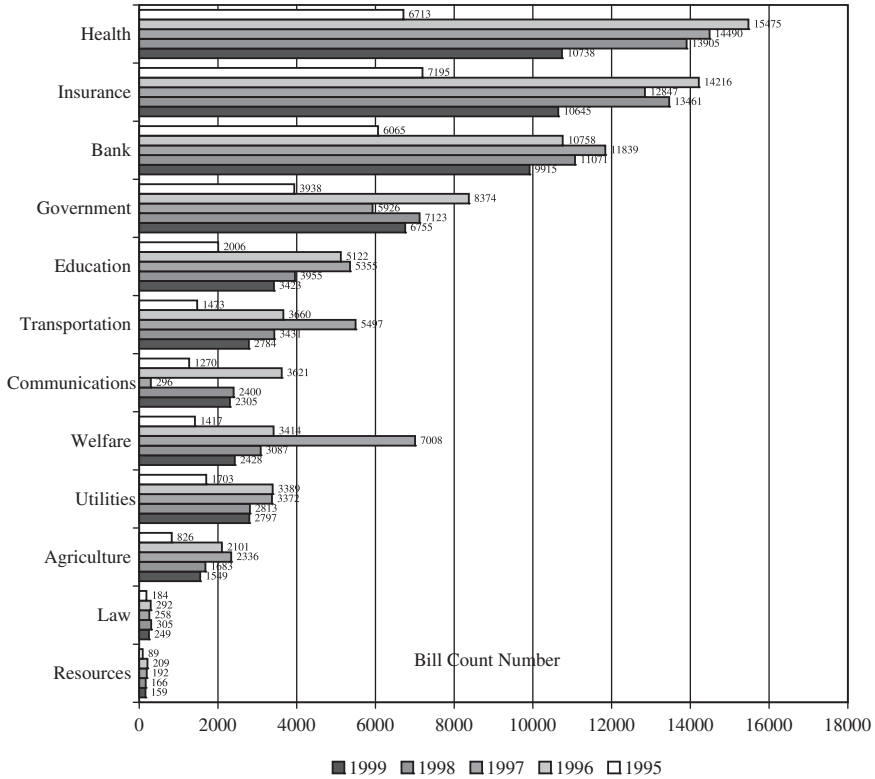


Figure 2 State bill count number by topic, 1995–1999.

first year of the time series, something we will address below.<sup>6</sup> There is also some periodicity in our ultimate independent variable, even if it is not entirely consistent across all of the policy agenda topics we examine. That is, the simple correlation of the pooled 1995–1996, 1996–1997, 1997–1998, and 1998–1999 percentage change scores with their lags is  $-0.533$ . This does not seem to reflect the fact that many state legislatures are either biennial or run short sessions every other year. Examining the count data indicates that bill introductions continue in all legislatures over both years of the legislative cycle. Rather, we believe that this periodicity reflects the same electoral cycle found in the Congressional hearings data. More importantly, the correlation of  $-0.533$  is sufficiently low in magnitude as to ensure us plenty of variation in changes of attention even across our very short time series.

To this point, our measure of state policy attention might well be criticized because it is a simple aggregate of all bill introductions in the states. It weights all states equally. A bill introduced in the Iowa Legislature counts as much as one

introduced in the California Assembly. This is not entirely implausible given that all states have two U.S. Senators who might pay attention to what their state lawmakers are doing. Further, in the age of the Internet, good ideas can be rather quickly found anywhere using Google. Indeed, some smaller states have proven especially innovative—such as Vermont and Maine on health policy. Still, there are also plausible reasons to expect—given our earlier discussion—that not all states will be equal. That is, if legislative entrepreneurship is what matters in the diffusion of attention, then we might expect that policy activity in states with more House members in Washington might be more influential and more likely to vertically diffuse policy ideas if they are attentive to what is happening in their own states. But it might be even more than simple number of legislators. That is, the better developed media market of a larger state like New York might more readily trumpet news about New York state policy activity that piques the interest of a legislator from Utah than his or her own more limited media market does about policy activity in the Utah legislature. Further, larger states tend to have more professional legislators, which might lead them to be more innovative in their attention to new issues or more dedicated in pursuing on-going concerns. Indeed, the correlation of the Squire index of state legislative professionalism (Squire 1997) and 1997 Gross State Product, one measure of state size, is fully 0.723. Thus, we might expect that Congress pays more attention to what is happening in larger states.

For all three reasons, we also employ a weighted bill count indicator that is multiplied by 1997 state Gross State Product in 100 billions.<sup>7</sup> The values for this weighted indicator are reported in figure 3. The broad pattern of attention appears to be quite similar to that reported for the unweighted bill count measure in figure 2 when we attend to the twelve broad topic areas. Indeed, the simple correlation between the weighted and unweighted measure is 0.992. But a substantial part of this high correlation is a function of the significant differences between the several topical groups of annual measures. Thus, all of the percentage changes in health bill introductions are greater than those for natural resources. Considerably, more variation is observed when looking at the correlations between the weighted and unweighted measures. That is, while most are above 0.900, the correlations for insurance (0.419), health (0.673), and government (0.764) point to considerably weaker relationships. Indeed, the correlation between the two measures for law bill introductions was  $-0.074$ . For this reason, we use both measures. Finally, the simple correlation of the pooled 1995–1996, 1996–1997, 1997–1998, and 1998–1999 weighted percentage change scores in legislative bill introductions with their values lagged one year is  $-0.492$ . Again, we should have sufficient variation in changes of attention over our series.

To this point, our model includes only one variable—if in a weighted and unweighted form—to explain variation in the percentage change in Congressional

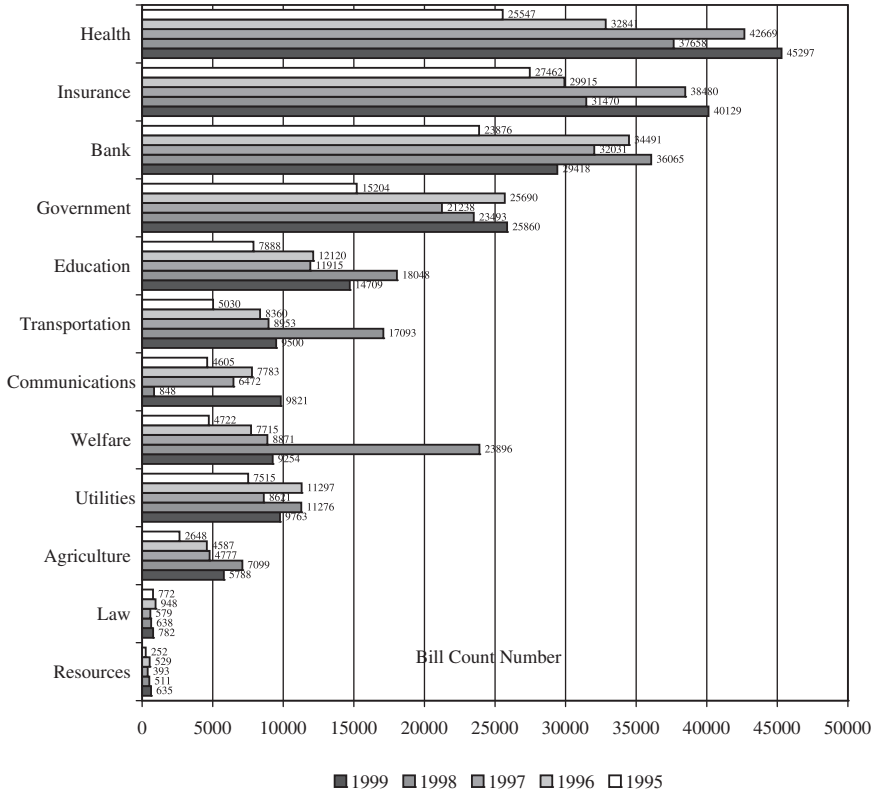


Figure 3 Weighted state bill count by topic, 1995–1999.

hearings. But, based on similar analyses of how Congressional policy attention influences state attention to issues over the short term (Baumgartner, Gray, and Lowery, 2009), this one variable should be able to tell us a great deal about how short-term changes in state policy agendas influence those at the national level. Most importantly, we include our percentage change in total state bill count variable in the model in both its contemporary and lagged form. If the estimate of the contemporary value of change in state policy attention generates a positive value, this would indicate that both the states and the Congress are responding to pressures for more or less policy attention in the same manner at the same time. This would provide, thus, support for the ‘*contemporaneous effect*’ hypothesis. In contrast, none of the hypotheses would lead us to expect a negative estimate for the contemporaneous measure of change in state policy attention. It would not, for example, be indicative of a substitution effect since some time would need to pass in which states failed to respond to some pressure or opportunity for policy change

before federal activity could be viewed as a compensatory response. Thus, we use a one-tailed test of this hypothesis.

Inclusion of the lagged value of change in the percentage of attention to policy in the states allows us, in contrast, to distinguish the *substitution effect* and *stimulation effect* hypotheses. If the estimate of the lagged value is negative, this would suggest that a weakening of state attention to an issue in one year leads to more attention to it by Congress in the next. Or, more attention to the issue in the states in one year may signal that Congress does not need to be so attentive to it in the following year. This result—a negative relationship between Congressional attention and the lagged value of change in state bills—would support the ‘*substitution effect* hypothesis; national and state attention to policy issues replace each other. In contrast, a positive value for the lagged measure of change in state attention would provide support for perhaps the most common expectation about the link between federal and state policy processes—the *stimulation effect* hypothesis. Here, activity in the states would signal to members of Congress that it would be politically useful to follow suit or perhaps even to preempt state activity. Given these competing expectations, we employ a two-tailed test for the lagged measure.

We include, of course, several other variables in the model as general controls. First, the lagged nominal value of Congressional hearings is included as a control for ceiling or floor effects in the rate of change in policy attention. That is, the value of a given percentage change in hearings on a given topic may differ depending on whether there are already few or many such hearings. And we have seen some evidence of periodicity, if not entirely consistent across all of the policy areas, in both the national and state measures of change in policy attention. This is potentially troubling because it could bias our results toward finding a relationship between the two. We also saw (see note 6) that the 1995 observations on bill counts tend to be systematically lower than those for later years. To control for this as well as artifactual synchronicity associated with the electoral and state legislative cycles, we include a set of year dummies in the estimating models. Again, given a full set of year dummies, all election effects—state, presidential, and congressional—should be controlled as well as any other processes operating exclusively on an annual level across all of the topic areas. Finally, in terms of model specification, given the relative skimpiness of the specification in terms of substantive variables, we employ a set of dummies for eleven of the twelve topics as general controls for their unknown difference that might influence the results on our substantive variables, something that is important given that the above noted periodicity in the data is not fully consistent across all of the policy areas.

Our pooled model is estimated via LSDV regression with robust standard errors clustered on the policy topics. We do not present, however, the results for either set of dummies given their lack of substantive relation to our core hypotheses about the linkage of national and state policy agendas, although the full results are

available on request. We also do not include measures of party control of the Presidency or Congress since the GOP was in charge of the latter and the Democrats the former during all of the years we examine. More importantly, theoretically, we are examining policy attention which, unlike policy diffusion and learning, may entail negative as well as positive interactions across the levels of government. Thus, while a Democratic Congress might well have a *positive stimulative* response to a pattern of liberal policy adoptions in the states while a Republican Congress would have a *negative stimulative* response, both would result in the same positive estimate for the lagged value of state policy attention. Thus, because the focus of the agenda literature is on the level of attention and its timing, not its direction, patterns of party control of Congress or state legislatures should not matter. Similarly, we explicitly chose not to include measures of interest group activity in the states on the several topics given prior work demonstrating that interest group activity in the states lags or responds to changes in state policy agendas rather than leading those agendas as is often supposed (Lowery, Gray, and Fellowes 2005; Lowery et al. 2004). Finally, given the low number of observations and the use of a full array of dummies for the policy agenda topics and years producing up to seventeen independent variables, a simple interpretation of the coefficient estimates may at least potentially be complicated by problems of collinearity, an issue we will discuss fully following presentation of the initial results.

## Findings

The LSDV regression results for the models including the unweighted and weighted percentage change in bill measures of policy attention are presented in tables 1 and 2, respectively. Given their striking similarity, however, they can be discussed together. Across all of the models, the lagged nominal value of number of Congressional hearings produces uniformly negative estimates that are discernibly different from zero in six of the eight models. This suggests that percentage change in number of hearings is smaller (larger) when there are already large (fewer) numbers. Thus, there is some suggestion of a ceiling/floor effect. Still, this effect is not especially strong since significant estimates are discernible at only the 0.10 level, a relaxed criterion we employ given the relatively few degrees of freedom.

Turning to our central concerns, the models provide almost no support for any of our substantive hypotheses.<sup>8</sup> Model one in each of the tables reports the results for models including both the contemporary percentage change in bill counts and its lagged value. These models are estimated with the thirty-six cases representing change in Congressional hearings from 1996–1997, 1997–1998, and 1998–1999, the cases for which we have complete data given inclusion of the lagged value of percentage change in state policy attention. In model 1 of both tables, the estimates



**Table 1** Pooled LSDV regression analysis of state determinants of changes in Congressional hearings with unweighted measure of state policy attention

Independent variables	Dependent variable: Percent. change in hearings by subject					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Number of hearings $t-1$	-0.064#	-0.029	-0.048#	-0.061#	-0.058#	-0.064##
Percent change in state bill introductions $t$	0.154	0.054	-	0.087	-	0.154
Percent change in state bill introduction $t-1$	0.601	-	0.091	-	-0.142	0.601
Constant	1.747	1.513	2.040	2.105	2.349	1.747
$R^2$	0.622	0.455	0.552	0.616	0.606	0.622
$N$	36	48	48	36	36	108

Figures under the coefficients are  $t$ -values estimated with robust standard errors clustered on twelve topic areas; the models were estimated with a full set of year and topic area dummies, the estimates of which are not reported. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ , two-tailed tests; # $p < 0.10$ , ## $p < 0.05$ , ### $p < 0.01$ , one-tailed tests

**Table 2** Pooled LSDV regression analysis of state determinants of changes in congressional hearings with weighted measure of state policy attention

Independent variables	Dependent variable: Percent change in hearings by subject					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Number of hearings $t-1$	-0.061	-0.029	-0.048#	-0.061#	-0.059#	-0.061##
Percent change in weighted state bill introduction $t$	0.079	0.064	-	0.096	-	0.079
Percent change in weighted state bill introduction $t-1$	-0.127	-	0.085	-	-0.376	-0.127
Constant	2.046	0.935	2.017	1.988	2.622	2.046
$R^2$	0.618	0.456	0.550	0.617	0.613	0.618
$N$	36	48	48	36	36	108

Figures under the coefficients are  $t$ -values estimated with robust standard errors clustered on twelve topic areas; the models were estimated with a full set of year and topic area dummies, the estimates of which are not reported. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ , two-tailed tests; # $p < 0.10$ , ## $p < 0.05$ , ### $p < 0.01$ , one-tailed tests.

for the contemporaneous value of percentage change in state policy attention are positive, which hints at a '*contemporaneous*' effect. More problematically, the estimate for the lagged unweighted measure of percentage change in state bills is positive in Model 1 of table 1, which suggests a *stimulation* effect, but negative for the lagged weighted of percentage change in state bills in Model 1 of table 2, which suggests a *substitution* effect. But despite the hints of support for all three types of effect, the more problematic result is that none of these estimates are statistically discernible at even relaxed criterion values.

In short, we have failed to find evidence supportive of any of our substantive hypotheses, which suggests that the null hypothesis must be maintained, something we have already suggested is not itself without theoretical foundation. Before opting for the null, however, one important rival explanation for our findings remains: the problem of collinearity. That is, with relatively few observations (thirty-six) and seventeen independent variables (including the year and topic dummies), our analyses may simply lack sufficient statistical power to identify with any sensitivity what are in fact substantively important relationships. And, indeed, there is evidence of considerable collinearity in the estimates. (We largely focus again on the unweighted results since the unweighted results were essentially identical.) When each independent variable in Model 1 of table 1 was regressed on the remaining independent variables, the resulting  $R^2$ -values for the three substantive variables were as follows: lagged Congressional hearings ( $R^2=0.899$ ), percent change in state bill introductions (0.685), and the lagged percentage change in state bill introductions (0.837). Thus, there is considerable collinearity in the estimates.

More to the point, does this high level of collinearity account for our null findings? For several reasons, we think it unlikely. First, the highest level of collinearity, as evidenced by the auxiliary  $R^2$ -value of 0.899, was observed for lagged Congressional hearings, an independent variable for which we were able to generate statistically discernible estimates in all but one of the models in both tables 1 and 2. In interpreting these estimates, we admittedly employed somewhat relaxed criterion-levels (one-tailed tests, 0.10  $\alpha$  level). Still, the  $t$ -values of *all* of the estimates for lagged Congressional hearings are higher than *any* of the estimates for percent change in state bills or its lagged values, most of which were considerably smaller than 1.00. Our finding of a significant effect for this variable and not the others is, therefore, not an artifact of employing different criteria for the several independent variables. Rather, it is likely that the large number of independent variables and high levels of collinearity, per se, did not preclude finding significant effects for the most collinear of the independent variables.

Second, much of the collinearity for our two independent variables of most substantive interest for our hypotheses, percent change in state bills and its lagged value, is due to their somewhat strong relationship with each other ( $r=-0.533$  for the unweighted measure;  $r=-0.492$  for the weighted measure). When one or the

other is removed from model, as is done in estimates presented in Columns 2 and 3 of table 1, the  $R^2$ -values generated by regressing these two variables on all of the remaining independent variables fall considerably—to only 0.276 for percent change in state bills in model 2 and 0.351 for its lagged value in Model 3. And in addition to considerable lower levels of collinearity, the coefficients in Models 2 and 3 in table 1 were estimated using more cases (forty-eight instead of thirty-six) as a result of dropping one or the other variable, thereby reaching one year earlier or one year later in the limited time series available to us than is possible if both the contemporary and lagged state bill variables are included in the model at the same time.<sup>9</sup> In column two in both tables, with the inclusion of twelve more cases from 1995–1996, percentage change in state bills generated the expected positive estimate (contemporaneous effect), but again neither is significant. And in column three of both tables, with the inclusion of twelve other cases from 1999–2000, percentage change in state bills also generated positive (stimulation effect) estimates, but again not significant. As a check to see if our inclusion of the additional cases in Models 2 and 3 influenced the results, Models 3 and 4 of each table re-estimate them with the original thirty-six cases used to estimate the full models in both tables. While the estimates for the lagged value of percentage change in bills in Model 5 in both tables switch sign, they are again not significant at even relaxed criterion values. In short, even when we sharply reduce the collinearity in the model through recourse to partially specified models, we do not generate significant estimates for our state bill variables.

Third and finally, we assessed the severity of the collinearity problem by artificially inflating the number of cases in the analysis, thereby simulating what the effect of greater statistical power would be while holding all else—the actual values of the variables—constant. That is, we replicated the number of cases used to test Model 1 in table 1 three times to produce 108 cases rather than thirty-six. This simulated data set contained no new information about the distribution of the variables since it is merely our original data set reproduced three times. Thus, keeping in mind that collinearity generates inflated standard errors rather than biased slope coefficients, the coefficient estimates generated from re-running model 1 in table 1 and reported in Model 6 in that table remain identical. Still, this model has much more statistical power, albeit generated artificially. More to the point, does this increased statistical power enable us to discern a significant effect in the estimates? The answer is no. With 108 cases, the slope of 0.154 for percent change in state bills generated a  $t$ -value of only 1.340 and a probability value of only 0.104, one-tailed test. The slope estimate of 0.601 for the lagged value of percent change in state bills generated a  $t$ -value of only 0.910 and a two-tailed probability value of only 0.381. Similar results reported for the weighted measures in Model 6 of table 2 are even weaker. Thus, the lack of discernible effects for these two variables does

not seem to result solely from limited statistical power in the face of high levels of collinearity. Instead, for all three reasons, we think it more likely that attention to policy agendas at the national level is not closely linked in any of the ways we have identified to attention to policy agendas in the states.

## Conclusion

Contrary to traditional expectations that the states are ready policy laboratories for the national government, we find little evidence that changes in state policy agendas *in the aggregate* influence national patterns of policy attention. By default, our failure to find support for the substantive hypotheses lends support to the null expectation as already supported via a number of studies, as discussed earlier, using less aggregated data and focusing on specific policy topics (Weissert and Scheller 2008; Boekelman 1992; Mossberger 1999; Thompson and Burke 2007). But as noted in our discussion of the null hypothesis, there are good substantive reasons to expect that policy attention in the states in the aggregate will have little direct impact on patterns of policy attention at the national level over the short term. Perhaps most importantly, while federal funding and regulatory activity ensure that the national government can exercise influence over policy making in the states (Gormley 2006; Dinan 2008; Krane 2007), the obverse is not nearly so clear. The states can rarely compel the national government to pay attention to issues that concern them or to avoid dealing with issues that they wish to keep under the rug. In this sense, the failure to find evidence of either a *substitution* or *stimulation effect* running from the states to the national government comparable to the one found by Baumgartner, Gray, and Lowery (2009) running in the other direction is perhaps understandable.

At the same time, however, our null results agree with elements of their findings. While they found that the national agenda has numerous and quite complex impacts on the states' attention to issues, they did not find that that Congressional hearings had a contemporaneous positive response on state bill introductions. Our results also indicate that this perhaps most common expectation of democratic polities—that both national and state legislators would respond to common problems at the same time—may not be valid. To a considerable degree, state and national legislatures still have their own policy agendas and their own policy cycles. More generally, we do not think that our null findings result from our policy attention measures or because of our attention to short-term influences. Baumgartner, Gray, and Lowery (2009) certainly demonstrated such linkages, excepting the *contemporaneous effect* just noted, with similar data when the causal arrow was reversed. At the same time, however, we must caution that our findings were based solely on observations from well within the term of a single administration in Washington and covered only a single configuration of party

control of the institutions of national government. Further work is thus needed to further probe these findings.

These results matter, we think, because we have an interest in such powerful metaphors as “laboratories of democracy” and such important literatures as that on vertical policy diffusion and policy learning. Both focus on specific policy solutions that are picked up from one government by another. But attention to such specific policy solutions presumes a prior diffusion of policy attention. Someone must have already perceived a problem for which he or she must find a solution. And indeed, their shared attention may lead to quite different policy responses, something that would indicate a diffusion of attention with a diffusion of policy instruments. Nevertheless, our results suggest that, at least over the short term, there is little transfer of policy attention from the states to the national government. This does not mean, of course, that the national government does not learn from the states in specific cases. We have too many very specific examples ranging from the income tax to zero-based budgeting to suggest that. And clearly, it would be useful to extend our analysis to a longer-term consideration of linkages between patterns of policy attention. But over the short term, at the macro-level, and for the period we have examined, albeit covering a broad array of policy topics, patterns of attention in the states have far less impact on policy attention at the national level than does attention at the national level of government on the states.

## Notes

We appreciate the assistance of Galen Irwin in helping to facilitate our collaboration. F.R.B. acknowledges the support of National Science Foundation (grant # 0111611) for data collection and Bryan Jones for related research projects from which this article draws.

- 1 A harsh form of negative stimulation is “preemption”—when federal action precludes state action on an issue (Pickerill and Chen 2007). A good health care example occurred in 1974 when Congress enacted the Employee Retirement Income Security Act (ERISA), which preempts state laws that “relate to” employee benefit programs (including health plans) unless such laws are part of the traditional state function of regulating insurance. The 1996 DOMA arguably attempted preemption as well in the face of state actions and proposed actions on gay marriage.
- 2 The matching procedures are described more fully in Baumgartner, Gray, and Lowery (2009, available at *Political Research Quarterly Online First*, <http://prq.sagepub.com/cgi/rapidpdf/1065912908322407v1>).
- 3 For details on the matching of state bill topics and the Policy Agenda Project categories, see Appendix 1, available at Publius online.
- 4 Several measures of state agendas were considered. Ferguson (1996) measured the governor’s legislative agenda in all fifty states through a content analysis of the 1994

“state of the state” speeches. Fording, Woods, and Prince (2002) analyzed thirty-seven 1999 “state of the state” speeches, identifying nine different policy initiatives pursued by governors. Perhaps the measure best matching our needs is Gerald Wright’s collection of roll call data for all 7,424 legislators between 1999 and 2000 (Wright and Winburn 2002). While each of these measures of legislative agendas has virtues, our analysis requires a measure of legislative activity in many different issue areas, a level of specificity that is not reached by extant measures. Further, we required a measure of the entire state legislative agenda, and not only bills of high priority to governors or those with roll calls.

5 The bill count data was collected from the “State Full Text of Bills” database on Nexis Academic Universe, a database maintained by LexisNexis, a division of Reed Elsevier Inc, and is available at <http://www.nexis.com>. The database contains bill text files for all bills considered by each statehouse in a calendar year and provides a separate listing for each revised version of a bill in the database. For example, Alabama House Bill 175, which appropriated \$4,564,831 to the Department of Public Health in 1997, was listed five times in the database: one entry was the introductory version, three were revisions, and the fifth was the enacted bill. In most cases, we used their search terms to code the number of times that a state bill was considered with content germane to each guild’s interests. In some cases, however, additional subject or topic search terms were created when the provided search terms did not include a term corresponding with our guild topics. The banking guild, for example, includes both banks and real estate organizations. In such cases, multiple search terms were employed to tap this diversity. The search terms for the twelve guilds were as follows, with the search terms in parentheses: Agriculture (agriculture), Bank (banking and finance, real estate), Communications (media, telecommunications), Education (education), Health (health), Insurance (insurance), Law (legal), Government (local municipality, public employees, police, fire), Resources (gas, oil, minerals), Transportation (highways, transit, airports), Utilities (utilities), and Welfare (social services, charities). Two issues concerning our measure of the size of the policy agenda facing each interest guild deserve further comment. First, we do not believe that the search terms provide a comprehensive count of all of the bills the several guilds attend to as they lobby state legislators. Rather, the measure is designed to tap variations in legislative activity across states and across guilds. After reviewing the issue counts, we are quite confident that they tap this variation. States with extensive natural resources, for example, generated much higher bill counts than those without oil, natural gas, or mining industries. Second, as noted earlier, some bills are counted more than once if they were revised as they moved through the legislative process. Rather than a drawback, we view this aspect of the coding scheme as quite appropriate for our purpose. That is, policy attention should be heightened as bills proceed further on the road toward becoming law. Our coding scheme taps this greater attention.

6 We are concerned about the 1995 bill count data: its low totals raise the question of whether Lexis/Nexis was getting truly reliable counts this far back. We address this concern by including year dummies in the main model and estimating several models

excluding the suspect 1995 data. The problem also highlights, however, one of the difficulties in extending this analysis further in the past.

- 7 An alternative weight could be population. However, state population is correlated with GSP at the 0.99 level, which suggests that selection of one or another is an entirely arbitrary issue.
- 8 We also examined a number of models that, following Baumgartner, Gray, and Lowery (2009), interact each measure of state-level policy attention with a variable measuring whether the policy area involved considerable federal involvement with state policy through funding or regulation. These more extended set of results also failed to generate any evidence in support of our substantive hypotheses.
- 9 As noted earlier, the series of the dependent and independent variables do not fully overlap, which means that we necessarily lose one year's worth of cases if both the contemporaneous and lagged values of percentage change in state bills are included in one model.

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