Party Effects in State Legislative Committees[*]

Short Title: "Party Effects"

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Abstract

"Party Effects in State Legislative Committees"

      Legislative scholars have theorized about the role of committees and whether they are, or are not, tools of the majority party. We look to the states to gain more empirical leverage on this question, using a regression discontinuity approach and novel data from all state committees between 1996 and 2014. We estimate that majority party status produces an eight and a half percentage point bonus in committee seats and a substantial ideological shift in the direction of the majority party. Additionally, we leverage a surprisingly frequent, but as-if random occurrence in state legislatures—tied chambers—to identify majority party effects, finding similar support for partisan committees. We also examine whether the extent of committee partisanship is conditional on party polarization or legislative professionalism, but find that it is not. Our results demonstrate that parties create non-representative committees across legislatures to pursue their outlying policy preferences.

Keywords: Legislative Organization; State Legislatures; Legislative Parties; Committees

In both Congress and the American state legislatures, much of the work of the body is completed within standing committees. This fact has been recognized since the earliest congressional research by McConachie (1898) and Wilson (1885, 79), who famously observed that "...Congress in session is Congress on public exhibition, whilst Congress in its committee rooms is Congress at work." State legislatures use committees in similar ways to Congress, yet their structure and operation remain subject to debate. Are legislative committees primarily organizational tools to achieve efficiency of legislative outcomes, or are they designed to bias those outcomes in the majority party's favor? In particular, how ideologically representative of their parent chambers are legislative standing committees? Whether partisan bias exists in committees had been a staple of legislative research for decades (Krehbiel 1991, Cox & McCubbins 1993, Groseclose 1994, Adler & Lapinski 1997, Aldrich & Battista 2002, Richman 2008), but we re-investigate the topic in the context of American states in order to learn more about the causal effects of party influence.

Previous research has generally failed to find evidence of partisan stacking on committees. Yet, we argue that the literature has not been well-suited to discerning local treatment effects of majority party status on committee composition. Thus, we test the extent to which committees represent their parent chambers using a regression discontinuity design and leveraging a natural experiment which occurs in some states: tied chambers with no majority party. We argue that both empirical approaches allow for direct comparisons between parties having majority status to those that, as-if randomly, fall just short of commanding a chamber majority.

These two empirical approaches demonstrate that state legislative majority parties receive membership "bonuses" on committees that translate into significant ideological shifts away from chamber medians. Our interpretation thus conflicts with much existing research that fails to find that committees are statistical "outliers" and confirms a key prediction of how party power might matter in legislative organization.

This research contributes to a long standing debate in the literature and does so in a

substantively informative way. We also examine the extent to which compositional bias has downstream effects on legislative agendas and policies (Anzia & Jackman 2012, Cox & McCubbins 1993, Kiewiet & McCubbins 1991, Jackman 2013, Maltzman 1998) by analyzing coalition sizes (a proxy measure of policy extremity) in tied, majority-less chambers and chambers with bare-majority party control. In tied chambers, coalition sizes are significantly larger, implying that party effects are consequential for policy outcomes. Our findings are especially important as state legislatures increasingly take responsibility for legislating on such salient topics as religious freedom, gun control, health policy, and voting rights and ballot access (Moncrief & Squire 2013).

An additional advantage of examining committees at the state level is the variation in institutional and political contexts across states. The recent rise in the strength and influence of party leaders in Congress (Lee 2009, Curry 2015, Koger & Lebo 2017) might imply that leaders are increasingly able to stack committees with ideologically extreme (partisan) members. We thus investigate whether more polarized legislatures allow the leadership greater autonomy in creating ideologically extreme, non-representative committees (Rohde 1991, Aldrich & Rohde 1997). Similarly, we examine whether the committee-stacking effects we find across states differ based on levels of legislative professionalism. Interestingly, we find no evidence that either of these two features change the relationship between majority status and party effects in state committees.

Previous attempts to assess committee representativeness in the states have proven difficult because data limitations have driven most research to focus on select sets of committees in a few states and/or for a limited set of years. We introduce a new dataset which measures membership in all standing committees in all state chambers from 1996-2014, resulting in far more observations than is typical in congressional studies or previous state research. The data provide sufficient variation across different contexts to explore how committee composition changes as a result of partisan and institutional factors, and will be useful to other researchers interested in questions of committee composition and

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state legislative behavior and outcomes.[1]

Theories of Committee Composition and Legislative Outcomes

In majoritarian legislatures, the median must be included in any winning coalition (Black 1948), and competition between two coalitions should lead to outcomes situated at exactly the median's ideal point (Downs 1957).[2] Thus, standing committees would seem to have to serve the needs of the median or pivotal member, perhaps acting as an efficient information gathering mechanism (Krehbiel 1991).[3] Median-oriented theories of legislative organization suggest that while committees are not representative of their parent chambers in terms of policy-level expertise, in most cases their collective ideological preferences or demand for distributional goods should not diverge much from the median's (Gilligan & Krehbiel 1990, Krehbiel 1990).[4]

American legislatures have long been viewed as having weak parties compared to their counterparts in parliamentary systems (Carey 2007, Cox 2000), but recent trends, including a reduction in committee autonomy and the rise of polarization, have led to a re-examination of the extent to which committees serve partisan interests (Smith 2000). Notably, committee appointments are largely controlled by the party leadership at both the congressional and state levels, providing a mechanism by which committees may be stacked with ideological outliers (Kanthak 2009, Masters 1961, Rosenthal 1998, Sinclair 1983). Because the party leadership has an interest in dictating the legislative agenda, stacking committees ensures that only relatively extreme, non-median policies will be

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[1] Alexander Fouirnaies and Andrew B. Hall have collected similar data (Fouirnaies & Hall 2018 (Forthcoming)). We collected our data independently (and concurrently), and note that our data are available at the individual member-committee level. Fouirnaies and Hall have instead classified members as belonging only to an issue area based on committee names, rather than documenting committee-specific membership. Thus, our data allow for a more refined measure of member committee assignments, uniquely allowing us to conduct the analysis presented here.

[2] This assumes open proposal rules, but in majoritarian legislatures, the median should ensure the rules empower it to make proposals.

[3] In non-majoritarian legislatures such as the U.S. Senate, similar inferences hold, but the pivotal member changes based on the institutional rules of the chamber (Krehbiel 1998).

[4] A few select committees may have significantly different preferences than the chamber median's if the committee can engage in low cost specialization.

considered by the floor while issues that divide the majority party will be kept off the agenda, preventing potentially damaging policy proposals from reaching the pivotal member (Anzia & Jackman 2012, Cox & McCubbins 1993, Cox & McCubbins 1994, Cox & McCubbins 2005, Cox, Kousser & McCubbins 2010, Jackman 2013, Sinclair 1994). Co-partisans are thus able to realize disproportionate gains from legislation and collective benefits accrue when non-median policies become law and promote the party's reputation in the minds of voters, helping to increase party reelection rates (Cooper & Brady 1981, Cox & McCubbins 2005, Sinclair 1995, Koger & Lebo 2017).

The observable implications of these theories differ. Partisan theories of committee composition suggest that committees should be made up of partisan or ideological outliers who represent the party median rather than the chamber median. Median-based theories do allow for some extreme (non-representative) committees if the members of the committee can engage in low cost specialization. As Krehbiel (1991, 96) says, these "exceptional cases" may exist if a rational legislature wishes to tap the special preferences and expertise generated by these members. Though these committees might exist, they should be extremely rare and occur only when a subset of members have prior expertise on an issue area. And, even in these cases, membership is likely to be composed of heterogeneous preferences (Krehbiel 1991, 139-141), which dilutes measures of ideological extremity such as the committee median, producing committees that look very ideologically similar to their parent chambers.

The evidence we find for non-representative committees along compositional and ideological dimensions is important empirical support for partisan theories of legislative organization and the claim that the laws produced by the legislature are partisan, rather than median or distributive in type. Given the amount of data we analyze, it is unlikely our findings are driven by "exceptional cases" of outlying committees, but instead are a systematic feature of state legislatures across a long period of time.

Existing Empirical Evidence on Committee Composition

There has been significant scholarly attention devoted to identifying committees as representative (or not) of their parent chamber, in both Congress and the states. The standard approach in the literature is to use interest group or ideology scores to approximate the congruence between the mean or median of the committee and the chamber mean or median. Early work used interest group ratings from organizations like *Americans for Democratic Action* to measure the similarity between committee members and the entire House of Representatives on a small number of votes within specific issue areas (Groseclose 1994, Krehbiel 1990, Hall & Grofman 1990, Maltzman & Smith 1994). Later work focused on ideological scores that incorporate all votes taken within Congress through the use of NOMINATE scores (Cox & McCubbins 1993, Londregan & Snyder 1994, Poole & Rosenthal 1997). Other approaches have sought to examine constituency characteristics (Adler & Lapinski 1997, Adler 2000), or to combine various aspects of each theory to refine predictions about which types of committees should be preference outliers (Maltzman & Smith 1994, Maltzman & Sigelman 1996), and though there is some evidence that a few congressional committees may be composed of policy "high demanders" (Shepsle & Weingast 1981), evidence that outlying partisan committees are a systematic feature of Congress remains scant.

Committee Composition in the States

Most of the research on committee composition has focused on the U.S. House (Ray 1980, Groseclose 1994), limiting inferences about whether states also have outlying committees, and preventing analyses of whether different legislative institutions affect the extent to which committees mirror the preferences of chamber floors. State legislatures offer a more diverse institutional context in which to study committee composition and a growing body of work seeks to leverage this variation (Battista 2004, Overby & Kazee 2000). Not only is there significant variation in party strength across the states (Wright & Schaffner 2002), but there is cross-sectional and within-state variation on im-

portant factors which may affect party influence, including the size of the majority party, the level of polarization within the legislature (Shor & McCarty 2011), and the level of professionalism of legislators.

State-level empirical work has largely followed the congressional model of comparing interest group ratings of committee members to the floor. Overby & Kazee (2000), Overby, Kazee & Prince (2004), and Prince & Overby (2005) all use interest group ratings of legislator ideology generated by the *National Federation of Independent Business*, and find little evidence of systemic partisan committees. Aldrich & Battista (2002) and Battista (2004) generate NOMINATE scores for legislators for a handful of states and find that committees are representative of their parent chamber, while Hamm, Hedlund & Post (2011) take a different approach by showing that the occupations of committee members reflect committee jurisdictions, which they argue supports the claim that committees provide information and expertise to the median. Richman (2008) finds empirical support for the claim that the median has incentives to create unrepresentative committees when its level of uncertainty about how policy choices map onto outcomes is high. The notable exception to state legislative research broadly supportive of representative committees is Hedlund, Coombs, Martorano & Hamm (2009), who use a much larger dataset than previous research, drawing on 49 states over two sessions, to show that majority party members are disproportionately represented or "stacked" on committees.

Estimating the Treatment Effect of Majority Party Status

From the majoritarian standpoint outlined above, it is unsurprising that examinations of chamber and committee congruity fail to find significant differences. Chamber majorities (reflected by the median's preferences) should not countenance many committees that are statistical outliers from their collective preferences (Krehbiel 1991, Krehbiel 1993). Yet, as even proponents of nonpartisan theories of legislative organization admit (e.g., Krehbiel 1991), this lack of evidence of party stacking is not dispositive, especially considering that committee representativeness is usually posed as the null hypothesis and

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party theories are burdened with a high standard of proof. Thus, there is often controversy over which critical value constitutes a "significant" difference, and even small changes in how committees are classified (outlying or not) have large effects on the conclusions that are drawn (Hall & Grofman 1990, Groseclose 1994). While using a .05 critical value minimizes the probability of Type I errors, scholars have made other choices, including using a .15 (or even .25) critical value to identify an outlying "tendency" (Battista 2004, Battista 2009, Sprague 2008). By categorizing committees as discretely representative or not, this approach ignores relevant information about how far majority parties can move committees toward their preferences.

In addition, tests of significant differences suffer from endogeneity problems, as party platforms, campaigns, voters, and a multitude of other factors affect both the types of legislators on committees as well as the partisan composition of the chamber. Chambers that are dominated by a strong majority party may not need to "bias" their committees much to pass their preferred policies; yet, these very majorities would have the most potent ability to stack committees. Evidence that committees are not outliers in this context is thus largely uninformative regarding the existence of party effects. It is likely that majority parties in this position know how much to extract policy gains from friendly committees without stacking the committees enough to produce statistical outliers and thus triggering institutional backlash or negative electoral consequences. In our view, we cannot learn much about party effects from these extreme cases of party dominance. Instead, we would like to know precisely how much parties can benefit from majority status when that status is most tenuous. That is, we are interested in local treatment effects where the two parties exist in a highly competitive environment, rather than global cross-sectional comparisons of politics under one-party dominance.

Our strategy offers a more comprehensive approach and has a number of advantages over previous research. First, most previous work limits its focus to partisan stacking in terms of number of party members on committees (e.g., Hedlund et. al. 2009), but as Krehbiel (1993) notes, ideological stacking may occur even if partisan committee mem-

bership reflects chamber membership, and vice versa. Here, we measure both committee membership and committee ideology, allowing us to account for the different ways in which committee may be outlying. Second, we use a much larger dataset than previous research, measuring far more committees across a longer timespan than any previous research. Finally and most importantly, our approach not only overcomes endogeneity problems, but more broadly, changes the counterfactual used in the literature. All previous research has implicitly adopted a null hypothesis of representative committees and sought evidence for the alternative of partisan stacked committees. Instead, we compare committee composition under majority party status to committees under near majority party status. This is a more direct test of majority party power and overcomes many of the limitations described above, such as defining "significant differences."

Regression Discontinuity Design

Here, we use two techniques that allow us to gauge the local treatment effect of majority status on committee composition, overcoming the issues described above. The first is use of a regression discontinuity (RD) design, an approach that is now quite common in political science and economics (Skovron & Titiunik 2016, Lee & Lemieux 2010). Regression discontinuity designs are used to make claims of local average treatment effects in observational data. Treatment status in an RD design is a deterministic and discontinuous function of some "running variable" (also known as a "forcing variable" or "score"). The power of the RD design lies in the fact that the running variable can smoothly control for variation in the outcome, so that any observable discontinuity in the relationship between the running variable and the outcome can be attributed to treatment status, especially in the "neighborhood" of the assignment threshold. This is because, in a correctly specified RD study, observations just to the left of the assignment threshold (control group) and those just to the right of the threshold (treatment group) are as-if randomly assigned.

The "treatment" is majority status, which gives the majority party control over cham-

ber procedures and an advantaged role in selecting members for committees.[5] Because majority party status is exogenously imposed as a discontinuous function of a continuous running variable (namely Democratic [Republican] seat share in the chamber reaching the threshold of greater than 0.5 of the chamber), the necessary components exist to specify an RD study of majority party bias in committee composition. Our setting is similar to recent studies that examine the treatment effect of incumbency on legislator outcomes (Lee 2008, Fouirnaies & Hall 2014, Eggers, Fowler, Hainmueller, Hall & Snyder Jr. 2015, Hainmueller, Hall & Snyder Jr. 2015). Instead of examining individual vote margins as a running variable conferring an election treatment, we are exploiting chamber-level variation in seat shares and its deterministic assignment of majority status.[6] In the case of committee composition, those factors which cause both majority status and the types of legislators serving on committees are balanced between the treated committees (those in a legislature with a slim Democratic [Republican] majority) and the control committees (those in a legislature with a slim Republican [Democratic] majority), overcoming the inferential concerns associated with conducting t-tests on committee representativeness. We are principally concerned with the possible effects majority status has on a critical institutional outcome: the extent to which state legislatures produce party-"biased" committees. The concepts used to capture committee representativeness within a state legislature are majority party seat share on a committee, and median

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[5]Here, majority party status (the treatment) is conferred if and only if the seat share for one major party exceeds that of the other.

[6]Our setting does differ from individual vote margins in a potentially important way. Chamber seat shares are aggregations of individual elections that may or may not themselves be closely contested. Some suggest using a multidimensional RD design that uses the geometric distance from an aggregated threshold comprised of multiple running variables (Feigenbaum, Fouirnaies & Hall 2018 (Forthcoming)). In fact, Feigenbaum, Fouirnaies & Hall (2018 (Forthcoming)) assert that seat share, as we use, is "not a valid running variable" for their purposes, due to weak balance test results. For our sample, though, we demonstrate that lagged (at both $t-1$ and $t-2$) chamber Democratic seat shares are strongly balanced around the treatment assignment threshold, indicating that future majority status is not generally manipulable by close majorities. Although majority parties no doubt seek to use their resource advantages to perpetuate their status (and may well successfully focus on individual races), they do not systematically hold that advantage in future close aggregate elections. While the Feigenbaum, Fouirnaies & Hall (2018 (Forthcoming)) multidimensional RD technique is promising for analyzing thresholds for divided government across different institutions (Kirkland & Phillips N.d.), within chambers, we consider elections that contribute to party seat shares to be analogous to test items that are aggregated to test scores for use in an RD design (Thistlethwaite & Campbell 1960).

committee ideology. In each case, just above the seat share threshold of majority party assignment, we predict statistically significant treatment effects in the direction of the majority party.

Majority Party Treatment Hypotheses:

1A. A significant, positive discontinuity in committee seat share occurs when a party becomes the chamber majority.

1B. A significant discontinuity toward the majority party in median committee ideology occurs when a party becomes the chamber majority.

Tied Chamber Design

Our second empirical technique is to leverage a natural experiment that occasionally occurs in some states: tied legislative chambers. Many state chambers have an even number of seats and partisan ties are not uncommon. A tied chamber requires the parties to create power sharing agreements and forces legislative compromise. While the parties and their leadership may find tied chambers a frustrating inconvenience, they offer a unique opportunity to test theories of party influence in legislatures.

In a tied legislative chamber, there is no majority party and no median member. Chamber votes, including procedural votes such as those for a slate of committee members, can only be decided by capturing at least one vote from a member of the opposing party. Median voter theory suggests that outcomes should collapse to the space halfway between the *two* middle voters such that each receives the closest possible outcome to their ideal point which can still achieve a majority. Thus, any committee assignments made which disproportionately favor one party can be defeated by a simple majority vote of all members on the other side of the two middle voters. This should lead to perfectly representative committees and our primary expectation is that committees in tied chambers are more reflective of chamber preferences than they are when one party can assert full control over committee selection and skew committee preferences away from

the chamber and toward the majority party.[7]

Tied Chamber Hypotheses:

2A. A significant, positive discontinuity in committee seat share occurs when a majority party exists as compared to tied chambers.

2B. A significant discontinuity toward the majority party in median committee ideology occurs when a majority party exists as compared to tied chambers.

Additional Tests for Heterogeneous Treatment Effects Across States

The variation in contexts across states allows for comparisons in committee representativeness between legislatures based on party strength and polarization (Aldrich & Battista 2002, Wright & Schaffner 2002), and on levels of legislative professionalism (Squire 2007). We leverage state differences on these two dimensions to investigate whether heterogeneous treatment effects occur when the state context differs.

Conditional party government theory suggests that internally cohesive parties which substantially differ from the other party have incentives to tighten their control on legislative procedures, including the committee system, in order to produce more partisan bills (Aldrich & Rohde 1998, Bianco & Sened 2005, Cox & McCubbins 1993). In the context of the U.S. House, there is some evidence that committee medians move toward the majority party when it is more ideologically unified (Young & Heitshusen 2003). As a result,

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[7]The parties in tied chambers typically set up power sharing rules that differ from state to state. We argue that for our purposes, understanding the details of these power sharing arrangements is unimportant given that the rules themselves are endogenous to the chamber and are thus the product of median-oriented rules creation (Krehbiel 1991). Power sharing agreements which give one party an advantage can be defeated by the other party, and any abuse of the rules can be stopped by the other party. Though power sharing agreements may produce some stacked committees in a legislature for one party, these committees should be canceled out by other committees stacked in favor of the other party. Occasionally, members of the majority party defect and join with members of the minority party to create a new majority coalition. Just such a situation occurred in the New York State Senate in 2012 when a group of eight Democrats caucused with Republicans to give Republicans control of the chamber. If a minority of the majority join with the minority to form a new ruling coalition, committees should either be 1) not stacked and distributed in proportion to each party's coalition size and ideological distribution or 2) stacked with members from the minority party and majority party defectors. In either case, this will mitigate the relationship between the observed majority party and control of committee seats. In the case of New York, the observed majority party is Democratic, but the committee seats, based on anecdotal accounts, were biased toward Republicans. Any such idiosyncratic occurrences in our data would bias findings against our expectations.

partisan control over committees may be electorally induced (Aldrich & Rohde 2000) and exacerbated when parties become more willing to delegate to their leadership as a means of enforcing collective action and distributing party-based benefits (Rohde 1991). This should translate into a greater ability for leaders to stack committees in their favor, exactly what observers of Congress have surmised in the current era of high polarization (Lee 2009, Curry 2015, Koger & Lebo 2017, McCarty, Poole & Rosenthal 2006). We test whether more polarized legislatures produce more heavily stacked committees, extending previous research which, because of a lack of institutional variation, has not examined how variation in levels of polarization influences committee stacking. If parties control committee assignments even in non-polarized chambers (that is, there are no hetero-geneous treatment effects in polarized chambers) it suggests that committee control is an important tool of party power even in relatively bipartisan or ideologically moderate legislative environments.[8]

The second institutional context we are interested in is the level of professionalism within the state legislature. In less professionalized legislatures, members have low levels of information about policy problems and solutions (Berry, Berkman & Schneiderman 2000, Berkman 2001) and are subordinate to the governor and other institutional actors (such as lobbyists) (Kousser & Phillips 2009, Kousser & Phillips 2012). This might lead legislators to grant more discretion to the leadership, which in turn would create more partisan committees. Indeed, legislators themselves report depending more on the party in less professionalized chambers (Anderson, Butler & Harbridge 2016). Still, members in more professional legislatures are career-oriented, frequently face credible reelection chal-lengers, and depend heavily on resources and support from the party and partisan donors, increasing individual levels of extremity (Barber 2016). These factors may make members more willing to defer to the party to organize collective action and structure the legislative development process. Before the modern era of legislative polarization in states, Squire (1988) speculated that greater career opportunities promote more professional legisla-

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[8]We have also examined whether majority party homogeneity increases committee stacking, and find no evidence it does (results not shown).

tures with more autonomous members, but even those types of chambers had relatively powerful speakers. Further, previous empirical work on professionalism finds that higher levels are associated with more chamber polarization (Hinchliffe & Lee 2016, Overby, Kazee & Prince 2004).[9] Our data allow for a test of these competing claims about the relationship between professionalism and party strength.

Committee Composition Data

As reviewed above, there is a substantial amount of research on the representativeness of committees in both state legislatures and Congress, but state-level work focuses exclusively on a small sample of state legislatures or a cross-section of the states at a point in time. We aim to estimate more general party effects across legislatures and over time. To this end, we constructed information on committee memberships from various digital editions of the *State Yellow Book* for all states from 1996-2014.[10] This source includes a list of each standing committee in each state chamber for each year.[11] Included here are lists of the full membership of each committee, as well as party and leadership identification. We recorded these lists and merged them with Shor and McCarty's (2011) data on individual state legislators' ideal points. The resultant individual-level dataset includes information on legislator names, party identification, Shor-McCarty ideal points, and committee memberships. We then aggregated these individual data to the level of the individual committee, calculating a count of the number of Democrats, Republicans, and third party legislators in each committee, as well as measures of central tendency

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[9]For example, California is the most professional and the most polarized state legislature according to Shor & McCarty's (2011) ideology scores.

[10]For 1996-2007, we scanned print copies of each yellow book and used optical character recognition software to create digital copies. For 2008-2014, we purchased access to digital copies of the yellow books though the publisher, Leadership Directories (`http://www.leadershipdirectories.com/`).

[11]We are concerned only with *standing* committees and exclude information on "special", "select" or joint chamber committees. This leads us to completely omit Connecticut and Massachusetts, where state legislatures do not have any standing committees. We also omit Nebraska, as it has a nonpartisan legislature.

(medians) for the ideal points of aggregated committee members.[12]

Aggregating by committee-year and omitting joint or special ad-hoc committees, we have 31,699 committees in the data, and 30,509 committees where we have ideology data. The mean number of committees in a chamber in a given year is high compared to Congress (where the House averages 20 committees and the Senate 16 in recent years): just over 26 committees (SD: 14.2, Min: 4, Max: 85). The average size of these state committees is 10.85 members, with a standard deviation of 7.39 and a range from 1 to 124 members.[13] For each committee, we are able to calculate the proportion of the membership belonging to each party, in addition to aggregate ideology scores, each of which we will examine in our empirical analyses. This dataset, encompassing all states with standing committees across 19 years, is the most comprehensive state legislative committee data available and offers an opportunity to test theories of partisan committee composition across a range of institutional and ideological contexts. We describe these data in more detail in appendix A below.

RD Design Validation and Results

The committee outcomes we analyze in our RD design are the proportions of committee members belonging to the majority party and the ideological medians of the committees (Battista 2009, Groseclose 1994). If parties are able to stack committees disproportionately and ideologically, these outcomes are likely to be starkly distinct from each other in the neighborhood of the threshold that determines majority status in the chamber. The running variable is Democratic seat share in the chamber which determines the treatment (majority status for the Democrats) that should theoretically affect the outcomes of interest (proportions of committee members in the majority party and the ideological medians of committees). As described above, we have committee composition

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[12]We follow the advice of Groseclose (1994) to focus on medians, as they are more theoretically relevant to policy-making. We have alternatively assessed committee means as an outcome and reached the same substantive conclusions.

[13]We alternatively omit very small committees (membership less than 3 members) from the sample, with no substantive changes to our overall findings.

data at the level of the individual committee. Yet, since the treatment (majority status) is conferred at the chamber level, we aggregate these committee measures of Democratic seat share and median ideology up to the chamber level, by taking the mean of each across committees in each chamber-year.[14] We thus have the necessary elements of a credible RD design, but they are not sufficient to guarantee causal inference and we undertake additional efforts to validate the design in our setting.[15]

As developed above, our design takes advantage of the fact that majority status is conferred whenever either party achieves a simple majority of the two party seat share. State legislatures sometimes have third party legislators, so to calculate this as a running variable, we measure the number of Democrats in a chamber divided by the total number of Democrats plus the total number of Republicans in that chamber.[16] In order for the RD design to produce valid inferences about treatment effects, the distribution of the running variable needs to be smooth near the threshold (which is $.5 + \epsilon$ for our running variable). This property is what allows us to assume that observations immediately to the left and to the right of the threshold are as-if randomly determined.[17] If, instead, units (in this case, chambers) were able to manipulate treatment status endogenously, the primary benefit of the RD design is lost. In our case, we can be sure that parties within

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[14]We do this so that we do not artificially deflate our standard errors for committees clustered in chambers. In aggregating committee measures to the chamber-year level, we are assuming that each committee contributes equally to the chamber means. This masks much committee-level variation, such as might exist between so-called "control" committees (Rules, Ways and Means, Appropriations) and less broad jurisdictional committees. Although we leave work on committee-level microfoundations to future work, we do present confirmatory results using the committee-level outcomes in appendix E.

[15]Some states have rules or norms that require the number of partisan members on committees to reflect the number of members in the chamber. The extent to which these norms or rules are enforced is unclear, but they should bias any partisan effects on committee membership *downward*, producing more conservative tests of our hypotheses. In order to assess this we have included Appendix C, which splits state chambers according to whether the majority party has discretion to disproportionately stack committees versus those chambers whose rules explicitly mandate committee composition to be proportional to chamber membership. These results clearly indicate that "party power" chambers drive the aggregate results and "proportional representation" chambers exhibit no discernible majority party bias.

[16]Therefore, without loss of generality, calculating the Republican chamber share gives us the same inferences. Our committee data include the population of state legislators in a given chamber-year and party identification, so we calculate the running variable using these data from the *State Yellow Books*. By contrast, our measures of committee proportions include third party legislators in the denominators.

[17]A continuity-based RD approach assumes that the running variable is a continuous random variable and thus, that, no observations exist exactly *at* the threshold. In our setting, we do have tied chambers, but these are theoretically distinct from the majority/minority dynamics that normally exist. Thus, we omit these tied chambers for now and deal them specifically in a subsequent section.

chambers *each* want to be the majority party and will expend resources to manipulate this outcome, especially in close elections (near our assignment threshold.) Yet, since each party aims for this and the running variable is zero-sum between the parties, close elections can be considered to be determined by chance, on average (Eggers et. al 2015, though, see Caughey & Sekhon 2011). Nonetheless, it may be the case that Democrats or Republicans win more close state races than the other because of systematic resource advantages or other factors.[18]

The specific framework we rely on to estimate the causal effect of majority party on committees is the continuity-based approach that is easiest to demonstrate visually (Skovron & Titiunik 2016, p. 11). Figure 1 plots average values for the first outcome ("Proportion Democrats in Committee") by fine bins (length of 0.001) of the running variable.[19] The top of the figure combines all of the lower house and upper house data together, and the bottom separates out state lower houses from upper houses, as Sinclair (1997) finds that there is less of a majority party bonus for the U.S. Senate than there is for the House of Representatives.

The underlying relationship between the running variable and the outcome is approximated with two third order global polynomials, one for values of the running variable from 0 to less than 0.5 and the other for values larger than 0.5 to the maximum value. These are meant to estimate the relationship between the two variables for the control and treatment groups, respectively. Of course, one cannot observe this relationship for control observations to the right of the threshold or for treatment observations to the left of the threshold, given than the running variable sharply determines treatment. However, we can compare the two curves *locally*; that is, very near the threshold. The vertical

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[18]Although the assumption of exogeneity of treatment assignment is untestable itself, there are a number of standard ways to test the implications of exogeneity (Skovron & Titiunik 2016), and we conduct these validation checks and falsification tests in appendix B. Most specifically to the validity of the running variable, we plot the density of our running variable and use the local polynomial density estimator described in Cattaneo, Jansson & Ma (2015) in appendix figure B1. We also demonstrate, in appendix table B1 and figures B2 and B3 that Democratic seat share in periods $t-1$ and $t-2$ are balanced around the threshold of the running variable.

[19]Binned averages are usually more informative than raw scatterplots, especially with as many observations as we have.

distance between the two curves (about eight and a half percentage points in figure 1) approximates the local average treatment effect of majority status. As useful as this figure is in demonstrating a discontinuity in the relationship between chamber seat share and committee seat share, we can be more formally precise with our estimate.

First, we need to specify the "local neighborhood" of the assignment threshold. The general approach is to choose a bandwidth that includes observations both to the left and right of the threshold and then fit local polynomials to the control and treatment data. The point estimate of the treatment effect can then be calculated by the vertical distance between where these polynomials intercept the threshold. The standard way to select a bandwidth is to choose the one that minimizes the mean squared error of the polynomial fit.

Table 1: RD Effects of Democratic Chamber Majority on Committee Outcomes in Chamber. Local Polynomial Analysis.

Outcome: Chamber Mean of Democratic Seat Share in Committees

|			Conventional		Robust	
Polynomial	Bandwidth	Point Estimate	p-val	95% CI	p-val	95% CI
State Lower Chambers and Upper Chambers						
1	0.065	0.084	<0.001	[0.048, 0.120]	<0.01	[0.045, 0.131]
2	0.147	0.090	<0.001	[0.058, 0.122]	<0.01	[0.055, 0.129]
State Lower Chambers Only						
1	0.055	0.089	<0.001	[0.042, 0.136]	<0.001	[0.043, 0.150]
2	0.144	0.105	<0.001	[0.064, 0.145]	<0.001	[0.065, 0.156]
State Upper Chambers Only						
1	0.052	0.066	<0.10	[-0.006, 0.138]	<0.20	[-0.032, 0.145]
2	0.105	0.045	<0.25	[-0.033, 0.124]	<0.45	[-0.055, 0.127]

Outcome: Chamber Mean of Median Ideology (Shor-McCarty) in Committees

			Conventional		Robust	
Polynomial	Bandwidth	Point Estimate	p-val	95% CI	p-val	95% CI
State Lower Chambers and Upper Chambers						
1	0.057	-0.559	<0.001	[-0.749, -0.369]	<0.001	[-0.805, -0.339]
2	0.109	-0.573	<0.001	[-0.776, -0.369]	<0.001	[-0.838, -0.376]
State Lower Chambers Only						
1	0.046	-0.510	<0.001	[-0.783, -0.237]	<0.001	[-0.859, -0.216]
2	0.102	-0.585	<0.001	[-0.851, -0.318]	<0.001	[-0.926, -0.320]
State Upper Chambers Only						
1	0.074	-0.488	<0.001	[-0.726, -0.249]	<0.001	[-0.762, -0.173]
2	0.157	-0.441	<0.001	[-0.687, -0.196]	<0.01	[-0.716, -0.152]

 Note: Dependent variables measured using individual committee data and aggregating to the chamber level by taking the mean of all chamber committees for each outcome. Output from `rdrobust`. For Polynomial order=1, point estimated by OLS, regressing outcome on the running variable on either side of the 0.5 threshold within the optimal bandwidth. For Polynomial order=2, point estimated with quadratic function of running variable. Conventional and robust (Calonico, Cattaneo & Titiunik 2014*b*) p-values and 95% confidence intervals.

In the top half of table 1, we present such estimates for our first outcome.[20] Here, we

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[20]Regression discontinuity designs are sometimes criticized on external validity grounds; the local treatment effect is specific only to those cases which are close to the treatment assignment. However, as Aronow & Samii (2016) point out, regression suffers from the same issue. As they say, "There is no general external validity basis for preferring multiple regression on representative samples over quasi-experimental or experimental methods [250]." Also see appendix D which shows the states that contribute most to the RD models.

Figure 1: Regression Discontinuity Plot. Outcome: Chamber Mean of Democratic Seat Share in Committees
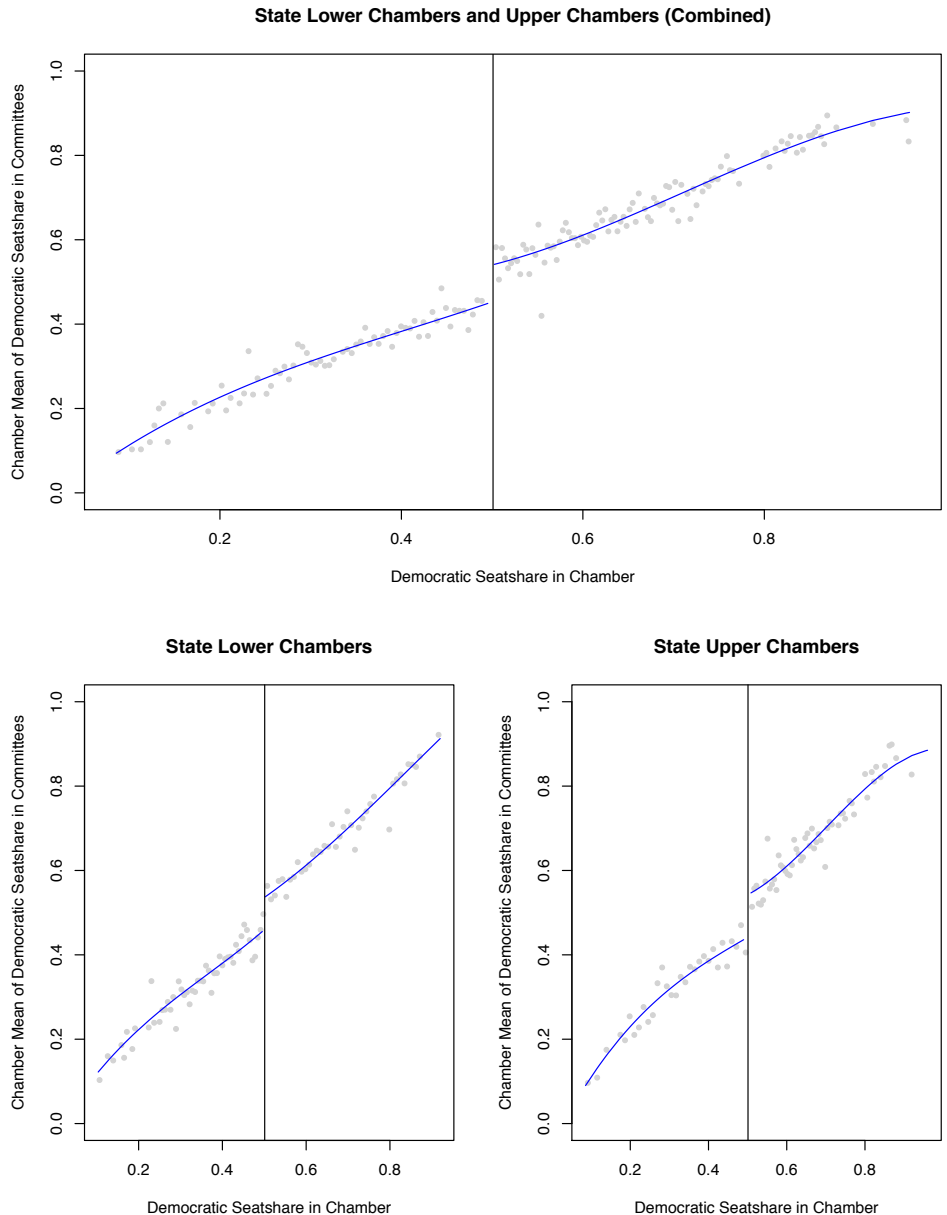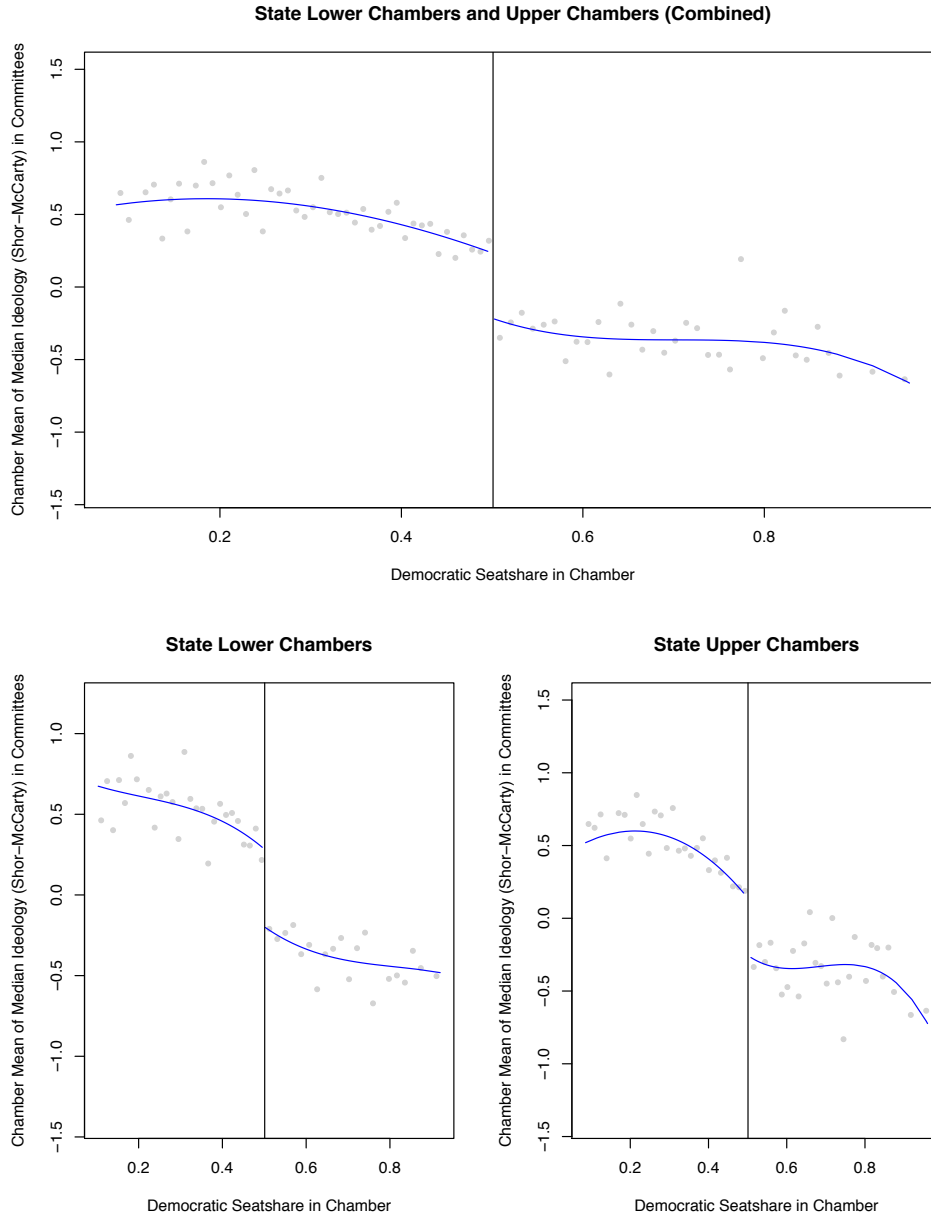

Note: Plot shows averages of the proportion of Democrats in committee by 1 percentage point bins of the proportion of Democrats in the chamber running variable. When the Democratic party crosses the threshold (located at 0.5) from minority party to majority party in a chamber, its share of seats in that chamber's committees increases substantially.

start by fitting an OLS regression (first order polynomial) to the observations on either side of the threshold.[21] We see that the optimal bandwidth here is 0.065 (on the 0-1 scale

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[21]`rdrobust`, developed in Calonico, Cattaneo & Titiunik (2014*a*) and Calonico, Cattaneo & Titiunik (2014*c*) implements optimal bandwidth selection, along with robust estimation of confidence intervals for RD estimators. See Calonico, Cattaneo & Titiunik (2014*b*) for the details on robust estimation.

Figure 2: Regression Discontinuity Plot. Outcome: Chamber Mean of Median Ideology (Shor-McCarty) in Committees

Note: Plot shows averages of median committee ideology by 1 percentage point bins of the proportion of Democrats in the chamber running variable. When the Democratic party crosses the threshold (located at 0.5) from minority party to majority party in a chamber, the ideology of the chamber's committees becomes noticeably more liberal.

of our running variable). The point estimate of 0.084 is thus estimated using only those observations where the running variable is between 0.435 and 0.565. This is a statistically significant treatment effect and can be interpreted to mean that majority status allows a party nearly an eight and a half percentage point bonus in the number of committee

seats they get, smoothly controlling for the fact that higher seat shares in the chamber will usually bring higher committee seat shares. This finding is virtually identical to the vertical distance between the two curves as shown in figure 1 and is strongly supportive of hypothesis 1A. We replicate this analysis with a second order polynomial and see that the treatment effect increases somewhat and is still significantly distinguishable from zero, using conventional and robust standard errors (Calonico, Cattaneo & Titiunik 2014b). Just as in figure 1, we separately estimate treatment effects for state lower and upper chambers and see that, while the point estimates are larger for lower chambers, they are not statistically significantly so.

The effect sizes that we find are all statistically discernible from zero, and for an average committee size of 11 members, indicates that the majority party receives an additional seat, which is not particularly surprising given that we should expect the majority party to earn at least bare majorities on committees. What is more interesting and consequential is that this numerical advantage produces large effects on aggregate ideology. In figure 2 and the bottom half of table 1, we replicate our design with respect to a new outcome: the median ideology score on each committee (averaged across all committees in a chamber, as with the seat share outcome). As noted above, we used the Shor and McCarty (2011) scores to measure an individual's ideology and create the committee median from these data. These scores are time invariant for individuals, so over time changes are driven entirely by compositional change. Positive scores indicate increasingly conservative ideology and negative scores indicate liberal ideology.

Figure 2 demonstrates the apparent discontinuity that occurs in committee ideology when Democrats become the majority party. Although the averages in the outcome variable are more spread out than they were in figure 1, the apparent treatment effect is quite large. The empirical estimates found in the bottom portion of table 1 support hypothesis 1B, indicating that the effect of majority status on committee median ideology is close to -0.56 in the full data and is statistically distinguishable from zero for both polynomial specifications. This amounts to about a standard deviation-sized "bonus" in how liberal

a committee's ideology is when the Democrats have a chamber majority as compared to when they narrowly do not. This suggests that even the smallest of majorities are able to use their majority status to exercise ideological control over committees. This result is strongly consistent with theories of majority party power (Fortunato 2013).

There are two possible mechanisms driving these committee ideology results. The first is simply that the numerical stacking in majority party seat share that we have found simply translates into ideological stacking as well. There is no doubt that this occurs. For example, we can regress the median committee ideology variable on the proportion of committee members who are Democrats. This bivariate regression explains 55% of the variance in median committee ideologies. Including chamber fixed effects increases the R^2 to 0.83. This leaves some room for a second mechanism: that party leaders can shift the ideology of committees simply by distributing ideologically extreme members efficiently across committees, without necessarily stacking them in terms of seat share. Yet, appendix C demonstrates that when chamber rules preclude seat share stacking, there is no discernible evidence of ideological stacking. This indicates that the first mechanism is mostly, though not exclusively, at play here.

As discussed above, it is best practice to validate the design of a regression discontinuity study by showing that treatment effects are not found for placebo outcomes or placebo cutoffs (Skovron & Titiunik 2016). In appendix B, we verify that placebo thresholds at 0.4 and 0.6 do not produce significant treatment effects and that there is no systematic evidence that an unrelated committee outcome (the size of chamber committees) is affected by majority party status.

Empirical Tests for Heterogeneous Treatment Effects

Having established that majority parties succeed in systematically biasing committee composition and ideology in their own favor, we assess whether their ability to do so is conditioned by chamber characteristics, specifically the level of polarization and professionalism within chambers. An exclusive focus on Congress has limited the ability of

researchers to empirically estimate changes in majority party power resulting from differ-
ent institutional conditions, but our state legislative committee composition data offers
cross-sectional variation in professionalism and cross-sectional and temporal variation in
the level of polarization.

Conditional party government theory suggests that as the parties become more po-
larized, members will delegate to the leadership, which will exert a heavier hand over
legislative functions, including committee assignments. In order to evaluate this claim,
we apply our regression discontinuity approach from above on particular subsets of our
data and look for differential results. We first categorize state chambers as clearly "polar-
ized" or "non-polarized." We take an empirical approach to such categorization and look
to one of the indicators commonly used in studies assessing conditional party government
at the national level: inter-party differences (Schickler 2000). We consider a chamber
"polarized" if and only if the chamber difference between the Democratic and Republi-
can party medians is greater than the mean difference in the full data. If this condition
is not met, we consider a chamber "non-polarized." Table G1 in Appendix G presents
the results from the same RD estimator used above in table 1, but now applied to split
samples of chambers based on level of polarization. As these results show, though both
types of chambers exhibit significant evidence of party stacking on committees, there
is no statistically significant effect in polarized chambers *as compared to* non-polarized
chambers. That is, while the full sample analyses demonstrate committee stacking, there
is no additional membership or ideological bonus in more polarized chambers and we
conclude that all chambers, no matter how polarized, engage in committee stacking to
the same extent.

With respect to legislative professionalism, the literature provides mixed guidance.
More professionalized legislators are more career oriented and as a result, depend on
their parties more for campaign support and for developing legislative reputations, but
less professional legislators have lower levels of information and resources, and may look
to the party leadership for instructions more frequently on substantive votes or during the

legislative development process. Our data allow us to assess these competing empirical predictions using the variation in state legislatures.

The results are in Appendix G, table G2. For these analyses, we use the National Conference of State Legislature's coding of "green," "light green," "gray," "light gold," and "gold," (see table F2 for more details on this coding) categorizing "green" and "light green" states as more professional, and all other categories as less professional.[22] As with polarization, the results in table G2 show statistically significant discontinuities when majority status is reached, but there are no heterogeneous effects for more or less professionalized chambers. We find these null results to be additional evidence that legislators in these different institutional environments face competing pressures which have the effect of balancing party power across different levels of professionalism. While our tests do not clearly adjudicate in favor of either set of theoretical claims, these null results constitute an important empirical contribution in understanding when the party stacks committees. As with polarization, the leadership engages in this organizational practice under all relevant institutional conditions.

Tied Chambers and the Absence of a Majority Party

We supplement our regression discontinuity analysis by leveraging a quasi-natural experiment which occurs in states when party seat shares are equal across the major parties. Tied chambers are a somewhat regular occurrence in the states with 16 occurrences since 1996, the year that our committee data commence (see table 2).

Although the full data include 32,304 committee years, we here limit our sample to *closely* contested chambers, defined as chambers where the absolute value of the difference between the number of Democrats and the number of Republicans is less than three. We do this because of the difficulty in controlling for majority size in studying divergence in committee composition and chamber composition. Although larger majorities construct

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[22]Alternatively, we split state legislatures at the mean level of professionalism using Squire's (2007) coding, and the results are substantively similar.

committees that look very much like the majority party caucus, as the size of the majority grows, the party itself looks very much like the chamber, which can mask party influence or create the illusion of it where it may not exist (Krehbiel 1993). Therefore, including a covariate for majority size is problematic, as the same information (party size) would be on both the right hand and left hand sides of the regression equation. To avoid this issue, and to take advantage of the fact that the distinction between a tied chamber and one that is a single seat or two from being tied is due to random chance,[23] we limit our sample to close chambers. This mimics the logic of the local RD design from above and allows us to make the most of the randomness inherent in a tied chamber, while effectively controlling for majority size. All told, there are 3,756 committees in this subsample analysis, with 1,415 in tied chambers, and 2,341 committees in chambers that are at most a two seat swing from being tied.

Table 3 displays results from complementary models of two dependent variables: *Difference in Party Proportions between Committee and Chamber*; and *Difference in (Shor and McCarty) Medians between Committee and Chamber*. The first three columns treat the relationship between covariates and each outcome variable with a linear model, with state-chamber fixed effects to control for all sources of chamber level heterogeneity that may affect committee composition (e.g., chamber size, legislative professionalism, chamber rules concerning committee assignment, etc.).[24] Columns 4 through 6 display results

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[23]A recent example gives some credence to the as-if random assumption. Early returns from Oregon in the November 2010 election had the House tied 30-30 and the Senate deadlocked at 15-15. Newspapers were calling both chambers tied on election night ("Oregon will likely have a legislative tie, in both Senate and House." Floyd McKay, November 3, 2010, accessed at `http://tinyurl.com/jwscgvo` on March 28, 2014.), only for a tight Senate race to give the Democrats a one seat advantage when all the votes were counted a few days later. A more recent illustration comes from the 2017 Virginia House of Delegates elections. Here, there appeared to be a 50-50 tied chamber for weeks after the elections until a recount in the 94th district was deemed a tie (after a 3-judge panel validated a ballot that had previously been considered a double vote). Virginia law mandates a random drawing in cases of a tie and on January 4, 2018, the luck of the draw gave the Republicans a 51-49 chamber majority.

[24]If a chamber tie is assigned as-if randomly, then the values on these factors and other unobservable factors should be balanced in the treatment (tied chambers) and control groups (close, but not tied chambers). The fixed effects offer a fail-safe for any endogeneity in tied chamber outcomes. The models that include state effects are identified via within-chamber variation in the tied chamber variable. That is, the coefficients reflect the average difference in the dependent variables for when a chamber is tied and when that same chamber is close, but not tied. There are many such chambers in our data, as indicated in the note to table 3.

Table 2: Tied State Legislative Chambers Since 1996

State Chamber	Term	Party Division
1. Indiana House	1996-1997	50-50
2. Virginia House	1997-1998	50-50
3. Washington House	1998-1999	49-49
4. Arizona Senate	2000-2001	15-15
5. Maine Senate	2000-2001	17-17
6. Washington House	2000-2001	49-49
7. New Jersey Senate	2001-2002	20-20
8. North Carolina House	2002-2003	60-60
9. Oregon Senate	2002-2003	15-15
10. Iowa Senate	2004-2005	25-25
11. Montana House	2004-2005	50-50
12. Oklahoma Senate	2006-2007	24-24
13. Alaska Senate	2008-2009	10-10
14. Montana House	2008-2009	50-50
15. Alaska Senate	2010-2011	10-10
16. Oregon House	2010-2011	30-30

Source: National Conference of State Legislatures (`http://tinyurl.com/m9uzebq`). Note: We consider tied chambers which are relatively permanent, not a temporary occurrence as the result of a resignation or death (e.g., the New York Senate for one month in 2009, or the Wisconsin Senate for a few months in 2012). One other tied chamber occurred in our study period which is not listed here. The Virginia Senate was tied 40-40 in 2011-2012, but the Lieutenant Governor was empowered to break the tie, giving Republicans effective control of the chamber.

of analogous random effects models which more explicitly take into account the fact that committees exist in chambers, which are nested in states. These results largely confirm the OLS results, and are presented as robustness checks.

Column (1) shows that tied chambers produce committees that are statistically significantly closer (by about 3%) to their chambers with respect to the proportions of each party in each organization. While this does not amount to a huge substantive effect, the coefficient is nearly half the size of the standard deviation of the dependent variable. Columns (2) and (3) tell a more ambiguous story with respect to the divergence in medians between committees and chambers. The effect in column (2) is not discernible from zero, and while the inclusion of chamber fixed effects means that we do not generally need to control for chamber-level time invariant factors, the varying distribution of ideal points in a legislature may confound the true effect. In particular, the model presented

in column (3) controls for the standard deviation in the chamber-level Shor and McCarty scores and shows that high variance chambers are likely to produce higher degrees of ideological deviation in committees. Controlling for this additional factor, committees in tied chambers show marginally less ideological bias (significant at $p < .05$, one-tailed test) than when they are close, but not tied. Columns (4) through (6) confirm the negative and statistically distinguishable impacts of tied chambers in the context of a random effects model with random intercepts and random slopes for chamber by state and an unstructured covariance matrix. These results have shown, using a variety of specifications, that tied chambers are more representative of their parent chambers than the narrowly untied chambers, which supports hypotheses 2a and, more weakly, 2b and indicates that majority parties are able to take advantage of these latter situations to bias committees away from being representative of their chambers.

Finally, combining the logic of the regression discontinuity and tied chamber approaches, we have also used the full committee data to estimate difference-in-difference regressions of the proportion of Democrats in each committee on an indicator for the Democratic majority treatment (variably controlling for state and year fixed effects and the proportion of Democrats in the chamber). The results, found in appendix F below, confirm the robustness of our findings from the main analyses and serve to bracket our interpretation of the magnitudes of the treatment effects.

Winning Coalition Sizes in States

Given that we find strong and consistent results of partisan stacking of committees, in this section we examine whether more partisan committees produce more partisan legislation. Ultimately, if party control over committees is meaningful, bills passed by the state legislature should more closely reflect the preferences of the majority party; after all, that is the purpose of stacking the committee with partisan legislators. Conversely, classical social choice theory, where the median controls the committee system, and receives exactly their preference, predicts substantially less partisan policy outcomes. To

Table 3: Effects of Tied Chambers on Committee Divergence from Chamber

DV: Difference in...	Linear Regressions			Random Effects Models		
	Party props (1)	Medians (2)	Medians (3)	Party props (4)	Medians (5)	Medians (6)
Tied Chamber (1=Yes)	-0.030 (0.006)	-0.002 (0.013)	-0.019 (0.011)	-0.027 (0.006)	-0.0007 (.013)	-0.018 (.012)
SD of Chamber Ideology			0.614 (0.050)			0.558 (0.046)
(Constant)	0.101 (0.022)	0.260 (0.048)	-0.133 (0.058)	0.087 (0.013)	0.209 (0.028)	-0.215 (0.045)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Chamber Fixed Effects	Yes	Yes	Yes	No	No	No
N	3,756	3,357	3,313	3,756	3,357	3,313
Adjusted R-squared	0.249	0.171	0.224			
Chamber Var.				0.007 (0.002)	0.018 (0.007)	0.013 (0.005)
Residual Var.				0.016 (0.0004)	0.074 (0.001)	0.069 (0.001)
Wald Chi-squared				337.8(p <0.001)	295.8(p <0.001)	213.1(p < 0.001)

Note: Linear regressions are OLS models of (1) the absolute value of the difference between the proportion of Democrats in a committee and in its parent chamber and (2) the absolute value of the difference between the median ideology of a committee and its parent chamber. Random effects models are maximum likelihood regression estimations on each of the dependent variables, columns (4)-(6), with random intercepts and random slopes for chamber by state and an unstructured covariance matrix. The samples of both sets of analyses are restricted to close chambers (absolute value of the difference between the number of Democrats and the number of Republicans less than 3). Chambers included: *AK Senate*; AR House; *AZ Senate*; CO House; CO Senate; GA Senate; IL House; IL Senate; *IN House*; KY Senate; LA House; LA Senate; *ME Senate*; MN Senate; MO Senate; MS Senate; MT House; *MT Senate*; *NC House*; NC Senate; NH Senate; *NJ Senate*; NM House; NV Senate; NY Senate; OK House; *OR House*; *OR Senate*; PA House; SC Senate; TN Senate; TX Senate; VA House; *VA Senate*; VT Senate; WA House; *WA Senate*; WI House; WI Senate. Tied chamber effects identified off of within-chamber variation in above italicized chambers.

assess these claims, we compare coalition sizes, a proxy for legislative extremity, in tied chambers to those in chambers with a majority of one party.

In a one-dimensional space, policy located at the median's ideal point will attract the median, all voters on the opposite side of median away from the status quo, as well as all voters on the same side of the status quo (relative to the median) who are closer to the median than to the status quo (Krehbiel 1998). In contrast, policy outcomes located at some non-median ideal point (e.g., the party median) will generate smaller coalitions. Only those members who are closer to the outcome than they are to the status quo will

be part of the winning coalition. This includes the median, but not necessarily additional members on the same side of the median as the status quo. As outcomes are made more partisan, more majority party members (including the median) will prefer the status quo and the smaller coalitions will become, potentially down to minimum sized (which is rarely observed in American legislatures.) Partisan theorists have suggested that the majority can buy-off majority party members with inducements to keep them in the winning coalition, and there is empirical evidence for this in Congress, which explains both how outcomes can be made more extreme and coalitions remain larger than minimum-sized (Cox & Magar 1999, Cox & McCubbins 2005, Jenkins & Monroe 2012). Thus, coalitions are partisan, majority dominated, and smaller as compared to those in tied chambers, but still larger than minimum-sized as most members of the majority party vote for the proposed bill. We thus expect to find larger coalitions on final passage in tied chambers, controlling for other factors.

We control for a number of other factors that may affect coalition size. These include the size of the majority party, measured as a percentage of total members. If members consistently vote with their party, due to ideological alignment or party influence, then we would expect the size of the majority to have a positive effect on coalition size. We also account for the influence of other pivotal actors by including a variable measuring the number of legislators required to override a veto, as higher override thresholds should increase coalition size (McGrath, Rogowski & Ryan 2015), and an indicator variable for divided government. We include a continuous measure of chamber size, an indicator for chamber and for the legislative session from which the data are drawn. As there are few available sources of coalition size data across state legislatures, we use two sources from two different time periods (1999-2000 and 2011-2012).[25]

Table 4 displays regression results where the dependent variable is winning coalition size and the unit of analysis is the individual final passage vote. The first model shown in column (1) includes all final passage votes in our dataset that are defined as competitive

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[25]The 1999-2000 data come from Wright (2004). We compiled the 2011-2012 data using information available from Open States, a project funded by the Sunlight Foundation (`www.openstates.org`).

votes (having a winning coalition size of $\leq$ .95). As expected, coalitions are significantly larger in tied chambers, even when accounting for the size of the majority. All else equal, according to the first model, tied chambers have coalitions about 5.6% larger, or about three member votes in an chamber with the median number of members. This supports our claim that legislation is moderated in these chambers, and suggests that in non-tied chambers, the majority is able to pull policy outcomes toward a more extreme party preference.

Including all votes with less than 95% of members voting in favor may be too lenient; a number of votes included in the analysis are likely trivial or symbolic and do not concern meaningful or controversial issues. While it is unclear how the inclusion of these trivial votes may bias the results, most analyses seek to screen out these lopsided votes and account only for those bills which made substantive changes and generated significant conflict within the legislative chamber (Krehbiel & Woon 2005). We do the same here by setting a threshold of a coalition size equal to or smaller than 75% of members voting in favor (coalition size $\leq$ .75).[26] Column (2) shows the results using only these "highly competitive" votes, and again, the tied chambers variable is positive and significant.

The regression results are suggestive, but to better account for across chamber differences, we also estimate random effects models in columns (3) and (4) of table 4. The random effects models allow votes to be nested within chambers, and allow for the inclusion of random intercepts for chambers within states. The results are consistent with those from the regression models and in each model, tied chambers have a significant, positive effect on coalition size. The substantive effect is a coalition 4.4% larger when the model includes all competitive votes, and 2.1% larger for only those votes in which the winning coalition size was 75% or smaller.

As coalition sizes within a chamber decrease, bills become more ideological, and these results unequivocally demonstrate that coalition sizes are larger in chambers with a ma-

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[26]This is an inclusive standard and should tend to bias any significant effects downward as a greater number of bipartisan votes will be captures by the 75% threshold. Previous work at the congressional level uses a 65% cut-off, which may be too low given that many states require coalition sizes of at least 67% to override a veto (Masket 2008, Snyder Jr. & Groseclose 2000).

Table 4: Effects of Tied Chambers on Winning Coalition Sizes

	Linear Regressions		Random Effects Models	
	All Comp. Votes	Highly Comp. Votes	All Comp. Votes	Highly Comp. Votes
	(1)	(2)	(3)	(4)
Tied Chamber (1=Yes)	.056	.06	.044	.021
	(.026)	(.02)	(.006)	(.009)
Veto Override Requirement	-.001	-.0002	-.005	-.0007
	(.001)	(.001)	(.0003)	(.0003)
Majority Size	.089	.122	.111	.162
	(.082)	(.074)	(.014)	(.018)
Divided Government	.004	.027	-.017	.001
	(.013)	(.014)	(.002)	(.003)
Legislative Professionalism	.086	.164	.515	.421
	(.056)	(.044)	(.012)	(.015)
Chamber Size (*100)	-.0001	-.001	-.07	-.037
	(.026)	(.011)	(.004)	(.004)
Chamber (1=Senate)	.005	.006	-.045	-.022
	(.022)	(.016)	(.006)	(.007)
Session Dummy	.041	.006	.011	-.008
	(.013)	(.012)	(.002)	(.002)
(Constant)	.714	.477	1.07	.545
	(.107)	(.104)	(.027)	(.03)
Chamber Var.			.001	.001
			(.0004)	(.003)
Residual Var.			.026	.018
			(.0001)	(.0002)
Adj. R-squared	.015	.031		
Wald Chi-squared			2124.31; ($p <0.001$)	933.26; ($p <0.001$)
N	73,819	28,054	73,819	28,054

Note: Linear regressions are OLS models of coalition sizes (1) using all competitive votes (coalition size $\leq .95$) and (2) and highly competitive votes (coalition size $\leq .75$). Random effects models are maximum likelihood regression estimates on all competitive votes (3) and highly competitive votes with random intercepts and random slopes for chamber by state and an unstructured covariance matrix regression. Tied chambers included in the analysis are: Arizona Senate, Maine Senate, Washington House, Virginia House.

jority party than in chambers without a majority. Indeed, if bills were just as moderate in chambers with a majority party as in those without, we would expect no differences in coalition sizes between the two. This is "downstream," evidence that parties can use control over standing committees to enforce a more partisan agenda, resulting in more

polarized bills.

Conclusion

The search for party influence over legislation through committee work has been frustrating, as very little empirical evidence has been uncovered at the congressional or state levels. Most research has compared committee members' aggregate ideology to chamber characteristics. We have taken a different tack, by leveraging a unique dataset of all state legislative committees from 1996 to 2014. These data allow us to take advantage of the fact that majority status can be thought to be randomly assigned around the threshold for majority control. Using a regression discontinuity design, we are able to show that as the proportion of Democrats in a chamber crosses this threshold, the proportion of Democrats in committees "jumps," and, more importantly, so does the extent to which the committee's median ideology becomes more liberal. The discontinuities we find are statistically significant and robust, indicating that parties achieve a bonus on committees, both in terms of membership and ideology. We estimate local average treatment effects for a party membership bonus of about 8.5%, and an ideology bonus of a full standard deviation of median committee ideology, indicating that the difference between a bare minority of members to a bare majority of members is substantively large.

The results regarding membership bonuses comport with conventional wisdom: majority parties enjoy the spoils of victory and hold majorities on most committees. The ideology results are entirely novel and strongly suggest that majority parties make the most of their advantages to drastically shift aggregate preferences in their favor.[27] Indirectly, this offers evidence that parties can use committees to create partisan legislation rather than median legislation, and use committees to block bills favored by the median from ever reaching the floor. Not all committees are equally important to the legislative process, and we speculate that more valuable committees are also likely to be more

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[27]Contra the implications of recent work by Fortunato (2013) that shows that in Congress, majority party delegations are more representative of their party than minority delegations.

partisan. Future research should examine the level of party-stacking to verify this claim.

We have further shown that winning coalitions in bare majority chambers are significantly larger than coalitions in tied chambers, indicative of more partisan legislation in the majority-controlled chambers. The results hold for both the proportion of party members and committee medians under all of the empirical specifications used. Though we cannot measure policy outcomes directly, this is strong evidence that the partisan stacking of state legislative committees produces non-median oriented policies.

Finally, we find no evidence that the effect of majority parties depends on the level of polarization within the chamber, or the level of legislative professionalism. Neither variable mitigates the substantive and significant effects we find and these null findings constitute an important result as previous studies that focus on committee composition in Congress have lacked theoretically relevant institutional variation. Yet, there is ample state-level variation left for research to examine. For example, future research could catalogue the extent to which seniority norms constrain the party effects we have uncovered; or, whether states with stronger Speakers see pronounced effects.

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