Stalemate in the States

Negative Agenda Control, Veto Players, and Legislative Gridlock in the American States

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Abstract: This paper examines how negative agenda control influences policy change in the American state legislatures, by conceptualizing of negative agenda control as the introduction of an additional "veto player" into a state legislature. In doing so, I use the logic of "absorption" detailed by Tsebelis (2002) to synthesize predictions generated by Cox and McCubbins (2005) and Krehbiel (1998) to show that negative agenda control increases legislative gridlock. More specifically, I argue and find: 1) that the largest distance between any two veto players in a political system (i.e., the size of the "core" of a political system) positively correlates with gridlock, and 2) even conditional on a state's preference dispersion/polarization, negative agenda control drives policy change downward. To date, few studies have examined legislative gridlock in the American states, and even fewer have made use of prominent theories of policy change in the Congress and comparative legislatures literatures. This paper seeks not only to do just that, but also to suggest a means of synthesizing two major theories of policy change in American government using a prominent comparative theory of policy change (Tsebelis 2002). Finally, along the way, it also develops a new, easily calculable measure of bill "significance" at the state level (using ACA implementation bill counts from the National Council of State Legislatures), enabling it to add to previous studies by incorporating important differences between bills when assessing the magnitude of policy change.

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For decades now, scholars of American politics have studied legislative gridlock, exploring both its determinants and ramifications. Explanations for gridlock have taken many forms, with some studies focusing on the importance of interest-group diversity, others on ideological polarization, and still others on divided government and separation of powers. Results in these studies have proven similarly diverse. Indeed, nearly every purported explanation of gridlock has received some level of both substantiation and lively criticism. Still, such studies have remained consistent in at least one important regard. That is, they have focused almost entirely on the U.S. Congress. Indeed, scholars have shown only limited interest in applying theories of legislative gridlock to "out of sample" cases, such as the U.S. state legislatures.

Limiting studies of gridlock to Congress limits our understanding of gridlock in a number of regards. First (and perhaps most obviously), relatively little is known about how common determinants of gridlock in Congress might influence gridlock in other important legislatures, such as those in the American states. While scholars of Congress have carefully examined the effects of party power, preference polarization, divided government and many other factors, such examinations have not occurred to the same extent within state governments. Second, by focusing on just one legislature/government, congressional studies of gridlock lack variance on a number of important institutional variables. In particular, despite the importance of partisan agenda control to studies of American legislatures in the past two decades, scholars have been limited by the fact that Congress has itself changed very little in its agenda-control characteristics over the past century. The American states offer variance on agenda, yet comparatively few studies have exploited this institutional variety in an effort to better understand gridlock/policy stability.

This study begins to fill this gap by examining how negative agenda control and preference distance between institutional pivots influence gridlock in the American states. Using theoretical findings from George Tsebelis's *Veto Players* to synthesize canonical theoretical expectations on policy change in Congress, I show that the number of and preference distance between partisan and institutional veto players in state governments influences legislative gridlock at the state level. More specifically, I find that: 1) negative agenda control increases the size of a state's gridlock interval or "core"; 2) core size positively predicts a state's levels of gridlock; and 3) even when conditioning on distance between institutional pivots (Krehbiel 1998), the presence of negative agenda control positively predicts gridlock. Taken together, these findings provide support for the idea that negative agenda control introduces a new, partisan veto player into a system of governance (Tsebelis 2002), thereby increasing gridlock in the American states.

The paper proceeds as follows. First, I examine current literature on congressional and state legislative gridlock. I argue that state-level gridlock has remained understudied, and that current studies of congressional gridlock suggest important extensions in the study of state legislatures. Second, I demonstrate how negative agenda control may increase gridlock, by relating Tsebelis's *Veto Players* to relevant literature on agenda control in American government. Third, I test three hypotheses about agenda control, core size, and gridlock, using both standard measures of gridlock, as a well as a new measure involving Affordable Care Act implementation. Finally, I conclude with a discussion of some implications of these findings.

LEGISLATIVE GRIDLOCK IN AMERICA: WHAT ABOUT THE STATES?

Legislative Gridlock in Congress

While scholars have studied policy stability and change in the American context for decades, the study of legislative gridlock in particular was rekindled in Mayhew's classic study, *Divided We Govern* (1991). Mayhew's major finding in the study was as simple as it was striking: divided government did not positively predict legislative gridlock in Washington. To arrive at this conclusion, Mayhew compiled a large dataset of "major" legislative successes, by tediously scouring the pages of the *New York Times, Washington Post,* and end-of-year *Congressional Quarterly* wrap-ups. Using these counts of major legislative successes, Mayhew found divided government did not lead to gridlock during the years examined in his sample (1946-1991).

This finding and its puzzling nature was met with dozens of tests and retests throughout the 1990s. Indeed, numerous scholars attempted to show that Mayhew was simply mistaken: divided government surely ought to lead to gridlock. Nevertheless, as summarized by Fiorina (1996) and others, Mayhew's central finding remained intact—at least until Sarah Binder published the next landmark study on legislative gridlock in Congress, "The Dynamics of Legislative Gridlock" (1999; see also Binder 2003). In her re-examination of legislative gridlock since 1946, Binder argues that Mayhew neglects to consider an important component of gridlock measurement. That is, he measures only the *count* of legislative successes and fails to consider the *proportion* of successes to overall opportunities for success (i.e., the size of the agenda). Binder accounts for differences in the denominator of the success proportion, and ultimately finds that divided governments *are* in fact more gridlocked. Importantly, she also finds that, as the number of moderate members in Congress decreases, gridlock increases.

But while Binder's findings seemed to square better with *prima facie* expectations about divided government and gridlock, her study was by no means met with less scrutiny than Mayhew's. Indeed, Binder's study has been challenged on a number of fronts. First, because Binder's data tends to include bills of lower status than does Mayhew's data, scholars have questioned whether her findings are robust to various levels of statutory importance. Howell et al. (2000), for example, add to Binder's data and create four levels of statutory significance for all bills—not just ones mentioned in major newspapers. Using this organizational structure, the authors find that divided government only predicts gridlock within the highest strata of bill significance ("landmark" bills). In fact, among minor pieces of legislation, divided government correlates with *less* gridlock—not more.[2] Additionally, Binder's analysis was subject to criticisms regarding her measures of ideology. In a 2008 paper in *Political Analysis*, Chiou and Rothenberg demonstrate that Binder's findings regarding ideology are not robust when tested with Common Space DW-NOMINATE scores. Such scores are better-suited for ideological comparisons across states and legislatures than the W-NOMINATE scores that Binder uses in her analysis, and the authors find that the W-NOMINATE scores bias Binder's findings.

Beyond these methodological criticisms of Binder and Mayhew's work, however, a different

[2] This result is similar to Tsebelis's approach and result for "important" versus "nonimportant" laws, in Chapter 7 of *Veto Players*.

sort of "criticism" of the divided government/gridlock literature began to arise in the late 1990s. Indeed, beginning with Keith Krehbiel's *Pivotal Politics*, congressional scholars began (and continue today) to think about gridlock in terms of spatial voting models. Krehbiel's *Pivotal Politics* model offers perhaps the simplest prediction for gridlock in Congress. Developing a party-less model of unidimensional preferences in Congress and the presidency, Krehbiel argues that gridlock occurs when the status quo lies between the president's veto point and the Senate's filibuster veto point. Thus, the larger the gap between the 60th vote in the Senate and the president's veto point, the more gridlock Congress will experience.

Krehbiel finds modest support for this theory in his book, but like all previous studies of gridlock, his study engendered some lively debate. In particular, many scholars disagreed that parties are unimportant when attempting to explain voting patterns and policy stability in Congress. While a number of different scholars voiced this concern, Gary Cox and Mat McCubbins offer the most complete and critically acclaimed "strong"-party model of congressional behavior. Their study, *Setting the Agenda* (2005), posits that parties in Congress exercise power in Congress via negative agenda control. That is, the majority party in Congress can protect its brand and guide policy outcomes by preventing bills opposed by a majority of the party from ever reaching the floor.

Within the spatial-voting context, Cox/McCubbins' negative agenda power model essentially adds another pivot to Krehbiel's party-less model. In doing so, the model expands the gridlock range to those cases in which the status quo lies between the Senate filibuster pivot and the reflection point of median voter on the other side of the party median. By adding this additional pivot, the gridlock range grows (and Cox/McCubbins' model predicts more gridlock than does Krehbiel's). Indeed, some policies that might pass under Krehbiel's model (all policies preferred by 60 senators, 50% of House members, and the president) will not even be considered under Cox/McCubbins' model (i.e., those bills for which there is no "majority of the majority party" support).

Empirically speaking, there is evidence that the majority party in the U.S. House of Representatives does possess something akin to negative agenda control. For example, in 2003, then-Speaker Dennis Hastert announced that he would never allow a floor vote on a bill that did not enjoy support from "a majority of the majority" party. This "Hastert Rule" (as it is now known) met with staunch criticism by pundits and members of the minority party, but political scientists have since discovered that nearly every Speaker since the early 1900s has followed Hastert's "majority of the majority" guideline. In fact, since 1900, no Congress has passed more than 5 bills without a majority of majority-party members in support (Cox and McCubbins 2005). Cox and McCubbins count this fact as support for their theory, at least in principle.

Since the publication of *Setting the Agenda* in 2005, a series of papers attempted to test/discriminate between the negative agenda control and pivotal politics models, with limited success. In perhaps one of the most thorough examinations of each model's predictions regarding gridlock, however, Jon Woon and Ian Cook (2011) find that Cox/McCubbins' "gatekeeping" model (see Figure 1) generally outperforms other spatial models of congressional voting (including Krehbiel's). In the paper, the authors estimate their own statistical model of gridlock and test each

of the theories under two different kinds of data-generating functions for legislative proposals. Under both data-generation specifications, the gatekeeping model performs quite well, lending support to Cox/McCubbins predictions concerning gridlock (though by no means discrediting Krehbiel's model, either).

[FIGURE 1 HERE]

Taken together, empirical evidence provided by Woon and Cook and others demonstrate that both institutional (a la Krehbiel) and partisan (a la Cox and McCubbins) pivots matter for legislative gridlock. Indeed, the best-performing models of gridlock in Congress are those that are "hybrids" – ones that account for differences in both institutional pivots and party gatekeeping (Woon and Cook). However, even "hybrid" studies leave gaps in our understanding of both gridlock and agenda control in American politics. First and foremost, none of these models offer explicit predictions about how gridlock will fluctuate in cases when *both* chambers of a legislature have negative agenda control. Traditionally, only the House is seen as having negative agenda control power in Congress. However, many state legislatures possess agenda control in both chambers. Second (and relatedly), many studies do not adjust for "absorption." While I discuss absorption further below, the important point here is that, though the gridlock interval in systems with agenda control is generally larger than in chambers without such control, this does not necessarily have to be the case. Chambers of different parties, for instance, might have more distance between them than two chambers with negative agenda control, if the latter share partisanship. As detailed below, Tsebelis's *Veto Players* provides a framework for dealing with absorption, allowing for integration of the Krehbiel and Cox/McCubbins approaches.[3] I use this framework to generate predictions about gridlock in state governments.

Legislative Gridlock in the American States

Before generating predictions about gridlock in the American states, however, I first examine here the current state of the state-level gridlock literature. Without a doubt, studies of state-level gridlock have uncovered fascinating trends within state legislatures, some of which remain unexamined at the federal level. These studies notwithstanding, though, much of the most sophisticated research regarding gridlock has centralized around Congress—particularly research that employs spatial theory to generate predictions about gridlock.

Nevertheless, a small literature has emerged around gridlock in state legislatures. In particular, beginning with Gray and Lowery (1995), studies of gridlock in the state have considered the effects of interest-group density and diversity on legislative gridlock. In their 1995 study, Gray and Lowery find that interest group diversity does not appear to predict legislative productivity, but that the number of interest groups does positively predict productivity. In 2001, Cynthia Bowling and Margaret Ferguson re-examine gridlock in all fifty state governments, testing hypotheses

[3] Woon and Cook come closest to accounting for absorption in their study of competing gridlock models. However, as Woon and Cook rely on time-linked Markov Chain simulations to derive their results, their findings apply mainly to large, unbroken time-series, due to the dependence of their gridlock predictions on the status quo in the previous simulation period. In other words, their approach and results do not necessarily transport well discrete comparisons of *different* legislatures – or even to comparisons of the same legislature in separate time periods.

regarding divided government, interest-group diversity, and policy differences. The authors find that, at least for "conflictual" policies, divided government does engender gridlock. Moreover, they find mixed results regarding interest-group diversity: interest-group diversity influences gridlock in some issues areas, but not others.

In terms of spatial analysis of state-level gridlock, however, very little work has been done. James Rogers (2005), for example, examines how divided government affects legislative productivity in the states, but he does not explore how preferences or negative agenda control might play out. Hicks and Smith (n.d.) go a step further and show that, in addition to divided government, direct democracy, term limits, and a host of other variables might influence productivity. But here again, this study does not examine how differences in preference distributions and/or negative agenda control might influence productivity levels in the states—nor does any study to date. In this study, I utilize Tsebelis's *Veto Players* to synthesize the claims and findings of Krehbiel and Cox and McCubbins' and use them to examine the effects of negative agenda control in the states. My findings aim to contribute to our currently limited understanding of gridlock in the states, as well as to expand our understanding of institutional and partisan veto players and pivots in general.

THEORETICAL EXPECTATIONS: INSTITUTIONAL AND PARTISAN VETO PLAYERS IN THE AMERICAN STATES

As suggested above, few studies have explored legislative gridlock in the states from a spatial-theoretic perspective. This study serves as the first to do so. However, the study aspires to more than a re-test of theories from the congressional literature. Rather, as noted earlier, it seeks to synthesize existing American theories of gridlock, in order better explain gridlock in the states. To this end, this paper relates George Tsebelis's *Veto Players* theory to Cox and McCubbins' theory of negative agenda control and Keith Krehbiel's *Pivotal Politics*.

In *Veto Players* (2002), Tsebelis employs multidimensional spatial analysis to show that both the number of veto players in a governmental system, as well as the distance between them, influence policy stability. More specifically, the more veto players in a lawmaking system, and the more preference distance between them, the larger the "core" of the political system. Tsebelis defines the core much like Krehbiel and Cox and McCubbins define the "gridlock interval:" the core is the set of status quo policies that cannot be defeated by any policy proposal.[4] Thus, the larger the core, the more policy stability a system encounters.

Tsebelis defines a veto player as any individual or collective actor whose support must be earned in order for a measure to take effect. Unlike the spatial models discussed earlier, however, Tsebelis's findings regarding veto players and policy stability generalize to multiple issue dimensions – not just one. Tsebelis describes two basic types of veto players, institutional veto players and partisan veto players—one of which one derives from the governmental structure system itself (institutional veto players), and the other from the political and party system in a country (partisan

[4] The primary difference between Tsebelis's model and the model Krehbiel details is that, while Krehbiel's model is one-dimensional, Tsebelis's model generalizes to *n* policy dimensions.

veto players). The introduction of either kind of veto players may increase policy stability. Moreover, the further apart the preferences of veto players, the greater the level of policy stability, all else equal.

When combined, the number of and distance between veto players jointly influence a system's policy stability. However, additional veto players do not *always* increase policy stability. Indeed, if a particular veto player is "absorbed" by another veto player, that veto player will not increase policy stability. A veto player is absorbed if she, by virtue of her preference set, would not choose to independently veto a change to the status quo (as in the brief example above). Put differently, in the cases in which an absorbed veto player might choose to veto legislation, there will always exist another veto player who also would choose to veto that piece of legislation. In the context of American legislatures, a moderate Republican governor may likely be absorbed in a political system that features one Democrat-controlled legislative chamber and one radical Republican-controlled legislature. Indeed, while the governor may object to, say, a liberal piece of legislation originating in the Democratic chamber, the radical Republican chamber will also oppose such legislation. Similarly, while the moderate governor may occasionally disapprove of conservative legislation originating in the Republican chamber, the Democratic chamber will also oppose such legislation. In sum, the moderate Republican governor, as an absorbed veto player, has no incentive to unilaterally veto any proposal. As a result, the governor (like all absorbed veto players) *does not add to the political system's policy stability*. Rather, policy stability remains unaffected.

In the American context, the federal government (and most state governments[5]) is thought to have three institutional veto players: Congress (lower house), the Senate (upper house), and the president (executive). However, when one considers American government in view of Cox/McCubbins' theory of negative agenda power, the possibility of additional veto players in the American system becomes apparent. Indeed, beyond the three veto players inherent in America's presidential, bicameral system, negative agenda control theory posits an additional *partisan* veto player: the majority party. In order for legislation to ever reach the floor in a chamber with majority party negative agenda control, a majority of the majority party must support the measure. In effect, then, the majority party acts as an additional veto player, located at the median of the majority party: if they do not approve of a policy, the policy will (essentially) never reach the floor. Consequently:

> **H1:** *American state legislatures with majority parties that exercise negative agenda control should,* ceteris paribus, *have larger cores on average than do those without such parties (and, consequently, fewer veto players).*

In *Veto Players* (2002), Tsebelis proves that the addition of veto players into a system of governance cannot possibly decrease the size of the core and will, at worst, not increase its size. However, assuming that at least some veto players are not absorbed, the addition of veto players should increase the size of the core. Nevertheless, because this study will compare legislatures of varying degrees of polarization, the addition of veto players may or may not correlate with overall core size in the interstate aggregate. In this case, we learn nothing from the data about how negative agenda control (i.e., the addition of veto players) might increase the size of the core. Thus, this study

[5] Nebraska has a unicameral legislature.

first tests to see that the number of added veto players (i.e., the number legislative chambers with negative agenda control) does in fact correlate positively with core size.[6]

It is important to note that here that the American states possess many institutional similarities that are often difficult or even impossible to find in comparative institutional studies across countries. Indeed, nearly every state (with the exception of Nebraska, which has been removed from the data, due to its lack of legislative parties) possesses three institutional veto players—a lower house, upper house, and executive—and have "presidential," two-party systems. Thus, not only do the states offer an opportunity to isolate the potential impact of negative agenda control on gridlock, but they also offer a particularly direct test of the original veto-players theory and predictions regarding core size. In this sense, the findings of this study may interest both scholars of both American and comparative politics.

Should the existence of negative agenda control in fact predict core size, the next test of this study's theory directly relates to outcome of interest, legislative gridlock:

H2: *The larger the core, the more gridlock a system will encounter.*

Given that legislatures have multiple veto players, a number of preference differences merit attention. Below (and in Figure 2), I detail which of these preference distances should matter, and when.

In addition to the relationship between veto players and core size, and core size and gridlock, this study strives to synthesize Krehbiel and Cox and McCubbins' models of gridlock, as noted above. Thus, the final part of this study's analysis attempts to demonstrate that the theoretical conceptualization of negative-agenda-control-as-additional-veto-player adds to the literature's current understanding of legislative gridlock. To do so, this study will attempt to show that negative agenda control does indeed matter for gridlock, even when accounting for the size of a Krehbiel-like gridlock interval. Thus:

H3: *Even conditional on distance between institutional veto players, negative agenda control should lead to higher levels of gridlock in states with legislative majorities that possess it.*

DATA AND ANALYSIS:

H1 (NEGATIVE AGENDA CONTROL AND CORE SIZE)

As detailed in H1, the first step to untangling the relationship between negative agenda control, core size, and gridlock is to affirm that agenda control and core size are indeed positively correlated. If they are not, there is little reason to believe that agenda control will influence gridlock as predicted. Thus, this study first tests to see that the number of veto players in a state (determined in part by the number of chambers possessing negative agenda control) positively predicts core size.

To determine whether or not a chamber possesses negative agenda control, I rely on

measurements found in Anzia and Jackman's (2013) recent study on agenda control in the state legislatures. In their study, the authors examine the relationship between the existence of agenda-control outlets in a legislature and the "majority roll rates"[7] that the legislature experiences—finding, as Cox and McCubbins do for Congress, that the presence of agenda control rules do indeed prevent the majority party from being rolled. One of the major agenda control rules that the authors examine is majority-leader control of the floor-vote agenda. Under this rule, majority party leaders decide which bills, among all the of the bills that could come up for a floor vote, will actually receive a floor vote—and when. Some bills may receive a vote right away, while others are delayed impossibly long (so as to never come to a vote at all). This paper utilizes the existence or non-existence of this power as its measure of negative agenda control for a given chamber of a legislature. Because the variable is a dummy, each chamber assumes a value of either 1 (possesses negative agenda control) or 0 (lacks negative agenda control). When pooled across entire legislatures, this variable may take on three possible values: 2 (negative agenda control in both houses), 1 (control in just one chamber), or 0 (no negative agenda control). Given that all American states (besides Nebraska) have the same number of institutional veto players, differences in this negative agenda control variable will also represent overall differences in a state's total number of veto players.

Core size, of course, is a function of more than just the number of veto players in a system. That is, preference distance between veto players also helps to determine the size of the core. Thus, to measure core size, one first needs a measure of veto player preferences. In this study, I utilize Shor and McCarty's (2011) Common Space scores to measure these preferences. In using these scores, of course, I assume unidimensionality of issue space in the state legislatures. Doing so precludes me from offering the most direct test of a veto-players theory, since a major contribution of Tsebelis's theory is its generalizability to *n* dimension. Still, I make this decision for two primary reasons. One is practicality: Shor and McCarty's scores are the most accessible preference measures for for state legislatures. The other is simplicity: unidimensionality allows one to calculate and examine core size and its effects in a very straightforward manner. That is, once one determines the number and position of veto players in a system of governance, unidimensional core size is simply the *maximum distance between any two veto players*.

Figure 2 provides greater detail on how this maximum distance is calculated. In short, to measure the size of the core, I simply choose the largest of the relevant veto-player distances for each legislature. In theory, chambers with negative agenda control carry with them the potential for much larger cores than the average non-agenda-control chamber. In Cases 3 and 4 of Figure 2, for example, both chambers possess negative agenda control. In cases where one chamber is controlled by Democrats and the other Republicans, Cases 3 and 4 should produce a larger core than those depicted in the other cases in Figure 2. The same logic applies for Cases 5 and 6 (just one chamber with negative agenda control), which should create a larger core than Case 7 (no negative agenda

[7] Traditionally, studies of negative agenda control test the agenda-setting power of a majority by examining voting phenomena call "roll rates." A majority party is thought to be "rolled" when a bill passes through a chamber without majority support from the majority party. Roll rates, then, are the percentage of bills overall that pass without support from the majority of the majority party. The lower the roll rate, the story goes, the more likely it is that the majority party possesses some kind of agenda control.

control), all else equal.

Legislatures with larger numbers of veto players do not *always* possess larger cores of course, due to the logic of absorption highlighted above. Moreover, just because a legislature possesses two chambers with negative agenda control does *not* mean that the relevant gridlock interval lies between the party medians. Indeed, due to absorption, some veto players in a system of governance may not contribute to the size of the core at all. Cases 1 and 2 of Figure 2 (two chambers with negative agenda control, controlled by the same party) provide an example. Here, the distance between the party medians is zero—the majority parties in these chambers are very similar. Instead, the relevant gridlock interval or core distance is a tie between $|M_{hm} - C_{hm}|$, $|M_{sm} - C_{sm}|$, $|M_{hm} - C_{sm}|$, and $|M_{sm} - C_{hm}|$ (the distances between the lower house majority party and chamber medians, upper house majority party and chamber medians, lower house majority party and upper house chamber medians, and upper house majority party and lower house chamber medians, respectively). Thus, instead of identifying which of the intervals will serve as the core, the number of veto players simply identifies, *which pivots (and therefore which preference distances) must be maximized over, in order to calculate the core*. Majority party medians ostensibly do not matter in systems lacking any sort of negative agenda control. As a result, after sorting out which preference distances were relevant in each state and calculating those distances, I maximized over each state's relevant distances to obtain the proper core measurement. These measurements are calculated biannually for each state, from 2002 to 2014.

Figure 4 plots the number of additional veto players due to negative agenda control against core size. As expected, an increase in number of veto players correlates with an increase in core size. However, as demonstrated by the core size overlap between the agenda-control categories, absorption is a very real phenomenon in the data.

[FIGURE 4 HERE]

To examine whether or not this relationship between negative agenda control and core size is robust to the incorporation of other covariates relevant to core size, I estimate a model of core size using truncated MLE estimation, with cluster-robust standard errors by state and dummy variables for years. Along with number of veto players/negative agenda control, I also include an indicator for divided government, party of the governor, state interest group population, and size of state economy. Table 1 summarizes the results.

[TABLE 1 HERE]

As Table 1 demonstrates, the presence of negative agenda control does indeed positively correlate with a state's core size. This result is significant at the $p < .01$ level ($p = .007$). As predicted, negative agenda control does appear to matter for core size in the aggregate: larger number veto players (via the introduction of negative agenda control) are associated with larger core sizes. This is not a particularly surprising result, given that the presence of negative agenda control is used to calculate core size itself. However, it does establish that these data square with theoretical expectations regarding veto players and core size and offer an opportunity to test H2 and H3.

H2: DOES CORE SIZE ACTUALLY PREDICT GRIDLOCK?

Testing Theoretical Expectations Using Bill Counts

While the above relationship between core size and negative agenda control is an important one, this study is not interested in just core size. Rather, it seeks to explain differences in levels of legislative gridlock between state governments. According to this study's theory, core size should play a central role in predicting gridlock. Indeed, the larger the size of the core—that is, the larger the set of policies that, theoretically, are unacceptable to at least one veto player—the less policy change will occur. This study will test this central expectation in two ways, first using the currently accepted measure of legislative productivity in state legislatures (bill passage counts) and then using a new measure (based on ACA implementation).

To date, models of legislative gridlock in the American states have relied exclusively upon raw bill passage counts as a means of measuring gridlock. As Tsebelis, Binder, and Mayhew each detail, this measure possesses a number of important flaws. Nevertheless, due to the difficulty of collecting data on "significant" legislation in all 50 state legislatures, studies continue to focus on raw productivity numbers. As a result, I test H2 with these measures, in order to relate my findings to current studies of gridlock. Bill enactment counts are taken from the Council on State Government's *Books of the States,* for the years 2002-2014. Under this measurement, the more bills passed, the greater the movement of the status quo.

Before examining the relationship between core size and bill passage, one final aspect of core size merits attention. That is, core size does not pertain just to legislative veto players: because of the veto power of governors, the location of the governor matters for the size of the core. As a result, the truest measurement of core size would incorporate the governor's preferences. However, because scholars have yet to develop reliable ideology scores for governors, measuring distance between the governor and legislature proves more difficult. This study introduces a *divided government* variable, in place of these scores. The variable takes on the value "1" if the governor is of a different party than both chambers of the legislature and "0" otherwise.[8] While certainly a suboptimal measure of preference distance, I expect that, *ceteris paribus*, divided government will positively correlate with gridlock.

To test these expectations, I estimate a negative binomial model of bill enactments from 2002-2014, with unconditional fixed effects for year and clustered standard errors by state. Here again, I group core size and enactment data biannually, in order to account for legislatures that meet only once every other year. Negative binomial regression is well suited for these data, since they are overdispersed counts. Finally, in addition to the core size and divided government covariates noted above, I also include a variety of typical explanatory variables in the model, including size of state economy (and size squared), interest group population,[9] initiative difficulty (Bowler and Donovan 2004), and number of bill introductions (also taken from the *Book of the States*).

Table 2 summarizes the results. As predicted, core size does negatively predict bill passage rates, as result that is significant at the $p < .0001$ level. Interesting, however, divided government

[8] Divided government is calculated in this way because, while legislative elections occur at the district level, governors are elected at the state level. As a result, governors are likely more moderate than legislators, rendering them absorbed by any system that features one Democratic and one Republican legislative chamber (per the example of absorption articulated above).

[9] Special thanks to James Strickland for providing data on interest group populations and economy size.

does not negatively predict enactments at a statistically significant level. All other variables behave as expected.[10] Taken together, these results provide tentative support for H2, that core size (which is driven upward by negative agenda control) does indeed decrease policy change (increase) gridlock. However, as detailed below, these measures of gridlock are flawed, which calls for another test of this study's theory—this time, accounting for bill significance.

Measuring Gridlock – Improving Upon Bill Passage Counts

Although current studies of state gridlock rely upon bill-count measures, most studies of policy change and gridlock in Congress and other countries have avoided measuring policy change in this way, due in particular to concerns about bill significane. As Tsebelis (2002) argues in his original test of the veto players theory, core size should not necessarily predict bill counts, per se. Rather, it should predict *changes to the status quo*: systems with large numbers of veto players (and larger core sizes) will not pass legislation that moves the status quo in large increments. Bill counts relay only limited information about such movements: one government could pass 10 incremental pieces of legislation that do not move the status quo as much as another government's single piece of landmark legislation.[11] Thus, Tsebelis chose *not* to measure policy stability via raw bill counts. Instead, to address this challenge, he focuses his analysis on a single policy area, labor policy, and utilizes data gathered from lawyer/advocacy handbooks to determine the "significance" of a given piece of legislation. That is, if a law is included in the handbook, it is considered to be above some level of significance, and is therefore included in the dependent variable of his study. Using this measure, Tsebelis finds support for his prediction that the number of veto players will (positively) predict a system's policy stability.

This study takes seriously the arguments made by Tsebelis regarding legislative significance. Tsebelis, of course, is not the only scholar to make such arguments regarding policy significance: Mayhew (1991) and Binder (1999) also focus their studies on "significant" legislation. Still, Tsebelis, echoes many of their arguments, and provides a compelling case for focusing on a single issue area. Consequently, this study attempts to retest its claims regarding negative agenda control, preferences, and gridlock, taking policy significance into account. To do so, I focus on a single policy area: health policy. In particular, I focus on implementation of the Affordable Care Act (ACA) in the states from 2011 to 2013.

Focusing on this specific policy area offers a number of benefits. First, it allows for better data collection regarding bill "significance" (discussed further below). Second, it offers a way to deal with the "problem" of the status quo. That is, just because a government does not move the status quo does *not* mean that it is *unable* to do so. Rather, relevant veto players may simply prefer the status

[10] It is worth noting here that the "Initiative Difficulty" term behaves in the way it should, according to Tsebelis (2002) (as well as Gerber 1996). According to his theory, initiatives act as the addition of another veto player, which decreases policy change. In other words, the easier (or less difficult, as it were) it is to suggest and vote on initiatives, the less likely policy change will be.

quo to feasible alternatives. By subjecting all 50 states to all implementation requirements and incentives, the Affordable Care Act shifts the health policy status quo in such a way that all 50 states must respond in some way (even if only to deny funds or offer ACA alternatives). Third, it allows for a more manageable data collection effort regarding core size (detailed below). This is especially important, given that core size allows this study to synthesize predictions regarding both preference distance and number of veto players and account for the concept of absorption.

In order to measure bill significance, I utilize National Council of States Legislatures (NCSL)'s database on ACA-related bills in the state legislatures. NCSL has developed 10 policy categories for ACA-related bills, with which they classify bills according to all relevant categories. For instance, NCSL would categorize a bill twice that addresses *both* Medicaid *and* insurance exchanges: once in the Medicaid/CHIP category, and once in the insurance exchange category. Classifying bills in this manner offers a means by one which may create a measure of "bill significance." That is, because significant bills can be "double-counted" (or triple- or quadruple-counted, for that matter) under this classifying scheme, one may utilize the sum total of relevant bills in each category as a means of measuring a state's ability to move the status quo on ACA compliance.

This, then, is the approach this study utilizes to measure policy change and gridlock. For each legislature, I count the total number of bills listed in each NCSL ACA implementation category. Then, I add together the bill counts from each category to arrive at a single count for each state. This number represents the total number of ACA implementation bills passed in each state; however, as noted above, the double-counting of more complex legislation allows this count to capture bill significance in a way that raw bill counts cannot. Figure 3 depicts the search interface used to collect these data, along with some of the categories used to classify bills.[12]

[FIGURE 3 HERE]

Using this measure of policy change, I retest the relationship between core size and policy change (H2). In short, the data again provide support for the idea that core size drives policy change downward.[13] Even without adjusting for differences in size of state economy and other important control variables, the data exhibit a modest negative relationship between core size and movement of the status quo. This relationship is depicted in Figure 5. Nevertheless, I control for a number of other variables in the model. In addition to economy, I include two variables related to partisanship and ideology: the mean Common Space score within each legislative core, and the partisanship of the state's governor. I do this to account for differences in ACA compliance due to distaste for or strong opposition to the ACA (a controversial bill, passed entirely by Democrats). Finally, I include a dummy variable for divided government, which I expect to be negative. When divided government is present, as discussed earlier, the governor is likely not absorbed, and will therefore increase the size of the core.

[12] To be clear, these are cross-sectional data, as I add up all enactments over the three-year period covered by the data (2011-2013).

[13] It should be note that H1 was also retested on these data from 2011-2013 and again received support.

[FIGURE 5 HERE]

Table 3 summarizes the results. Here again, I estimate a negative binomial model, using cluster robust standard errors by state. All variables behave as expected. First (and somewhat unsurprisingly), states with more liberal legislatures and/or with Democratic governors tend to score higher in the ACA implementation dependent variable, though this result is not statistically significant at conventional letters. Second, economy size performs as expected: states with larger economies legislate more than states with smaller economies. Most importantly for this study, however, the main independent variable of interest behaves as expected. As depicted in Figure 6, core size negatively predicts policy change. Divided government also negatively predicts policy change, though this result does not reach statistical significance.

[TABLE 3 HERE]

These results again provide support for my theoretical expectations regarding core size and gridlock. Indeed, larger core sizes appear to be associated with smaller changes to the status quo. Figure 6, a marginal effects plot of core size and predicted policy change, captures the estimated magnitude of the relationship between core size and gridlock. As demonstrated in the plot, a move from the 20^{th} to 80^{th} percentile in core size (0.114 to 0.679) is associated with a decrease of approximately 6 units ACA status quo movement.

[FIGURE 6 HERE]

Taken together, these results provide evidence for my theoretical predictions regarding negative agenda control, preference distance, and the size of the core. Indeed, as shown above, the number of veto players positively correlates with the size of the core (H1). Moreover, as the size of the core increases, so too does gridlock (H2). However, as discussed below, these models are not entirely able to address the role that negative agenda control *itself* plays in the increase of gridlock. Thus, the final portion of analysis will attempt to establish that the addition of a partisan veto player via negative agenda control is responsible for higher levels of gridlock in state governments.

H3: IS NEGATIVE AGENDA CONTROL ITSELF RESPONSIBLE FOR GRIDLOCK?

In *Pivotal Politics* (1998), Keith Krehbiel argues that a government's level of gridlock is determined by the distance between its pivots. In the American system, he argues, the most important such distance lies between the filibuster and veto override pivots. Krehbiel's theory, then, underscores the importance of institutional veto players, both in the executive and legislative branches. Cox and McCubbins, on the other hand, argue that party has a role to play in the policy change process. In particular, a party with negative agenda control powers may keep any law that "rolls" the majority from ever receiving a vote. This study has operationalized this claim by arguing that such systems possess an additional partisan veto player, located at the majority party median.

By only regressing policy change on core size, however, this study has not yet been able to test whether negative agenda itself impacts policy change, or whether the core-size/policy-change relationship is simply a relic of a generally polarized set of state governments (a la Krehbiel 1993).

Were this to be the case, institutional gridlock intervals may predict gridlock practically as well as the core size calculated here. In order to assess the impact of negative agenda control on gridlock, then, one would need to first condition on the distances between institutional veto players, and then assess whether or not negative agenda control displays a negative relationship with policy change.

This study undertakes a matching analysis to address this challenge. In addition to the benefits generally attributed to matching, such as an avoidance of structural imposition/interpolation on the data and a sensitivity to data (non)overlap, matching seems to suit this study's goals particularly well. That is, this study seeks to claim that, even when conditioning institutional distances and other characteristics that matter for gridlock, *systems with negative agenda control still have lower levels of policy change (higher levels of gridlock) than do systems without negative agenda control*. To be clear, matching is not an identification strategy—it in no way unquestionably establishes causal links between variables. Nevertheless, matching enables this study to first match on relevant institutional characteristics/distances and then assess whether there exist relevant differences between systems with and without negative agenda control.

Here, I examine one primary type of treatment. That is, I match and compare systems with any amount of negative agenda control ($W_i = 1$) with systems lacking any kind of negative agenda control ($W_i = 0$). Future versions of this study may match and compare systems with two chambers possessing negative agenda control to systems with just one such chamber. The former treatment assignment would examines whether negative agenda control matters at all, and the second whether or not the addition of two partisan veto players is actually associated with more gridlock than a system with just one such veto player. However, this study will focus on just the first treatment definition.

In order to match treatment and control units, I aim to condition on an **X** vector that includes: 2008 presidential vote share, party of the governor, divided government, size of state economy, and number of interest groups. Importantly, this **X** also includes five different preference distances: $|M_{hm} - M_{sm}|$, $|M_{hm} - C_{sm}|$, $|M_{sm} - C_{hm}|$, $|M_{hm} - C_{hm}|$, $|M_{sm} - C_{sm}|$, and $|C_{hm} - C_{sm}|$ (see Figure 2). These distances capture all the possible inter- and intra-chamber combinations of institutional and partisan veto players in a system with the maximum number of veto players. Of course, not all of these distances should matter in all states. However, conditioning on all of these distances ensures that I am matching state governments with maximally similar player spacing. If differences are found between cases matched on these distances, even when some distances are (theoretically) irrelevant in a given state, such a result will (and does) suggest that negative agenda control itself impacts gridlock, beyond the impact of state polarization. To test this assertion, I will estimate the average treatment effect of treatment on the treated:

$$ATT = \boldsymbol{E}[Y_i(1) - Y_i(0) \mid W_i = 1]$$

Before actually matching and estimating, however, it is important to first discuss important assumptions underlying the analysis, and whether or not such assumptions are justified. First and foremost, of course, is SUTVA—Stable Unit Treatment Value Assumption. In short, does the

assignment of negative agenda control in one state affect the outcome (policy change) in another? The map in Figure 7 suggests that geographically similar states have similar treatment assignments; however, it does not necessary suggest that a given state's assignment affects that state's *outcomes*. Instead, the presence of negative agenda control in, say, Arizona, would have to influence *policy change* in, say, Nevada, in order for SUTVA to be violated. This, of course, is entirely possible: if Arizona is unable to overcome negative-agenda-control-induced gridlock and pass something like a time-sensitive tax policy, Nevada may more vigorously pursue such policies, perhaps to attract regional businesses.

[FIGURE 7 HERE]

For a number of reasons, however, this concern is a minimal one for this study. First, every state has to comply with the ACA, so the aforementioned "race" is unlikely to occur. Second, because of absorption, negative agenda control may not affect gridlock so drastically that it would become noticeable and influence a neighboring state's actions. In the first place, many states have possessed their negative agenda control rules for decades—long before current legislative leaders ever took office. If so, it would be hard for them to attribute differences in gridlock to the presence or absence of negative agenda control. Moreover, even if negative agenda control is present in one state and not the other, *negative agenda control need not lead to gridlock*. If it doesn't, then it is unlikely that legislative production in the neighboring state will respond in any meaningful way to the presence of a procedural rule like negative agenda control.

In addition to SUTVA, the assumption of unconfoundedness also merits attention. That is, after conditioning on **X**, outcomes should be orthogonal to treatment assignment. While it is impossible to test for the presence or absence of unobserved confounders, this study attempts to account for a wide variety of covariates commonly found in studies of legislative gridlock. By conditioning on these common determinants of gridlock, confounding from these variables should be accounted for.

Finally, matching analysis rests crucially on the presence of overlap in each of the dimensions of **X.** That is, the conditional distribution of controlled units ought to share a common support with treated units on pertinent covariates. Figure 8 examines the overlap assumption graphically. Observing the treatment versus control density plots for each of the elements of **X,** the data in this study appear to possess healthy levels overlap, at least in varying degrees. When combined with the unconfoundedness assumption, the presence of overlap suggests that the data in this study meet the criteria for strong ignorability necessary for matching analysis.

[FIGURE 8 HERE]

In order to match treated and control units, I implement nearest neighbor matching, using the GenMatch() function in **R** (Diamond and Sekhon 2013)**.** GenMatch() optimizes covariate balance by using a genetic search algorithm to generate weights for each covariate. Previous iterations of this analysis used propensity score matching, though this method interestingly resulted in far worse balance post-matching than pre-matching. This is not the case for the matched sample from GenMatch, however. As Table 4 summarizes, the mean values of each covariate in **X** do not

significantly differ between treatment and control groups. The weakest matched covariate, number of interest groups, generates a t test that does not achieve significance at the $p < .1$ level. Table 5 lists the actual matched cases in the treatment and control groups.

[TABLE 4 HERE]

With these matches, one may finally estimate the ATT, in order to determine whether or not negative agenda control is associated with lower levels of gridlock, even when matching states by preference spacing and other relevant covariates. As Table 6 indicates, a *t* test of the difference between mean levels of policy change in the treated and control groups is in fact significant at conventional levels ($p < .05$). The approximately 13-unit difference is strongly negative between the two groups, as predicted. This result is consistent with the claim that negative agenda control does contribute to gridlock in the American state legislatures as expected.

[TABLE 5 HERE]

No statistical analysis is perfect, of course. It is important to note here, for example, that the state of California presents a particularly different case with which to match, particularly in terms of economy size. Ideally, future robustness checks may remove California from the sample for this reason. However, this study has opted not to do so for a number of reasons. First and foremost, the sample size is already extremely small; removing any number of observations is undesirable, particularly for non-parametric methods. Second, despite its large economy and high levels of professionalization, California is part of the control group, which is notably smaller than the treatment group. Removing California would impoverish the control group even further. Finally, despite California's inclusion, balance between economy sizes remains quite healthy. Still, future iterations of this study will explore removing California, in order to ensure that this large, high-productivity state is not driving the differences between mean levels of productivity between treatment and control groups.

[TABLE 6 HERE]

DISCUSSION: IMPLICATIONS AND CONCLUSIONS

To summarize, this study finds evidence for three primary hypotheses related to its veto-players theory of gridlock. First, the number of veto players in a state positively predicts that state's core size. Because the only way to add a veto player to a state's system of governance (at least in this model) is by the introduction of negative agenda control, one may conclude that negative agenda control is in fact positively associated with core size, as predicted. Second, this study finds that core size positively predicts gridlock (negatively predicts policy change), as measured by both bill passage counts and ACA implementation in the states. Finally, this study finds that, even conditional on preference polarization and other gridlock-relevant factors, negative agenda control appears at least partially responsible for the differences in gridlock between large-core and small-core states. In other words, as predicted, this study finds that negative agenda control by majority parties leads to greater levels of gridlock in American state legislatures.

This study contributes to the current understanding of gridlock, legislative parties, and state policymaking in a number of regards. First, as mentioned throughout, it suggests a synthesis of Krehbiel's and Cox and McCubbins' theories of gridlock using Tsebelis's *Veto Players* framework. In doing so, it accounts for veto player absorption and finds the resulting core sizes to consistently predict levels of gridlock. Second, the study also posits a new measure of gridlock in the American states. That is, while previous studies rely on bill passage counts (which assume that all bills are of the same level of significance), this study develops a measure of status quo movement that captures important differences between bills that address multiple aspects of a policy issue (in this case, ACA implementation). Moreover, because all states must come into compliance with federal ACA mandates, this measure ensures at least some level of dissatisfaction with the status quo—all states must desire to move the status quo in at least some way. Finally, by building on current research on agenda control in the states (e.g., Anzia and Jackman 2013) and extending spatial models of gridlock to the states, this study advances our current understanding both of the determinants of gridlock in state legislatures, and of the impact of negative agenda control on gridlock generally.

Still, one may build on and improve upon these findings in a number of ways, in addition to some of the ways highlighted above. First, a more precise test of this paper's theory would incorporate the position of a filibuster pivot (a la Krehbiel 1998) into its calculation of the core. Similarly, future studies may improve on this one by developing some way to measure gubernatorial preferences. With such preferences, a divided government control variable no longer becomes needed, and a more complete/accurate calculation of the core can occur. Finally, future studies can and should test this study's theoretical expectations in contexts other than health policy. Major federal mandates provide useful opportunities for testing this study's predictions, and extending those predictions to other policy areas could increase one's confidence in the robustness of the findings.

Most political scientists and political observers would likely agree: gridlock and policy stability matter in a representative democracy. Indeed, while some research shows that institutionally limited governments allow the economy to grow and prosper more freely, for example, countless studies and anecdotes also document the negative ramifications of a legislature's inability to address a polity's problems. In America's federal system, state governments wield power over a number of important policy areas, so their ability to address policy problems remains an issue of vital interest. This study explores the determinants of gridlock in the American states and demonstrates that institutional and partisan factors (like negative agenda control) carry significant ramifications for a state's ability to move the status quo.

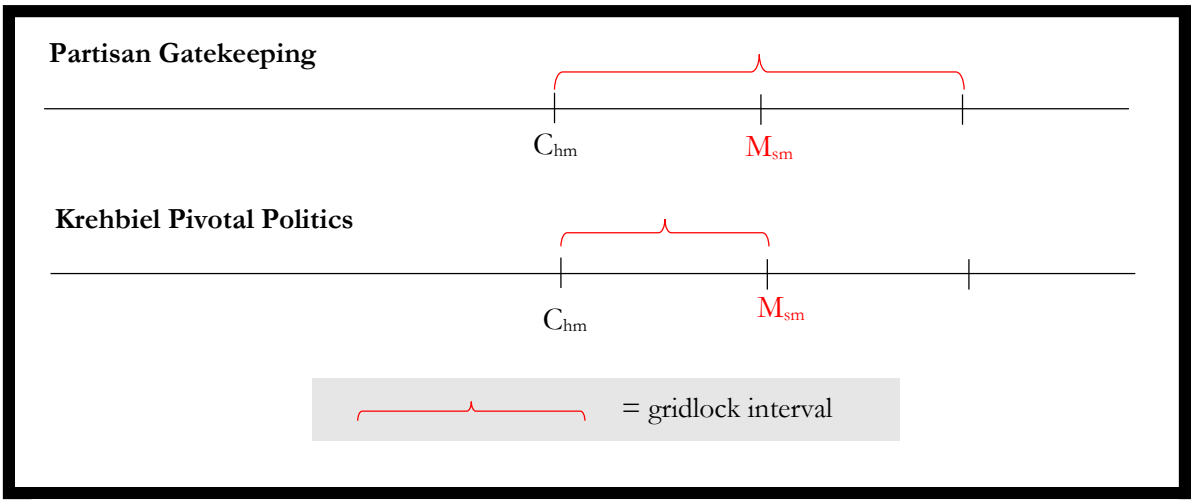
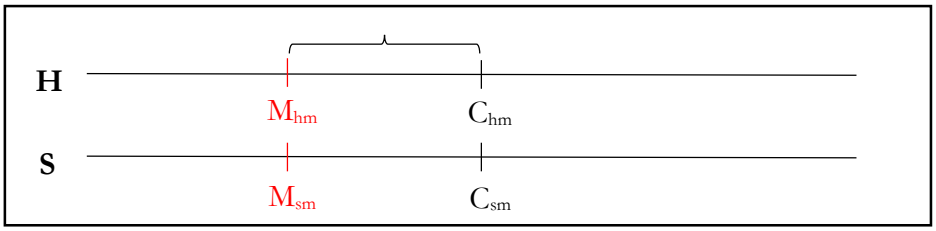


FIGURE 1. Negative-Agenda Control (Gatekeeping) Gridlock Range, Compared to *Pivotal Politics* Model*

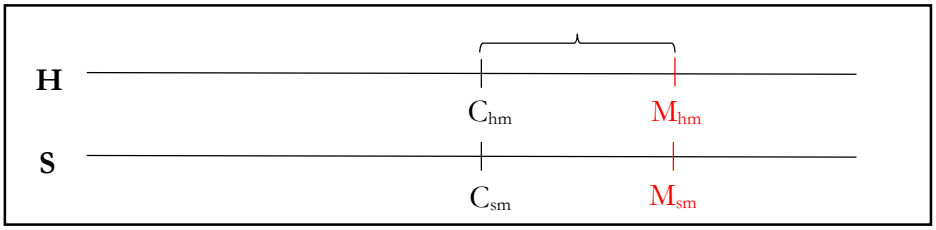
**Figured modeled on a figure in Woon and Cook (2011), "Competing Models of Gridlock."*



Core =

$\max\{|M_{hm} - C_{sm}|;\ |M_{sm} - C_{hm}|;\ |M_{hm} - C_{hm}|;\ |M_{sm} - C_{sm}|;\ |C_{hm} - C_{sm}|\}$

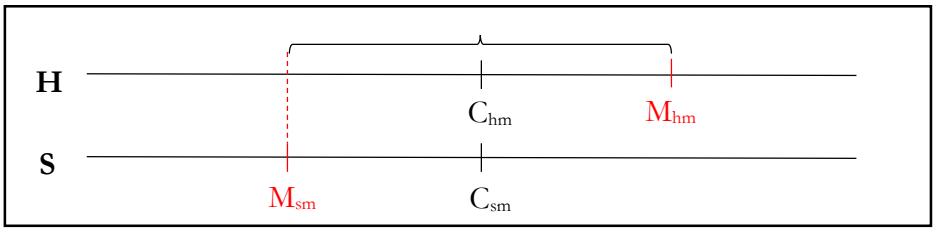
CASE 1: *Negative Agenda Control in Both House; Democrats Control Both Chambers*



Core =

$\max\{|M_{hm} - C_{sm}|;\ |M_{sm} - C_{hm}|;\ |M_{hm} - C_{hm}|;\ |M_{sm} - C_{sm}|;\ |C_{hm} - C_{sm}|\}$

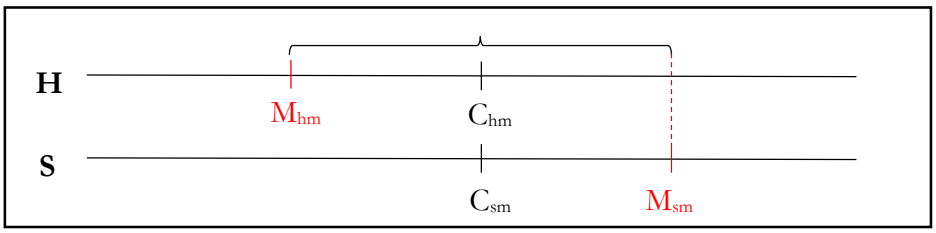
CASE 2: *Negative Agenda Control in Both House; Republicans Control Both Chambers*



Core =

$\max\{|M_{hm} - M_{sm}|;\ |M_{hm} - C_{sm}|;\ |M_{sm} - C_{hm}|;\ |M_{hm} - C_{hm}|;\ |M_{sm} - C_{sm}|\}$

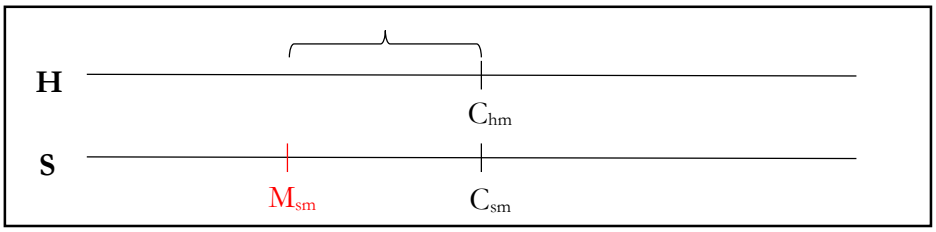
CASE 3: *Negative Agenda Control in Both House; Republicans Control House, Democrats Control Senate*



Core =

$\max\{|M_{hm} - M_{sm}|;\ |M_{hm} - C_{sm}|;\ |M_{sm} - C_{hm}|;\ |M_{hm} - C_{hm}|;\ |M_{sm} - C_{sm}|\}$

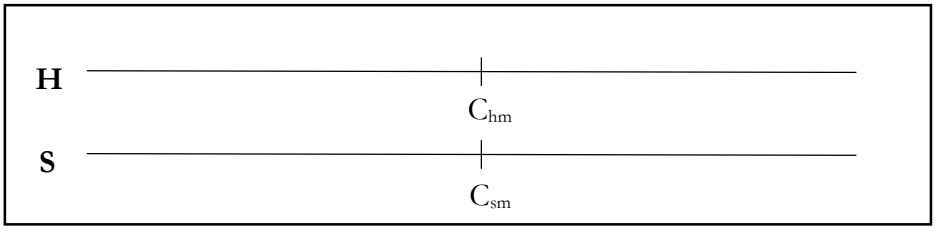
CASE 4: *Negative Agenda Control in Both House; Democrats Control House, Republicans Control Senate*



Core =

$\max\{|M_{sm} - C_{hm}|;\ |M_{sm} - C_{sm}|;\ |C_{sm} - C_{hm}|\}$

CASES 5 & 6: *Negative Agenda Control in Senate Only (core values vice versa in House-only case)*



Core = $|C_{sm} - C_{hm}|$

CASE 7: *No Agenda Control*

FIGURE 2. Agenda Control and Calculation of the Core

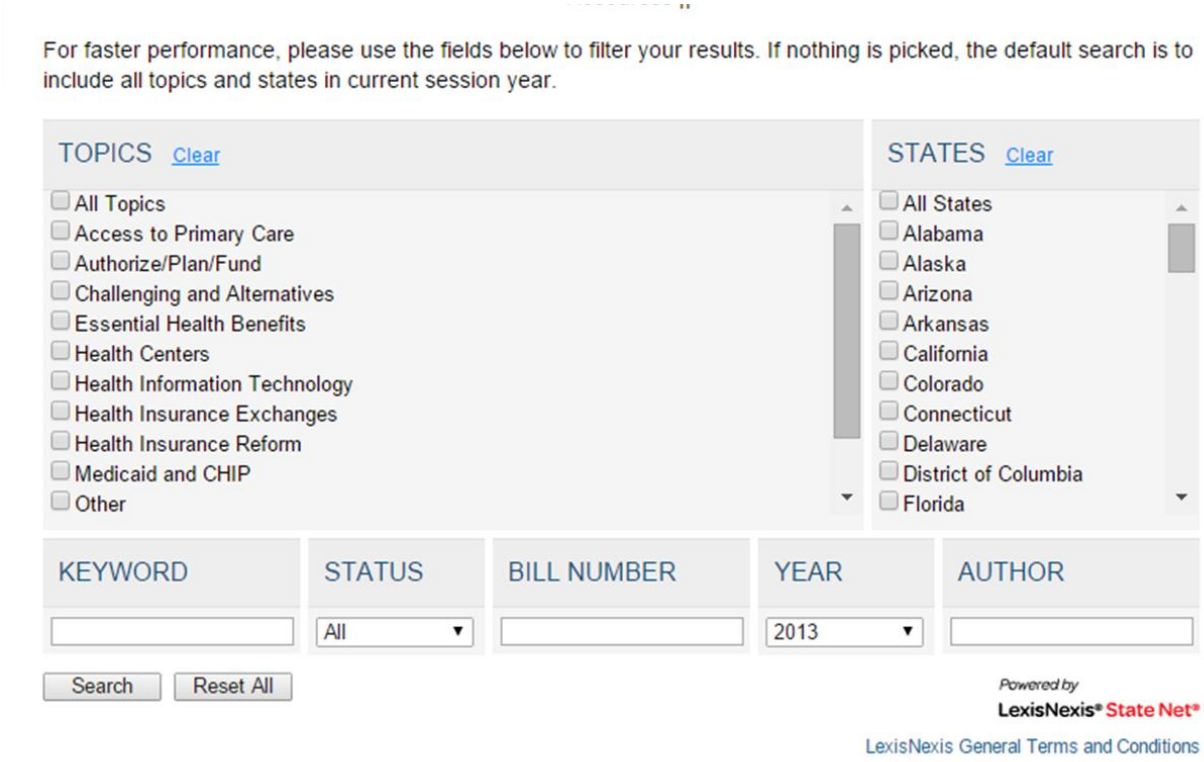


FIGURE 3. NCSL ACA Implementation Database Search Interface (2011-2013)

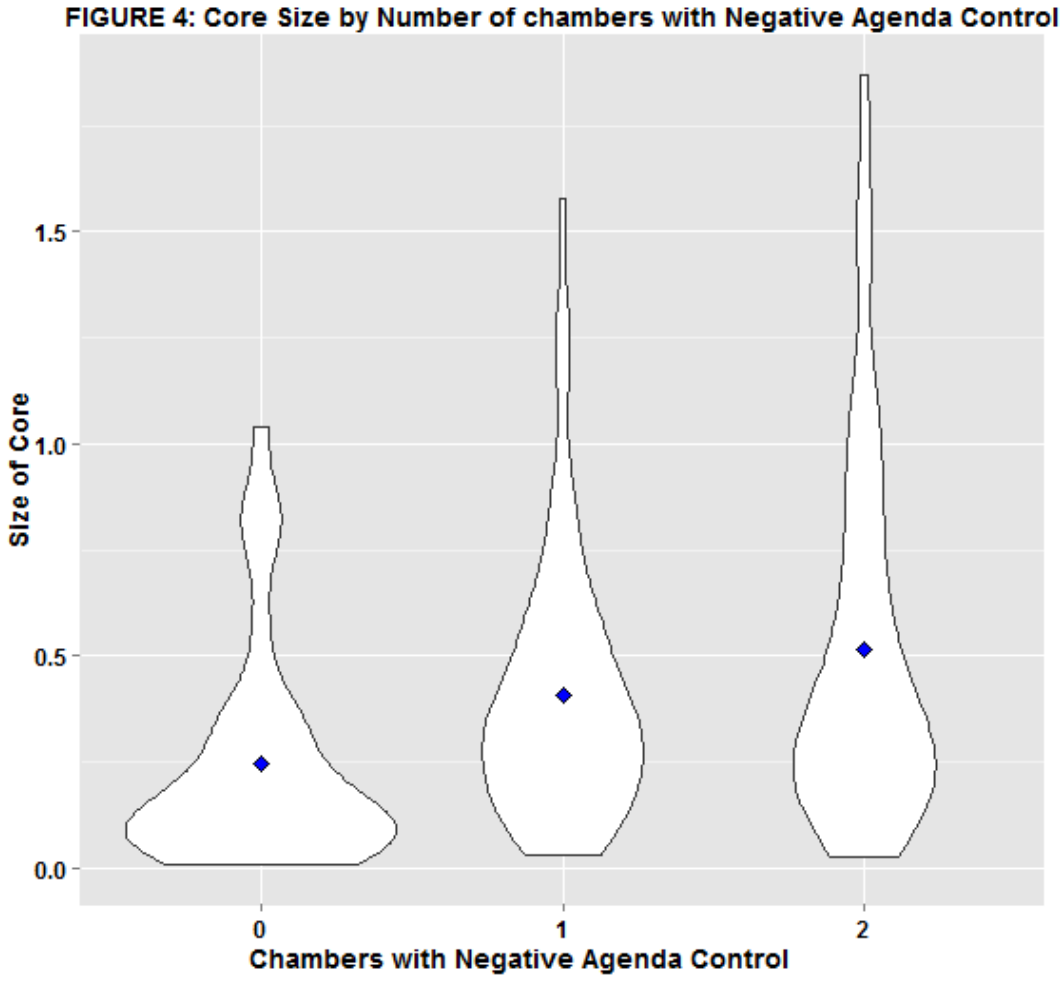


Table 1: Negative Agenda Control and Core Size: 2002-2014

	Dependent variable:
	Core Size
Negative Agenda Control	0.108***
	(0.040)
Interest Group Population	0.0001
	(0.00009)
Divided Government	.086***
	(0.028)
Size of State Economy	−5.76e-08
	(2.25e-07)
Initiative Difficulty	−.002
	(.012)
Constant	0.134
	(0.117)
Observations	49
Log Pseudolikelihood	−111.861
Upper Limit	2
Note:	*p<0.05; **p<0.01; ***p<0.001

FIGURE 5: Core Size v. ACA Status Quo Movement

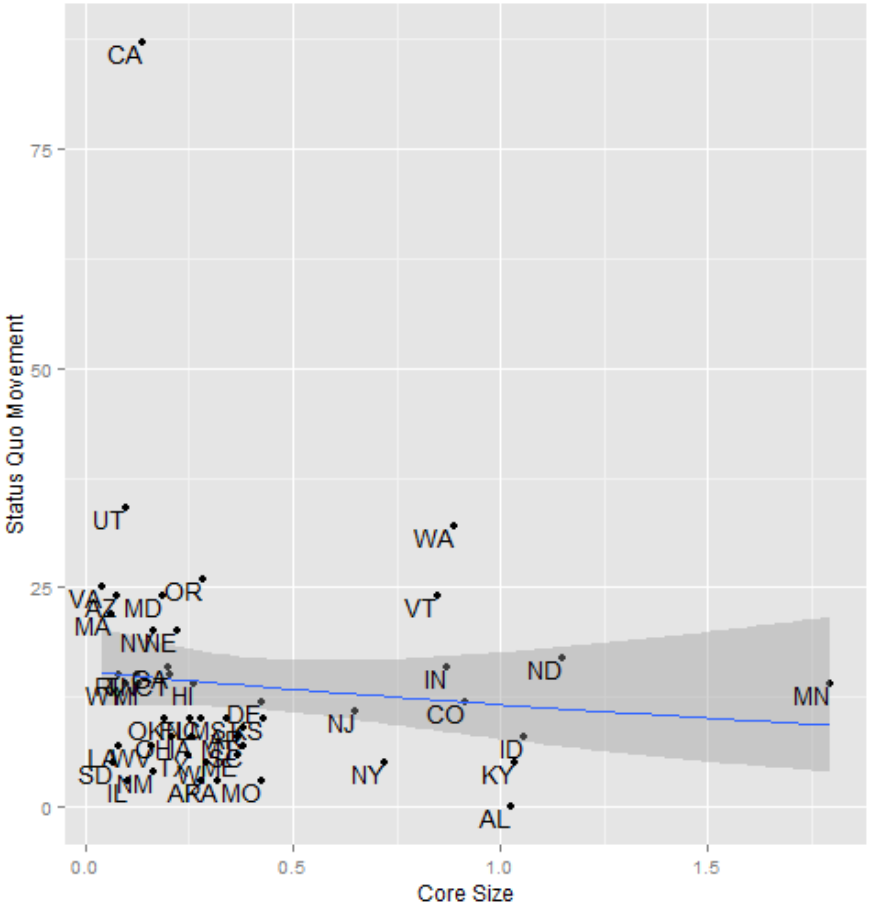
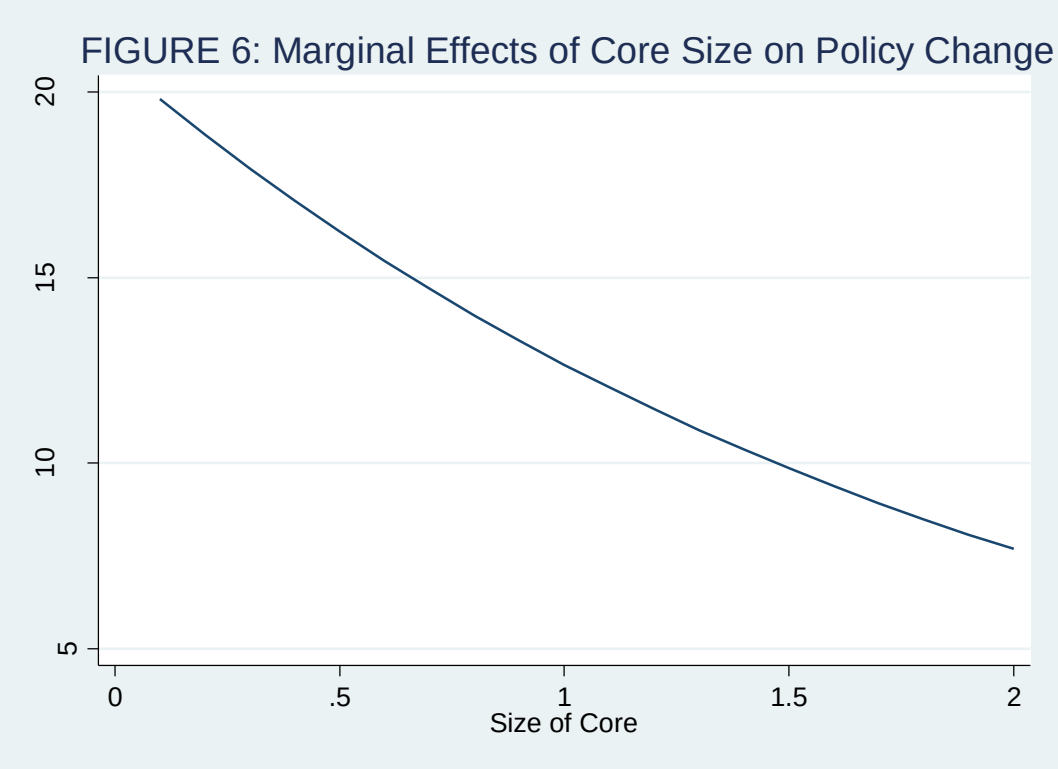


Table 2: Core Size and Legislative Productivity: 2002 - 2014

	Dependent variable:
	Number of Enactments
Size of Core	−0.419***
	(0.104)
Divided Government	−.004
	(0.060)
Interest Group Population	−0.0001
	(0.0001)
Size of State Economy	6.58e-07***
	(3.00e-07)
Initiative Difficulty	−.045 **
	(0.020)
Introductions	0.00005**
	(0.000001)
Constant	6.833***
	(.199)
Observations	293
Log pseudolikelihood	−2,148.2629
Note:	*p<0.1; **p<0.05; ***p<0.01

Table 3: Movement of the ACA Compliance Status Quo

	Dependent variable:
	ACA Compliance
Size of Core	−.498**
	(.238)
Mean Core Ideology	−0.152
	(0.127)
Governor Party	0.463**
	(0.191)
Divided Government	−0.110
	(0.074)
Size of Economy	6.21e-07**
	(2.54e-07)
Constant	2.449***
	(0.173)
Observations	49
Log Likelihood	-163.549
Note:	*p<0.1; **p<0.05; ***p<0.01



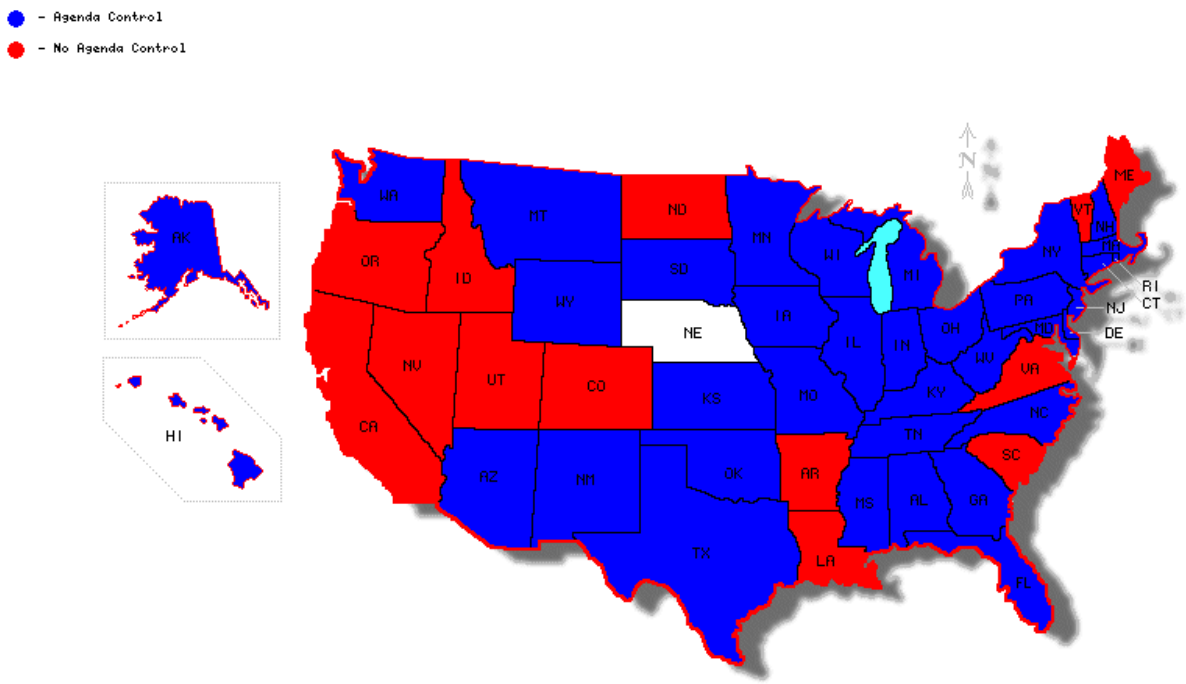


FIGURE 7. Map of Treated and Controlled Units

Source: diymaps.net (c)

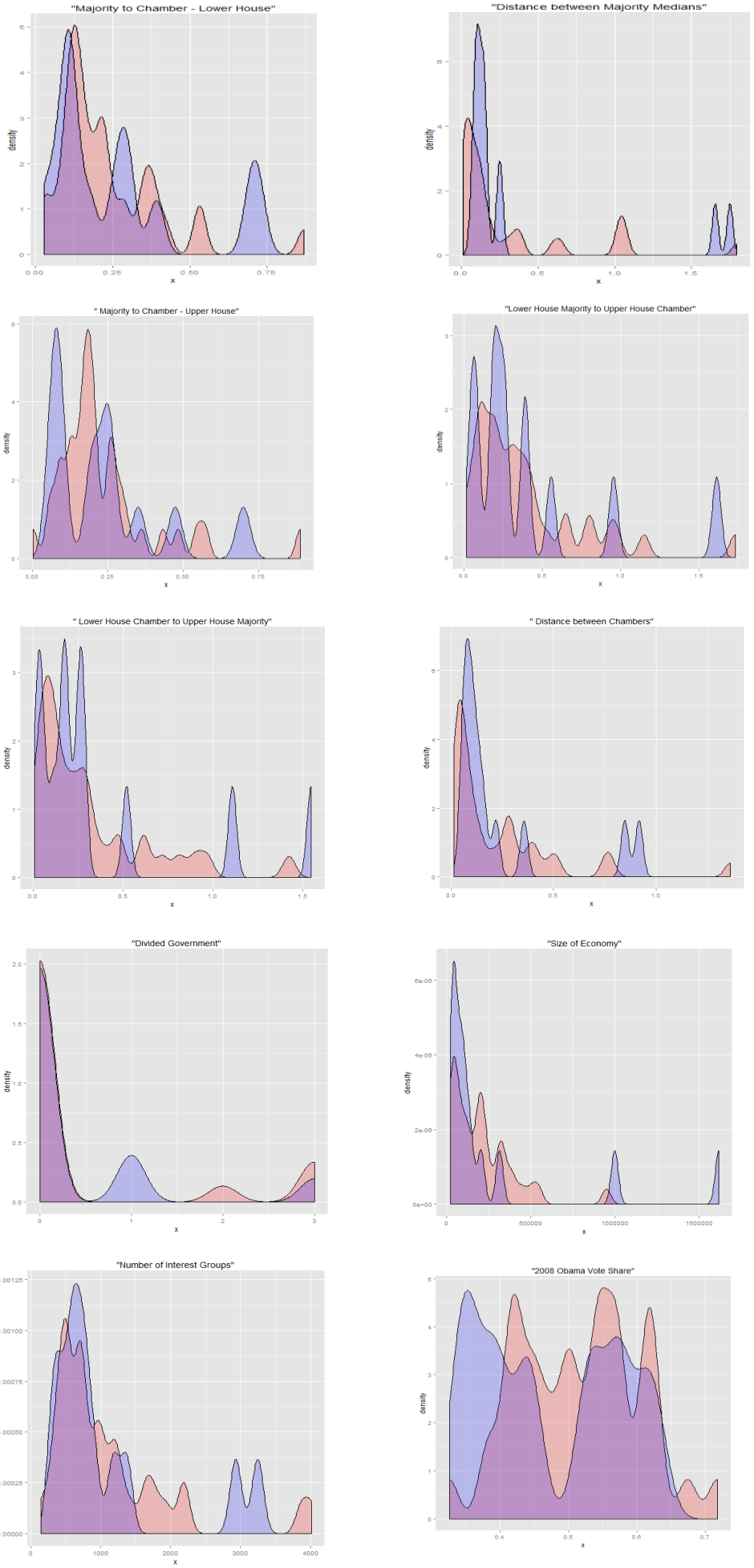


FIGURE 8. Covariate Overlap Between Treated and Controlled Groups

TABLE 4: Covariate Balance Before and After Matching

	Before Matching	*After Matching (ATT)*	*After Matching (ATC)*
Majority Median Distances			
Mean Treatment	0.31868	0.31868	0.30546
Mean Control	0.38054	0.33033	0.38054
Standardized Mean Differences	-14.406	-2.7141	-14.512
T-test *p*-value	0.73341	0.82652	0.146
Majority to Chamber - Lower House			
Mean Treatment	0.23439	0.23439	0.20416
Mean Control	0.24964	0.23503	0.24964
Standardized Mean Differences	-8.2186	-0.34426	-42.591
T-test *p*-value	0.80268	0.97807	0.17567
Majority to Chamber - Upper House			
Mean Treatment	0.21928	0.21928	0.25764
Mean Control	0.28851	0.25968	0.28851
Standardized Mean Differences	-42.041	-24.53	-13.892
T-test *p*-value	0.26446	0.11971	0.47024
Lower House Majority to Upper Chamber			
Mean Treatment	0.371	0.371	0.44479
Mean Control	0.46531	0.39955	0.46531
Standardized Mean Differences	-25.765	-7.799	-4.6275
T-test *p*-value	0.49346	0.23697	0.57153
Lower House Chamber to Upper Majority			
Mean Treatment	0.31708	0.31708	0.29805
Mean Control	0.34474	0.30781	0.34474
Standardized Mean Differences	-8.5254	2.8569	-11.004
T-test *p*-value	0.84268	0.81438	0.42549
Distance between Chambers			
Mean Treatment	0.22419	0.22419	0.27979
Mean Control	0.26644	0.224	0.26644
Standardized Mean Differences	-15.155	0.066424	3.7593
T-test *p*-value	0.6556	0.9946	0.80653
Divided Government			
Mean Treatment	0.44444	0.44444	0.23077
Mean Control	0.38462	0.36111	0.38462
Standardized Mean Differences	5.8277	8.1172	-18.49
T-test *p*-value	0.84132	0.25604	0.15013

~ Table 3 continued ~

Governor's Party			
Mean Treatment	0.44444	0.44444	0.30769
Mean Control	0.38462	0.33333	0.38462
Standardized Mean Differences	11.872	22.048	-16.013
T-test *p*-value	0.71828	0.24726	0.31847
Size of State Economy			
Mean Treatment	236858	236858	211963
Mean Control	230507	241205	230507
Standardized Mean Differences	2.751	-1.8829	-7.1752
T-test *p*-value	0.95976	0.93854	0.75644
Interest Group Population			
Mean Treatment	1173.6	1173.6	979.73
Mean Control	905.73	1006.6	905.73
Standardized Mean Differences	28.453	17.742	9.4972
T-test *p*-value	0.29136	0.10241	0.3988
2008 Obama Vote Share			
Mean Treatment	0.51136	0.51136	0.45007
Mean Control	0.49475	0.48222	0.49475
Standardized Mean Differences	17.892	31.391	-63.051
T-test *p*-value	0.61687	0.18347	0.1262

TABLE 5: Matched Sample (from GenMatch function)

Treated Units	*Control Units*
AK	VA
AL	ND
AZ	LA
CT	LA
DE	VT
FL	CA
GA	LA
HI	ND
IA	OR
IL	OR
IN	UT
KS	SC
KY	VA
MA	LA
MD	LA
MI	LA
MN	CO
MO	NV
MS	ME
MT	NV
NC	AR
NH	AR
NJ	NV
NM	ME
NY	CO
OH	LA
OK	ID
PA	CA
RI	ND
SD	UT
TN	UT
TX	CA
WA	OR
WI	UT
WV	VT
WY	ND

TABLE 6: Results - ATT for Negative Agenda Control

	ATT	**ATC**
Estimate	-13.222	-13.538
AI Standard Error	5.9143	6.6016
T-statistic	-2.2356	-2.0508
p-value	0.025375	0.040289
Original Number of Observations	49	49
Original Number of Treated (Control) Obs.	36	13
Matched number of Treated (Control) Obs.	36	13

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