How Elections and Parties Influence Legislative Effectiveness

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

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This paper considers why some legislators are more effective than others at advancing their preferred policies. Existing studies on this subject, since they do not control for underlying proposal content, fail to distinguish between effective and accommodative legislating, and are difficult to connect to broader questions regarding representation and policy-making. In this paper, I use new data on U.S. Senate Amendments to address these shortcomings. Specifically, I apply a novel text-processing algorithm to identify a series of "natural experiments", wherein one Senator introduces identical amendments under different institutional settings. My results, unlike all previous studies, suggest that majority-party membership does *not* increase effectiveness, but running for re-election does, at least for minority-party Senators and especially for the most electorally vulnerable among them. Overall, my results suggest that Senate policy-making institutions may be oriented more towards incumbents' electoral incentives, regardless of party, and less towards partisan competition than some recent scholarship suggests.

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In July of 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, but he provided swine packers triple the amount of time to come into compliance as their beef and poultry counterparts. Despite providing a remarkably apt reminder of the similarities between the legislative and sausage-making processes, Senator Johnson's amendment not only failed to clear the Senate, it never even received a vote.

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

What accounts for these different outcomes? Since, the amendment had the same sponsor each time it was introduced, Senator-fixed traits will not provide an answer and it is natural to turn to Congress' policy-making institutions for clues. One difference that may have been salient to Senator Johnson's case especially 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, 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). I argue that such cases are particularly useful for uncovering how electoral and partisan incentives influence Senators' capacity to legislate effectively, and for connecting individual legislative outcomes to models of policy-making that disagree over the relative influence of parties and electoral institutions.

Approach and Contributions of the Paper

The general aim of legislative effectiveness research has been to identify the institutions and individual traits that cause some legislators to be more successful in pursuing their policy objectives.This paper makes related theoretical, methodological, and substantive contributions towards answering these questions.

Theoretically, this paper offers a precise definition of legislative effectiveness that clearly connects to the underlying concepts of representation with which legislative scholars are generally concerned. Most importantly, this definition distinguishes between effective and accommodative legislative behavior, which exposes a missing variable problem endemic in the existing research, and which makes the democratic and representational consequences of existing results ambiguous.

This paper's methodological contribution is to solve the missing variable problem and resolve this interpetive ambiguity. This is accomplished with a new corpus of Senate amendments consisting of more than 50,000 proposals made between the 100^{th} and 113^{th} Congresses. In this, data I identify and analyze a "natural experiment" where the same Senators proposes the same legislative proposals in different institutional settings. I empirically exploit this behavior to identify and estimate the causal effect of party-membership and electoral vulnerability on individual legislative effectiveness and make three main findings related to Senators' effectiveness in their chamber's amendment process: (1) majority-party membership has no influence on effectiveness; (2) electoral vulnerability has a positive effect for minority party members, which is strongest for Senators facing the most challenging electoral conditions, and (3) electoral vulnerability has no effect on Senators in the majority.

After outlining and illustrating these findings, I discuss what to make of them by considering whether the empirical approach is likely to present an accurate picture of the Senate amendment process or if focusing only on repeatedly offered amendments skews the results. After finding little evidence to suggest that identically offered amendments materially differ from the broader sample, I discuss the implications of the empirical results for theories of agenda-setting and policy-making both in the Senate and in general.

The paper also helps fill in gaps in our knowledge about important but largely understudied stages of Congress' policy-making process. Most studies of Conngressional legislating consider bills introduced and debated in the U.S. House. While scholars of previous generations have studied legislative effectiveness in the Senate, I offer the first empirical analysis that utilizes modern econometric techniques. I also consider amendments rather than bills, which despite receiving little attention from academics, have proven to be extremely important and even decisive in several recent prominent legislative debates.[1]

Procedures and Productivity in the Senate

Since there are relatively few studies that utilize data from the Senate amendment process, and because this process is procedurally distinct from that which applies to regular order bills, I provide an extremely brief procedural overview of the amendment process below.[2]

During floor consideration of most bills, Senators may submit any amendment 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* - 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 frequently uses this right to preempt other amendments from consideration in a process known as "filling the tree". Once amendments are made

[1]For example, a Senate amendment doomed the most promising recent effort to change national immigration laws and saved the Affordable Care Act from looming defeat. Senate amendments were also critical to the passage of the so-called stimulus passed at the start of the Obama Administration and several other high-profile bills considered during this time period.

[2]Interested readers can consult `www.senate.gov/CRSReports/crs-publish.cfm?pid=\%26*2\%3C4RL08\%0A` for additional background.

pending they are generally resolved either by unanimous consent, vote (which can be either recorded or voiced), or rule - e.g., non-germane amendments might fall due to an affirmative Senate vote to invoke cloture which renders such amendments out-of-order. 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 Senators' robust legislative ambition, creates an environment of scarcity where there are more amendments than the Senate as a collective body is able to consider (Walker 1977).

While my empirical focus on the Senate is a departure from the usual focus on the House (though not a unique one, see Matthews (1960), Moore and Thomas (1991)), the literature on legislative effectiveness in general goes back half a century and is well-reviewed elsewhere (Anderson, Box-Steffensmeier and Sinclair-Chapman (2003),Cox and Terry (2008)). 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.

Numerous variables are considered 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 and also considers individual-level variables like gender, race, and ethnicity (Anderson, Box-Steffensmeier and Sinclair-Chapman (2003), Volden, Wiseman and Wittmer (2013), Frantzich (1979)). Also, some have also found evidence that innate ability, or personal legislative style, such as speech frequency, also can influence individual legislative effectiveness (see Volden and Wiseman (2014) and Anderson, Box-Steffensmeier and Sinclair-

Chapman (2003), respectively).

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 critique 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 that were constant in a single Congress. As a remedy, CT emphasize 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 provide 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. This general concept - that legislative success is defined with respect to both procedural and final policy outcomes - is universally accepted in the literature and is adopted here as well (e.g. Anderson, Box-Steffensmeier and Sinclair-Chapman (2003), Volden and Wiseman (2014)).

Finally, and perhaps most importantly, CT explicitly link 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 (Moore and Thomas (1991), Anderson, Box-Steffensmeier and Sinclair-Chapman (2003)).

Volden and Wiseman (2014) have recently made a significant contribution to this literature in their book-length treatment on the subject of indvidual legislative effectiveness. Their work highlights at least two ideas that are applicable to this paper. First, recognizing that existing research is held back because it lacks a single coherent empirical measure of its main quantity of interest, the authors develop a normed measure of legislative effectiveness for each legislator-year. Secondly, the authors hint at the importance of differentiating between distinct types of legislative activity. This continues the trend away from measuring Congressional output with unqualified counts, a trend that this paper aggressively adopts.

My paper considers the same subject and agrees with the general perspectives motivating and expressed in Volden and Wiseman (2014). However, while the authors make numerous impressive contributions, neither the scalar measure of legislative effectiveness they develop nor their deployment of this measure address the identification issues that I identify and then address below.

Effectiveness or Accommodation?

To evaluate existing approaches to studying legislative effectiveness, we first need to define the underlying quantity of interest we are trying to capture. While there is no canonical definition, 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 it is defined by the ability to advance policy proposals that achieve one's legislative goals.[3] This definition seems simple, but it importantly distinguishes between effective and *accommodative* legislating, where the latter is the inclination to offer legislation that is ideologically agreeable to one's colleagues. The distinction is obviously material to questions of representation and policy-making, since the preferences of citizens with more effective legislators will play a larger role in shaping realized policy outcomes.

This definition of effectiveness also clarifies that quantifying the concept by counting the number of bills that advance to a given legislative stage is, without further refinement, subject to a missing variable problem. To use observed results from the policy-making process to measure X's causal influence on legislative effectiveness we need to also measure its influence on legislative content. Otherwise, the relationship between X and legislative success is confounded by *X's* simultaneous relationship with legislative content, and

[3] In a standard spatial policy-making model, such a definition implies that the expected outcome for effective and ineffective legislators will differ given an ideologically equivalent proposal. For example, with 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. This approach to thinking about individual effectiveness was suggested by Nolan McCarty at a roundtable on legislative effectiveness research at a recent conference.

associated statistics fail to distinguish between effective and accommodative behavior.[4]

This problem undermines attempts to connect existing measures of legislative effectiveness to the core concepts of legislative representation and democratic policy-making that Congressional scholars ultimately care about. For example, studies have found that legislation sponsored by female legislators may be more likely to advance in Congress' policy-making process (Volden, Wiseman and Wittmer 2013). If this finding is interpreted as evidence that woman are more effective, then the representational implications are that districts who vote for male legislators are willing to endure substandard legislative records for some unknown reason. If, on the other hand, we interpret this finding as evidence that women tend to be more accommodating in their proposals, then this may indicate that female legislators are given ideological "slack" by their constituents not enjoyed by their male counterparts.

Empirical Approach

The basic takeaway from the discussion above is that in order to measure how institutions influence legislative effectiveness, we need to consider their simultaneous affect on proposal content. In this section, I develop an empirical approach that meets this need and apply it to estimate both the effect of *majority-party membership* and the Senate's trifurcated *electoral-cycle*.

I emphasize the role of these variables because of their natural connection to the broader debate in the Congressional literature about the relative importance of partisan and individual electoral incentives, respectively, in structuring the agenda and influencing legislative results. This debate centers on whether parties are a key driver of the policy-making process or, alternatively, whether the empirical appearance of party power is an artifact of the correlation between party affiliation and underlying preferences (Mayhew (1974); Cox and McCubbins (2005); Krehbiel (1999, 2010)).

The link between this debate, which is concerned primarily with aggregate-level outputs

[4] This issue seems even more pertinent given recent findings in Adler and Wilkerson (2013) which suggest 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.

and individual legislative productivity in the Senate, is found primarily in the chamber's agenda-setting institutions. Policy-making models, partisan or otherwise, at their core are concerned with how legislative institutions 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, and theories that attempt to explain the former will make predictions regarding the latter, as well.

While other effectiveness studies have attempted to frame their findings as speaking to questions of institutional influence on policy-making, without addressing the missing variable issue introduced above, such interpretations are difficult: policy-making theories render their predictions in ideological terms while existing metrics proxy for individual effectiveness without controlling for ideology. As a result, to take one example, cross-party parity in individual legislative productivity might not indicate a lack of majority-party agenda power if the minority tended to propose legislation that was consistent with the majority's preferences. On the other hand, if legislative content is controlled for, then these ambiguities do not arise, and these competing models of the policy-making process define three hypotheses that are empirically testable in the data.

Hypothesis 1: Majority-Party Advantage The prevailing model of partisan agenda setting is built on the idea that the majority party enjoys a monopoly over agenda-setting and other procedural resources and is thusly often referred to as the Procedural Cartel Model (PCM). This theory predicts that the majority party will restrict agenda access for the sole benefit of its members and that these members will enjoy other material resource advantages. Thus, this theory would predict, and indeed draws strength from existing findings which purport to show, that the majority-party will be more effective than minority-party colleagues.

A leading alternative perspective to these party centered approaches traces back to the seminal contribution in Mayhew (1974) and is therefore often called the Electoral Connection Model (ECM). Under this model, legislative institutions are primarily designed to help incumbents keep their jobs regardless of their party, while parties have little to no meaningful influence once legislators' and voters' prior preferences are taken into account Krehbiel (1999). This model predicts that party affiliation will have little influence on legislative effectiveness. These contrasting positions define the paper's first hypothesis:

Majority Party Effect Hypothesis

Null Hypothesis 1: Senators in the majority and minority party are equally effective.

Alternative Hypothesis 1: Senators in the majority party are more effective than Senators in the minority.

Hypothesis 2: Electoral-Cycle Advantage If voters' decisions are influenced by a "What-Have-You-Done-for-Me-Lately" bias - a fairly weak, frequently employed, and empirically defensible proposition - then it is optimal for Senators to prefer to backload their legislative accomplishments into the session when they are in-cycle.[5] Since, the ECM model stipulates that institutions will evolve to maximize the overall electoral prospects for incumbent legislators, it predicts in this context, that Senators running for reelection, who are said to be "in-cycle", will be more effective than their electorally secure, "out-of-cycle" colleauges. This defines the second hypothesis:

Electoral-Cycle Effect Hypothesis

Null Hypothesis 2: In-cycle and out-of-cycle Senators are equally effective legislaors.

Alternative Hypothesis 2: In-cycle Senators are more effective than their out-of-cycle colleagues.

Hypothesis 3: Party mediation of electoral-cycle effect Finally, the ECM and PCM perspectives define a third, slightly subtler hypothesis regarding the partisan distribution of an electoral-cycle effect should one be found. Under the PCM, a party organizes and sustains collective activity because doing so allows its members to monopolize the agenda, in turn yielding electoral benefits. The underlying logic of the PCM highlights

[5]In fact, Shepsle et al. (2009) show that such behavior can plausibly be consistent with non-cooperative, utility-maximizing equilibria behavior as well.

the majority party's unconditional *negative agenda-control*, which it applies to restrict the minority party's ability to use the legislative agenda to further their own electoral goals. If the effect of legislative accomplishment depends in part on electoral proximity, then the majority will be especially motivated to use their agenda-control to constrain minority-party Senators up for reelection. Alternatively, the ECM would predict that the majority party either is unable or unwilling to structure policy-making so as to exclude the minority from using the agenda to pursue their electoral objectives. These competing viewpoints give rise to the paper's third and final hypothesis:

Party Mediation of Electoral Effect Hypothesis

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

To evaluate these hypotheses I employ a new corpus of Senate amendments and a novel methodology to analyze and quantify legislative similarities across the proposals in it. Below I introduce the new data set first, and then discuss how and why I measure legislative similarity in it.

This original data consist of the vast majority of Senate amendments offered from the 100th through the 113th Congresses. Containing more than 50,000 policy proposals, this corpus is to the best of my knowledge the most comprehensive source data on Congressional amendments analyzed in the literature. Within this data set, I define a range of variables consistent with long-standing practice in the literature. The non-obvious ones are described in Table 1.[6]

In any empirical study of policy-making productivity, one crucial feature is the definition of legislative success. Perhaps the most obvious approach, classifying a proposal as successful if and only if it is enacted into law, is inappropriate and unhelpful for analyzing

[6]While the Congressional Bill Project and others have made corpi of Congressional legislative texts easily accessible, no corresponding source existed for *amendments*.

individual productivity for two related reasons. First, the percentage of individual proposals enacted into law is so small that defining success to cover only these cases ignores the vast majority of variation in legislative outcomes. Second, legislators also make and seek to advance proposals because they offer "credit-claiming" and "position-taking" opportunities that they perceive to be electorally valuable (Arnold (1992); Mayhew (1974)). For these reasons, the definition of legislative success in existing studies includes procedural, rather than just final, outcomes in the definition of legislative success because, to quote one influential study, doing so offers "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, Box-Steffensmeier and Sinclair-Chapman 2003). This "positive action" can be electorally valuable regardless of its direct impact on prevailing policy because it helps Senators develop their reputations individually and parties communicate their priorities collectively (Mayhew (1974); Lee (2009)).[7]

For these reasons, research on individual legislative succeess, from its earliest to its most recent incarnations does not solely define the concept solely according to whether a proposal is enacted into law (Anderson, Box-Steffensmeier and Sinclair-Chapman (2003); Cox and Terry (2008); Volden and Wiseman (2014)). I follow this precedent in the approach here, assuming that proposals can be "successful" even when not enacted, as long as they progress far enough to provide their sponsors with politically beneficial, and not universally available opportunities. 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.[8] Specifically, I focus on three different, overlapping, binary definitions of success depending on whether an amendment (a) was approved by the Senate, (b) received a recorded vote on the Senate floor, and (c) was approved or received a vote or both (i.e., the union of

[7] In addition to legislator's individual incentives, the amendment outcomes short of enactment are worth understanding because they can influence (positively or negatively) Congress' ability to expeditiously move forward with a broader agenda and contribute to the overall socio-political milieu in which policy ideas evolve (Walker 1977).

[8] It is probably most similar to the definition of success in Cox and Terry (2008), which is based on the reporting of a bill through committee.

Table 1: Selected Covariate Abbreviations and Measurements

Variable Name	Explanation
Pres. Vote	Democratic presidential candidate's two-party vote share in most recent election in amendment sponsor's state
NOMINATE	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	Absolute difference between Senator's NOMINATE score and the chamber mean NOMINATE score for that session
First Introduction (First Intro'd)	Equal to 1 the first time an amendment is introduced (chronologically), equal to 0 otherwise.
Tough Race	Dummy variable indicating a competitive electoral climate. Equal to 1 if most recent presidential race in amendment sponsor's state was decided by $< 5\%$ *or* that Presidential candidate from sponsor's party lost in the sponsor's state.

the first two categories).These definitions are intended to echo the conceptualization of success in the literature as closely as possible but, of course, the correspondence is not perfect. However, the substantive results are robust across various approaches to defining legislative success that I have considered.[9]

The Senate amendment data provide the foundation for this paper's central empirical innovation - the identification of two "matched" subsets of observations consisting of amendments repeatedly offered by the same Senator in different electoral and partisan settings respectively. As depicted in Figure 1, the subset that I refer to as the *party-match data* comprises legislatively equivalent 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 content is independent of which party holds the majority, and in the cycle-match data, proposal content is independent of whether

[9]Other definitions of success that I have analyzed include categories based on whether amendments become pending, multinomial definitions based on variations of these categories, and outcomes which treat dependent variables separately.

Figure 1

Criteria for Party-Match and Cycle-Match Data Sets

Identify proposal tuples with same ideological content
(*matched amendments*)

⇓

If matched amendments are sponsored by same Senator *AND*

⇓

at least one was offered when that Senator was both

↙ ↘

in-cycle and out-of-cycle *OR* in majority and minority

↓ ↓

Cycle-Match Data	Party-match Data
▸ Treatment groups defined by whether amendment sponsor is up for reelection ▸ n=209	▸ Treatment groups defined by whether amendment sponsor is in majority party ▸ n=122

the amendment sponsor is in-cycle.[10]

The ultimate goal of creating these two matched data sets is to identify data where all relevant covariates are distributed as we would expect them to be in a randomized trial. So, for example, the party-match data is intended to correspond to what would be observed if Senators wrote and introduced their amendments prior to the start of a session and were then randomly assigned to receive the institutional and resource advantages of membership in the majority party or not. If this goal is achieved, then causal quantities of interest can be estimated without bias and analyzed using the potential outcomes, causal inference framework (Rubin (1974); Sekhon (2008)). Under this framework, each observation is assumed to have two *potential outcomes.* One of these outcomes is observed and corresponds to the outcome that the unit realizes under the treatment condition that the unit actually received. The other outcome, referred to as the "counterfactual",

[10]These data are constructed via a text matching algorithm which identifies amendments with essentially identical legislative content. Several examples illustrating the ideological equivalence of the matched amendments are provided in the appendix and the full sample of matched amendments are available in their redlined form at `scholar.harvard.edu/bengruenbaum/data`. One complication that arises in creating these data concerns the treatment of amendments that meet the criteria for inclusion, and which are also offered more than once in a single legislative session. For example, what is the appropriate way to deal with a proposal which is offered once by Senator A as a member of the majority during session t, and 3 times by Senator A in session $t + 1$ as a member of the minority. While there are several possible options, I implement the simplest and consider for each amendment a single outcome for each session the amendment appears in the matched data, with this outcome defined by the most successful result recorded in that session. This means that the estimated effects should be interpreted as applying over the course of the session not for any single legislative debate. Generally speaking, the results are robust to other reasonable approaches as well. I take several other steps in identifying the matched data sets. For example, I filter out amendments offered to the Senate Budget Resolution because, for these amendments only, all Senators can insist on on receiving a simple-majority vote at a time certain.

is the outcome that would have been observed if the same unit had received the other treatment assignment. So, for the party-matched data, each amendment has two potential outcomes corresponding to the outcome that would occur when the amendment sponsor is a member of the majority and minority party respectively, and analagously for the cycle-match data. The causal effect of treatment then is defined theoretically at the unit level by the difference between these two potential outcomes. Since we only observe an observation under a single treatment condition, estimating causal effects requires that we impute unobserved, counterfactual outcomes, and the causal validity and statistical efficiency of resulting parameter estimates depends on this imputation process.

In a potential outcomes framework, matching is aimed at ensuring that the distribution of potential outcomes is the same in the treatment and control groups. This is accomplished by defining a matching criteria that selects pairs (or any tuple with at least one treatment and one control obseration) that are similar "enough" according to the criteria.[11] These selected observations then form a new database in which, hopefully, the joint distribution of potential outcomes is independent of treatment assignment, as it would be in a truly randomized experiment (Ho et al. 2007).

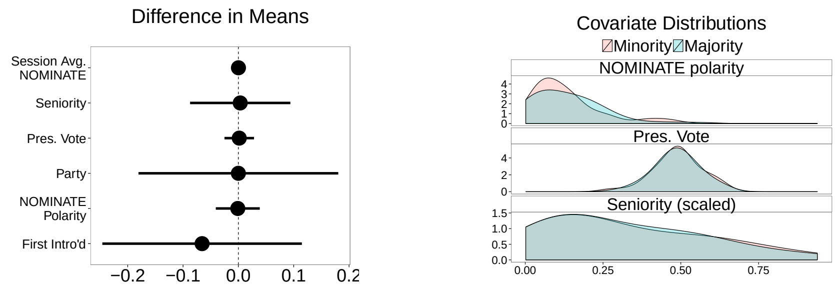
Therefore, the key to assessing the success of any matching process (when intended for causal inference) is to consider the distribution of covariates are across the treatment and control groups. If the matching "worked", then the covariates should be distributed in the treatment and control groups as they would be under an experiment where treatment was determined randomly (Ho et al. (2007) Sekhon (2008)).

These considerations have natural application to the cycle and party-match data. Notably however, there are two variables, legislative content and Senators' fixed latent ability, whose distribution are not easily quantified but which are almost certainly correlated with legislative effectiveness. These are the covariates around which the matching criteria is constructed and so they are balanced by construction: amendments are only included in the party-match or cycle-match data if there is an amendment offered under the opposite treatment condition, sponsored by the same Senator and with the same legislative

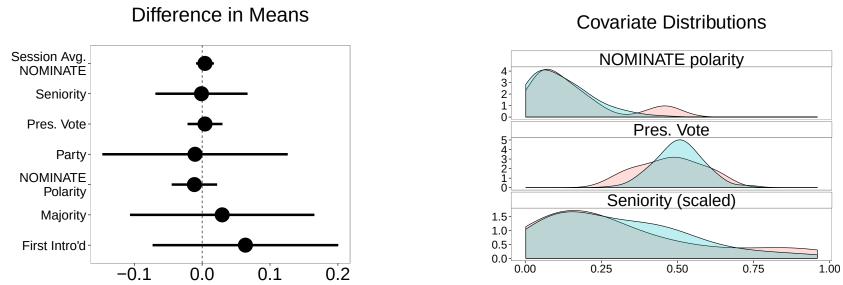
[11] As long as the matching criteria is not a function of the outcome variable, this process does not induce bias.

Figure 2: Covariate Balance

Party-Match Data



Cycle-Match Data



Note: Black lines in the left-hand panels indicate 95% confidence intervals.

content.

Even with this requirement though, other variables could still confound the relationship between potential outcomes and treatment assignment; in fact the matching process itself can induce a confounded relationship where none exists in the original data (Ho et al. 2007). For this reason, I empirically test for imbalance, by comparing covariate distributions across treatment conditions. The results of running these balance tests on the two matched data indicate that covariates are generally well-balanced, and none of the differences in means are statistically significant. These results, shown in Figure 2, indicate that covariate means in the two treatment groups are statistically indistinguishable from each other and generally appear to be similarly distributed.

The one possible exception relates to the "First Intro'd" variable, which is equal to 1 the first time an amendment appears (chronologically) in the data. In both the party and cycle-match data, the differences across treatment groups are not statisticially distinguishable from zero, but appear large enough to warrant further consideration. In a subsequent section, though, I show that there is little reason to be concerned about this variable because, despite intuitive reasons to suspect otherwise, there is no evidence to suggest that it is associated with successful legislative outcomes. As a result, the measured imbalance is unlikely to bias the empirical results.

The balance tests along with the strict criteria used in the matching process make a strong argument that in both matched data sets, amendments in the treatment and control groups are fundamentally comparable. The distribution of the treatment variable appears to be independent of all other measured variables that might influence legislative outcomes and so, as far as we can measure it, the matched data look how we would expect them to under the hypothetical experiments discussed above.

At the same time, it is important to acknowledge that internal balance in the data only gets us so far. Interpreting subsequent results requires a consideration of whether the matched data represent fairly the Senate amendment process in general. Given the criteria used in creating the matched data, and the fact that Senators are free to offer or not offer amendments depending on external political factors, several concerns can be raised. These concerns, unlike internal covariate balance, are considerably harder to get at empirically. For now, however, I proceed with analyzing the matched data because these concerns are easier to deal with when the results being assessed are concrete.[12]

Empirical 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, as presented in this section, simple techniques can be used to compute unbiased estimates of the average treatment effect of both electoral vulnerability and majority-party membership on

[12] One possible concern that can be addressed here, which is inherent in any study of electoral pressure in the Senate, relates to the treatment of retiring Senators. These Senators are technically in-cycle but clearly not subject to the same electoral incentives as their colleagues seeking re-election. However, classifying all retiring Senators as out-of-cycle has problems as well, since such Senators may not have decided, or be perceived as likely, to retire until their in-cycle session is largely completed. Thankfully, the data show that retiring Senators are, evidently, considerably less active in the amending process. There are only five amendments offered by retiring Senators in the cycle-match data. Results are robust to categorizing these five amendments as either in-cycle or out-of-cycle. I treat them as in-cycle because this seems likely the more conservative approach.

indivudal legislative effectiveness (Imbens and Rubin (2009), Ho et al. (2007)).

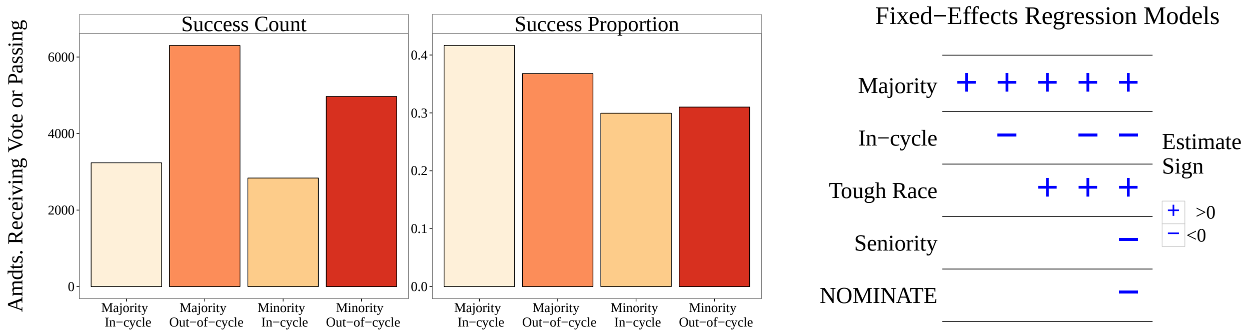
Original Data

Before turning to the matched data, it is helpful to consider first the full data for both descriptive and inferential purposes. Descriptively, despite the centrality and dynamism of the Senate's amendment process, little is known about its empirical contours, either in terms of which Senators make proposals or which proposals succeed. Inferentially, the original data provide information about the value added through the amendment matching process since results derived from it provide a baseline against which results from the matched data can be compared. Any differences then speak to whether the standard approach to estimating effectiveness is likely to induce bias as I have speculated it might.

Results calculated in the original data, obtained by calculating top-line differences in mean probabilities of amendment success and with individual Senator fixed-effects regression, are presented below. The former is adopted because of its expositional simplicity, the latter because it represents the current cutting edge approach to estimating causal effects in the individual effectiveness literature (Cox and Terry 2008).

With more than 50,000 observations and several hundred Senators represented in the data, statistical power is abundant and provide clear results both with respect to majority-party power and the role of the electoral cycle: Whether results are measured in terms of first-order differences in means or with fixed-effect regressions, the the original data replicate the literature's finding of a positive majority-party influence on effectiveness. In fact, the influence of majority-party membership is substantially larger than the estimated influence of any of the other variables. On average, the probability of receiving a vote or passing an amendment is approximately eight percentage points higher with a majority-party sponsor, while the estimated associations for other variables are around one or two percentage points. With respect to electoral cycle influence, the top-line and regression results show a negative assocation. However, it is considerably smaller in magnitude relative to the effect of majority party membership. These results are given in Figure 3.

Figure 3: Patterns of Success in the Original Data



Note: The left and middle panels show success frequency in the original data. In the middle panel bar height is determined by proportion of amendments that are successful in the indicated category (not proportion of overall successes in the data). The right panel summarizes estimates from a fixed-effect regression model, indicating the coefficient on the majority All coefficients in each model are significant at the $\alpha = 0.01$ confidence level.

With respect to other definitions of legislative success, if we classify outcomes according to whether an amendment passes, the results are unaltered. However, if we define success only according to receipt of a vote, then as shown in the appendix, the full data indicate that minority-party Senators are somewhat better positioned to achieve a positive outcome. This finding is consistent with the conventional view of Senate agenda-setting which stipulates that the minority party uses the Senate's procedural lattitude to force votes on their priorities (Lee (2008, 2009)). However, the majority's advantage in passing amendments is much larger than the minority's advantage in receiving a vote, and passing an amendment is - at least to a first order approximation - no less valuable than receiving a vote. Thus, the original data appear to support, and, at the least, are not contradictory of, the prevailing finding in the literature that finds majority-party legislators are more effective.

With respect to the influence of the electoral-cycle, the original data offers no evidence to suggest that the minority party is able to strategically allocate amendment opportunities, either measured as recorded votes or amendment approval, to its electorally vulnerable members. In fact, the evidence in the original data suggests that running for re-election *decreases* effectiveness for both parties. To the extent party membership mediates the electoral-cycle effect, it is, as would be predicted under the PCM, to the benefit of the majority party. Its worth noting, though, that the estimated influence of running for reelection, or any other variable, is considerably less important than whether or not a Senator is a member of the majority party.

These same results are supported by the results of applying fixed-effect models to the original data as well. These are given in the far right panel of Figure 3. Here each column summarizes the results for a different covariate model with the sign of the coefficient indicated by the "+" or "-" sign. Each of these variables is significant at the $\alpha = 0.01$ level and substantively, the results replicate with Senate amendment data those in CT which, using a similar fixed-effects approach, found that majority-party members were more effective legislators in the House.

Interpreting these results in light of the paper's original hypotheses suggests a substantial positive majority party influence on effectiveness as long as the measure of effectiveness isn't restricted to the receipt of a recorded vote. The analysis of the unprocessed data also suggests that the electoral-cycle likely *decreases* effectiveness and to the extent it is mediated by party-membership it is to the advantage of the majority party. Overall then original data appear to suggest that a party-centric Procedural Cartel Model does a reasonably good job predicting patterns of effectiveness while the Electoral Connection Model fares much worse.

The Matched Data

The analysis above suggests that analyzing the full collection of amendments would yield results that are substantively very similar to those found in the extant literature, especially with respect to party-strength. However, without considering the relationships between the variables considered and the content of proposed legislation, the appropriate substantive interpretations of these results are ambiguous. Also, as estimates of causal influence on effectiveness these results are likely biased by their confounded relationship with legislative content.

In this section, I use the matched data to address these issues that arise in estimating the causal determinants of legislative effectiveness. I start by presenting two simple charts that capture all of the main empirical themes. I then outline and demonstrate the paper's three main empirical findings, which I show are robust to estimation using both non-parametric and logit regression models.

While the presentation below, in order to clarify the exposition, focuses mostly on a single definition of legislative success, determined by whether an amendment receives a vote or passed the chamber, the main empirical results are extremely robust across different dependent variables.[13]

Three general empirical findings emerge from the analysis of the matched data. First, after controlling for proposal content, majority-party Senators are no more effective in the amendment process than their minority-party colleagues. This is indicated in the left panel of Figure 4: the ratio of the success to failure indicated by the adjacent bars is approximately equal indicating that frequency of success in party-match data is about the same across the party-determined treatment and control groups.

Second, running for reelection positively affects minority-party Senators' probability of success, but this effect is largest for minority party Senators facing the most challenging electoral circumstances. Third, and in contrast, there is no evidence of a similar electoral-cycle effect for majority-party Senators. These patterns are reflected in the right panel of Figure 4, which shows clearly that minority-party, in-cycle Senators clearly succeed at a higher rate than their out-of-cycle co-partisans, but that no such advantage accrues to Senators in the majority-party.

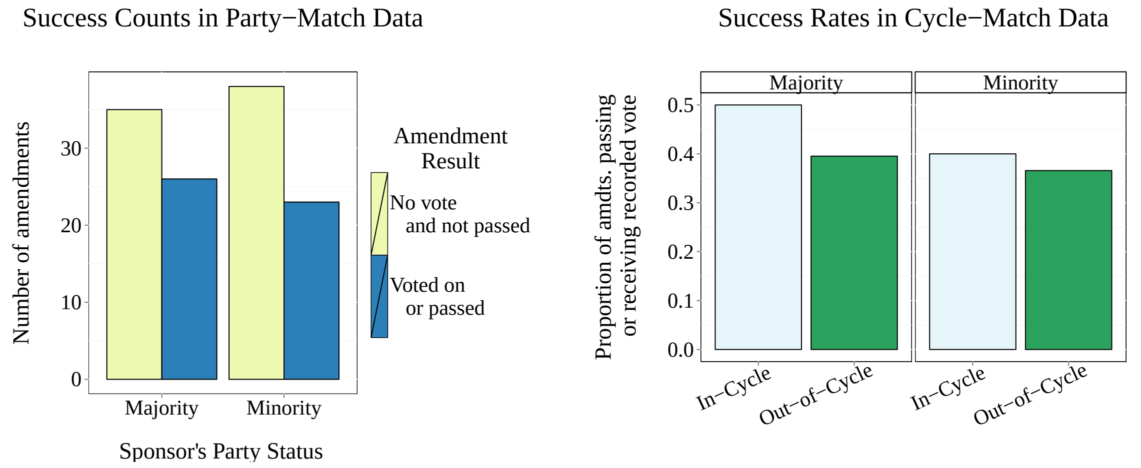
The rest of this section probes the robustness of and seeks to more fully illuminate these initial findings.

While the observed patterns are suggestive, drawing inferences from them requires more robust estimation and assessment of their precision. For this purpose, I compute average treatment effects using both non-parametric and logit regression models.

Since the treatment and dependent varables are binary, non-parametric p-values estimated with a Fisher Exact Test can quantify the uncertainty of the difference-in-means estimates (Imbens and Rubin 2009). This test is well-suited for the party-match and cycle-

[13]In addition to other binary dependent variables, I have obtained essentially the same substantive results using a multinomial approach, as well as with an approach that breaks out procedural and policy votes into separate categories. Specifications and information about these models are available in an online appendix and can be replicated with the code and data publicly available on my website.

Figure 4: Initial Evidence - Conditional Electoral-Cycle Effects and Partisan Parity



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).

In addition to the non-parametric tests, I consider results from logit regression models.[14] Unlike in other studies of legislative effectiveness, the matching approach taken here implies that controlling for covariates is not necessary to make unbiased effect estimates. Still, incorporating them is valuable for at least 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. Second, while covariates should not substantially change point estimates, it might improve their precision (Angrist and Pischke 2008).

In order for the regression results to serve as a robustness check, and because there are many possible regressions that could be specified with the available data, 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.[15] I applied this criterion to all regression models

[14]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.

[15]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

that could be estimated with the variables listed in Table 1. For models that contained at least two binary variables, regressions were estimated by both including and excluding the interaction of the binary terms.[16]

The robustness and substantive significance of the empirical results is also tested by giving special attention to the patterns of effectiveness among amendments offered by the most electorally vulnerable Senators.[17] Doing so clarifies 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 the most challenging electoral circumstances.

Result 1 - Null Majority Party Effect

Perhaps the most robust, and surprising, empirical result is that, once proposal content is accounted for, majority-party membership does not appear to influence individual Senators' effectiveness. The sample difference, which given the matching process used to create the data also provides an unbiased estimate of the average treatment effect of majority-party membership is given in Table 2. This table shows that, regardless of the definition of success, once proposal content 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.[18] Across all tested specifications, even the most extreme deviation from the null hypothesis reaches a p-value of only 0.3.

The regressions paint a similar picture. The majority-party treatment variable is not

and Anderson 2002).

[16]The full table of results from this process is available in the appendix.

[17]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 percentage points or less, or (b) if the candidate from the Senator's party lost in that Senator's state. I have considered a range of cutoffs and found them to have essentially no effect on the substantive results.

[18]Additionally, though I do not report them here, equivalent results occur when the in-cycle and out-of-cycle amendments are analyzed separately.

Table 2: Party Effects - Non-parametric estimates

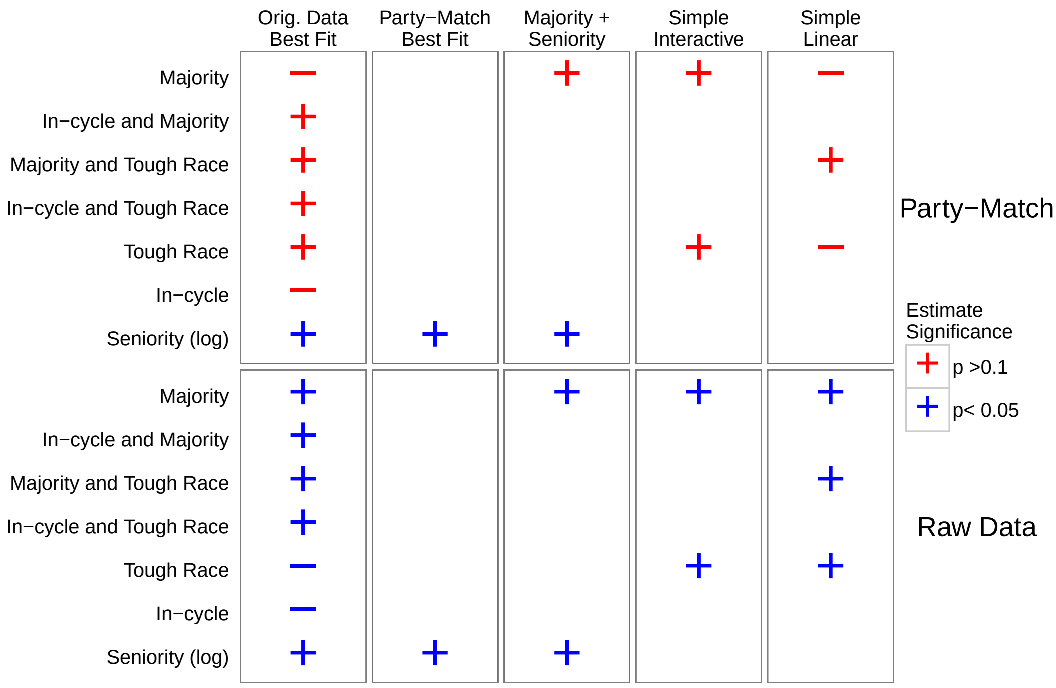
Seat Subset	Minority $\frac{\text{Successes}}{\text{Total}}$	Majority $\frac{\text{Successes}}{\text{Total}}$	Maj. Party Treatment Effect (Fisher Test p-value)
Dependent Variable: Pass + Vote			
All Seats	23/61	26/61	0.05
			(0.71)
Competitive Seats	11/30	16/32	0.13
			(0.32)
Dependent Variable: Vote			
All Seats	9/61	11/61	0.03
			(0.81)
Competitive Seats	4/30	8/32	0.12
			(0.34)
Dependent Variable: Pass			
All Seats	21/61	23/61	0.03
			(0.85)
Competitive Seats	10/30	13/32	0.07
			(0.61)

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.

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 5, with the model selected according to the AIC criteria applied to the party-match data listed in the second column. Other logical alternatives are presented alongside it for comparison and to demonstrate the null finding is not an artifact of the selection process.

The bottom panel of Table 5 returns to the original data, giving the regression results for the same model, but estimated using the full data. These results show why previous studies have found such a robust relationship between majority-party affiliation and effectiveness. When proposal content, 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 content is accounted for however, these relationships disappear, suggesting the possibility that previous results in the literature may have overstated the effect of party affiliation by ignoring the extent to which majority-party membership influences proposal content.[19]

Figure 5: Party Effects - Regression Results



Note: The sign of the estimated coefficient is given by the symbol in the chart. The blue 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.

Results 2 and 3 - Electoral Cycle Effects

The first look at the data at the start of this section suggested a positive electoral-cycle effect exists, but may only apply to Senators in the minority-party. As a first step towards

[19] The regressions for the original data here do not account for indvidual fixed-effects as the ones presented earlier did.

more fully illuminating this pattern, consider the treatment effects estimated in the last column of Table 5. These results, and the corresponding Fisher Exact test p-values reported underneath them, show that when the cycle-match data is pooled across parties, the evidence of an electoral-cycle effect is suggestive but inconclusive. However, when the parties are considered seperately, the influence of the electoral-cycle becomes clear. For the minority, running for reelection increases the probability of success by about 16 percentage 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 seperately. 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 dramatically and the result is significant at $\alpha = 0.01$ for both dependent variables. While the size for the analysis of competitive seats is fairly small, it still serves as a valuable robustness check. If the estiamtes moved in the opposite direction, it would call into question whether the observed patterns were truly reflecting political institutions and incentives rather than random noise.

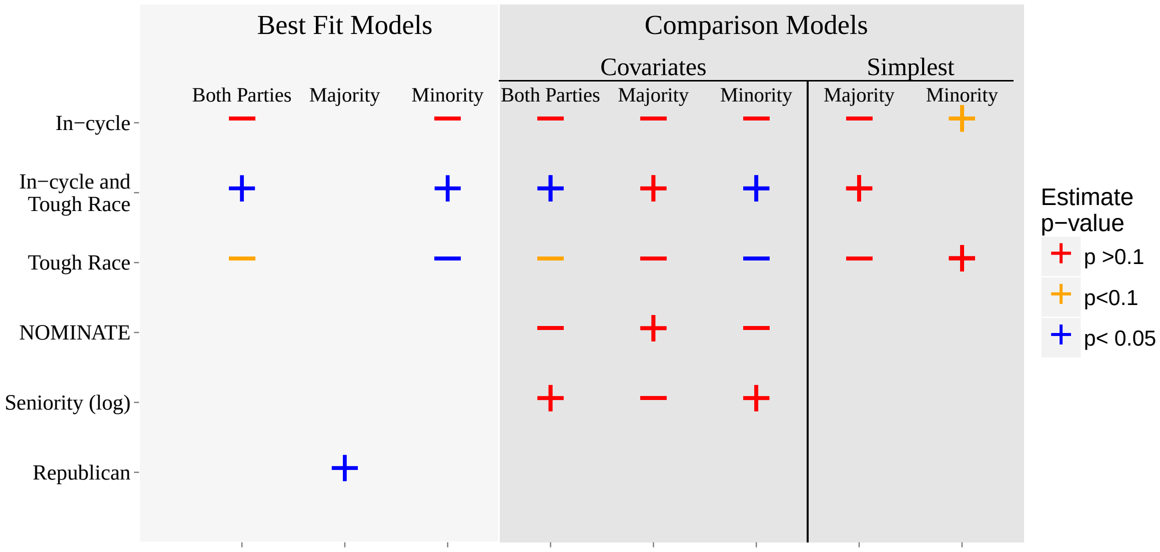
Table 3: Electoral-Cycle Effects -Non-parametric Estimates

Seat Subset	Party	Out-of-Cycle $\frac{\text{Successes}}{\text{Total}}$	In-Cycle $\frac{\text{Successes}}{\text{Total}}$	Electoral-Cycle Effect (Fisher p-value)
Dependent Variable Pass + Vote				
All Seats	Both	36/109	42/100	0.09 (0.2)
	Majority	20/49	17/42	0 (1)
	Minority	8/22	9/23	0.03 (1)
Competitive Seats	Both	10/43	20/39	0.28 (0.01)
	Majority	8/22	9/23	0.03 (1)
	Minority	2/21	11/16	0.59 (0)

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. Results for other dependent variables in the appendix show that, substantively, these results are robust.

These non-parametric patterns carry through the regression analysis, as well. The model-selection results for the cycle-match data are given in Figure 6. The results indicate that for the minority party data, the best-fit model contains only the treatment variable, the dummy variable indicating a competitive seat, 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 this does not have a clear substantive interpretation here given the 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 ($p < 0.001$). Another approach is to drop the interaction term, which, as shown in the last column of Figure 6, leads to an estimated electoral cycle effect that is positive and very nearly reaches statistical significance at the $\alpha = 0.05$ level ($p = 0.06$), and a positive coefficient on the "Tough Seat" variable but one which is hardly distinguishable from zero (p=0.83)

Figure 6: Electoral-Cycle Effects: Regression Results



Note: The sign of the coefficients is indicated by the "+" or "-" symbol. 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.

In contrast to the strong evidence of an electoral cycle effect for minority-party Senators, majority-party Senators appear to have the same chance of being successful whether they are running for reelection or not. The evidence suggesting this pattern is found across dependent variable definitions, seat-types (i.e. competitive or all seats), and estimation methods. In fact, as seen in Table 5, three of the four relevant point estimates of the majority-party, electoral-cycle effect deviate less than 5% from the exact count predicted by the null hypothesis.

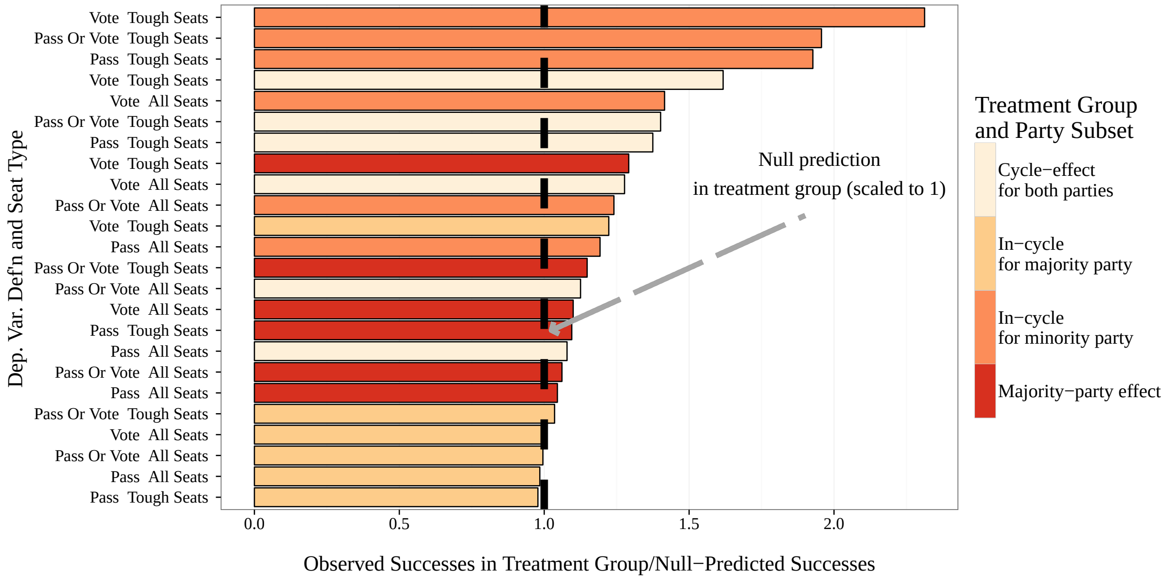
Therefore it is not surprising that the regression results for the majority-party in the cycle-match data (shown in the middle of the left panel of Figure 6) stipulate that the best-fit model does not contain the electoral treatment variable. In the right panel of the same figure, we see that when the treatment variable is included in majority-party models, neither it nor an interaction term containing it is ever significant.

Normally, estimating treatment effects with logit models requires converting coefficients into substantive quantities of interest. Here though, the regression results suggest that the best estimates of the effect size to be gleamed gleam from the logit models are equivalent to the estimates obtained non-parametrically in Tables 2 and 5. The best fitting regressions do not include continuous covariates or in the case of the party-match data do not include the treatment variable at all (and when it is included is nowhere near distinguishable from zero). This result is consistent with what we would expect under an idealized matching process, since in a randomized experiment the estimated effect magnitudes from a regression should be equivalent to a difference in means. Thus both the regression and non-parametric approaches suggest that the majority-party effect, and the electoral-cycle effect for majority-party Senators is nil. Both approaches also suggest that running for re-election causes the probability of success to increase by about 16 percentage points on average for minority-party Senators, and considerably more than this for minority-party Senators in more electorally competitive seats.

We have now seen a wide range of estimated treatment effects that have considered numerous ways of slicing the data. In the matched data these estimates have considered different dependent variable definitions and have been computed both for all states combined and separately for the most electorally competitive states. Within the cycle-match data we have also looked at results estimated for both parties, and for the majority and minority party considered separately.

We have now also seen that, because of the apparent success of the matching process, a simple difference in means provides the best possible treatment effect estimates. As a result, we can compare all treatment effect estimates on a single chart by focusing on the relative divergence between the observed outcomes and the outcomes predicted under the null hypothesis of no treatment effect as seen in Figure 7. In this graph, the y-axis labels first give the dependent variable definition and then indicate whether the results pertain to either all seats or are limited to the most electorally vulnerable ones. The bar colors indicate whether the results pertain to the electoral-cycle or majority-party treatment

Figure 7: Observed Outcomes vs. Null Predictions



Note: As explained in the main text, the electoral-cycle and majority-party effects shown are estimated with the cycle-match and party-mach data respectively. The bar length indicates the number of successes in the treatment group relative to the number of successes for this group predicted under the null hypothesis of no treatment effect. The y-axis labels give the dependent variable definition considered and whether the estimate was computed for all seats or just the most electorally competitive ones.

effect and whether they are estimated for majority-party Senators, minority-party Senators, or Senators from both parties simultaneously. Bar length then is determined by the ratio of observed successes for the treatment group to the number of successes predicted under the null hypothesis. Each null prediction has been normed to 1 so that those bars that extend beyond the vertical black dashed line correspond to data where there is evidence of a positive treatment effect.

Thus, Figure 7 shows in the top row that the largest deviation from the null hypothesis, and therefore the largest average treatment effect estimate, applies to minority-party Senators, when treatment is defined by receiving a recorded vote and results are calculated for Senators running in the most competitive states. In other words, the top row suggests that the largest influence on individual effectiveness is the electoral-cycle effect on the ability of minority-party Senators in politically competitive seats to get votes on their amendments. Generally, the concentration of same-colored bars at the top of the figure crystalizes the pattern that strong electoral-cycle effects are strongest for minority-party

Senators facing the most daunting reelection campaign.

To conclude this section, the empirical results can be summarized quickly because the main patterns are robust across different estimation techniques. First, once proposal content is accounted for, there is little evidence to suggest that majority-party membership causes Senators to be more effective or legislatively productive. Second, a large and statistically robust electoral-cycle effect on legislative effectiveness was found, but only 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 substantively 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. For majority-party Senators, however, no evidence of a positive electoral-cycle effect is found in the data. These findings stand in stark contrast with the conclusions that would have been reached if the full data had been analyzed using the standard approaches in the literature, which would have suggested that majority-party membership was the strongest predictor of legislative success and that running for reelection, in fact, decreased the probability of a positive amendment outcome. The difference between the results obtained in the original and matched data suggest that existing studies linking individual effectiveness with majorty-party membership are likely in large part measuring partisan influence on legislative accommodation as much as its influence on effectiveness.

Discussion: External validity and Theoretical Significance

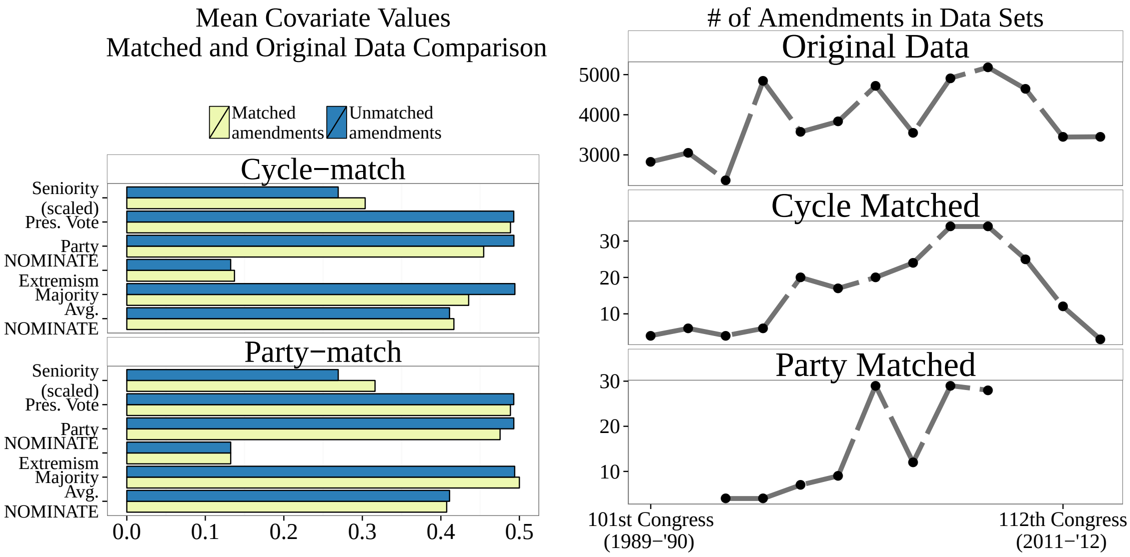
While the matched data present a clear picture of legislative effectiveness, the implications of these results for how we understand policy-making and representation in the Senate depend on whether these data are fair proxies for the Senate amendment process. In this section, I first discuss evidence that indicates that matched amendments are similar to the full set of amendments across a number of key dimensions. I then specifically consider concerns that the matched data is censored to exclude amendments that would evince a positive majority-party effect and find that the data provide no evidence for finding that this concern materially threatens the validity of the findings presented in the last section.

I then move on to discuss the implications of the empirical results for the stipulated hypotheses and interpret them in light of Congress' multi-faceted policy-making process. As part of this discussion, I speculate that the lack of majority-party power found in the Senate amendment process may point towards a multi-stage procedural equilibrium (or norm) in which incumbents from both parties are afforded preferential access to legislative opportunities but at different stages of the legislative process.

Matched and Original Data Similarities

Below I provide evidence that the the matched data sets and the original data have the same first-order political-legislative features. These results are of course in no way definitive, but they seem to indicate that matched amendments are not obviously distinguishable from other proposals.

Figure 8: Similarities Betweeen Matched and Original Data



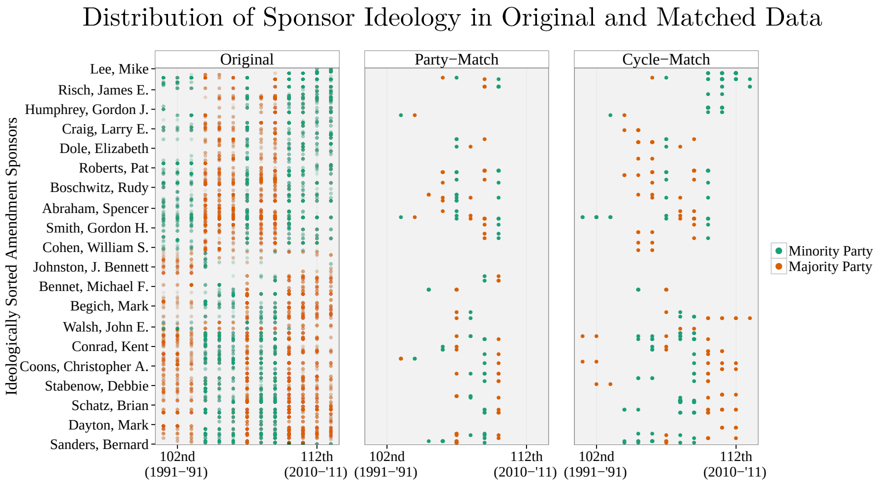
Note: In the left panel, the mean value of different covariates are given both as they are calculated in the matched data and the original data. For the two dummy variables included, "Majority" is equal to 1 if the amendment is sponsored by a majority-party member and equal to 0 otherwise, "Party" is equal to 1 for amendments sponsored by Republicans and equal to 0 otherwise.

Figure 8 shows that with respect to the measured covariates there is no evidence that the matched data is unrepresentative of the broader data. The left panel of this figure shows, average covariate values in the matched and original data. Across each of the covariates the differences observed for the matched and unmatched amendments are extremely small and thus provide little if any reason to expect that the effects found in the matched data are artifacts of the process by which these amendments were chosen. In other words, the covariate distribution in the matched and unmatched data is approximately what we would expect it to be if the matched amendments were drawn randomly from the full set of amendments rather than selected systematically based on their content.

In the right panel, we see evidence that changes in the intensity of amendment activity found in the original data are also reflected in the matched data, as well. More amendments are offered in the middle of the data set's time coverage, and we find that there are more amendments in the matched data from this period also.

Another perspective on the representativeness of the matched data is given in Figure 9, which shows that amendment introduction, and repeated amendment introduction, occur across both time and the range of the Senate's ideological spectrum. If the matched data provided a skewed sample of either the ideological or temporal features of the full amendment data then we would expect to see a "clumping" of dots somewhere in the matched-data panels, indicating that more matched amendments are coming from Senators with a particular ideological view point or at a particular time then would be expected in a random sample.

Figure 9: Sponsor Ideology in Matched and Original Data



Note: The names on the y-axis are representative but not exhaustive. They are sampled at even intervals from most liberal to most conservative Senators (from bottom to top) over the time-period in the data. The x-axis provides a single column for each session of Congress in the data. Each dot then represents an amendment in the indicated data set, with the vertical position representing its sponsor's ideological position and the horizontal position indicating when it was offered. In the original data we see that offering amendments is essentially a universal and frequent activity. In the matched data we see a good mix of moderate and extreme amendments offered though later in the time-window in both data sets there appears to be slightly disproportinate representation from more extreme Senators.

Censoring of Successful Amendments in the Matched Data

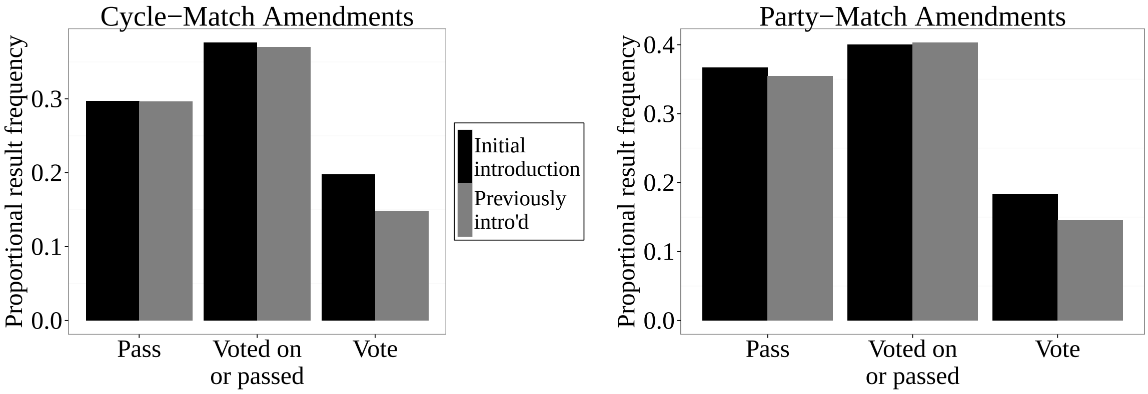
The general resemblance between the matched and original data does not rule out the possibility that certain types of amendments are excluded from the matched data and therefore are not being captured in the treatment effect estimates in the previous section. This variant of selection bias could still be present if, for example, more effective Senators tended to offer fewer amendments in the matched data because these Senators achieved their desired level of success with just a single introduction. Given the differences between the results presented in the previous section and the existing literature, we might be particularly concerned that membership in the majority-party both conferred an effectiveness advantage, but also suppressed the tendency to offer the same amendment repeatedly - a pattern that would emerge if majority-party Senators more frequently achieved their legislative goals during their proposals' initial consideration.

Indeed, intuition about the legislative process might at first suggest such an association, since we might suppose that once a proposal succeeds it will not be offered again. This intuition is likely in part because terms like "legislative success" evoke images of White House signings or district ribbon-cutting ceremonies. In other words, legislative success is most readily brought to mind in its most extreme and unlikely manifestation - enactment into law.

As already discussed, however, studying indiviudal legislative effectiveness requires that the concept be defined to include outcomes other than enactment. As a result, legislative success does not *a priori* exclude an amendment from inclusion in the matched data except in rare circumstances. Of course, the concern would be equally problematic if successful amendments are excluded *de facto*, as they would be if, for example, Senators tended to move on to new policy priorities once they achieved a modicum of success on old ones. However, it seems also plausible that initial success short of enactment might *increase* a Senator's inclination to bring up a proposal a second time since the proposal has shown itself to have political currency, or because its procedural success has attracted positive attention and resources from outside groups and other Senators.

The point is not that one of these two explanastions is more appealing, but rather that the question is an open one upon which empirical evidence can be brought to bear. If effective Senators are censored out of the matched data, then there should be empirical traces. In particular, we ought to see more amendments fail initially and then succeed then the reverse.

Figure 10: Success Frequency for First-time vs. Previously Introduced Amendments



Note: The height differences between adjacent black and gray bars reflect differences in success frequency for amendments being considered for the first time relative to amendments that have previously been offered. For example, the height of the left-most black bar shows that slightly less than a third of amendments in the cycle-match data are approved the first time they are considered on the Senate floor. Similarly the height of the adjacent gray bar indicates that a very similar proportion of amendments that previously have been offered achieve pass as well.

When the data is consulted however, the empirical evidence we would expect to find if majority-pary influence were understated in the matched data is absent. We see in Figure 10 that the success or failure of an amendment is evidently independent of whether it has been considered before. In other words, just as many amendments succeed the first time they are offered and fail the second as the reverse outcome order, and this pattern holds in both the party-match and cycle-match data.[20]

[20] I also show in the appendix that it holds for different dependent variable definitions of success.

Table 4: Introduction Order and Success: Null Results in Party-Match Data

	Dependent variable:					
	Pass or Vote		Pass		Vote	
Majority	0.622	0.207	0.368	0.140	0.519	0.223
	(0.538)	(0.371)	(0.542)	(0.378)	(0.689)	(0.493)
Previous Intro'd	0.442	0.027	0.188	−0.042	0.069	−0.265
	(0.536)	(0.371)	(0.543)	(0.378)	(0.726)	(0.493)
Majority:Previous Intro'd	−0.809		−0.450		−0.629	
	(0.748)		(0.760)		(1.000)	

Note: Estimates are for logit regression models. For all three binary dependent variable definitions, neither the "Previous Introduction" variable nor its interaction with the amendment sponsor's party ever approaches statistical significance in the party-match data. As shown in the appendix, similar results hold in the cycle-match data.

To search even deeper for evidence that majority-party influence is obscured by censoring in the matched data, we can consider whether there is any relationship between an amendment sponsor's party upon initial introduction and the outcomes observed. Such a relationship would be evident in a regression of amendment outcomes on the variable capturing whether the amendment had been previously introduced, whether its sponsor was in the majority-party, and the interaction of these two terms. However, as seen in 4, such regressions show no evidence of this sort of systematic relationship: Knowing an amendment is introduced for the first time by a member of the minority (or majority) gives us essentially no information about the likely result likely for that amendment.[21]

The arguments above do not guarantee that concerns about censoring in the matched data are unwarranted - doing so would require a content-based text analysis of the matched and unmatched amendments that is beyond the scope of this paper.[22] They do, however, significantly complicate attempts to write-off the results of the previous section

[21] In the appendix, I show that similar regressions in the cycle-match data yield equivalent results.

[22] Though the amendments in the matched data clearly represent a broad range of legislative subjects as can be seen partially in the appendix and in full by viewing these amendments online at `scholar.harvard.edu\BenGruenbaum\data`

as an artifact of the process by which amendments are included in the matched data. If a positive majority-party influence on effectiveness was limiting the representation of that influence in the matched data, then we would expect to see some evidence that the matched data disproportionately sampled amendments that failed the first time they were introduced. That we do not would seem to restrict the possibile mechanisms by which censoring could be taking place to less plausible, seemingly more *ad hoc* scenarios.

Connecting Effectiveness and Institutional Policy-Making Theories

In light of the previous section's results suggesting that the matched data is a fairly good sample of the broader amendment process, I return to the question of what individual productivity can tell us about aggregate forces in the policy-making process.

Recalling the ECM and PCM perspectives on agenda-setting discussed previously, the results presented in the last section unambiguously are more in line with predictions of the Electoral Connection alternative. In-cycle, minority-party Senators were more productive than their out-of-cycle co-partisans, and this difference was largest for Senators in the most competitive elections. Meanwhile, there was no evidence of a similar effect for majority-party Senators. Additionally, no advantage is found to accrue to members of the majority party. These results run against both the existing literature on individual legislative productivity and the PCM's perspective on agenda-setting institutions. The incongruence of the results with the PCM's predictions is particularly acute since such models emphasize the majority party's negative agenda control.

At the same time, the estimated electoral-cycle effects are consistent with, if not comprehensively supportive of, the electoral connection perspective. While electoral incentives influence 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 within the 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). This marks an area of future research using the approach adopted here.

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 PCM 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); Den Hartog and Monroe (2011); Crespin and Monroe (N.d.)). Meanwhile, the ECM is partially predictive of and potentially consistent with the estimates gleamed from the cycle-match data and correctly predicts that productivity would be about equal across treatment groups in the party-match data.

At the same time, though, significant care must be exercised in interpreting the results suggesting minimal partisan influence on individual legislative outcomes. Given how dramatically this contrasts with the existing literature, as well as with general intuition regarding modern Congress, it is worth clarifying how this result should be interpreted in light of the methods used to estimate it, and what it potentially adds to our understanding of Senate policy-making.

Specifically, the estimates here should not be read as suggesting that the majority party has no power over agenda-setting or individual legislative activity. Instead, these results advance two weaker claims. First, partisan influence seems likely to be conditional on the stage of the legislative process. It evidently is not a strong influence, controlling for legislative content, in determining which amendments pass or receive recorded votes. This is not an entirely novel observation, yet at the same time, numerous studies discuss the extent to which partisanship in the Senate is more and more resembling that which can be seen in the House. The result here then can be seen as a counterweight to these studies and one which also follows previous scholars in counseling that, without measuring legislative outcomes and behavior precisely, party effects will often be misattributed or conflated.

Conclusion

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

To pursue this goal, I first distinguished theoretically between effective and accommodative legislative behavior and argued that without making strong assumptions, existing empirical results are unable to differentiate between the two. I argued that addressing this shortcoming required a method of accounting for legislative content when measuring legislative effectiveness. For this purpose I developed two matched data subsets of Senate amendments that hold constant the relationship between legislative content on the one hand and party-affiliation or electoral proximity on the other. In these data, covariates were distributed as we would expect them to be in a randomized experiment, implying that causal inferences drawn from them were at least internally unbiased.

In terms of substantive results, I first showed that existing approaches to calculating effectiveness would suggest that majority-party membership had a positive influence on effectiveness and running for re-election had a negative one. However, when the matched data were considered, significantly different results emerged: First, there was no evidence of a majority-party effect. Second, electoral vulnerability was found to positively influence legislative productivity for minority-party Senators, but almost certainly not for majority-party Senators. Additionally, this electoral-cycle effect was found to be largest for those minority-party Senators in the most electorally competitive states. These results were found to be robust across non-parametric and logit regression models, and to different definitions of legislative success.

In comparing the matched data to the full data set, I found no evidence across a range of measures to conclude that censoring was obscuring the true effect of the majority-party or otherwise skewing the results. Partially as a result, I then argued that the empirical findings hold broader implications for competing theories of partisan agenda control and Congressional institutions in the Senate. Specifically, they tend to support models that emphasize the institutional influence of incumbent's shared desire to retain office, and to run against models that suggest the majority part enjoys a monopoly on procedural resources on the Senate floor.

Future research can delve deeper into the null majority-party effect. This paper's analysis considered only amendments, but its method can be applied to earlier stages of the policy-making process as well. There are good reasons to speculate that majority-party membership may matter more at these earlier stages. 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 results of this paper suggest the benefits of paying close attention to measuring legislative outcomes with theoretical precision. 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.[23] Only recently have tools become available to facilitate metrics of representation that go beyond broad counts of outcomes or behavior, but now that these tools are available, empirical scholars of Congress should put them to use aggressively.

[23]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 deplores the practice of "counting things" (Binder 2015)

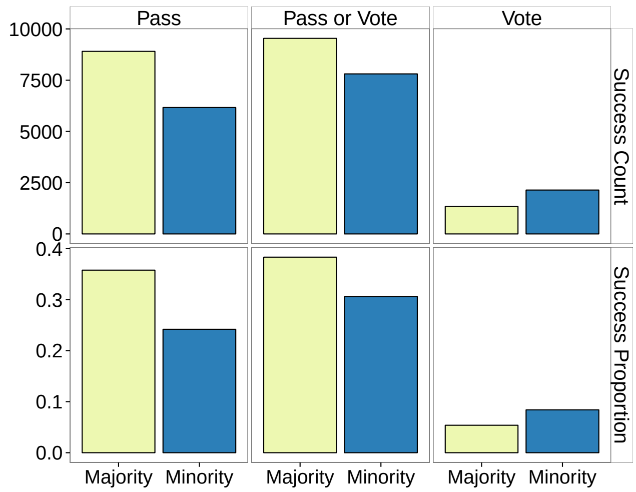
Appendix

Original Data Summary Tables

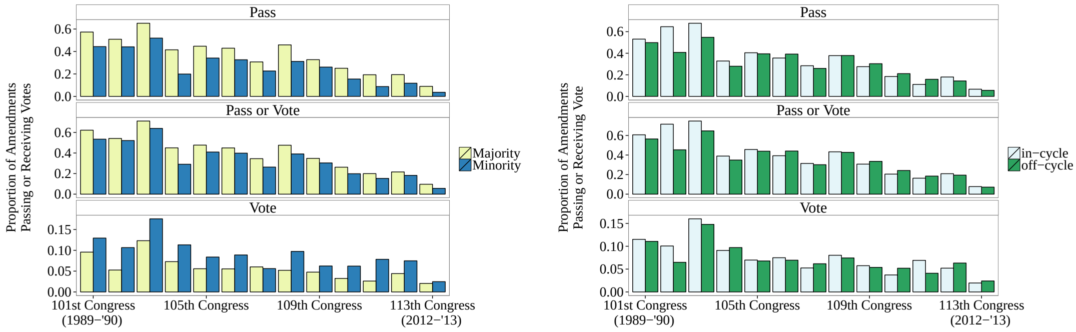
	Success Count		Success Proportion	
	Majority	Minority	Majority	Minority
Vote	1340	2141	0.054	0.084
Pass or Vote	9539	7807	0.383	0.306
Pass	8905	6166	0.358	0.242

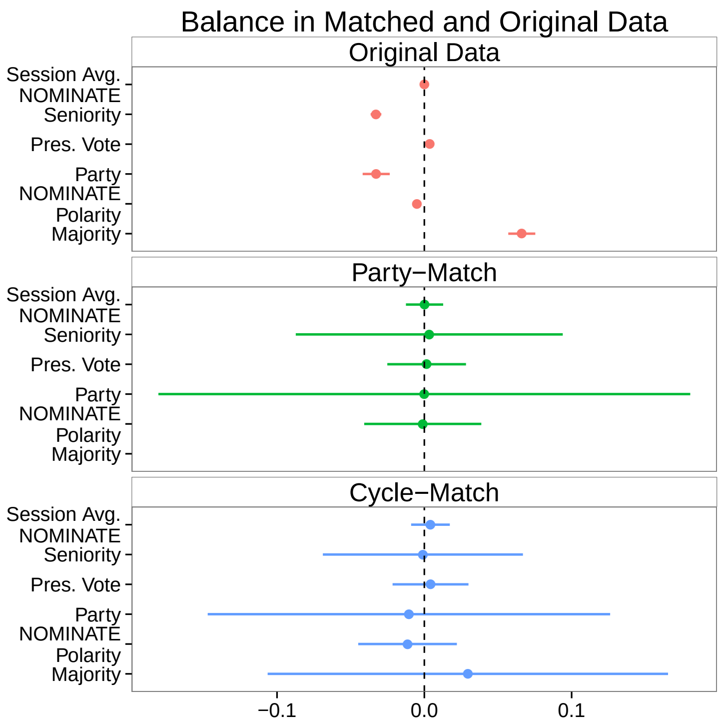
	Success Count				Success Proportion			
	Majority		Minority		Majority		Minority	
	In−cycle	Out−of−cycle	In−cycle	Out−of−cycle	In−cycle	Out−of−cycle	In−cycle	Out−of−cycle
Vote	487	853	728	1413	0.063	0.05	0.077	0.088
Pass or Vote	3236	6303	2838	4969	0.417	0.368	0.299	0.31
Pass	3028	5877	2284	3882	0.39	0.343	0.241	0.242

Success in the Original Data - All Dependent Variables



Proportional Success By Treatment Groups in Original Data - All DV's





Introduction Order and Success - Cycle-Match Data

	Dependent variable:						
	Pass or Vote		Pass			Vote	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
In-Cycle	0.399	0.222	0.241	0.235	0.213	0.665*	0.475
	(0.289)	(0.416)	(0.307)	(0.306)	(0.444)	(0.377)	(0.506)
Majority	0.273	0.253	0.572*	0.566*	0.569*	−0.322	−0.346
	(0.291)	(0.293)	(0.307)	(0.306)	(0.309)	(0.384)	(0.387)
Previous Intro'd	−0.073	−0.244	−0.067		−0.092	−0.368	−0.621
	(0.290)	(0.410)	(0.308)		(0.433)	(0.375)	(0.595)
In-Cycle:Previous Intro'd		0.343			0.052		0.429
		(0.582)			(0.618)		(0.773)

*p<0.1; **p<0.05; ***p<0.01

Note: Estimates are for logit regression models. For all three binary dependent variable definitions, neither the "Previous Introduction" variable nor its interaction with the amendment sponsor's party ever approaches statistical significance in the party-match data. In the cycle-match data, significant coefficient for the "Pass" dependent variable is likely due to the relationship between being in the majority party and proximity to the chamber's median ideology. The full regression results show that it is ideology, not majority-party status that is the best predictor of success for this specification. For the other dependent variable definitions, party-affiliation is insignificant. More relevant to the immediate question regarding introduction order, the coefficient is essentially unchanged whether or not the initial introduction variable is included and this variable is never significant. Thus the evidence suggests that amendments are just as likely to succeed the first time and fail the second as vice versa. This is equally true for amendments introduced by the majority and minority parties.

Introduction Order and Success - Party-Match Data

Dependent Variable: Pass

	Not Passed	Pass
Initial Intro	38	22
Not Initial Intro	40	22
	Odds Ratio	1.052
	Fisher test p-value	1

Dependent Variable: Vote

	No Vote	Vote
Initial Intro	49	11
Not Initial Intro	53	9
	Odds Ratio	1.319
	Fisher test p-value	0.63

Dependent Variable: Pass or Vote

	No Vote and Not Passed	Voted on or Passed
Initial Intro	36	24
Not Initial Intro	37	25
	Odds Ratio	0.987
	Fisher test p-value	1

Introduction Order and Success - Cycle-Match Data

Dependent Variable: Pass

	Not Passed	Pass
Initial Intro	71	30
Not Initial Intro	76	32
	Odds Ratio	1.004
	Fisher Test p-value	1

Dependent Variable: Vote

	No Vote	Vote
Initial Intro	81	20
Not Initial Intro	92	16
	Odds Ratio	1.417
	Fisher Test p-value	0.364

Dependent Variable: Pass or Vote

	No Vote and Not Passed	Voted on or Passed
Initial Intro	63	38
Not Initial Intro	68	40
	Odds Ratio	1.025
	Fisher Test p-value	1

All Regression Results

Model Code	Party Subset	Matching Type	Outcome Variable	Variable	Coefficients	p.value
1	Both Parties	Party-Match	Pass or Vote/No Pass or Vote	Seniority (log)	0.44	0.02
1	Both Parties	Party-Match	Pass/Not Pass	Seniority (log)	0.50	0.01
2	Both Parties	Party-Match	Vote/No Vote	In-cycle	0.77	0.14
2	Both Parties	Party-Match	Vote/No Vote	Pres. Vote	-6.86	0.04
2	Both Parties	Party-Match	Vote/No Vote	NOMINATE Polarity	-4.12	0.15
3	Both Parties	Party-Match	Pass or Vote/No Pass or Vote	In-cycle	0.14	0.80
3	Both Parties	Party-Match	Pass or Vote/No Pass or Vote	Majority	0.13	0.78
3	Both Parties	Party-Match	Pass or Vote/No Pass or Vote	In-cycle X Majority	0.28	0.72
3	Both Parties	Party-Match	Pass/Not Pass	In-cycle	0.36	0.52
3	Both Parties	Party-Match	Pass/Not Pass	Majority	0.24	0.60
3	Both Parties	Party-Match	Pass/Not Pass	In-cycle X Majority	-0.29	0.72
3	Both Parties	Party-Match	Vote/No Vote	In-cycle	0.59	0.42
3	Both Parties	Party-Match	Vote/No Vote	Majority	0.16	0.81
3	Both Parties	Party-Match	Vote/No Vote	In-cycle X Majority	0.28	0.78
4	Both Parties	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle	-0.14	0.70
4	Both Parties	Cycle-Match	Pass or Vote/Not Passed and No Vote	Tough Race	-0.76	0.08
4	Both Parties	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle and Tough Race	1.39	0.02
5	Both Parties	Cycle-Match	Pass/Not Pass	NOMINATE Score (abs. Value)	-1.70	0.05
6	Both Parties	Cycle-Match	Vote/No Vote	In-cycle	0.59	0.27
6	Both Parties	Cycle-Match	Vote/No Vote	Majority	-0.06	0.93
6	Both Parties	Cycle-Match	Vote/No Vote	Tough Race	-2.44	0.05
6	Both Parties	Cycle-Match	Vote/No Vote	In-cycle and Tough Race	2.53	0.04
6	Both Parties	Cycle-Match	Vote/No Vote	In-cycle X Majority	-2.32	0.05
6	Both Parties	Cycle-Match	Vote/No Vote	Majority X Vuln. Seat	2.09	0.08
7	Majority Party	Cycle-Match	Pass or Vote/Not Passed and No Vote	Republican Party	0.97	0.03
7	Majority Party	Cycle-Match	Pass/Not Pass	Republican Party	0.95	0.03
8	Majority Party	Cycle-Match	Vote/No Vote	Seniority	-0.05	0.13
8	Majority Party	Cycle-Match	Vote/No Vote	NOMINATE Polarity	-6.42	0.11
8	Majority Party	Cycle-Match	Vote/No Vote	Republican Party	1.54	0.02
4	Minority Party	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle	-0.11	0.81
4	Minority Party	Cycle-Match	Pass or Vote/Not Passed and No Vote	Tough Race	-1.67	0.04
4	Minority Party	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle and Tough Race	3.15	0.00
9	Minority Party	Cycle-Match	Pass/Not Pass	In-cycle	-0.66	0.25
9	Minority Party	Cycle-Match	Pass/Not Pass	Tough Race	-1.47	0.08
9	Minority Party	Cycle-Match	Pass/Not Pass	NOMINATE Score (abs. Value)	-2.13	0.11
9	Minority Party	Cycle-Match	Pass/Not Pass	In-cycle and Tough Race	3.19	0.00
4	Minority Party	Cycle-Match	Vote/No Vote	In-cycle	0.48	0.37
4	Minority Party	Cycle-Match	Vote/No Vote	Tough Race	-10.53	0.91
4	Minority Party	Cycle-Match	Vote/No Vote	In-cycle and Tough Race	10.78	0.91
10	Both Parties	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle	0.73	0.06
10	Both Parties	Cycle-Match	Pass or Vote/Not Passed and No Vote	Majority	0.64	0.12
10	Both Parties	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle X Majority	-0.75	0.20
10	Both Parties	Cycle-Match	Pass/Not Pass	In-cycle	0.51	0.24
10	Both Parties	Cycle-Match	Pass/Not Pass	Majority	0.84	0.05
10	Both Parties	Cycle-Match	Pass/Not Pass	In-cycle X Majority	-0.55	0.37
10	Both Parties	Cycle-Match	Vote/No Vote	In-cycle	1.06	0.03
10	Both Parties	Cycle-Match	Vote/No Vote	Majority	0.23	0.68
10	Both Parties	Cycle-Match	Vote/No Vote	In-cycle X Majority	-1.06	0.17
11	Majority Party	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle	-0.01	0.97
11	Majority Party	Cycle-Match	Pass/Not Pass	In-cycle	-0.04	0.92
11	Majority Party	Cycle-Match	Vote/No Vote	In-cycle	-0.00	1.00
11	Minority Party	Cycle-Match	Pass or Vote/Not Passed and No Vote	In-cycle	0.73	0.06
11	Minority Party	Cycle-Match	Pass/Not Pass	In-cycle	0.51	0.24
11	Minority Party	Cycle-Match	Vote/No Vote	In-cycle	1.06	0.03
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	In-cycle	-0.11	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	Pres. Vote	12.24	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	Majority	0.23	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	NOMINATE Score (abs. Value)	-0.93	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	NOMINATE Polarity	-0.59	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	Republican Party	0.13	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	Pres. Vote (sqr)	-12.79	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	Seniority (log)	0.12	0.00
12	Both Parties	Original	Pass or Vote/Not Passed and No Vote	In-cycle X Majority	0.23	0.00
12	Both Parties	Original	Pass/Not Pass	In-cycle	-0.07	0.03
12	Both Parties	Original	Pass/Not Pass	Pres. Vote	10.39	0.00
12	Both Parties	Original	Pass/Not Pass	Majority	0.46	0.00
12	Both Parties	Original	Pass/Not Pass	NOMINATE Score (abs. Value)	-1.43	0.00
12	Both Parties	Original	Pass/Not Pass	NOMINATE Polarity	-1.04	0.00
12	Both Parties	Original	Pass/Not Pass	Republican Party	0.19	0.00
12	Both Parties	Original	Pass/Not Pass	Pres. Vote (sqr)	-11.09	0.00
12	Both Parties	Original	Pass/Not Pass	Seniority (log)	0.13	0.00
12	Both Parties	Original	Pass/Not Pass	In-cycle X Majority	0.17	0.00
12	Both Parties	Original	Vote/No Vote	In-cycle	-0.14	0.00
12	Both Parties	Original	Vote/No Vote	Pres. Vote	16.22	0.00
12	Both Parties	Original	Vote/No Vote	Majority	-0.60	0.00
12	Both Parties	Original	Vote/No Vote	NOMINATE Score (abs. Value)	0.89	0.00
12	Both Parties	Original	Vote/No Vote	NOMINATE Polarity	0.62	0.00
12	Both Parties	Original	Vote/No Vote	Republican Party	-0.09	0.04
12	Both Parties	Original	Vote/No Vote	Pres. Vote (sqr)	-15.82	0.00
12	Both Parties	Original	Vote/No Vote	Seniority (log)	0.05	0.01
12	Both Parties	Original	Vote/No Vote	In-cycle X Majority	0.40	0.00
13	Both Parties	Original	Pass or Vote/Not Passed and No Vote	In-cycle	-0.05	0.08
13	Both Parties	Original	Pass or Vote/Not Passed and No Vote	Majority	0.26	0.00
13	Both Parties	Original	Pass or Vote/Not Passed and No Vote	In-cycle X Majority	0.25	0.00
13	Both Parties	Original	Pass/Not Pass	In-cycle	-0.01	0.82
13	Both Parties	Original	Pass/Not Pass	Majority	0.49	0.00
13	Both Parties	Original	Pass/Not Pass	In-cycle X Majority	0.21	0.00
13	Both Parties	Original	Vote/No Vote	In-cycle	-0.15	0.00
13	Both Parties	Original	Vote/No Vote	Majority	-0.61	0.00
13	Both Parties	Original	Vote/No Vote	In-cycle X Majority	0.39	0.00

Model Key for Regression Table

Full.Model	Model.Code
Seniority (log)	1
In-cycle, Pres. Vote, NOMINATE polarity	2
majority, In-cycle, In-cycle and Majority	3
In-cycle, Tough Race, In-cycle and Tough Race	4
NOMINATE Score (abs. value)	5
In-cycle, majority, Tough Race, In-cycle and Tough Race, In-cycle and Majority, Majority and Tough Race	6
Republican	7
seniority, NOMINATE polarity, Republican	8
In-cycle, Tough Race, NOMINATE Score (abs. value), In-cycle and Tough Race	9
In-cycle, majority, In-cycle and Majority	10
In-cycle	11
Raw Party Treatment Best	12
cycle, majority, cycle x majority	13

Table 5: Electoral-Cycle Non-Parametric Results - All Outcomes

Seat Subset	Party	Out-of-Cycle $\frac{\text{Successes}}{\text{Total}}$	In-Cycle $\frac{\text{Successes}}{\text{Total}}$	Electoral-Cycle Effect (p-value)
Dependent Variable: Pass + Vote				
All Seats	Both	36/109	42/100	0.09
				0.2
	Majority	20/49	17/42	0
				1
	Minority	8/22	9/23	0.03
				1
Competitive Seats	Both	10/43	20/39	0.28
				0.01**
	Majority	8/22	9/23	0.03
				1
	Minority	2/21	11/16	0.59**
				0
Dependent Variable: Vote				
All Seats	Both	14/109	22/100	0.09*
				0.1
	Majority	7/49	6/42	0
				1
	Minority	3/22	5/23	0.08
				0.7
Competitive Seats	Both	3/43	10/39	0.19**
				0.03
	Majority	3/22	5/23	0.08
				0.7
	Minority	0/21	5/16	0.31**
				0.01
Dependent Variable: Pass				
All Seats	Both	30/109	32/100	0.04
				0.54
	Majority	18/49	15/42	-0.01
				1
	Minority	7/22	7/23	-0.01
				1
Competitive Seats	Both	9/43	17/39	0.23**
				0.03
	Majority	7/22	7/23	-0.01
				1
	Minority	2/21	10/16	0.53**
				0

Note: This table has the same set up as in the text but now includes all dependent variable definitions.

Text Matching Background

Below I summarize the algorithm used to identify bills with shared policy-content. These steps can also be adopted to other research questions involving identical legislative proposals and customized by researchers to allow for larger samples with higher tolerance for textual deviation or more exact matches with fewer documents retained as matches. This approach can significantly simplify the process of locating the origin and development of legislative ideas and the computational challenge of identifying similar legislative proposals. More information about the tools and approaches utilized here can be found in the Computer Science literature on text reuse(Schleimer, Wilkerson and Aiken (2003) Heintze et al. (1996)).

Algorithmically, the process of identifying legislative identical amendments works as follows:

1. Pre-filter amendments: In the current case, we are interested in measuring the causal effect of the electoral-cycle and the majority party on the probability of receiving legislative attention. This effect is absent when procedural rules prevent agenda-setters from exerting control over the amendment schedule as during debate on the Budget Resolution or certain tax and spending reconciliation" bills. Thus amendments to these bills should be removed.

2. Create a document "feature-print" for each amendment by retrieving from the full amendment text the following features: Public Law (e.g. P.L. 109-1234 or Public Law 109-1234), United States Code (e.g. 12 U.S.C. 1234), and other statutory references. Also retrieve references to existing institutions and programs (which can be identified because they are title-cased e.g. "National Institutes of Health", "Manhattan Project for Energy Independence", "Gun Show Loophole Closing Act"). Treat each document (temporarily) as if it were only made up of these features.

3. Compare each document feature-print using any standard document similarity method (I use cosine similarity based on an inverse term document frequency

matrix) and retain as potential matches only those documents that are essentially exact matches (cosine score>0.999) according to their feature-print.

4. For those documents identified as potential matches in (2), compare the fingerprints created from the operative amendment text and retain as exact matches those amendments that share a proportion of fingerprints above a chosen threshhold.

   With respect to step 3, there are numerous fingerprinting approaches available - most of them are aimed at addressing the trade-off between computational burden and comprehensiveness. Given the sizable reduction in the number of comparisons required achieved via steps one and two, the computational requirements in step 3 are fairly minimal and thus a fingerprinting approach favoring comprehensive comparisons between documents of interest can be efficiently employed. Once fingerprints for full individual documents are generated they can be easily compared to the match candidates generated in steps 1 and 2 in order to identify a final list of equivalent amendments.

The most significant shortcoming to this approach is that it will miss shorter matches (a challenge encountered throughout the document-matching literature) since amendments without features searched for in steps 1 and 2 are filtered out. These are amendments that make short changes to the operative text of the bill itself without making reference to external statute or government institutions. For example, an amendment to increase the amount of funding provided to the National Institutes of Health by $1 million in 2007 would not be classified as equivalent to an identical amendment from 2006 if it didn't reference NIH specifically ("On page 17, strike $1 million and replace with $2 million. Similarly an amendment that would only insert the word "not" to negate a bill provision also would not be considered as a potential match. This might be mitigated in the future by incorporating dollar figures into the document 'feature-print' though it would be a challenge to identify whether the underlying account being given or stripped of x dollars is the same across amendments. However, the absence of these types of amendments is of fairly limited concern to the current analysis. First, only a small proportion of the total amendment database meets this criteria ($\approx$ 10%). Second, amendments that are only defined by the changes they make to a specific bill are unlikely to be proposed in substantively identical form more than once because the Senate rarely considers identical or nearly identical bills repeatedly within the time frame under consideration.

Illustration of amendment similarities

Below are several examples of selected amendment matches. While the paper analyzes only exactly matching amendments,

All matched amendments included in either of the matched data analyzed in the main text, as well as code to reproduce matching process are available at `scholar.harvard.edu/bengruebaum/data`.

Examples of matching amendments "redline versions"

SA. 14 (110th) vs. SA. 32 (111th)

SEC. __. PROTECTION OF WORKERS' POLITICAL RIGHTS.

Title III of the Labor Management Relations Act, 1947 (29 U.S.C. 185 et seq.) is amended by adding at the end the following:

``SEC. 304. PROTECTION OF WORKER'S POLITICAL RIGHTS.

``(a) Prohibition.--Except with the separate, prior, written, voluntary authorization of an individual, it shall be unlawful for any labor organization to collect from or assess its members or nonmembers any dues, initiation fee, or other payment if any part of such dues, fee, or payment will be used to lobby members of Congress or Congressional staff for the purpose of influencing legislation.

``(b) Authorization.--An authorization described in subsection (a) shall remain in effect until revoked and may be revoked at any time.''.

SA 285 (101st) vs. SA 810 (102nd)

~~At the appropriate place in the bill insert the following~~
SEC. 611~~—~~. POLICY TOWARD THE FUTURE OF TAIWAN.
(a) Findings: ~~.—~~The Congress finds that--
(1) although peace has prevailed in the Taiwan Strait for the past decade, on June 4, 1989, the Government of the People's Republic of China showed its willingness to use force against the Chinese people who were demonstrating peacefully for democracy; and
(2) in the Taiwan Relations Act, the United States made clear that its decision to enter into diplomatic relations with the People's Republic of China rested upon the expectation that the future of Taiwan would be determined by peaceful means.
(b) Sense of Congress.--It is the sense of the Congress that--
(1) the future of Taiwan should be settled peacefully, free from coercion, and in a manner acceptable to the people of~~on~~ Taiwan; and
(2) good relations between the United States and the People's Republic of China depend upon the ~~Chinese authorities'~~ willingness of the Chinese authorities to refrain from the use or the threat of force in resolving Taiwan's future.
On page 4, after the item relating to section 610, add the following new item:
Sec. 611. Policy toward the future of Taiwan.

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~~[Page: S8282]~~

SA 5393 (110th) vs SA 1492 (111)

SEC. 2822~~2832~~. LAND CONVEYANCE, F.E. WARREN AIR FORCE BASE, CHEYENNE, WYOMING.

(a) Conveyance Authorized.--The Secretary of the Air Force may convey to the County of Laramie, Wyoming (in this section referred to as the ``County'') all right, title, and interest of the United States in and to a parcel of real property, including any improvements thereon and appurtenant easements thereto, consisting of approximately 73 acres along the southeastern boundary of F.E. Warren Air Force Base, Cheyenne, Wyoming, for the purpose of removing the property from the boundaries of the installation and permitting the County to preserve the entire property for healthcare facilities.

(b) Consideration.--

(1) In general.--As consideration for the conveyance under subsection (a), the County shall provide the United States consideration, whether by cash payment, in-kind consideration as described under paragraph (2), or a combination thereof, in an amount that is not less than the fair market value of the conveyed real property, as determined by the Secretary.

(2) In-kind consideration.--In-kind consideration provided by the County under paragraph (1) shall~~may~~ include the acquisition, construction, provision, improvement, maintenance, repair, or restoration (including environmental restoration), or combination thereof, of any facilities or infrastructure relating to the security of F.E. Warren Air Force Base, that the Secretary considers acceptable.

(3) Relation to other laws.--Sections 2662 and 2802 of title 10, United States Code, shall not apply to any new facilities or infrastructure received by the United States as in-kind consideration under paragraph (2).

(4) Notice to congress.--The Secretary shall provide written notification to the congressional defense committees of the types and value of consideration provided the United States under paragraph (1).

(5) Treatment of cash consideration received.--Any cash payment received by the United States under paragraph (1) shall be deposited in the special account in the Treasury established under subsection (b) of section 572 of title 40, United States Code, and shall be available in accordance with paragraph (5)(B)(ii) of such subsection.

(c) Reversionary Interest.--

(1) In general.--If the Secretary determines at any time that the County is not using the property conveyed under subsection (a) in accordance with the purpose of the

~~the~~ conveyance specified in such subsection, all right, title,
and interest in and to the property, including any improvements thereon, shall revert, at the option of the Secretary, to the United States, and the United States shall have the right of immediate entry onto the property. Any determination of the Secretary under this subsection shall be made on the record after an opportunity for a hearing.

(2) Release of reversionary interest.--The Secretary shall release, without consideration, the reversionary interest retained by the United States under paragraph (1) if--

(A) F.E. Warren Air Force Base, Cheyenne Wyoming, is no longer being used for Department of Defense activities; or

(B) the Secretary determines that the reversionary interest is otherwise unnecessary to protect the interests of the United States.

(d) Payment of Costs of Conveyance.--

(1) Payment required.--The Secretary shall require the County to cover costs to be incurred by the Secretary, or to reimburse the Secretary for costs incurred by the Secretary, to carry out the conveyance under subsection (a) and implement the receipt of in-kind consideration under paragraph (b), including survey costs, appraisal costs, costs related to environmental documentation, and other administrative costs related to the conveyance and receipt of in-kind consideration. If amounts are received from the County in advance of the Secretary incurring the actual costs, and the amount received exceeds the costs actually incurred by the Secretary under this section, the Secretary shall refund the excess amount to the County.

(2) Treatment of amounts received.--Amounts received as reimbursements under paragraph (1) shall be credited to the fund or account that was used to cover the costs incurred by the Secretary in carrying out the conveyance and implementing the receipt of in-kind consideration. Amounts so credited shall be merged with amounts in such fund or account and shall be available for the same purposes, and subject to the same conditions and limitations, as amounts in such fund or account.

(e) Description of Real Property.--The exact acreage and legal description of the real property to be conveyed under subsection (a) shall be determined by a survey satisfactory to the Secretary.

(f) Additional Terms and Conditions.--The Secretary may require such additional terms and conditions in connection with the conveyance under subsection (a) as the Secretary considers appropriate to protect the interests of the United States.

SA 5265 (110th) vs. SA 1477 (111th)

SEC. 642.~~___.~~ MODIFICATION OF OFFSET AGAINST COMBAT-RELATED SPECIAL COMPENSATION FOR CHAPTER 61 DISABILITY RETIREES.

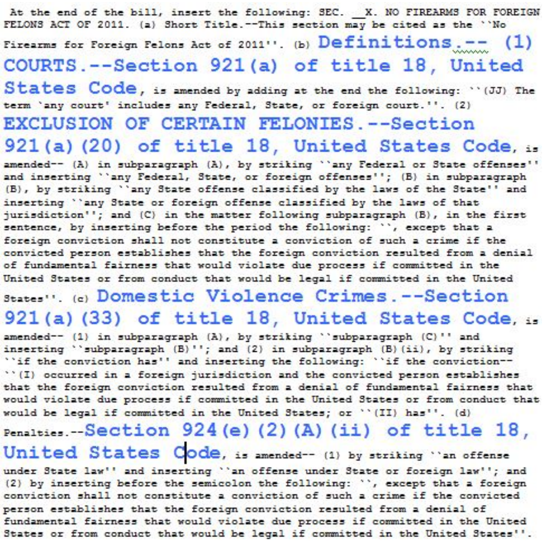
Section 1413a(b)(3) of title 10, United States Code, is amended--

(1) in subparagraph (A), by striking ``shall be reduced'' and all that follows through ``exceeds'' and inserting ``may not, when combined with the amount of retirement pay payable to the retiree after any reduction under sections 5304 and 5305 of title 38, cause the total of such combination to exceed''; and

(2) in subparagraph (B), by striking ``shall be reduced'' and all that follows through ``exceeds'' and inserting ``may not, when combined with the amount of retirement pay payable to the retiree after any reduction under sections 5304 and 5305 of title 38, cause the total of such combination to exceed''.

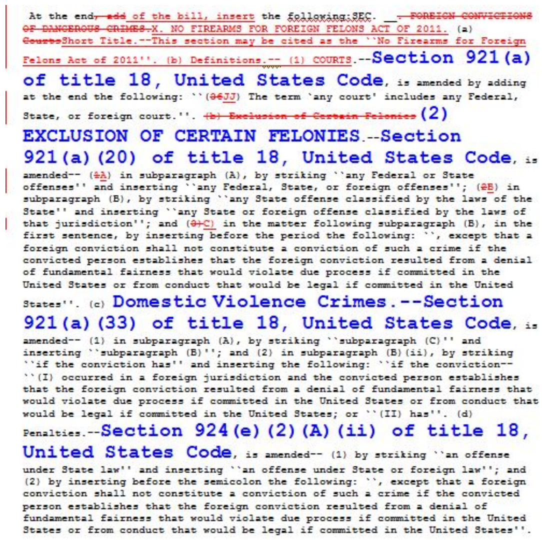
"Feature Print Matching"

Match 1



At the end of the bill, insert the following: SEC. __X. NO FIREARMS FOR FOREIGN FELONS ACT OF 2011. (a) Short Title.--This section may be cited as the ``No Firearms for Foreign Felons Act of 2011''. (b) Definitions.-- (1) COURTS.--Section 921(a) of title 18, United States Code, is amended by adding at the end the following: ``(JJ) The term `any court' includes any Federal, State, or foreign court.''. (2) EXCLUSION OF CERTAIN FELONIES.--Section 921(a)(20) of title 18, United States Code, is amended-- (A) in subparagraph (A), by striking ``any Federal or State offenses'' and inserting ``any Federal, State, or foreign offenses''; (B) in subparagraph (B), by striking ``any State offense classified by the laws of the State'' and inserting ``any State or foreign offense classified by the laws of that jurisdiction''; and (C) in the matter following subparagraph (B), in the first sentence, by inserting before the period the following: ``, except that a foreign conviction shall not constitute a conviction of such a crime if the convicted person establishes that the foreign conviction resulted from a denial of fundamental fairness that would violate due process if committed in the United States or from conduct that would be legal if committed in the United States''. (c) Domestic Violence Crimes.--Section 921(a)(33) of title 18, United States Code, is amended-- (1) in subparagraph (A), by striking ``subparagraph (C)'' and inserting ``subparagraph (B)''; and (2) in subparagraph (B)(ii), by striking ``if the conviction has'' and inserting the following: ``if the conviction-- ``(I) occurred in a foreign jurisdiction and the convicted person establishes that the foreign conviction resulted from a denial of fundamental fairness that would violate due process if committed in the United States or from conduct that would be legal if committed in the United States; or ``(II) has''. (d) Penalties.--Section 924(e)(2)(A)(ii) of title 18, United States Code, is amended-- (1) by striking ``an offense under State law'' and inserting ``an offense under State or foreign law''; and (2) by inserting before the semicolon the following: ``, except that a foreign conviction shall not constitute a conviction of such a crime if the convicted person establishes that the foreign conviction resulted from a denial of fundamental fairness that would violate due process if committed in the United States or from conduct that would be legal if committed in the United States''.

Match 2



At the end~~, add~~ of the bill, insert the following:SEC. __~~. FOREIGN CONVICTIONS OF DANGEROUS CRIMES.~~X. NO FIREARMS FOR FOREIGN FELONS ACT OF 2011. (a) ~~Courts~~Short Title.--This section may be cited as the ``No Firearms for Foreign Felons Act of 2011''. (b) Definitions.-- (1) COURTS.--Section 921(a) of title 18, United States Code, is amended by adding at the end the following: ``(~~36~~JJ) The term `any court' includes any Federal, State, or foreign court.''. ~~(b) Exclusion of Certain Felonies~~(2) EXCLUSION OF CERTAIN FELONIES.--Section 921(a)(20) of title 18, United States Code, is amended-- (~~1~~A) in subparagraph (A), by striking ``any Federal or State offenses'' and inserting ``any Federal, State, or foreign offenses''; (~~2~~B) in subparagraph (B), by striking ``any State offense classified by the laws of the State'' and inserting ``any State or foreign offense classified by the laws of that jurisdiction''; and (~~3)~~C) in the matter following subparagraph (B), in the first sentence, by inserting before the period the following: ``, except that a foreign conviction shall not constitute a conviction of such a crime if the convicted person establishes that the foreign conviction resulted from a denial of fundamental fairness that would violate due process if committed in the United States or from conduct that would be legal if committed in the United States''. (c) Domestic Violence Crimes.--Section 921(a)(33) of title 18, United States Code, is amended-- (1) in subparagraph (A), by striking ``subparagraph (C)'' and inserting ``subparagraph (B)''; and (2) in subparagraph (B)(ii), by striking ``if the conviction has'' and inserting the following: ``if the conviction-- ``(I) occurred in a foreign jurisdiction and the convicted person establishes that the foreign conviction resulted from a denial of fundamental fairness that would violate due process if committed in the United States or from conduct that would be legal if committed in the United States; or ``(II) has''. (d) Penalties.--Section 924(e)(2)(A)(ii) of title 18, United States Code, is amended-- (1) by striking ``an offense under State law'' and inserting ``an offense under State or foreign law''; and (2) by inserting before the semicolon the following: ``, except that a foreign conviction shall not constitute a conviction of such a crime if the convicted person establishes that the foreign conviction resulted from a denial of fundamental fairness that would violate due process if committed in the United States or from conduct that would be legal if committed in the United States''.

Note: Amendments are first filtered according to policy "feature-prints". Such as those in blue. Potential matches must have essentially identical feature-prints to be an exact match.

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