

# How Elections and Parties Influence Legislative Effectiveness

Assessing Causes and Institutional Implications in the U.S. Senate

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*Abstract:* I provide a new perspective on individual legislative productivity by analyzing a subset of Senate amendments that were offered in identical form at least twice by the same Senator. This data makes it possible to account for the confounded relationship between institutional variables, legislative content, and individual legislative success. In contrast to the results in the existing literature, I find no evidence that majority-party affiliation has a causal effect on legislative productivity. On the other hand, the data does suggest a positive electoral-cycle effect on effectiveness. This effect is strongest for the most electorally vulnerable Senators, but appears only to apply to members of the minority-party. These findings suggest that policy-making institutions in the Senate may be oriented more towards incumbents' electoral incentives, regardless of party, and less towards partisan competition than recent scholarship on the subject suggests.

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## Legislative Productivity and Representation

In July 2000, Senator Tim Johnson proposed an amendment on the U.S. Senate floor to ban livestock packers from owning animals for longer than two weeks prior to slaughter. Since the Senate's legislative process is oft compared to sausage making, perhaps few were surprised that under this amendment, swine packers were afforded triple the amount of time to come into compliance with the ban as their beef and poultry counterparts. Nevertheless, Senator Johnson's amendment not only failed to clear the Senate, it didn't even receive a vote.

Undeterred, in the next session of Congress, Senator Johnson introduced the exact same proposal as an amendment to another bill. This time, the amendment received a roll-call vote and passed through the chamber and into law.

What accounts for these different outcomes? Since variation on individual factors won't give provide an answer, its natural to turn to Congress' policy-making institutions for clues. One difference, that may have been especially salient to Senator Johnson's case, was that the first time he introduced the amendment he did not have to win an election to retain his seat in the next Congress. The second time he did. Another difference is that the first time Senator Johnson offered his amendment his party was in the minority, whereas the second time they held the majority.

This paper takes on the task of estimating whether these institutional differences affect individuals' legislative effectiveness by looking at cases, similar to Senator Johnson's above, where the same Senator offers the same amendment but faces different institutional incentives and constraints (though thankfully, in most cases, only one of these two institutional forces varies across any pair of identical proposals). Such cases, as I will argue, are especially useful for uncovering how electoral and partisan incentives influence Senators' capacity to legislate effectively, and for connecting individual legislative outcomes to models of institutional policy-making that disagree about the relative influence of parties and electoral institutions.

## Approach and Contributions of the Paper

The general aim of legislative effectiveness research has been to search for what institutions and individual traits cause some legislators to be more successful in pursuing legislative objectives. The current project makes an empirical contribution to this research program by estimating how partisan and electoral incentives influence legislators' effectiveness, while, for the first time controlling for the association between these incentives and proposal ideology. This addresses a major shortcoming in the existing literature which ignores legislative content and thus implicitly assumes it to be independent of ostensibly causal variables despite evidence and intuition to the contrary (Adler and Wilkerson 2013).

My empirical strategy goes further than existing approaches, which at best only control for individual traits, by using panel data to hold constant both fixed-individual traits and proposal ideology. This strategy is built on a new text processing algorithm that identifies tuples (mostly pairs) of amendments have been offered identically under different institutional settings. Using two data subsets identified by this matching process, I estimate the causal effect of two variables, defined respectively by the Senate's electoral calendar and partisan majority control of the chamber, which naturally connect to the broader debate in the literature regarding institutions and policy making.

This empirical approach creates an explicit link between the distribution of individual legislative success and broader theories on parties and electoral incentives in policy making. In particular, I argue that the treatment effect estimates found for the variables introduced above can inform the debate about partisan influence in the policy-making process.

A final contribution of this work comes from its focus on the Senate in general, and the Senate amendment process in particular, which both represent understudied legislative settings. Most studies of legislative productivity (along with most studies of legislative activity in general), focus on the House and measure production according to *bill* outcomes. In contrast, I consider *amendments* offered on the U.S. Senate floor, and analyze legislative outcomes in this chamber with a modern econometric perspective for the first

time.

The primary results that emerge from this approach empirical analysis diverge sharply from the existing literature. First, I find that once an proposal ideology is controlled for, a Senator's party does not play a meaningful role in determining legislative effectiveness. Second, I find fairly strong evidence that running for reelection positively affects legislative effectiveness, at least for Senators in the minority party.

The rest of the paper proceeds as follows. Section 2 describes the Senate amendment process and related features that make it a particularly attractive source of data given the substantive goals. It also outlines existing research on individual legislative productivity, its shortcomings that the current paper seeks to address, and the hypotheses this paper will evaluate. Section 3 introduces the data and an approach to analyzing it that identifies causal relationships. Section 4 presents statistical results, interprets their implications for the literature on legislative productivity, and provides a more detailed argument about how these results fit with the different models of policy-making in Congress. Section 5 discusses possible research and concludes.

## **Procedures and Productivity in the Senate**

### **A Brief Sketch of Legislative Life in the U.S. Senate**

Congressional research, generally speaking, tends to focus on the House of Representatives, and specifically the legislative productivity literature defines outcomes in terms of which *bills* advance through the legislative process. In this paper, I depart from both of these traditions and draw on Senate amendments for data. Since the legislative path of a Senate amendment is procedurally and materially distinct from the path of an underlying bill, and may not be entirely familiar to readers, below I summarize the Senate's amendment process and its features that inform certain aspects of the rest of the paper.

During the consideration of any bill, Senators may submit an amendment almost at almost any time. These amendments are given to clerks on the Senate floor who index them and enter them into the Senate record. While there is no restriction on introducing

amendments, there are both technical and practical restrictions on the number of amendments that can become *pending* - that is to say only a limited number of amendments can actually be open to debate and resolution at any time. This number is determined by rule and depends on procedural context. Politically and practically, the subset of amendments that receive consideration is determined either by negotiations between the Majority and Minority leader or by Senators calling up their amendment for consideration as they are able to gain recognition. The Majority Leader, looms large in this process because he has the right of first recognition and can - and frequently does - use this right to call up amendments for the express purpose of filling all available amendment “slots”, thereby preempting any other amendments from consideration in a process known as filling the tree. Once amendments are made pending they are generally resolved either by vote (which can be either recorded or voiced) or by rule - e.g. non-germane amendments might fall due to an affirmative Senate vote to invoke cloture which renders such amendments out-of-order or, more optimistically, amendments might be accepted by unanimous consent. No matter the method, all amendments that become pending must be resolved in some fashion prior to final Senate disposal of the underlying bill.

A fair characterization of this process might be that the Senate’s standing rules allow wide latitude for individual prerogative but also endow the Majority Leader with the ability to restrict this prerogative in many settings, sometimes drastically (Lee 2009). This dynamic combined with the Senate’s limited time and attention bandwidths, and the robust legislative ambition exhibited by most U.S Senators, creates an environment of scarcity where there are more amendments than the Senate as a collective body is able to consider (Walker 1977). For example, in the 110th Congress the data contains 5175 amendments offered to senate bills, of which 3603 never became pending and only 244 of which received votes.

This process also is notable for its unique procedural flexibility. Regardless of the forces that determine which underlying bills reach the chamber floor, Senators are able to express their legislative priorities through amendments, and if they are able to engender enough support, can advance them regardless of other agenda constraints. To the extent

that these priorities are consistent over a over a Senator's career, the openness of the amendment process makes it reasonable to suppose that some proposals might be offered more than once in different political settings. Such behavior is essential to the empirical approach adopted in this paper as detailed in subsequent sections.

Apart from the amendment process, another advantage of focusing on the Senate is that its electoral calendar induces largely independent variation in electoral vulnerability. This constitutionally established feature prescribes that only a third of Senate seats are up for reelection in any given session of Congress, and Senators in these seats are said to be *in-cycle*. Other seats carryover to the next session of Congress and their occupants are therefore said to be *out-of-cycle*. Many observers of the Senate have found that variation in Senators' electoral time-lines induces variation in their electorally motivated behavior (Shepsle et al. (2009), Ahuja (1994), Warshaw (2013)). While the possibility that the Senate's electoral calendar affects Senators' behavior, and Senate outcomes is well-recognized, to date it has not been incorporated into studies of legislative productivity except indirectly and without a clearly interpreted result (Moore and Thomas 1991).

## Previous Research

While the empirical focus on the Senate is unusual (though not unique, see Moore and Thomas (1991) and Matthews (1960)), the literature on legislative effectiveness in general goes back half a century and is well reviewed elsewhere (Cox and Terry (2008), Anderson et al. (2003)). Most of this work computes legislative effectiveness according to either a count or proportion of proposals that reach a certain stage of the legislative process, and tests whether some subset of independent variables accurately predicts differences in these measures across legislators.

Many variables have been tested as potential explanations. Of these, majority party affiliation is perhaps the most frequently considered, and is found unanimously to have a positive association with productivity. Researchers have also found sometimes inconsistent evidence regarding the influence of seniority, leadership position, committee chairmanship, age, constituent ideology, personal ideology, and ideological extremism. Other

studies also consider individual-level variables like gender, race, and ethnicity (Anderson et al. (2003), Volden et al. (2013), Frantzich (1979)). Some have also found evidence that innate ability (Volden and Wiseman 2014), or personal legislative style, for example, speech frequency, can play a role in that legislator's relative productivity (Anderson et al. 2003).

My primary methodological objective is similar to several recent papers, especially Cox and Terry (2008) (henceforth CT), that seek to identify causal relationships between institutional variables (such as majority party membership) and legislative success. CT critiques previous studies by noting that they generally considered only a single Congress and therefore failed to isolate the effect of institutional variables from individual fixed-effects and/or other variables which were constant in a single Congress. As a remedy, CT emphasizes the advantages of panel data and its ability to control for unobserved individual level traits, a point which informs my perspective as well.

In addition, CT provides a prominent example of research on legislative effectiveness that measures outcomes according to both final passage and procedural outcomes, with the latter defined according to whether a proposal has advanced to a certain stage in the legislative process rather than by its ultimate passage through the chamber.<sup>1</sup> This general concept, that legislative success is defined with respect to both procedural and final policy outcomes, is generally accepted in the literature, and, as explained in more detail in a subsequent section, is adopted here as well (Anderson et al. (2003), Volden and Wiseman (2014)).

Finally, CT explicitly links the institutional theories of parties and electoral incentives to the distribution of individual legislative success. In confirming the unanimous finding that majority party legislators are more effective, the authors, echoing others, state this result is an indication of the power of the majority party. (On this point, see also Anderson et al. (2003) and Moore and Thomas (1991)).

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<sup>1</sup>No study that I am aware of defines success as enactment into law, and in fact I'm not aware of any study that even captures this outcome as a variable in this literature.

## Shortcomings in Previous work

While my approach to measuring and motivation for studying legislative productivity falls squarely in line with previous scholarship, I also address a critical shortcoming in it. Below I explain this shortcoming and its consequences before advancing an approach to address it.

While there is no canonical definition of legislative effectiveness in the literature, there seems to be at least implicit agreement about the concept's core features. In particular, both qualitative and more formal expositions on the subject suggest that effectiveness involves the ability to advance legislation independent of its underlying ideological effect.<sup>2</sup> A corollary to this definition is that effectiveness is distinct from *accommodation*, that is the inclination to offer legislation that is ideologically agreeable to pivotal legislators. The problem with existing approaches then, is that they fail to distinguish between the determinants of *effectiveness* and the determinants of *accommodation*.

This problem is a direct result of not accounting for the relationship between ideological content and institutional variation. From this perspective, ideological content can be seen as an omitted variable, and so observed statistical relationships between institutional setting and legislative effectiveness are only causal under the heroic assumption that proposal ideology is unaffected by institutional variation.<sup>3</sup>

Such an assumption seems dubious given recent research and intuition. For example, Adler and Wilkerson (2013) offer a detailed examination of the institutional determinants of policy-making and find that policies with expiration dates are far more likely to spur successful legislative action, and that such action was most often spearheaded by committee chairs.

This pattern is interpreted in the effectiveness literature as indicating that chairing a committee makes legislators more effective. However, as a brief anecdote illustrates, this

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<sup>2</sup>In the ubiquitous spatial policy-making model, such a definition implies that the expected outcome for two, ideologically equivalent, proposals may differ due considerations independent of this ideology. For example, in a world of uncertain policy effects and loss-averse legislators, two proposals might have the same expected ideological effect, but one might achieve this effect with lower variance and thus be preferred by all Senators to the higher-variance proposal.

<sup>3</sup>Technically, bias will occur only if legislations' ideological content is also associated with legislative outcomes, the converse however, seems absurd.



interpretation is problematic if we define wish to retain a connection between individual effectiveness and ideological representation.

When Democrats gained the Senate majority in the 110th Congress, Senator Chris Dodd went ascended from ranking minority member to Chair of the Banking Committee. Any metric in the literature would have found that his effectiveness in the Senate amendment process increased (dramatically) over this period because he passed 13 more amendments through the chamber in the 110th Congress. This nearly doubled his comparable result from the 109th Congress. Existing studies would count this increase as evidence that majority party Senators are more effective than minority party Senators (or similarly, that Committee chairs are more effective than ranking minority party members). However, this conclusion relies on the assumption that Dodd's proposals were equally ideologically *accommodating* to pivotal legislators in both sessions.

Such an assumption though is belied by the record which indicates that in the 109th Congress, none of Dodd's successful amendments were technical or substitute proposals, whereas in the 110th Congress, 12 were. This difference is relevant because substitute and technical amendments are almost certainly less reflective of Senators' personal ideology and instead, as Adler and Wilkerson (2013) show, more reflective of pressure from party leadership or externally imposed institutional requirements. Given that the total number of successful Dodd amendments increased by almost exactly the same number of successful technical and substitute amendments, it seems safe to conclude that the Senator's increased legislative success was the result of stronger incentives to *accomodate*, rather than his enhanced *effectiveness* as a committee chair. <sup>4</sup>

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<sup>4</sup>The substantive difference between accomodation and effectiveness is perhaps underscored (if only anecdotally and perhaps incidentally) by Dodd's declining political fortunes over this period. In January 2010, in the midst of the most active legislative period of his long career, he announced his retirement in order to allow his party a better chance at retaining the seat in the coming election - which it did.

# Empirical Approach

## Hypotheses

The preceding discussion was meant to clarify that, in order to measure how institutions influence legislative effectiveness, we need to consider their simultaneous affect on proposal ideology. In this section, I develop an empirical approach that meets this need and apply it to estimate separately, the effect of *majority-party membership* and the Senate's trifurcated *electoral-cycle*. Focusing on these variables allows the empirical results to speak directly to the central debate in the Congressional policy making literature regarding the influence of parties on legislative outcomes.

On one side of this debate, scholars such as Cox and McCubbins (1993) and Rhode (2000) posit a powerful party system in which majority party Senators are expected to be more effective because they have more resources at their disposal in the form of agenda-setting discretion, policy staff, recognition perogatives or other advantages.

Alternatively, the seminal "Electoral Connection" and "Pivotal Politics" models hold parties to be inconsequential, and instead conclude that legislative institutions are designed to serve incumbents' electoral objectives independent of their party affiliation (Mayhew (1974) Krehbiel (2010)). If this perspective is correct, then, under an additional, fairly weak assumption, Senators running for reelection, will more effective than those who are not, and party affiliation will be inconsequential.<sup>5</sup>

Also, and perhaps more subtly, these competing perspectives differ in their predictions over the partisan distribution of an electoral-cycle effect, should one exist. Party-based models predict that if an electoral-cycle effect is found, it will accrue disproportionately to the benefit of majority-party Senators, while the electoral connection model predicts

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<sup>5</sup>Predicting a positive electoral cycle effect based on Mayhew's model of Congressional institutions requires making the assumption that electoral returns to effectiveness increase as a Senator draws closer to his or her next election. This assumption will hold under several possible scenarios, the simplest being that voters' assessments of their incumbent Senators are influenced by a recency or, more colloquially, a "what-have-you-done-for-me-lately" bias, which causes them to place more weight on recent representative outcomes. This assumption appears frequently in research on both the Senate and Congressional representational behavior generally (Shepsle et al. (2009), Canes-Wrone (2015)). There is also a good deal of evidence that legislators perceive this bias operating in their electorates (e.g. Ahuja (1994), Thomas (1985), Warshaw (2013))

no such advantage will apply. The connection between partisan policy-making models and this prediction is elaborated later but, speaking generally, it is an intuitive and nearly immediate consequence of the majority party's assumed "negative-agenda" control, which is a foundational tenet of such models.

The preceding three paragraphs surely oversimplify the rich debate regarding Congressional institutions and legislative outcomes, but they are still sufficient to suggest three hypotheses to test the role of parties and electoral incentives in shaping legislative effectiveness.

### **Electoral-Cycle Effect**

*Null Hypothesis 1:* In-cycle and out-of-cycle Senators are equally effective legislators.

*Alternative Hypothesis 1:* In-cycle Senators are more effective than their out-of-cycle colleagues.

### **Majority Party Effect**

*Null Hypothesis 2:* Senators in the majority and minority party are equally effective.

*Alternative Hypothesis 2:* Senators in the majority party are more effective than Senators in the minority.

### **Party Mediation of Electoral Effect**

*Null Hypothesis 3:* The electoral cycle effect is no greater for the majority party than it is for the minority.

*Alternative Hypothesis 3:* The electoral cycle effect is stronger for Senators in the majority party than for their counterparts in the minority.

## **New Data Set**

I have argued that findings in the legislative effectiveness literature are potentially (indeed likely) biased because they were estimated without accounting for the relationship between institutional variables and proposal ideology. In this section, I describe an ap-

proach, depicted graphically in Figure 1, to address this shortcoming in order to identify causal determinants of effectiveness.

The first step in this approach is to compile the raw texts of all amendments and data on the sponsors of these amendments.<sup>6</sup> This raw data consists of the vast majority of all Senate amendments offered from the 100th through the 113th Congress. Containing more than 50,000 policy proposals this corpus is to the best of my knowledge the most comprehensive source of text data on Congressional amendments analyzed in the literature. A detailed statistical summary of this raw data is presented in the appendix.<sup>7</sup>

**Outcome definitions** Within this data I define both legislative successes and other variables consistent with long-standing practice in the literature. Most importantly, I follow the literature’s precedent in assuming that a proposal can be “successful” even if it does not ultimately pass the chamber, as long as it proceeds far enough procedurally to provide its sponsor with a chance for position taking or credit claiming (Cox and Terry 2008). Such a definition provides, to quote one influential study in this area “an intuitively satisfying conceptualization because it reflects the way that most legislators and interested constituents are likely to measure success - namely, as a function of the number of bills on which the member receives positive action” (Anderson et al. 2003). In this way, these procedural successes demonstrate legislators’ priorities to voters. Such “position-taking” activities are electorally valuable regardless of their immediate impact on policy because they help Senators develop their reputations individually and parties communicate their priorities collectively (Mayhew (1974) Lee (2009) Sinclair (1986), Fenno (1978)). Other reasons that Senators value procedural outcomes that fall short of changing policy include their influence (positive or negative) on the ability of Congress

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<sup>6</sup>While the Congressional Bill Project and others have made corpi of Congressional legislative texts easily accessible, no corresponding source existed for *amendments*. This is an important gap in the Congressional data landscape because a great deal of legislative activity occurs, especially in the Senate, as part of the amendment process, and many of the most important legislative activities and strategic interactions occur around these proposals.

<sup>7</sup>In addition to standard data on the amendment sponsor, I also generated and recorded various pieces of meta data for each amendment in the corpus as part of the process of identifying ideologically identical amendments including an ordered list of the most similar amendments offered when such a list was possible to create. This data will be made available online upon publication of this article if not sooner. More information about it is available from the author.

to expeditiously move forward with a broader agenda and because of their contribution to the overall socio-political context in which policy ideas evolve (Schattschneider (1960), Oppenheimer (1985), Walker (1977)).

In order to apply this concept of procedural success to data from the Senate amendment process, the precise criteria used to classify outcomes as successful or not differs slightly from the criteria used in previous research. Specifically, I consider three different (but overlapping) binary definitions of success depending on whether an amendment (a) received a recorded vote on the floor, (b) was approved by the Senate, and (c) either (a) or (b).<sup>8</sup>

My empirical approach adopted here also considers several other covariates familiar to Congressional scholars which are defined in Table 1.

Table 1: Covariates and Abbreviations

Variable Name	Explanation
Congress	Session of Congress in which amendment was proposed
In-Cycle	Treatment variable for cycle-match data. Equal to 1 if amendment sponsor is in-cycle
Majority	Treatment indicator for party-match data. Equal to 1 if amendment sponsor is in majority party
Party	Amendment sponsor's political party
Pres. Vote	Democratic presidential candidate's two-party vote share in most recent election in amendment sponsor's state
NOMINATE Score (abs. value)	Absolute value of sponsor's Poole and Rosenthal (2000)'s 1st Dimension NOMINATE Score
Session Avg. NOMINATE	Average NOMINATE score in Senate in the session when amendment was proposed
NOMINATE Polarity	Unsigned difference between Senator's NOMINATE score and the chamber mean NOMINATE score for that session
Strata Introduction Order	Equal to 1 if amendment was the first of matched amendment tuple introduced, equal to 0 otherwise.
Vulnerable Seat (Vuln. Seat)	Dummy variable indicating that most recent presidential race in amendment sponsor's state was decided by < 5% or that candidate from sponsor's party lost

<sup>8</sup>Note that an amendment can pass without receiving a vote as well as receive a vote and not pass. While initial intuition might suggest that passing amendments would be more likely to receive a vote, we will see that in fact the correlation runs in the opposite direction. I have also analyzed a model of polynary outcomes based on these three categories and found similar results. Details of this analysis are from the author.

## The Party-match and Cycle-Match Data

The most important data-processing step and the central empirical innovation I offer is to identify two “matched” subsets of observations consisting of proposals repeatedly offered by the same Senator in different electoral and partisan settings respectively.<sup>9</sup> As depicted in Figure 1, one subset, that I refer to as the *party-match data*, comprises ideologically identical amendments offered by the same Senator both when that Senator was a member of the majority party and when he or she was a member of the minority party. The second subset, which I refer to as the *cycle-match data*, is constructed analogously, but with inclusion and exclusion based on whether the same proposal was offered once when a Senator was in-cycle and once when he or she was not. Thus in the party-match data, proposal ideology is independent of which party holds the majority, and in the cycle-match data, proposal ideology is independent of whether the amendment sponsor is in-cycle.

**Potential Outcomes/Quasi-Experimental Approach to Estimation** The ultimate goal of preprocessing the data in this way is to simulate in the two matched data sets what we would observe if we ran two implausible experiments. The party and cycle-match data is intended to correspond, respectively, to what would be observed if Senators wrote and introduced their amendments and then were randomly assigned to either receive all of the institutional and resource advantages offered by the majority party or not. The cycle-match data is then intended to simulate what would be observed if instead of randomly assigning majority-party affiliation we instead randomly assigned which Senators had to run for reelection in a given session of Congress.

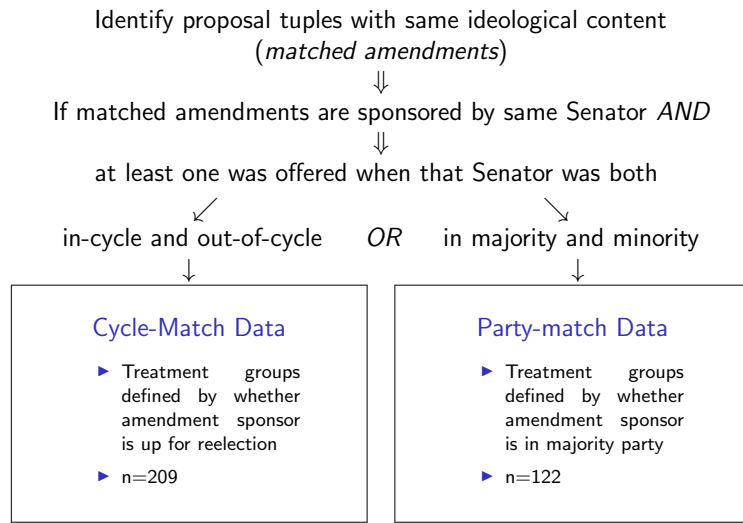
Once these matched subsets are formed, causal quantities of interest can be estimated using the potential outcomes, causal inference framework (Rubin (1974), Rosenbaum (2002), Sekhon (2008), Holland (1986)). Under this framework, each observation is assumed to have two *potential outcomes*, one of which is the observed outcome under that

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<sup>9</sup>These data sets are constructed via a new text matching algorithm which identifies amendments with essentially identical legislative content. This algorithm is based on filtering amendments based on certain signature legislative phrases or “tags” that differentiate types of legislative changes and then performing substring match comparisons on the filtered candidate list.

Figure 1

Criteria for Party-Match and Cycle-Match Data Sets



unit’s actual treatment assignment, the other of which is the “counterfactual” outcome for that observation which would have been observed had the unit received the alternative treatment. Under this model then, the causal effect of treatment is defined theoretically at the unit level by the difference between each unit’s two potential outcomes. Of course, we only observe an observation under a single treatment condition, and therefore, estimating causal effects requires the imputation of the unobserved, “counterfactual” outcome for each observation, and the causal validity and statistical efficiency of resulting parameter estimates depends on this imputation process.

As with any method of estimating causal relationships, parameter estimates under the potential outcomes model will be biased if there are unobserved variables which are associated with both treatment assignment and outcomes.<sup>10</sup> This observation clarifies the essential feature of the party and cycle-match data: That is, within these data, proposal ideology is independent of treatment assignment and therefore unconfounded by construction. Additionally, the matching criteria requires that each matched amendment pair be sponsored by the same Senator and therefore fixed individual traits are independent of

<sup>10</sup>Because the observed potential outcomes for one treatment group do not provide an unbiased estimate of the unobserved potential outcomes for the other treatment group

treatment assignment and unconfounded with outcomes as well.<sup>11</sup>

This empirical approach obviously leans heavily on the efficacy of the text-processing algorithm used to identify identical amendments. Therefore, and especially since this paper marks the first application of this algorithm, I present more information about it in the appendix and provide some examples illustrating the ideological equivalence of the matched amendments.

While within the matched data fixed-individual traits and proposal ideology are unconfounded by construction, other sources of bias could enter into parameter estimates if another variable is correlated with both treatment assignment and outcomes. Whether such a variable exists can be tested empirically (if the variable can be measured) with “balance tests” that compare a covariate’s distribution and mean value across the two treatment conditions. If the matching process was empirically successful - that is if potential outcomes are independent of treatment assignment - then these tests should show that the distributions and average values of relevant covariates are approximately the same in both treatment groups (Ho et al. (2007) Sekhon (2008)).

The results of running these balance tests on the two matched data sets come out largely as expected and intended as shown for the party-match data in 2. Here we see that with a single exception discussed below, observed covariates have approximately the same means and similar distributions for in the treatment and control groups.<sup>12</sup>

**Introduction Order and Amendments that are only introduced once** As indicated in Figure 2, there is one variable, *Introduction Order*, that appears to be imbalanced in the party-matched data and therefore has the potential to introduce bias into the estimates of the majority-party treatment effect. This variable is an indicator equal to one if the observation of a matched amendment pair  $t$  is the first of that pair to be introduced. Therefore, the imbalance shown in 2 suggests that, for two ideologically identical proposals in the party-match data, the first amendment introduced was more

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<sup>11</sup>Put slightly differently, in both matched data sets, the text matching process ensures that differences in outcomes across treatment groups are not due to proposal ideology.

<sup>12</sup>The corresponding figure for the cycle-match data, given in the appendix, shows no evidence of imbalance across any of the observed covariates. Not included on these charts are individual-fixed covariates which are trivially identical across treatment groups by construction.



likely to be introduced when the amendment sponsor was a member of the majority party.

Whether this covariate introduces bias into subsequent treatment effect estimates depends, as for all other covariates, on whether it is also associated with amendment outcomes. Readers' initial intuition might (and in my experience, most of the time has) suggest that such an association is likely since a twice-introduced proposal can't practically have been enacted into law the first time it was introduced. However, policies that achieve a more modest level of procedural success are not subject to this practical restriction, and in fact we might expect such reintroductions to be a common if procedural success rewards, encourages, or validates an amendment sponsor's effort along the often long path to enactment. In any case, since the current definition of amendment success - and all others in the literature - is not defined by enactment, the relationship between *Introduction Order* and potential outcomes is open to empirical questioning.

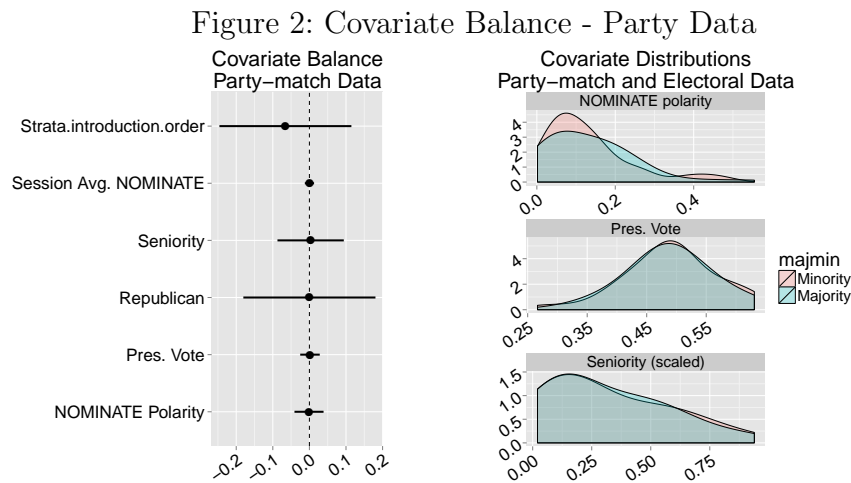
Table 2: Amendment Outcomes Independent of Introduction Order

	No Vote & Not Passed	Pass or Vote
Initial Amdt. Introduction	17	15
Subsequent Introduction	15	17

*Note:* For amendment pairs in the party-match data, more proposals are first made when the sponsor is in the majority. This would be problematic if legislative success was a function of the number of times a proposal had previously been made. However this does not appear to be the case. In a given matched amendment pair, failure is just as likely to follow success as the opposite.

When the data is consulted, in fact, no evidence is uncovered to suggest a meaningful relationship between the imbalanced covariate and legislative success. That is, as shown in Table 2, a matched amendment pair is just as likely to succeed first and then fail, as it is to fail first and then succeed. Thus, since a variable does not lead to bias if it is not associated with outcomes, the *Introduction Order* imbalance in the party-match data does not appear to materially undermine the possibility of drawing valid causal inferences from this data. Additionally, as illustrated more thoroughly in the appendix, when *introduction order* is included in different regression models it is insignificant. Therefore I continue on with the empirical task at hand, assuming that this variable can safely be

ignored.<sup>13</sup>



**Retirements** While the results from the balance tests are encouraging, other possible sources of bias must also be addressed.

While treatment assignment in the cycle-match data is constitutionally fixed and therefore independent of most relevant covariates by construction, there is at least one potential concern in this data, Senators' retirement decisions, that is not easily assessed via balance statistics or deduced *a priori* from the constitutional features of the electoral-cycle.

Senators who retire are technically in-cycle but clearly are not subject to the same electoral incentives as Senators actively running for reelection. However, classifying all retiring Senators into the control group is not without drawbacks either, since such Senators may not have decided, or be perceived as likely, to retire until their in-cycle session is partially completed. Thankfully however, the data show that retiring Senators are, apparently, considerably less active amenders. As indicated in detail in the appendix, the text-matching process identifies only five amendments from retiring Senators in the cycle-match data and five in the party-match data as well. The results given are robust

<sup>13</sup>While the discussion in this section speaks to the "internal validity" of causal inferences made using the party-match data - that is the identification and unbiased estimation of causal effects within the matched data - there is a different but related issue with respect to "external validity" - the extent to which the observations in the matched data resemble the observations that do not - especially with respect to the relationship between the treatment variables and potential outcomes. I treat this subject in a subsequent section.

to treating these five members as either in-cycle or out-of-cycle. I treat them as in-cycle because this seems like the more conservative approach.

While the exogeneity of the Senate's electoral-class rotation greatly reduces the threat of confounded variables in the cycle-match data, no analogous *a priori* arguments apply to the treatment variable in the party-match data. Unfortunately, which party holds a majority of seats, legislative success, and the distribution of legislator preferences are inescapably confounded because Senators are always afforded the institutional advantages of majority party membership just as the chamber's ideological center gravity shifts towards their own preferences.<sup>14</sup> As a result of this shift, if legislative proposals generally reflect sponsors' ideologies, then majority-party sponsored amendments will have a higher probability of success, even if parties have no institutional effect on the legislative process.

Several options exist for teasing apart this collinearity. The simplest and most convincing in this case however, is to note that the direction of the resulting bias, and therefore its relationship to relevant hypotheses tests, can be logically deduced under the weak assumption that individual ideology is constant, proposal and individual ideology positively covary, and that majority-party Senators are ideologically closer than their minority-party colleagues to pivotal legislators (this final assumption can be empirically tested but seems safe enough to assume here for convenience.) If these conditions are granted, then the bias induced by the collinearity between preferences and party might lead us to reject the null hypothesis of no party effect when it is true, but it can not lead us to fail to reject the null when the alternative is true. As shown in the next section, the latter case is the applicable one in this case: There is essentially no support in the data for rejecting the null hypothesis of no majority party effect and thus concern about any bias that would lead us to reject it inappropriately is immaterial.

**Sample-Size** One final additional critique, that applies approximately equally well to both the electoral and party-match data, is that the matching process leads to dramatically smaller samples sizes and therefore attenuated statistical power.

The face validity of this concern is hard to contest. The matching process results

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<sup>14</sup>This issue is endemic in some form to essentially all studies of parties in legislatures (Krehbiel 1999)

in a drastic reduction in sample size in both the electoral and party-match data. The master dataset contains more than 50,000 amendment-observations, the cycle-match data contains only 204, and the party-match data contains just 64.

For the sake of brevity, I save a more thorough response to this critique for the appendix and make three brief, general points here. First, while more data yields sharper estimates, this statistical power in the current case is inferential fools gold from a causal perspective. This paper's empirical approach is the only one I am aware of that accounts for the relationship between independent variables and proposal ideology, which is a necessary prerequisite for making inferences about the causal effect of institutional variables like parties and electoral incentives. Second, consistent with this perspective, other approaches to estimating causal effects with observational data also frequently, and often as a rule, require a dramatic reduction in sample size.<sup>15</sup> Third and finally, one of the the two approaches to estimation applied in the next section is non-parametric and provides valid uncertainty intervals regardless of sample size.

## Empirical Results

### An initial look at the data and three main results

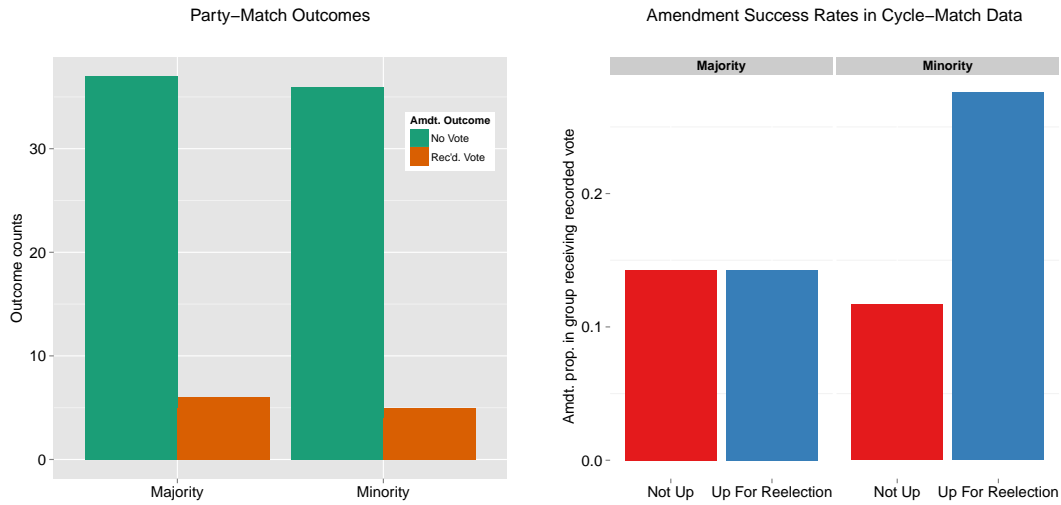
The previous section devoted significant energy to constructing data that could be analyzed from a quasi-experimental perspective. The payoff to this effort is that a simple difference in means across treatment groups will provide an unbiased estimate of the average treatment effect (Imbens and Rubin (2009), Ho et al. (2007)).

Three main patterns emerge which are summarized graphically in Figure 3. First, in the party-matched data, controlling for proposal ideology, the probability of success

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<sup>15</sup>A reasonable comparison can be gleaned from regression discontinuity analyses of elections which throw out the vast majority of elections and base inferences on a small subset of elections decided by the slimmest margins. For example, cutting-edge election research has drawn inferences about the causal effect of incumbency from as few as 600 state legislative elections out of a total sample of around 65,000 election-observations (Eggers et al. 2015). The information loss inherent in this approach is actually likely larger than in the current case, since included and excluded elections differ by construction on a background covariate that is unambiguously related to the dependent variable (future electoral outcomes), lagged value of that dependent variable (the result of the most recent election).

Figure 3: Initial Evidence of Positive Electoral Cycle and Null Majority Party Effects



is approximately the same for amendments sponsored by majority and minority-party Senators. This is indicated in the left panel of the figure: The ratio of the orange to the green bars is approximately the same indicating that the ratio of majority-party and minority-party success is about the same in the party-match data.

Second, in the cycle-match data, running for reelection positively affects minority-party Senators' probability of success, but, and third, facing reelection does not have a similar affect for amendments sponsored by majority party Senators. These patterns are shown in the right panel of the figure, which shows clearly that minority-party in-cycle Senators clearly succeed at a higher rate than found for the three other categories.

The rest of this section probes the robustness of and seeks to more fully illuminate these preliminary observations.

**Estimation Techniques** While the observed patterns are suggestive, drawing causal inferences from them requires an assessment of their precision. For this purpose, I consider approaches based on both non-parametric and logit regression models to estimate causal quantities of interest and associated uncertainty intervals.

Since the treatment and dependent variables are binary, non-parametric p-values estimated with a Fisher Exact Test are used to quantify the uncertainty of the difference-in-means estimates (Fisher 1922). This test is well-suited for the party-match and cycle-

match data because its validity does not rely on functional or distributional assumptions, and thus is not undermined by the relatively low counts in some of the outcome cells (Imbens and Rubin 2009).

I also, separately, report difference in means and exact test p-values for amendments offered by the most electorally vulnerable Senators, as defined by the result of the most recent presidential election in that Senator’s state.<sup>16</sup> Doing so is intended to clarify the political relevance of the results especially in the cycle-match data: If observed differences across treatment groups are artifacts of random variation, then there would be no reason to expect these differences to be larger for Senators facing stiff electoral challenges. On the other hand, if in-cycle Senators are afforded more opportunities because Senate institutions are built to serve incumbents’ electoral objectives, then this institutional bias should be greatest for those Senators who are both in-cycle and facing challenging races.<sup>17</sup>

One downside of the non-parametric approach is that it doesn’t easily accommodate incorporation of additional covariates and so, I also report results based on logit regressions, which do.<sup>18</sup> Unlike other comparable studies of legislative effectiveness, and because of the data pre-processing steps, choosing the appropriate set of covariates on which to condition estimates is not essential for causal identification. Still, incorporating covariates could be valuable for two reasons: First, if the conditions required for unbiased causal inference are met, then including covariates should not substantially change point estimates. In this way, the regression models can provide a further robustness check on the non-parametric results (Imbens and Rubin 2009).<sup>19</sup> Second, while covariates should not substantially change point estimates, it might improve their precision (Angrist and Pischke 2008).

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<sup>16</sup>I define a Senator as electorally vulnerable based on whether the most recent presidential election in that Senator’s state was either (a) decided by 5% points or less, or (b) if the candidate from the Senator’s party lost in that Senator’s state.

<sup>17</sup>Or alternatively, if the “effectiveness” premium is a function of electoral vulnerability, then it will not accrue (or accrue far less) to safe-seat, in-cycle Senators.

<sup>18</sup>Logit regression, which models the log-odds ratio for a binary choice, is standard when outcome variables are binary as all those considered here are.

<sup>19</sup>Under these models, the treatment variable coefficient parameter will have a causal interpretation under the same conditions and assumptions that impart such an interpretation to the difference in means and Fisher test statistics considered above, though the substantive effect size requires an additional computation as discussed below.

There are many possible regressions that could be specified with the available data. Thus, for the regression estimates to serve as a robustness check, we need an objective approach to choosing covariates to guard against confirmation bias. I therefore select a “best” model according to the Akaike Information Criteria (AIC), an easily calculated, likelihood-based statistic.<sup>20</sup> I applied this criteria to all regression models that could be estimated with the variables listed in Table 1, and with the dependent variable defined to be one if an amendment received a vote or passed (or both), and zero otherwise.<sup>21</sup> For models that contained at least two binary variables, regressions were estimated both including and excluding the interaction of the binary terms. One notable feature of this process, as shall be discussed more below, is that it allows for the selection of models that do not include the treatment variables themselves if these variables lack a significant association with the outcome.<sup>22</sup>

### **Result I - Majority-Minority Amendment Success Parity**

Perhaps the most robust, and surprising, empirical result indicates that, once proposal ideology is accounted for, majority party membership does not influence individual Senators’ effectiveness. The overall difference in the probability of success is given in Table 3. This shows that, regardless of the definition of success, once proposal ideology is controlled for, members are about equally as effective serving in the majority and minority party. This same pattern holds across different outcome definitions and when the data is restricted only to those Senators facing competitive races.<sup>23</sup> Across all tested specifications, even the most extreme deviation from the null hypothesis reaches a p-value of only 0.3.

The regression analysis paints a similar picture. The majority party treatment variable

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<sup>20</sup>The AIC measures the relative probability that a given model minimizes information loss and in the process penalizes for each covariate included. Models with lower AIC scores have lower expected information loss and so the “best” model is chosen by which produces the lowest AIC score (Burnham and Anderson 2002).

<sup>21</sup>I have performed regressions on the other dependent variables as well. These do not yield any new information and so are omitted here for brevity but are available on request from the author.

<sup>22</sup>The full table of results from this process is available in the online appendix.

<sup>23</sup>Additionally, though I do not report them here, equivalent results occur when the in-cycle and out-of-cycle amendments are analyzed separately.

Table 3: Party-Match Data Non-parametric Estimates

Subset	Majority $\frac{\text{Successes}}{\text{Total}}$	Minority $\frac{\text{Successes}}{\text{Total}}$	Maj. Party Treatment Effect (p- value)
<b>DV: Pass + Vote</b>			
All Seats	26/61	23/61	0.05 (0.71)
Competitive Seats	16/41	11/34	0.13 (0.32)
<b>DV: Vote</b>			
All Seats	11/61	9/61	0.03 (0.81)
Competitive Seats	8/41	4/34	0.12 (0.34)

*Note:* Treatment effects estimated as the difference between the proportions given in the second and third columns. The p-values given in parentheses below the effect estimates are non-parametrically computed using a Fisher Exact Tests. So for example, the last column, 0.05, indicates that the success rates for minority and majority party Senators for this specification is 0.05% points, and the number next in this row, 0.75, indicates the probability of observing a difference as extreme as this under a null hypothesis of no party effect.

is not included in the best fit model, and when other similar models are estimated for comparison, the sign of the coefficient changes (though it is never significant). These results are given in Figure 4. In this table, the model selected according to the AIC criteria is listed in the first column. Other logical alternatives are presented alongside it for comparison and to demonstrate the null finding is not an artifact of the selection process. In the presence of a significant majority party effect, we would expect that the majority party coefficient would be significant and approximately equivalent in each of these models. Thus, the regression analysis serves to confirm the results of the non-parametric tests, and offers no evidence that a majority-party effect is somehow being masked by the influence of other covariates.

The bottom panel of Table 4, gives the same results but estimated using the full, raw data set, rather than the matched data. These results show that why previous studies have found such a robust relationship between majority party affiliation and effectiveness. When proposal ideology, and Senator-fixed effects are not accounted for a significant



positive relationship is found between majority party status and legislative success in all four specifications. Additionally, these effects are stronger for majority-party members in vulnerable seats, and majority party members running for reelection. When proposal ideology is accounted for however, these relationships disappear entirely, which suggests that previous results in the literature may have dramatically overstated the affect of party affiliation by ignoring the extent to which majority party membership influences proposal ideology.<sup>24</sup>

Figure 4: Party-Match Data - Regression Results



*Note:* The sign of the estimated coefficient is given by the symbol in the chart. The red symbols indicate that the coefficient is significant at an  $\alpha = 0.05$  level. The best fit models refer to the models selected for the given data using the AIC criteria described previously. For the raw-data, this specification includes ideology scores for both individuals and time periods which is suppressed from the above for brevity. Full regression tables given in the appendix.

**Majority-party null finding interpretation and caveats** This null result with respect to majority-party affiliation is striking given the existing literature on the subject. However, two caveats need should be born in mind: First, the size of the party matched

<sup>24</sup>[Future versions will run a placebo test where samples are selected that control for fixed effects but not for proposal ideology to test whether the null result is a product of sample size-reduction]

data is smaller than the cycle-match data, so a systematic relationship may exist but require more observations to unearth.<sup>25</sup> Second, the always-applicable distinction between absences of evidence, and evidence of absences here bears repeating. Nevertheless, this result still contravenes against most previous work on the subject and appears to be the result this study being the first to consider the confounded relationship between majority-party membership, proposal ideology, and legislative success. This last point is supported both by the results presented for the regressions run on the raw, unmatched data, and by evidence presented in the appendix indicating that if without controlling for proposal ideology, we would be very likely to reject the null hypothesis with a high degree of statistical confidence.

## **Results 2 and 3 - Electoral Cycle Effect**

In addition to the null finding for the party-match data, the figures at the start of this section suggested that a positive electoral-cycle effect exists, but may only apply to Senators in the minority-party. This pattern is discussed and verified in the discussion of the non-parametric and regression results below.

**Finding 2: Electoral Cycle Effect for Minority Party Senators** In Table 4, the treatment effect estimates are given in the last column with corresponding Fisher Exact test results underneath. These results show that when the cycle-match data is pooled across the minority and majority parties, the evidence of an electoral-cycle effect is suggestive but inconclusive. However, when the parties are considered separately, the influence of the electoral-cycle becomes clear. For the minority party, running for reelection increases the probability of success by about 16% points. This effect always reaches an  $\alpha = 0.1$  confidence level with the Fisher Exact test procedure, and becomes even sharper when either outcomes are defined by receiving a recorded vote, or when the most competitive seats are analyzed separately. In the former case, the magnitude estimate is about the same but its significance reaches  $\alpha = 0.05$ , while in the latter case the magnitude increases

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<sup>25</sup>The smaller sample size is not surprising since Senators electoral status changes more frequently than which party holds the majority.

dramatically and the result is significant at  $\alpha = 0.01$  for both dependent variables. While the sample size is small, there is still no reason to expect that the estimates would change this way if the outcome observed patterns for the full cycle-match data were not reflective of substantive underlying political institutions and incentives.

Table 4: Cycle-Match Data Non-parametric Estimates

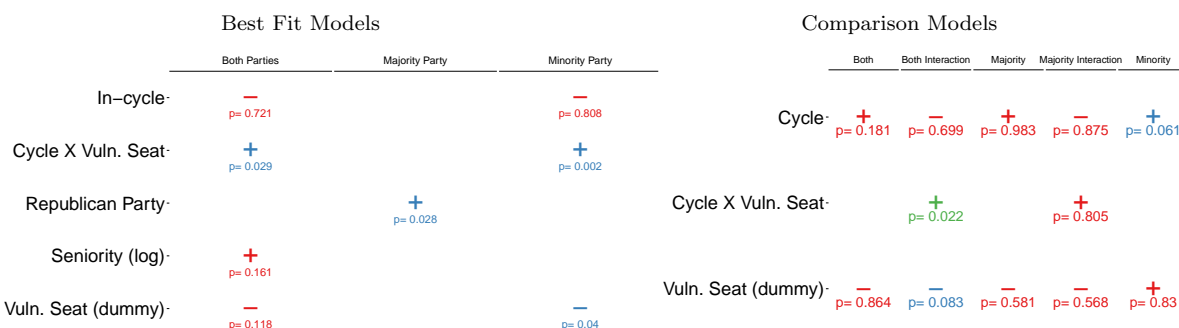
Seats	Party	Out-of-Cycle $\frac{\text{Successes}}{\text{Total}}$	In-Cycle $\frac{\text{Successes}}{\text{Total}}$	Electoral-Cycle Effect (p-value)
<b>Pass + Vote</b>				
All Seats	Both	36/109	42/100	0.09 (0.2)
	majority	20/49	17/42	0.003 (1)
	minority	16/60	25/58	0.16* (0.08)
Competitive Seats	majority	8/22	9/23	0.03 (1)
	minority	2/21	11/16	0.59** (0)
<b>Vote</b>				
All Seats	Both	14/109	22/100	0.09* (0.1)
	majority	7/49	6/42	0 (1)
	minority	7/60	16/58	0.16** (0.04)
Competitive Seats	majority	3/22	5/23	0.08 (0.7)
	minority	0/21	5/16	0.31** (0.01)

*Note:* Treatment effects estimated as the difference between the proportions given in the second and third columns. The parenthesized p-values beneath these estimates are calculated with Fisher's Exact test and refer to the null hypothesis of no electoral cycle effect. So for example, the first number in this column, 0.09, indicates that the difference across in and out-of-cycle Senators' success rates for this specification is 9% points, and the number next in this row, 0.2, is the Fisher Exact Test p-value for this estimate.

These non-parametric patterns carry through the regression analysis as well. The model-selection results for the cycle-match data are given in Figure 5. This figure shows

the best-fit models estimated for both parties, as well as the majority party, and the minority party subsets. The results indicate that for the minority party data, the best model available in the data contains only the treatment variable, the dummy variable for vulnerable seats, and the interaction of the two. The interaction term is positive and statistically different from 0 with a confidence level of  $\alpha = 0.002$ . The lower order, coefficient on the cycle term is not significant but its interpretation is ambiguous in the presence of the significant higher order term. One approach to resolving this ambiguity is to compare the model with the in-cycle treatment variable to a null model which contains only the indicator for electoral vulnerability. This comparison shows that the inclusion of the electoral cycle term increases log-likelihood and that this increase is statistically significant according to a standard likelihood ratio test ( $p < 0.001$ ). Also, as shown in Figure 5, the electoral-cycle treatment is positive and significant ( $p=0.06$ ) when the interaction term is dropped.

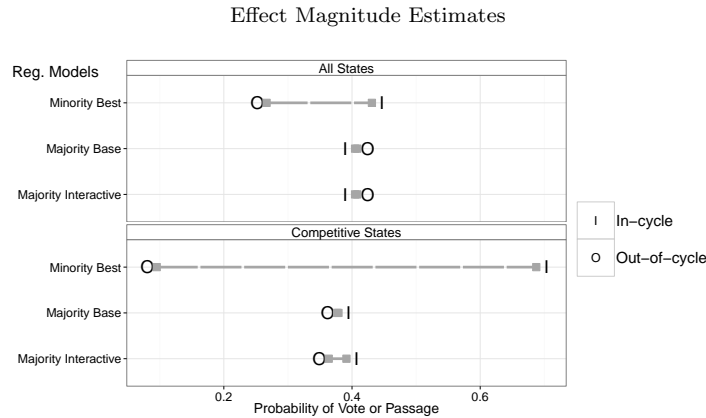
Figure 5: Cycle-Match Data Best Regression Results



*Note:* The sign of the coefficients in the two charts is given by the “+” or “-” symbol. The coefficient’s symbol is if the coefficient is significant at an  $\alpha = 0.05$  level. The left panel contains the models selected by the AIC criteria. The right panel models are for comparison and to demonstrate that the patterns in the best-fit models are not an artifact of the AIC criteria.

The evidence that emerges from this process confirms what we saw in the previous section. For the cycle-match data, best fit models for amendments sponsored by minority-party Senators include the electoral-cycle treatment variable and its interaction with electoral competitiveness for both the “Vote” and the “Vote or Pass” dependent variable definitions. The treatment coefficients in these models are statistically significant ( $\alpha = 0.05$ ) and the models pass standard likelihood ratio tests (see appendix for model likelihood ratio tests).

Figure 6: Effect Magnitude



*Note:* With respect to the Effect Magnitude chart, the horizontal length between the “O” and the “I” represent the difference in the fitted probabilities between the out-of-cycle and in-cycle observations in the cycle-match data. If the causal inference assumptions discussed previously hold, then this difference represents that average treatment effect.

Additionally, the model selection results continue to offer no support for concluding that electoral status is relevant for majority party Senators.

**Effect Magnitude** In addition to confirming the significance of the electoral-cycle treatment, the regression analysis also validates the difference in means’ estimate of the treatment effect magnitude. The magnitude estimates for the minority-party, best-fit regressions data as well as similar calculations based on comparable models applied to the rest of the cycle-match data are shown in the right panel of Figure 6 and indicate that that when running for reelection, minority party Senators have approximately a 0.42 probability of passing or getting a vote on an amendment, which is nearly twice the probability of success for an out-of-cycle minority party Senator.<sup>26</sup> What’s more, the electoral cycle effect estimated for minority-party Senators in vulnerable seats is nearly twice as large as that for minority-party Senators generally, though this is based on a limited sample and what may be an artificially low success rate for out-of-cycle, minority-party Senators in these seats.

<sup>26</sup>Generally, the use of logit models requires converting regression coefficients and covariates to predicted probabilities and comparing these predictions across different covariate values. However, since the best-fit model for minority party Senators is fully saturated model with the electoral cycle treatment variable, the electoral vulnerability dummy variable, and the interaction of the two, the difference in predicted probabilities are equivalent to the non-parametric effect size estimates found via the cross-tabulated difference in means.

**Finding 3: No Electoral Cycle Effect for Majority Party Senators** In contrast to the strong evidence of an electoral cycle effect for minority party Senators, there is almost nothing to suggest that the electoral-cycle meaningfully influences the effectiveness for majority party Senators. Across outcomes, seat-types (i.e. competitive or all seats), and estimation methods, amendments offered by in-cycle and out-of-cycle Senators in the majority appear to fare about the same. As for the minority-party subset, simple differences in means and associated Fisher Exact test p-values for majority party amendments in the cycle-match data are shown in Table 4. The results indicates that there is almost an uncanny similarity in outcomes across the in-cycle and out-of-cycle outcomes for the majority party. In fact, three of the four point estimates for the majority party Senators deviate less than 5% from the exact count predicted by the null hypothesis.

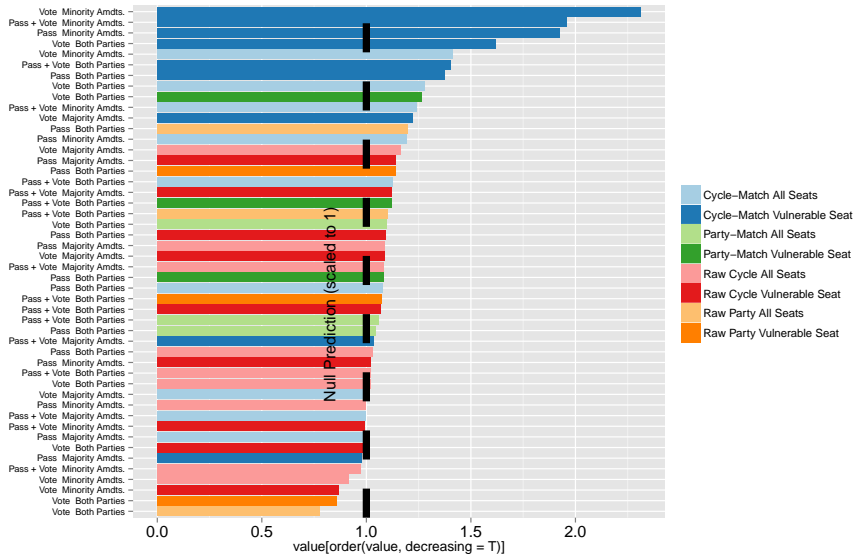
Additionally, when we consider the results of the model selection process shown in the middle of the left panel of Figure 5, we see that the best-fit model does not contain the electoral treatment variable. In the right panel of the same figure, we see that when this variable is included in majority-party models comparable to those selected for other subsets, the results indicate that neither the treatment variable, nor any related interaction terms even come close to reaching statistical significance.

Finally, strong evidence against finding a non-zero electoral cycle effect for majority-party Senators can be seen in the Figure 6. In contrast to the effect sizes estimated for the minority party, two of the four models for majority party amendments estimate a *negative* electoral-cycle effect for majority-party Senators, though this estimate is not statistically significant from zero. Evidently, the size of the effect for majority party Senators is far lower than it is for minority party Senators and not clearly distinguishable from zero.

**Visualizing Results** Figure 7 visualizes the empirical results in a single chart by focusing on the difference between the number of “successes” observed versus the number predicted under the null hypothesis for the treatment groups (i.e. for majority-party and in-cycle sponsors in the party-match and cycle-match data respectively).

In this graph, the y-axis labels first give the outcome definition and then indicate whether the category contains observations for the majority party, minority party or

Figure 7: Observed Outcomes vs. Null Predictions



both. The bar color indicates two features. First, whether the data comes from the cycle-match or party-match data, or, if neither, whether treatment is defined by party affiliation or electoral cycle. Second, the color also indicates whether the category consists of all seats or only those classified as electorally vulnerable.

The length of the bars then is determined by the ratio of the observed successes to the null prediction in the treatment group of that category. Each null prediction has been normed to 1 so that those bars that extend beyond the black dashed line correspond to those categories in which the treated group was more effective than would have been predicted under the null hypothesis of no treatment effect and thus, in these categories, some evidence for a non-zero, positive treatment effect can be found.

Thus, figure 7 shows in the top row that the largest positive deviation from the null hypothesis occurs when the dependent variable is defined by receiving a recorded vote, the data considered comes from the cycle-match data and comprises those amendments sponsored by electorally vulnerable, minority-party Senators. For this category, the length of the bar indicates that there are almost twice as many successful outcomes amongst in-cycle Senators, as we would expect to see if the electoral cycle didn't influence effectiveness in this data.

Generally, the concentration of blue bars towards the top of the figure crystalizes the

pattern that strong electoral cycle effects are evident, especially for the minority party, and most strongly for the minority party Senators facing the most daunting reelection campaign. Also, the one green, party-match bar amongst these blue cycle-match bars at the top is misleading in that the seemingly large deviation is the result of an extremely small number of observations in the implied treatment and control group cells. The Fisher Exact tests discussed earlier showed a p-value of 0.78 for this category and thus the deviation from the null hypothesis prediction is not statistically distinguishable from random noise. On the other hand, the same non-parametric approach the deviations from the null found in the blue bars at the top of the figure to not only materially substantial, but also statistically distinctive from zero.

To conclude this section, the empirical results can be summarized quickly because the main patterns appear robust across different estimation techniques. First, once proposal ideology is accounted for there appears to be little evidence to support concluding that majority party membership causes Senators to be more effective or legislatively productive. When it comes to individual legislative achievement, ideology apparently matters a great deal, but the apparatuses of party power may not.

Second, a material electoral-cycle effect on legislative effectiveness was found for minority-party Senators. That these estimates reflect political substance and not just random noise, is underscored by the fact that the strongest effects are observed for Senators facing the most competitive electoral conditions. These results are essentially the same whether estimated with logit regression models or non-parametrically. Adding covariates into the model does not meaningfully change the estimated electoral-cycle effect except for the covariate capturing electoral vulnerability. This pattern serves to further augment the argument that the observed electoral-cycle effects are both substantial and directly tied to the electoral connection between Senators and their constituents. This interpretation however is limited to Senators in the minority party. For majority-party Senators no evidence of a positive electoral-cycle effect is found in the data.



## Extension to Institutional Theories and External Validity

The empirical results just presented have their most obvious application to the literature on individual legislative productivity. In this section though, I discuss and interpret them in a slightly different context, and argue that they carry implications for the broader institutional literature on policy-making as well. Specifically, my argument is that estimated treatment effects found in the previous section can be used to evaluate the relative strength of the party-centered models of policy-making and a primary alternative to these models.

Also in this section, I discuss issues related to whether the estimated effects derived from the matched data sets extend to the data outside of these samples.

**Connecting Legislative Productivity and Institutional Policy-Making Theories** Modern research on Congress' policy-making process centers on the debate between perspectives which posit that parties are a key organizing feature, or alternatively, that the appearance of party power is mostly an artifact of the correlation between party affiliation and underlying preferences (Cox and McCubbins (1993), Krehbiel (2010), Rhode (2000))

The link between these perspectives and individual legislative productivity is found in the institutions which influence the rules and procedures of agenda-setting. Policy-making models are, at their core, concerned with this institutional influence and how it can affect the distribution of procedural resources. This distribution, almost by definition, also plays a large role in determining individual legislative success or failure. Therefore the forces that shape the policy-making process at the macro level should leave evidence of their presence in the distribution of individual successes.

In party-centered models of policy-making, the existence of parties is justified and their power rationalized by their ability to monopolize the procedural levers of legislative control. In particular, under the "Procedural Cartel Model" (PCM), the majority party monopolizes the legislative agenda which allows it to promote the party's "brand" (Cox and McCubbins 2005) which in turn provides electoral benefits to majority party legislators.<sup>27</sup>

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<sup>27</sup>The PCM and its many variants, posit that parties are supported and maintained because they provide electoral "public goods" to their members that can only be produced by groups of well organized

From the PCM perspective, a monopoly on procedural resources helps the majority party largely because it provides the means to block disfavored policies from the agenda. This so-called “negative-agenda” power is the focus of most empirical studies testing procedural theories of party power.<sup>28</sup>

While this empirical strategy measures the validity of the PCM perspective using final-passage outcomes, there is nothing in the underlying logic of procedural party power to suggest that agenda-control is valuable *exclusively* due to its influence on final policy outcomes. A majority party that can strongly influence policy outcomes by virtue of its negative agenda power should also strongly influence other politically relevant outcomes if they are as a function of the agenda-setting process.<sup>29</sup>

**Electoral Connection Models** The primary alternative perspective to these party centered theories, which trace back to Mayhew’s seminal contribution and is eponymously known as the Electoral Connection Model (ECM), posits that legislative institutions are primarily designed to help incumbents keep their jobs regardless of their party Mayhew (1974). This model sharply diverges from the PCM perspective in that, under the ECM, parties have little to no meaningful influence once legislators’ and voters’ primitive preferences are taken into account (Krehbiel (2010), Krehbiel (1999)).

Since it is often posed as an alternative to party-centered perspectives, empirical tests of the ECM also tend to focus on behavior in the final stages of the policy-making process. At the same time, analagous arguments apply to extending the ECM perspective to apply to the distribution of procedural legislative success. In fact, the earliest articulations of these models emphasized non-policy outcomes such as “credit-claiming” and “position-taking,” which we assume are a main reason why legislators would value successes that do not

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and relatively disciplined legislators (Cox and McCubbins 1993). The party brand is useful in part because it transmits information about candidates ideology to imperfectly informed voters who wish to reduce their uncertainty regarding the consequences of their vote choice (Snyder Jr and Ting 2002).

<sup>28</sup>In particular, these empirical approaches emphasize whether majority parties are able to avoid being “rolled”, which is defined to occur whenever a majority of the majority party votes against a bill which goes on to pass anyway.

<sup>29</sup>In fact, the authors of the Procedural Cartel Model specifically refer to the types of procedural outcomes measured in this paper. “[The] job of the majority-party leadership was to protect its members from having to vote on issues on which they did not want to vote” (Cox and McCubbins 2005).

immediately result in policy changes (Fenno (1978), Mayhew (1974), Anderson et al. (2003)).

The arguments for linking the PCM and ECM models to the estimated effects computed in the previous section are particularly strong given the methodological focus in this paper on controlling for legislation's ideological content. Previous studies of legislative productivity were, in some ways, ill-suited for evaluating institutional policy-making models because these models make predictions in terms of ideological outcomes while extant metrics proxy for individual effectiveness without controlling for ideology. As a result, for example, cross-party parity in individual legislative productivity would not indicate a lack of majority-party agenda power if the minority tended to propose legislation that was consistent with the majority's preferences. In the data analyzed in this paper however, legislative ideology is constant across treatment groups, and so it becomes straightforward to extend the institutional theories to proxies for individual legislative success.

Accepting the arguments above, the results estimated in the previous section are unambiguously more supportive of the ECM than the PCM perspective. In-cycle minority-party Senators were more productive than their same-party colleagues, and this difference was widest when comparing outcomes for Senators facing the most competitive elections. Meanwhile, there was no evidence of a similar effect for majority party Senators. This finding is inconsistent with the PCM, especially given that such models emphasize the majority party's negative agenda control.

At the same time, the estimated electoral-cycle effect is consistent with, if not entirely supportive of, the electoral connection perspective. While electoral incentives seem to play a role in determining the distribution of agenda access for minority party Senators, no such trend is found for the majority party. However, this result does not falsify the ECM hypothesis since the ECM model is not founded on the prevalence of incumbent-protecting institutions in a single legislative setting. One possibility is that majority party, in-cycle Senators are more successful at the committee stages, where Senate rules endow the majority with more tools to shape legislative output (e.g. there is no super-majority

requirement at the Committee stage).

Next, shifting attention to the party-match data, the previous section suggested that there is little if any evidence of a positive majority-party effect. Significant caution in interpreting this result is warranted because of the sample size. However, at the same time, this result clearly does not countervail against the evidence found in the cycle-match data that suggests the limitations of the PCM perspective as applied in the Senate.

Overall then, the partisan break down of the estimated electoral-cycle effect and the lack of evidence for a majority party effect suggest that efforts to apply the procedural cartel model in the Senate should proceed cautiously. Even if, as some research has found, final-passage roll-call votes follow a distribution in line with the PCM predictions, the underlying logic of this model appears to be contradicted by the distribution of procedural success and patterns of individual productivity discussed above (Gailmard and Jenkins (2007), Crespin and Monroe, Den Hartog and Monroe (2010)). Meanwhile, the ECM is partially predictive of and potentially consistent with the estimates gleaned from the Cycle-Match data and correctly predicts that productivity would be about equal across treatment groups in the party-match data.

**External Validity** A complete interpretation of the results in the last section, especially their extension to broader policy making theories, requires a discussion of the extent to which the estimated relationships in the matched data illuminate effects that apply generally to all amendments.

This discussion must start by acknowledging two facts. First, this paper's matching process has the unavoidable side-effect of discarding a large number of both treated and control group observations. As a result, the validity of causal interpretations are only immediately applicable within the sample. Second, determining whether the effects estimated for this sample can be extended more broadly is more art than science at this point. This last observation notwithstanding, below I look for indications of similarity or dissimilarity between observations in the matched data sets and the raw data consisting of approximately the full sample of amendments offered.

The basic question is whether or not policy proposals offered more than once are

comparable to policy proposals offered only once. This is a multi-faceted question that goes beyond the scope of this paper but a brief sketch of a response is possible.

Almost by definition, proposals repeatedly offered as amendments are not dependent on the underlying legislative proposal for their ideological character. These amendments have ideological consequences that are in some sense independent of the bill they seek to amend. On the other hand, a large portion of the unmatched observations are amendments that can only be ideologically interpreted in the context of the underlying legislation to which they are proposed. From a fairly broad random inspection of this data it appears that many of these amendments are more technical than ideological. Neither the extant literature nor the data here has much to say about these amendments, since policy-making research primarily focuses on outcomes that are ideological in nature, rather than procedural.

A second subset of unmatched amendments comprises those that are ideological in nature but defined locally and thus escape the text-matching process. For example, two amendments might be proposed to cut a specific appropriations account by a million dollars across different Congressional sessions, or two amendments might be proposed to cut different appropriations accounts by that amount. Because the text-matching process can't account for the underlying legislation, it can't determine ideological similarity of either pair and therefore doesn't retain either in the matched data. These amendments, because they depend on a particular proposal being brought to the floor repeatedly are more dependent on the legislative agenda than the amendments in the matched data and perhaps in some way this mediates the influence of membership in the majority-party or the in-cycle electoral-class however, it's not immediately clear why this would be the case.

Finally, a third, and likely the largest, subset of unmatched amendments comprises those which make proposals that are unique (at least in the sample collected). Here, the concern raised earlier regarding amendments that are only offered once because they are enacted the first time emerges again in a slightly different context. The question here is whether a majority-party or electoral-cycle effect results in a certain subclass of amendments not appearing in the matched data.

While it's impossible with the data at hand to address this concern definitively, there is

no evidence of such a dynamic where we would most expect to observe it. First, as noted already, there is no evident relationship between the number of times an amendment has previously been introduced and legislative success. Also, I present evidence in the appendix that the probability of legislative success in the non-matching data is not higher than the probability of success observed in the matched data as we would expect it to be if censoring was in fact taking place. That is to say, if the types of amendments in the matched data were fundamentally different, than we would expect that outcomes for all amendments would in part be a function of whether or not the amendment was in the matched data. As shown in the appendix, this relationship is not supported in the data.<sup>30</sup>

There are also indications in the data that suggest that the matched amendments are similar in many respects to the larger set of unmatched amendments. In the appendix, I present several evidence that Senators who are in the matched data sets are not ideologically distinctive from their colleagues, that the distribution of outcomes and covariates are broadly similar in the matched and unmatched data, and that similar trends over time seem to be operating at least to first-order approximation.

Another set of critiques relates to whether it is appropriate to evaluate broader theories of policy-making with the results estimated in the last section. For instance, and perhaps most intuitively the null majority-party effect may not indicate a lack of majority party power but that instead the party has superior avenues to advance the electoral objectives of its members. This is very possibly the case given that the Senate majority party has more resources to pursue partisan outcomes at the Committee stages of the policy-making process. Nevertheless, the inherent value of successful amendment outcomes for both parties seems difficult to dispute. Majority and minority party Senators *offer* amendments at approximately the same rate and the dependence between the electoral cycle and amendment success for minority-party Senators strongly suggests that such successes are perceived to have electoral impacts.

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<sup>30</sup>The set of unmatched observations contain the amendments that are only offered once *because* of a successful outcome (e.g. enactment into law) achieved in this initial introduction. Thus, all else being equal, the unmatched data should overall have a higher probability of success if a censoring dynamic is operating.

Thus, regardless of other legislative avenues available to majority-party Senators, the findings above still contrast with the predictions of the prevailing theories of parties in Congress in that these theories posit that the majority-party has both a strong incentive and powerful mechanisms at its disposal to prevent the minority party from achieving electorally valuable legislative successes.

Finally, regardless of the similarities or differences between the matched and unmatched data, the effects estimated above are arguably important in their own right. Amendments are offered and considered far more often than are bills, and thus improving our understanding of the determinants of success in the amendment process seems independently worthwhile. Also, amendments that are offered more than once obviously represent issues that legislators care about more than most, and care about independently of changing political conditions. Thus, especially given that proposal re-introduction does not seem to be materially censored by initial amendment success, the matched data can be seen as representing a subset set of proposals that reflect Senators' agenda-independent, legislative priorities.

## **Conclusion**

This paper's immediate goal was to determine whether the the electoral cycle or partisan affiliation causally effects Senator's legislative productivity as measured in the Senate amendment process.

To pursue this goal, a shortcoming in the empirical literature on legislative productivity had to be addressed. Specifically, the relationship between independent variables and the ideological content of legislative proposals had to be measured and accounted for. To address this shortcoming I created both an cycle-match and a party-match data set where legislative content and individual fixed effects were perfectly balanced across treatment groups defined by either electoral class and majority party membership, respectively.

Within these data, the arguments required to make unbiased causal estimates were argued and shown to be, for the most part, fulfilled. In particular covariate distributions across treatment groups were shown to be very similar, and the one variable for which

balance was not induced showed no meaningful relationship with outcome variables.

With this data, both non-parametric and logit regression models showed clear and consistent patterns: electoral vulnerability appears to affect legislative productivity for minority party Senators, but probably not for majority party Senators. The effect is largest for those Senators in the most electorally competitive states. These conclusions are robust across non-parametric and logit regression models, and to different definitions of legislative success.

On the other hand, there is essentially no evidence in the data for finding a non-zero majority party effect on legislative productivity. This finding is surprising given the unanimous result in the literature that the majority party enjoys a large and significant productivity advantage. This result may be due to the small sample size, but it also could be the result of the fact that previous studies did not account for the incentives legislators have to alter the ideological content of their legislation when their party gains or loses the majority.

These empirical findings, in part because of the causal framework in which they were estimated, have implications for competing theories of partisan agenda control and Congressional institutions. The family of theories that posit a strong majority party, predicated on exclusive control of procedural resources, would predict essentially the opposite of what the data support. Specifically, these theories would predict that, if an electoral-cycle effect existed, it would accrue to the benefit of majority party members - the data in fact says the effect benefits the minority and not the majority. These theories would also predict that holding all else constant, Senators would be more likely to achieve success on a given legislative proposal as members of the majority party rather than as members of the minority party - the data says the probability of success is approximately the same.

The electoral connection models on the other hand seem to fare better since they correctly predict that in-cycle Senators would be more productive. These models would not suggest that the electoral-cycle effect would apply only to members of the minority party, but they could accommodate such a finding, if an electoral-cycle effect existed for



the majority party in another stage of the legislative process such as during committee consideration legislative matters.<sup>31</sup> Future research investigating whether this pattern holds at the committee stage would therefore help evaluate how well electoral connection models predict legislative productivity.

More research is also needed to determine whether the null majority-party effect found is due to the small sample size, or if it is due to controlling for the confounded effect of majority party affiliation on legislative content.<sup>32</sup> Additionally, scholars could also examine the extent to which the amendments analyzed here resemble the collection of amendments overall. Such a project could incorporate both additional statistical tests regarding the distribution of amendment covariates as well as text-analytic tools to compare the distribution of topics in the matched data to the broader set of amendments.

The approach adopted in this paper can also be extended immediately to other legislative settings and processes, most obviously the House of Representatives and Senate bills (as opposed to Senate amendments, which were the empirical focus here). While legislative text matching may not work in all settings, since there is no guarantee that legislators will repeatedly offer identical amendments, given the surprising results presented above it seems worthwhile to at least attempt to apply this method more broadly.

Beyond the literature on legislative productivity, the approach here, and the surprising if tentative findings it produced, indicate a broader issue in the empirical literature on representation. Often measures intended to capture representational outcomes are based on quantities or counts of a behavior when what really matters is the quality of the behavior being investigated.<sup>33</sup> Only recently have tools become available to move beyond these quantity counts of representative behavior, but now that they have arrived,

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<sup>31</sup>Note here that if this speculative result is found in the future, it still doesn't save the Cartel Model Theories which make strong claims about the majority party's incentives and opportunities to restrict the minority party on the *floor* of the legislature.

<sup>32</sup>One initial piece of evidence suggesting that it may be the latter is that when the raw data is analyzed under the same procedures that were used for the matched data, a majority party advantage is found as would be predicted by the findings in the existing literature.

<sup>33</sup>For just one example in a related but distinct field, recent research has considered whether electorally vulnerable members speak more or less frequently on a legislative chamber floor, when it would seem that it is the content of these speeches that speak directly to their representational import (Eggers and Spirling 2014). And relatedly, Sarah Binder quotes a recent presentation in which Mayhew deplors the practice of "counting things" (Binder 2015)

they should be put to use in these situations and produce a higher resolution picture of forces at work in Congress and other legislatures.

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