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Inducing Polarization? The Effect of Congressional Procedure and Partisan Lawmaking on Ideal Point Estimation

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ABSTRACT

How do procedural innovations, such as committee bypass, affect our roll-call-based measurements of individual member ideology — and therefore our measurements of polarization? Congressional polarization, measured using member ideal points derived from scaling roll call data, has been steadily increasing over the last half-century. However, changes in legislative procedure that affect the construction of the roll call record have been concurrent with this apparent increase in polarization. In this paper, we explore the effect of one unorthodox procedure — the use of committee bypass in the House — on the measurement of member ideology and chamber polarization. We utilize matching to generate balanced subsets containing similar bills that bypassed committee to reach the floor and bills that went through "regular order." With these matched subsets, we estimate the effect of committee bypass on roll call votes and the resulting ideal points and polarization measures. We find that committee bypass has the effect of dampening, rather than exacerbating polarization.

Keywords: Ideal point estimation; legislative procedure; polarization; unorthodox lawmaking; legislative committees

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Introduction

There is a folk wisdom about Congress that has developed over the past few years that argues the decline in regular order has exacerbated partisanship and polarization in Congress. Implicit in this argument is that the modern set of procedural tools in Congress makes these problems worse and that returning to a more decentralized set of procedures–especially increasing the role of committees–would reduce partisan rancour and polarization. Of course, it is difficult to disentangle the conditions that have lead to this maligned procedural centralization from the outcomes and behaviors it is alleged to be driving. If the components of polarization — ideological agreement within parties and ideological disagreement across parties — are also pre-requisites for centralized procedural power, it is difficult to imagine how forced procedural decentralization would somehow alleviate the underlying polarization. The counter to this folk wisdom, then, is articulated by Rudolph Penner, former Congressional Budget Office director, who is quoted by Curry and Lee (2020a, p. 627) stating, "the problem is not the process; the problem is the problem."

There is a large body of literature focused on procedural changes to the legislative process since the Textbook Congress era of the 1950s through the early 1970s (e.g., Bach and Smith, 1989; Bendix, 2016; Howard and Owens, 2020; Sinclair, 2016). Much of this work posits that these procedural changes are driven by majority party leadership efforts to achieve non-median policy outcomes (Aldrich and Rohde, 2000a; Aldrich and Rohde, 2000b; Cox and McCubbins, 2005; Monroe and Robinson, 2008, but see Curry and Lee, 2020b). Further, this literature holds that the centralization of the legislative process, and the attendant violations of "regular order," have been made possible by increased intraparty homogeneity and ideological distance between the two parties (Aldrich and Rohde, 2001; Rohde, 1991). However, a related branch of the literature demonstrates that roll-call-based ideal point estimates and measures of party unity are sensitive to procedural context (Crespin *et al.*, 2013; Roberts, 2007; Roberts *et al.*, 2016). Therefore, our ability to measure the intra- and interparty ideological conditions that facilitate procedural changes such as committee bypass is potentially affected by the very use of these procedures. Stated differently, our measures of polarization may be biased by the strategic use of these procedures, rather than being a product of sincere spatial preferences.

Our paper seeks to disentangle this endogeneity by focusing on uses of committee bypass in the House of Representatives. Building on work done by Curry (2015) and Curry and Lee (2020a), we argue that bypassing committee creates an informational asymmetry between majority party leaders and rank-and-file legislators with regard to the contents of legislation. This informationally-constrained environment may affect legislators' vote choices, as members must rely on their party leaders for cues about how to vote. Because

party leaders may have different goals and motivations than rank-and-file members, the delegation of information gathering and communication tasks to these leaders is not trivial in terms of its potential impact on roll call votes.

While we cannot directly observe the counterfactual — how members would have voted on bypass bills if they had been considered and reported by a committee — we can get leverage on this question by matching on bill-level characteristics (including bill text). Using a variety of matching methods, we create balanced subsets of bills that bypassed committee and those that reached the floor after being reported by one or more committees. Because we assume that party leaders are constrained in their ability to engage in committee bypass, we think about our comparison group as the set of bills that would have bypassed committee in the absence of limits on the power delegated to party leaders.

We estimate ideal points for members on multiple subsets of bills, and also fit regressions on our matched datasets to estimate the effect of committee bypass on bill-level roll call outcomes. We conclude by comparing Congress-level measures of polarization between our matched sets and show how these procedural decisions have down-stream effects on not only roll calls themselves, but on how we measure the underlying preferences. The evidence we present in this article demonstrates that bypassing committee dampens, rather than exacerbates, polarization in the House. We argue, therefore, that reformers' interest in pushing Congress towards the procedures of the "Textbook" era would be unlikely to reduce polarization and partisanship.

Theory

Estimation and Bias in Ideal Point Modeling

Scaling methods that generate ideological scores for legislators based on roll call votes depend on an underlying spatial model of decision-making. It is assumed that each legislator has an ideal point in some n-dimensional policy space that represents their most-preferred outcome in a voting decision. The potential outcomes of a roll call vote can be depicted as two distinct points in the policy space — one associated with the policy outcome if the vote is successful, and one associated with the policy outcome if the vote fails. Exactly midway between those two potential outcome points is the point at which a hypothetical legislator would be indifferent between voting yea or nay. Legislators' voting decisions, then, are made as a function of their distance from, and orientation toward, this cutpoint. Legislators with ideal points on the side of the cutpoint closer to the outcome associated with a successful vote will be more likely to vote "yea" while legislators on the other side of the cutpoint will be more likely to vote "nay."

These scaling methods, though designed to work with all roll calls in Congress, have been shown to have some unexpected sensitivities to overarching content and context of legislation. Specifically, the uni-dimensional model of ideal points, which provides the basis for most of the polarization-in-Congress evidence, is itself susceptible to a multitude of problems. Mechanistically, there are many concerns about the foundations of how exactly we should think about ideal point models in the first place: what the utility function of a given legislator should look like (Armstrong *et al.*, 2014; Carroll *et al.*, 2009; Carroll *et al.*, 2013; Clinton and Jackman, 2009), what are the necessary identification assumptions and estimator properties (Clinton *et al.*, 2004; Londregan, 1999; Poole, 2005; Tahk, 2010; Tahk, 2018), and even how we should use these estimated ideal points in our analyses (Ho and Quinn, 2010; Lerner *et al.*, 2021). Further problems arise when trying to deal with the ever-changing legislative context under which roll calls occur, forcing legislative scholars to account for sensitivities to hidden intra-partisan multidimensionality (Aldrich *et al.*, 2014), procedural context (Jessee and Theriault, 2014; Lerner, 2020; Roberts, 2007), committee of origin (Crespin and Rohde, 2010), or policy content (Dougherty *et al.*, 2014; Roberts *et al.*, 2016).

Given all of this context, we should be concerned that our ability to measure intra- and interparty ideological conditions may be distorted by a myriad of factors that are themselves the consequences of changing procedural contexts in Congress. Though we believe it is unlikely that these endogenous factors are completely distorting our picture of polarization in Congress, it is indeed possible that our ability to credibly estimate this over time is entirely intertwined with changes to procedural contexts that magnify this very problem. If we are to consistently estimate polarization over time, and if we are to think through potential procedural changes that could feasibly decrease partisanship in Congress, we need to address this overarching distortion.

Information and Committees

An implicit assumption underlying the canonical spatial model of decision-making is that legislators have enough bill-specific information to be able to gauge the "distance" between their ideal point and the potential outcomes of the vote. Early work on legislator decision-making acknowledged the extensive informational demands placed on legislators who are asked to vote on thousands of bills dealing with a multitude of different, and often highly-complex, policy issues (Kingdon, 1989; Matthews and Stimson, 1975). Much of this work posited that legislators dealt with these informational demands by developing heuristics that simplified decision-making — including cue-taking from other relevant actors (party leaders, committee members, trusted colleagues, interest groups, etc.). This construction implies a principal-agent relationship in which a legislator delegates informational tasks to other actors or sets of actors, who

then provide the legislator with a picture of where a particular bill is located in ideological space.

Of course, information sources, or the actors to whom legislators delegate information-gathering tasks, are not chosen at random. If legislators already have a relatively accurate ideological map in their heads, they may be selecting on ideological proximity when they decide where to seek out bill-level information.[1] If the principal — the legislator who is making the vote choice — and the agent — the actor who is providing information about the ideological location of the bill — share an ideal point, provision of information about bill location should be straightforward and without bias. However, if legislators rely on actors who are not their ideological clones for bill-level information, the information provided about bill location may be biased. Specifically, if the information-providing agent is interested in the legislator voting for a certain bill — as would often be true in the case of a party leader — we could expect that the information shared about bill location would be biased towards the ideal point of the legislator.

Variations in the information environment, particularly changes in how costly it is for legislators to independently seek out bill-level information, should systematically affect legislators' perceptions of bill location in ideological space. In turn, these variations should affect ideological scores generated from the votes that are cast pursuant to legislators' perceptions of bill location. While we remain theoretically agnostic as to how variations in the informational environment will affect the estimation of individual legislators' ideal points, we sketch out two general possibilities below. First, we discuss our operationalization of two distinct informational environments in which legislators may seek out bill-level information to help them orient legislative proposals in ideological space.

Committee reports serve an important role of disseminating information to legislators who are asked to consider legislation on the floor. These reports summarize the need for the legislation in question, describe the changes that the legislation would make to current law, detail debate and amending activity that occurred during committee markup, and provide minority or dissenting views of committee members who oppose the reported legislation. Access to the information contained in these committee reports, then, should help legislators who do not serve on the reporting committee(s) learn about the bill

[1] Matthews and Stimson (1975, p. 48) interview a Southern Democrat who confirms this view: "Usually I follow the practice of seeking out a member of the committee that reported the bill, a member in whom I have confidence." Additionally, Kingdon (1989, pp. 75–82) finds that, when members seek out cues to inform their voting decisions, they prioritize members with whom they know they agree.

in question, and place the bill in ideological space relative to both themselves and the status quo policy being amended.[2]

If legislators do not have access to committee reports — such as in cases where a bill is brought to the floor without being reported by a committee — they must necessarily rely on other sources for information about the bill. As Curry (2015) demonstrates, legislators in recent Congresses often rely on party leaders for bill-specific information absent a committee report. In a principal-agent framework, when bills are considered under regular order, off-committee legislators delegate the tasks of gathering and communicating bill-specific information to members serving on the referral committee(s).[3] However, when committees are bypassed in the legislative process, legislators are, at least implicitly, deciding to delegate the information gathering and information provision roles to party leaders.

We argue that it is reasonable to expect systematic differences in the information that would be provided from committee reports compared to the information that would be provided by party leaders. For one thing, the ideological diversity of members serving on a committee — combined with the opportunity for minority party members to document their opposition to a bill in the committee's report — may provide a more comprehensive and holistic picture of the legislation being reported. On the other hand, when party leaders effectively monopolize bill-level information by bypassing the committee stage, they are likely to strategically and selectively communicate information that is tailored to convince legislators to support the bill. This is particularly consequential if, as Curry (2015, p. 23) argues, party leaders have different motivations and goals from rank-and-file members.[4]

Two Rationales for Committee Bypass

While we do not offer a theory of strategic party leaders that seeks to explain variation in the use of committee bypass, we do draw on the literature on

[2]We are not claiming here that committee reports offer unbiased or "better" bill-level information than members would get if they had to rely solely on party leadership. Committee reports will often be biased towards the viewpoint of the chair (Curry, 2019), and the extensive literature on outlier committees suggests that the information contained in committee reports will likely be skewed away from the floor median. Our argument is simply that the information contained in a committee report would be different from the information provided by party leaders.

[3]By "regular order" we are referring to, at minimum, a process in which a bill is referred to one or multiple committees, and is reported by at least one of those committees.

[4]For example, party leaders may be more directly concerned with how the party's reputation is affected by the passage or failure of a major policy initiative or important reauthorization bill, whereas rank-and-file members may have any number of idiosyncratic incentives to defect from the party's position. Speaker Paul Ryan's (R-WI) struggle with the House Freedom Caucus over the 2018 farm bill provides a good example of this discrepancy in motivations.

unorthodox legislative processes to identify two purposes for which bypass might be used. The first purpose is to attain a non-median outcome that moves policy away from the floor median and towards the median of the majority party. Adhering strictly to the spatial model of voting, such a policy outcome should not be possible absent non-ideological side payments. However, by routing the bill around the committee system, majority party leaders can monopolize bill-specific information and selectively communicate that information to moderate party members. This selective and strategic communication can help legislators justify a choice to support a bill that they may otherwise have opposed if they were privy to the kind of information that may have been available in a committee report. In this restricted informational environment, then, legislators' perception of the bill's location in ideological space may be biased towards their own ideal point.

An opposing view that has recently been articulated in the literature is that party leaders centralize negotiations and carefully guard bill-specific information generated from those negotiations in order to build broad coalitions of support and prevent them from unraveling (Curry and Lee, 2020b; Curry and Lee, 2020a). As Curry and Lee (2020a, p. 189) state in the conclusion of their recent book, "Today's unorthodox and party-led processes should be understood as new paths that allow Congress to continue doing what it has long done: building broad bipartisan support to enact laws." If this second perspective accurately describes at least some of the uses of unorthodox procedures like committee bypass, we may expect bypass to be associated with less divisive votes. Therefore, we would also expect ideal points estimated on the votes cast on bypassed bills to reveal lower levels of polarization between Republicans and Democrats in Congress.

The Effect of Committee Bypass on Roll Call Voting

We do not assume that unorthodox procedures like committee bypass are used exclusively in the service of one goal — whether it be partisan policy or coalition building and management in an increasingly fractious political environment. In a political environment in which legislators have strong incentives to blow up negotiations, bypassing committee may be leadership's attempt to keep a bill above the partisan fray and deliver a bipartisan compromise. Secrecy in bill-development can help facilitate deliberative negotiation across parties (Mansbridge and Warren, 2013), whereas the more open, committee-driven process can expose the bill to the centrifugal forces of lobbyists and the polarizing forces of the parties' activist bases (Curry and Lee, 2020b, pp. 636–637). However, bypass may also be used occasionally as a tool for majority party leaders to selectively sell partisan proposals to their more moderate members, thereby achieving non-median policy outcomes. Regardless of the purpose for which bypass is used, its use unquestionably creates a different

informational environment than would exist if a bill were referred to committee, marked up, and reported to the floor.

How does this variation in informational environment affect roll call votes on bypassed bills versus non-bypassed bills? Relatedly, how does the increased use of committee bypass over time affect aggregate measures of ideological polarization between Republicans and Democrats in Congress? As we will explain in more depth below, we attempt to answer these questions by comparing roll call votes, and the ideological scores derived from them, across comparable sets of bypassed and non-bypassed bills. Of course, because unorthodox procedures such as committee bypass are not deployed at random by party leadership (Curry, 2015, p. 122), we need a research design that allows us to examine, as closely as possible, the differences in roll call votes that are attributable to the procedure of bypass itself. Given a research design that adequately accounts for this selection process, we will be able to make inferences about the effect of committee bypass on roll call votes and measures of ideological polarization. Our results will allow us to make further inferences about how party leaders are using the informational monopoly that committee bypass affords.

Data

In order to test for the effect of committee bypass on roll call outcomes and ideological polarization measures, we first must define a universe of bills that are relevant for our purposes. We look specifically at the 103rd–113th Congresses (1993–2015). We limit our data to HR bills, as our analyses are focused exclusively on members of the House and the roll call votes they cast. Additionally, an HR bill must receive a roll call vote in the House to be included in our data — because, absent this, we would not be able to include the bill in any ideal point model.

We eliminate any bill that was brought to the floor under suspension of the rules. Because a motion to bring a bill to the floor under suspension of the rules requires a 2/3rds supermajority to succeed, the procedure is frequently used to deal with non-controversial legislation. Additionally, during the time period we study, a large majority of bills that come to the floor under suspension of the rules are not reported out of committee, and therefore would meet our definition for bypass (more on this below). However, it is clear that bypassing committee by making a successful motion to suspend the rules is substantively different than bypassing committee through other means, such as extraction by the Rules Committee.[5] Finally, because ideal point estimation models drop

[5]Additionally, while many of the bills that come to the floor under suspension of the rules are relatively minor, many of the bills that are routed around committee by a provision in a special rule for consideration are substantively significant. For example, the Medicare Physician Payment Reform Act of 2009, the FISA Amendments Act of 2008, the Secure

lopsided votes, a majority of bills brought to the floor under suspension of the rules would already be dropped in our analysis.[6] After dropping suspension bills and bills that did not receive a roll call vote on the House floor, we are left with 1,226 bills.

Identifying Committee Bypass Bills

Our decision rule for coding committee bypass is fairly straightforward: if a bill is considered on the House floor but has no committee report associated with it, we code it as a bypass bill. Conversely, bills that were reported out of committee are coded as non-bypass bills. This operationalization, in addition to being simple and intuitive, also matches our informational theoretical framework. Of course, we cannot directly measure the amount or diversity of information that each legislator has on any given bill, but we know that committee reports are important sources of bill-level information for legislators. By definition, legislators are deprived of that particular information source in the case of our bypass bills.

One byproduct of our coding rule for bypass bills is that we have to eliminate certain types of bills that could never be bypassed, by our operationalization. Because we will later use matching as a form of pre-processing, we need to be dealing with bills that, in theory, have some positive probability of bypassing committee. Two types of bills do not meet this criteria based on our operationalization of bypass — appropriations bills and budget reconciliation bills.[7] We eliminate these bills from our dataset, because the Appropriations and Budget committees, respectively, report them to the floor as original measures — making bypass impossible by our definition. After these bills are dropped, we are left with 951 bills in our dataset.

Limiting to Final Passage Votes

In this paper, we limit our analysis to roll call votes on final passage of bills. The logic of this is consistent with (Lerner, 2020). Non-final passage votes (including all procedural votes) are subject to kinds of strategic voting from which final passage votes are exempt; the public facing nature of final passage

Fence Act of 2006, and the Energy Policy Act of 2005 — all of which went on to become law — bypassed committee consideration in the House in this manner.

[6]The default threshold to drop a vote in the wnominate R package is when the minority side of a vote is $\leq$2.5% of the members voting. By this threshold, just over 65% of suspension bills would be automatically dropped in ideal point estimation.

[7]We maintain bills that are worked on by other (non-Budget) committees pursuant instructions in a budget resolution, because it is possible that these bills can get to the floor with or without a committee report. However, we exclude reconciliation packages reported by the Budget Committee, as those are always reported out of committee as an original measure.

votes arguably presents the member of Congress the most transparent vote choice (Carrubba *et al.*, 2008; Crisp and Driscoll, 2012). This is especially true for the majority party, as majority party legislators can assume the vast majority of bills put to final passage votes will pass (Cox and McCubbins, 2005). Though the selection of final passage votes will have an implicit bias (see Carrubba *et al.* (2008), for an example) it will be a *consistent* bias, whereas procedural and amendment votes introduce a variety of biases that are harder to track down. Indeed, there is some evidence from Lynch and Madonna (2022) that the increase in polarization is partially driven by the increase in (mostly meaningless) procedural or symbolic votes that do not change the substance of law. At least the outcome of a given final passage vote will always reflect the lawmaking preferences of members voting because all passed bills can, and often will, change the policy status quo.

Importantly, we cannot think of procedural votes and amending votes as ever being independent or identically distributed with final passage votes given the nature of how these votes *structure* final passage votes themselves, literally in the case of some procedural votes (voting on special rules for consideration), but metaphorically for the rest. Indeed, amending votes literally change the substance of the bill being considered by future final passage votes. Considering the extensive work on procedural manipulation and how the use of these tools have changed over time in response to changing partisan conditions (e.g., Dougherty *et al.*, 2014; Lynch and Madonna, 2013; Lynch *et al.*, 2016; Roberts, 2007), and on the multi-faceted incentive structure for amendment votes,[8] we can be sure that the utility space of voting on procedural and amendment votes is different from that for final passage votes.

Final passage votes remove four distinct sources of inconsistency in establishing spatial voting game: agenda model, tie breaking rule, decisiveness rule, and the status quo rule. We know that the agenda setting model for final passage votes is consistent; this goes back to Romer and Rosenthal (1978), but is elaborated on extensively in Cox and McCubbins (1993). The rules for tie breaking and decisiveness are also unchanging because, again, this is all established beforehand by the Rules Committee. Final passage votes all follow the same sets of procedures, which other sets of votes (amendments and procedural votes) do not. We also have consistent expectations with regards to the status quo, in that failure of the final passage votes means preservation of the status quo by definition. Final passage votes also have the benefit of being the "public facing" votes, in that it is an unambiguous signal the legislator has about the given proposal (Theriault, 2008). The layers of complexity in dealing with amendments and procedural votes means that it is rarely going to be clear to the public what is going on with an particular vote; final passage votes are what most people would expect voting in Congress to be.

[8]See the sophisticated voting literature on this point.

Research Design and Methods

A simple way to view the downstream effects of bypass on the subsequent stages of the legislative process is just to compare votes on bypassed and non-bypassed bills directly. The problem with doing this simple comparison is that bills do not go through any set of procedural paths randomly. There are a multitude of strategic and practical reasons why congressional leaders would bypass committees, and an overarching concern to any design looking to study questions on the effect of procedural choices is that those same considerations are likely to affect any and all outcomes we would be interested in (Bendix, 2016; Howard and Owens, 2020).

The presence of an effect on both the likelihood of a given piece of legislation bypassing committee *and* on any and all legislative outcomes we are interested in is a classic case of confounding. In causal inference terms, there are a large set of covariates describing each piece of legislation that strongly predict both treatment assignment–in this case committee bypass–and legislative outcomes of interest to us. A simple solution to dealing with this type of confounding bias is to condition your approach on these variables (Pearl, 2009). If this was a simple design, we could deal with this using a variety of tools, including just adding these variables to a regression. Because we are more concerned about how these affect treatment assignment and that the selection process is complex and unobservable, we are instead going to utilize matching as a non-parametric form of pre-processing that enables us to use whatever methods we want to after (Ho *et al.*, 2007; Imbens, 2015; Imbens and Rubin, 2015; Sekhon, 2009; Stuart, 2010).

As discussed above, there are some assumptions on the estimation of ideal points as a latent trait that might encourage re-estimation of ideal points on a more restricted set of bills. Matching as pre-processing allows us to condition on factors that might affect the conditions under which roll calls occur, which allows us to evaluate the effects of different procedural conditions on roll calls holding constant these mitigating factors. The good news is that with matching, we are retaining all of the bypassed bills and only selectively dropping the non-bypassed bills, so there is no risk we are missing any direct effect of bypass. Matching gives us the best credible effort for conditioning on observed bill characteristics in comparing the effect of committee bypass; even if there are unobservable characteristics that might be influencing the bypass decision, matching provides a more viable comparison than simply comparing all bypassed and non-bypassed bills. It also gives us a way to condition on a variety of factors in as transparent a way as possible.

As far as we know, there are no other methods designed to facilitate this conditioning without changing the underlying ideal point estimation method–since we are interested in keeping this simple, matching to pre-process followed

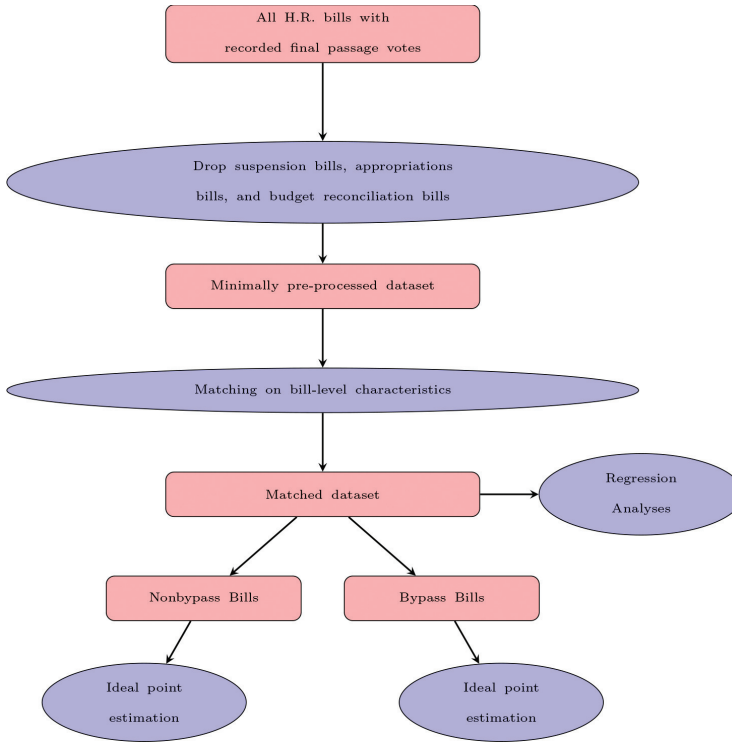

Figure 1: Data processing and analysis flow chart.

by running a conventional ideal point estimation method seemed like the most efficacious approach.

Figure 1 shows our general approach with each step identified along the way. We have already discussed treatment identification and bill selection in the data section. We use matching to create identically sized sets of final-passage votes for each set of roll calls: those that bypassed committees and those that did not. The ovals represent actions we take on the rectangles, which are the various different forms the datasets take throughout our analysis.

For the final stage of our estimation, we utilize W-NOMINATE for estimating ideal points. We also used a standard Bayesian IRT model for a robustness check, but given that we ultimately run our ideal point models repeatedly (once for each of the different matching specifications), we decided to report estimates from W-NOMINATE for speed considerations.[9]

[9]We use the same specification for W-NOMINATE each time to maintain consistency between each run. We use Tom Tancredo (R-CO) as our polarity reference point for W-

Matching Specifics

For each type of matching, we divided our features into two categories: Text based inferences and bill features. We also applied both Mahalanobis matching as well as propensity score matching to each set. For bill features, we matched on:

- Important Bill?[10]
- Number of Co-sponsors
- Number of Referral Committees
- Whether it has a Majority Party sponsor?
- Whether it has a committee chair sponsor?
- Whether it has a subcommittee chair sponsor?
- The absolute value of the distance between the sponsor's DW-NOMINATE score and the Median of the Majority party
- Number of Days left in Congress
- If the Bill is a Party Priority Bill (HR. 1-10)
- Congress
- Primary referral committee
- Policy Agendas Project topic codes

Beyond just looking at the features, we also need to address the substance of bills. To do that, we turn to natural language processing methods to identify which bills are substantively most similar.

One of the main concerns with the descriptive information about the bills we want to match on is that this doesn't capture the substantive similarities and differences between bills. We want to explore the most comparable set of bills and, though the conditions under which a bill is introduced are very important, it is likely that the most significant heterogeneities are going to be on the substance of the bills themselves. The majority party leadership

NOMINATE, since he is an extreme member of the Republican party that is present in multiple Congresses–we experimented with other members as the reference point, with only minor differences. W-NOMINATE was fit using the "wnominate" package in R with other specifications set at their defaults.

[10]As coded by the Congressional Bills Project (http://www.congressionalbills.org/codebooks.html). This is a fairly low threshold for importance, and for the most part only excludes bills naming buildings or transferring small plots of land.

decision to route a bill around the committee process likely hinges on the policy area and specific policy content of the bill. We would like to, as much as possible, match on the textual features of a given bill to make the agenda as comparable as possible.

For the text features, we utilize the structural topic model to summarize the text of the given bills (Roberts *et al.*, 2014). We used the structural topic model for several reasons: first of all, a simple topic model is the best generative model to describe the content of a given bill. Topic models are the most widely used unsupervised learning model for text data to provide concise summaries of the substance of documents that allows for meaningful comparisons. For matching in particular, we believe the information from the topics themselves provide a greater summary of the substance of a given bill than singular membership descriptions of bill substance would. Because all topic modeling based approaches are mixed-membership models, they provide information about the proportion of each document devoted to each topic. We believe this is a richer form of topic identification for comparing bills than single membership models that assign each bill to only one topic. Ultimately, we test both approaches in this article, as our matching procedures that used bill features incorporated Policy Agendas Project topic codes (Baumgartner and Jones, 2013) and our matching procedures that used text features incorporated topic proportions extracted from our structural topic model.

We used a $k = 21$ topic model–though we fit many different models on the data.[11] To get to our specific model, we evaluate a wide range of model-based characteristics in determining the ideal number of topics,[12] as well as a manual evaluation of the coherence and comprehensibility of the topics. We otherwise follow the recommendations from Roberts *et al.* (2020) and Mozer *et al.* (2020) in applying topic modeling to matching, though we try to stick to the approaches that were built around topic models.

We apply the information from the topic models in a set of matches that exclusively looks at the content of bills to make matches; we also look at a set of matches that exclusively matches on non-textual features. We then evaluate combination matching models that incorporate information from both sources. For comparison's sake, we use both propensity score matching and Mahalanobis distance matching, consistent with Roberts *et al.* (2020) and Mozer *et al.* (2020) in not relying solely on any one matching method.[13] For

[11]More information on these models and their diagnostics can be found in the Online Appendix.

[12]These metrics were provided by the searchK function in the STM package in R. These evaluation criteria include exclusivity, semantic coherence, held-out likelihood, and the residuals for each model. We tested different combinations from $k = 5$ to $k = 50$ and found that $k = 21$ was the most consistent.

[13]We are aware of the limitations of propensity scores for matching as discussed in King and Nielsen (2019), which partly motivates our use of Mahalanobis matching–we decided against using coarsened exact matching because of recommendations from Black *et al.*

our matching approach, we utilise the "MatchIt" package in R, which allows for easy comparison between propensity score matching and Mahalanobis matching on the different sets of covariates. Propensity scores are estimated with using logistic regression.

Results

Before moving on to any substantive results, we first have to evaluate how the matching methods performed. Since we have multiple different combinations of methods and variables used, we need to make sure that we only use methods that make sense and reduce balance between treated and control. We will examine, in a series of Love plots, the overall balance between bypassed and non-bypassed bills before and after matching.

Examples of Matched Bills

We begin by evaluating the matches qualitatively before examining the improvements in balance made by the various matching procedures. Here, we provide two example sets of bills that were paired together by our matching procedures and discuss the quality of those matches along a number of dimensions.[14] Primarily, we are interested in how similar the bills are in terms of policy content and ideological character, as these attributes should be the most closely tied to our outcomes of interest — all having to do with votes on the bills.

The first matched pair we examine includes HR 2746, the Helping Empower Low-Income Parents (HELP) Scholarship Amendments of 1997, and HR 2616, the Charter School Expansion Act of 1998, both considered during the 105th Congress (1997–1998). Both bills dealt with the same policy area — not just education, but charter schools specifically. HR 2616 sought to increase federal funding for charter schools, while HR 2746 would have created a scholarship program for low-income parents to send their children to charter schools. Both bills were central pieces of the House Republican Conference's legislative agenda on public education, which heavily emphasized school choice and increasing parents' control over their children's education. Reflective of their importance to the party's agenda, both HR 2616 and HR 2746 were sponsored by the chair of the House Education and Workforce Committee, Frank Riggs (R-CA).

(2020), given that we are matching on a large range of variables. We also explored using the covariate balancing propensity score for propensity score matching (Imai and Ratkovic, 2014), though minimal differences were noticed.

[14]Complete matched sets of bills from the different matching regimens are available upon request, and a further discussion of selected matched sets is provided in the Online Appendix.

Chairman Riggs introduced HR 2616 on October 6th, 1997. The bill was considered by the House Committee on Education and the Workforce and was favorably reported on October 14th. We mention as an indicator of the partisan and ideological content of the bill that the committee report included minority views written up by eight Democratic members who voted against reporting HR 2616.[15] Two weeks later, Riggs introduced HR 2746, the Helping Empower Low-Income Parents (HELP) Scholarship Amendments of 1997. This bill had the strong support of Speaker Newt Gingrich (R-GA) and other members of the Republican congressional leadership, and the House Rules Committee reported out a special rule for consideration of the bill the day after it was introduced. The full chamber's approval of the special rule, H.Res. 288 — which provided for the consideration of HR 2616 as well — allowed HR 2746 to bypass committee consideration and go straight to the floor.

HR 2616, the Charter School Expansion Act of 1998, passed on the House floor by a bipartisan vote of 367–57. HR 2746, the HELP Scholarship Amendments of 1997 — which had bypassed committee consideration because of leadership's fears that it would not be favorably reported — failed by a vote of 191–288.

Another matched pair we consider comes from the 106th Congress (1999–2000). The two bills included in this pair are HR 2723, the Bipartisan Consensus Managed Care Improvement Act, and HR 4680, the Medicare Rx 2000 Act. HR 2723 was a patients' rights bill while HR 4680 sought to add a prescription drug benefit to Medicare. While these bills were obviously not identical in terms of policy content, they both deal specifically with access to, and quality of, health care. Both bills were introduced by Republicans, with William M. Thomas (R-CA) sponsoring HR 4680 and Charles W. Norwood (R-GA) sponsoring HR 2723.

Representative Norwood's bill, the Bipartisan Consensus Managed Care Improvement Act, was introduced August 5th, 1999, and referred to three House committees — Commerce, Education and the Workforce, and Ways and Means. None of these committees took any action on the bill, though, and on October 5th, 1999, the House Rules Committee reported out a special rule for its consideration (H.Res. 323). The next day, H.Res. 323 passed the House, allowing the bill to bypass committee consideration and come straight to the floor. The day after the special rule was approved by the House, the Bipartisan Consensus Managed Care Improvement Act passed the House by a vote of 275–151. Surprisingly, the bill's passage rolled majority party Republicans, with 68 Republicans joining 206 Democrats in voting for the bill while the majority of the conference — 149 Republican representatives — voted no.

[15]Bill L. Clay (D-MO), Matthew G. Martinez (D-CA), Patsy T. Mink (D-HI), John F. Tierney (D-MA), Dale E. Kildee (D-MI), Donald M. Payne (D-NJ), Bobby C. Scott (D-VA), and Dennis J, Kucinich (D-OH).

Representative Thomas' bill, the Medicare Rx 2000 Act, took a more traditional route to the House floor. The bill was introduced June 15th, 2000, and was referred to the Ways and Means Committee and the Commerce Committee. The Ways and Means Committee considered the bill, and reported it favorably on June 27th. The same day, the Commerce Committee was discharged, and the bill was placed on the Union Calendar. The next day, the bill came to the House floor under the parameters of a special rule for consideration, H.Res. 539. The final passage vote on the House floor was a narrow, and almost perfectly partisan 217–214 victory for House Republicans.

The two matched pairs we consider here — a set of bills about charter schools and a set of bills about access to health care — are similar internally, in that each pair deals respectively with a consistent policy area and all bills are sponsored by copartisans. However, our qualitative sketches tell two different potential stories about committee bypass. In the case of the charter school bills from the 105th Congress, committee bypass of the HELP Scholarship Amendments seemed to be a reach by House Republican leadership to achieve a non-median policy outcome they would not have been able to achieve through regular order. As it turned out, Speaker Gingrich and Chairman Riggs were unable to achieve this policy outcome even with this unorthodox procedural reach. On the other hand, another bill sponsored by Riggs on a similar topic — the Charter School Expansion Act of 1998 — went through the typical committee process and garnered a bipartisan passage vote on the House floor. In the case of the health care bills, the Bipartisan Managed Care Improvement Act bypassed committee consideration and passed on the floor with bipartisan support, while the Medicare Rx 2000 Act went through regular order and passed with an almost purely partisan vote.

Ultimately, the success of our matching algorithms should be judged holistically based on improvements in covariate balance (discussed below) as well as qualitative comparisons between matched bills. Our inclusion of topic proportions in our matching procedures has allowed us to find pairs of bills that are similar to one another on substance in a fairly granular way. We would argue that this is an improvement over what can be done with topic codes that categorize an entire bill as fully one topic or fully another. While we cannot completely overcome the issue inherent to observational studies — that our treatment and control groups of bills are not literally identical on all relevant dimensions — our use of multiple matching procedures helps us to mitigate against whatever selection bias may be present in the decision to bypass committee.

Evaluating Covariate Balance

Moving on from these examples of matched pairs, we now look at the overall balance between two different matching methods and two different sets of

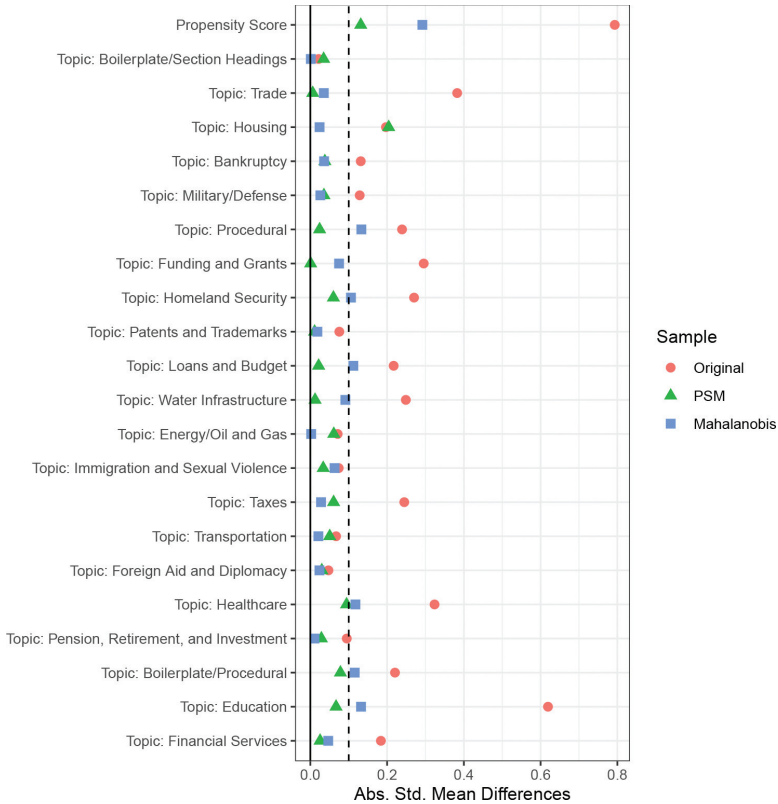

Figure 2: Text only love plot.

matching variables. Figure 2 looks at the balance between bypassed and non-bypassed bills comparing circles (no matching), triangles (propensity score matching), and squares (Mahalanobis matching).[16] The Love plots provide the covariates matched on the y-axis and the absolute value of the standardized mean differences on the x-axis. The dotted line represents the 0.1 threshold for good balance in a matched design specified by Stuart, 2010–scores below the threshold can be considered "balanced on" while those outside remain unbalanced.[17]

Overall, in Figure 2, we see a marked improvement in the overall balance between treated and control groups for either matching method over not match-

[16]Excluded from this plot, and other Love plots in the body of the article, are the Congress factors used in each of our matching procedures. Interested readers can find information about balance improvement on these factor variables in the Online Appendix.

[17]Other papers propose a 0.25 threshold for balance, but since we are concerned about selection effects, we chose the stricter criteria.

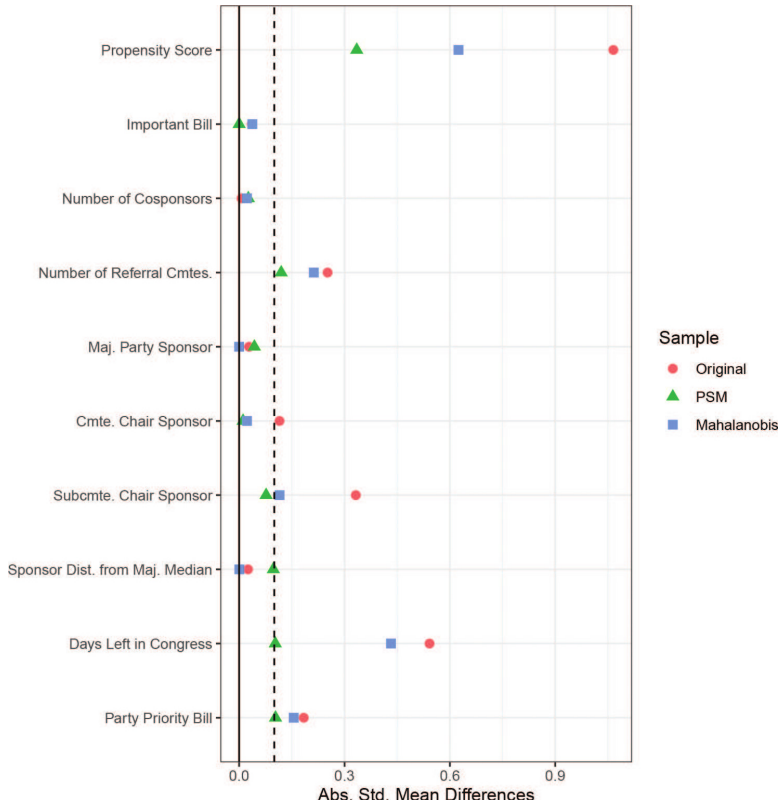

Figure 3: Features only love plot.

ing. Significant gains in balance are noticeable in topics concerning Trade, Bankruptcy, Military/Defense, Funding and Grants, Procedural, Homeland Security, Loans and Budget, Water Infrastructure, Taxes, Healthcare, Education, and Financial Services.[18] When evaluating the difference between the two matching methods, it seems that Mahalanobis matching does a little bit better across the board than propensity score matching, though both provide good balance compared to no matching. This is consistent with King and Nielsen (2019) and Mozer *et al.* (2020), which find propensity score matching lacking with lots of continuous variables, which is what topic proportions are.

Moving on to our next set of matches, we now compare how the matching approaches work on just the bill-level features, not the substance of the

[18]Note, as with all topic modeling based approaches, the labels for these topics were selected after a careful reading of the top words in each topic and the top documents representing each topic. A more detailed discussion of this is available in the Online Appendix.

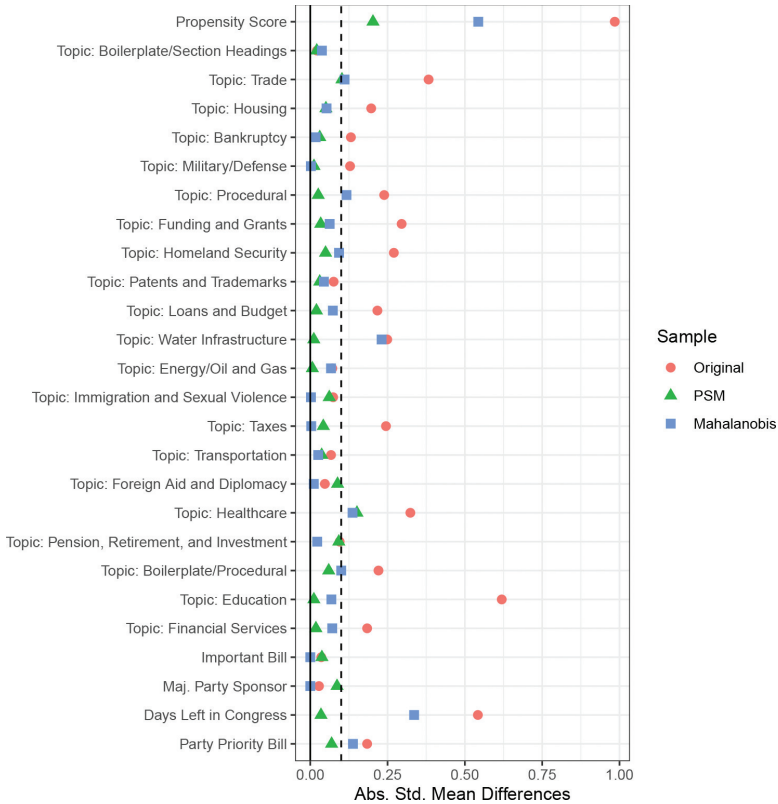

Figure 4: Combination love plot.

bills. We see this comparison in Figure 3.[19] We see overall improvements in balance between the unmatched and matched datasets on number of referral committees, if the sponsor is the relevant subcommittee chair, the number of days left in Congress, and if the bill is a party priority. In this set up, propensity score matching does better than Mahalanobis distance matching, especially on the days left in Congress variable.

We now move on to our final set of matches: combining both bill level features and text in matching. In Figure 4, we find results completely consistent with our previous two figures: matching methods improve fit across the board. With combining topics and bill level features, we find that propensity score matching and Mahalanobis distance matching both perform rather well in

[19]This Love plot also excludes the Policy Agendas Project topic codes and the primary referral committee factor variable. Interested readers can find information about balance improvement on these factor variables in the Online Appendix.

aggregate balance, though there is no method that strictly dominates. Because of this, we will continue to feature results from all of our matching methods, though specific polarization based metrics will be reported only from the combination models (though both propensity score matching and Mahalanobis distance matching will be reported).

Impact of Bypass: Regression Estimates

Now that we have a number of matched datasets with balanced subsets of bypass and non-bypass bills, we move on to assessing the impact of committee bypass on two outcomes of interest — vote margin and the presence of a party unity vote.

Our first set of models takes vote margin — the number of "yea" votes on a bill minus the number of "nay" votes — as a dependent variable. Because of the continuous nature of this dependent variable, we opt for OLS regression. These models will allow us to make inferences about the effect of committee bypass on the "closeness" of passage votes in the House. If committee bypass is successfully used to build coalitions behind legislation, we may expect the coefficient on our bypass indicator to be positive and statistically significant, demonstrating that bills pass by larger margins when they bypass committee. However, if committee bypass is generally used to achieve some non-median policy on the side of the majority party, we may expect our bypass indicator to be negative and statistically significant, indicating that bypass is picking up something close to a minimum-winning coalition of majority party members.[20] Our matching algorithms can be seen as attempts to mitigate selection effects — in other words, the coefficient on bypass should not be a function of some systematic difference between the *types* of bills that bypass committee as opposed to going through regular order. Instead, the effect shown should be the independent effect of the procedure of committee bypass itself.

As shown in Table 1, the coefficient on our bypass indicator is negative in all six of our models, and statistically significant in all models but two. Our model fit on the matched dataset constructed using Mahalanobis matching on bill feature variables is a notable outlier in that the magnitude of the bypass coefficient is considerably smaller than in the rest of the models. The bypass coefficient from the model fit on our matched dataset constructed with propensity score matching on the bill topic proportions also misses statistical significance by a wide margin. However, the balance of the results here, looking at the four other models, suggest that committee bypass has a negative effect on vote margin, such that bills that bypass committee gain fewer "yea" votes on the floor. The most extreme outcome from these models, the bypass coefficient from the model fit on our matched dataset constructed using Mahalanobis

[20]Each model presented below includes fixed effects for Congress, meaning that variations in the size of the majority party from one Congress to the next should not affect our results.

Table 1: Estimating the effect of committee bypass on vote margin.

	Dependent variable: Vote margin (Yea-Nay)					
	PSM text	Mahal text	PSM features	Mahal features	PSM combo	Mahal combo
Bypass	−19.370	−43.434**	−36.718**	−5.088	−27.938*	−40.110**
	(16.641)	(16.768)	(14.628)	(15.133)	(15.496)	(16.015)
Obs	362	362	362	362	362	362
Covariates	✓	✓	✓	✓	✓	✓
Cong. FEs	✓	✓	✓	✓	✓	✓

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

matching on bill topic proportions, demonstrates that bypassing committee costs just over 43 votes on the floor, relative to going through regular order.

Our next set of models considers a different dependent variable: the presence or absence of a party unity vote. For the construction of this variable, we opted for a 90/90 threshold, meaning that a vote is considered a party unity vote if at least 90% of the members from one party vote the opposite way from at least 90% of the members of the other party. Because of the dichotomous nature of this dependent variable, the models presented below are logistic regression models. The results in Table 2 are somewhat paradoxical when considered in combination with the results from Table 1. In all models, the coefficient on committee bypass is negative and statistically significant, demonstrating that bypass is associated with a decreased likelihood of party unity passage votes on the House floor. So, according to the results from both sets of regressions combined, committee bypass leads to smaller vote margins, but a decreased likelihood of party unity votes. One possible way to interpret this is that committee bypass is leading to relatively narrow, but at least minimally bipartisan, vote outcomes on the floor. If this is the case, these results are partially compatible with those reported by Curry and Lee (2020b) and Curry and Lee (2020a), who demonstrate that unorthodox legislative procedures do not consistently serve to polarize outcomes.

Impact of Bypass: Ideal Point Estimates of Polarization

We now move on to our direct estimates of bypass on polarization. Following Aldrich and Rohde (2000a), we measure polarization using the distance between the medians of the two parties. We compare results after fitting W-NOMINATE on all the different subsets of our data. Because we are interested in comparing non-bypassed bills to bypassed bills, the set of bypassed bills remains constant while we utilize different subsets of non-bypassed bills. For matching, this

Table 2: Estimating the effect of committee bypass on party unity vote propensity.

	Dependent variable: Party unity vote (90/90 threshold)					
	PSM text	Mahal text	PSM features	Mahal features	PSM combo	Mahal combo
Bypass	-0.973^{***}	-1.005^{***}	-0.924^{***}	-0.998^{***}	-0.976^{***}	-0.790^{**}
	(0.353)	(0.383)	(0.322)	(0.302)	(0.375)	(0.355)
Obs.	362	362	362	362	362	362
Covariates	✓	✓	✓	✓	✓	✓
Cong. FE's	✓	✓	✓	✓	✓	✓

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

is analogous to focusing on estimating the average treatment effect on the treated, wherein all treated units are always retained. All analysis in this section is based on the W-NOMINATE ideal point estimates on matched final passage votes.

We see the output of this analysis in Figure 5, which compares the absolute value of the distance between the party medians from the 103rd Congress to the 113th Congress. Each line in the Figure represents a different matched dataset, save for the black-solid line, which is just the bypassed roll calls. We use different shapes, shades, and line types to represent the six different matched subsets.

What is apparent from comparing the results in Figure 5 is that polarization is consistently greater among the matched non-bypassed roll calls than the bypassed roll calls, with the exception of the propensity score matching on bill features set. All other matched non-bypassed roll calls indicate a greater distance between the medians of the two parties, which seems to follow essentially the same pattern over time, with polarization generally increasing each Congress. It is significant that the consistent signal from all matched sets is that bypass *decreases* polarization.

The two matched sets we believe are the best balance between treatment and control are the two matched sets that use a combination of bill features and text features for both propensity score matching and for Mahalanobis distance matching. These are represented by the dotted lines with triangle points (and are close to each other on the figure).

We see how this plays out in each Congress in Figures 6 and 7, which compares the distribution of ideal points in each Congress by party for bypassed and non-bypassed (matched) roll calls. Figure 6 presents results from the combination Mahalanobis distance matched set, while Figure 7 presents results from the combination propensity score matched set.

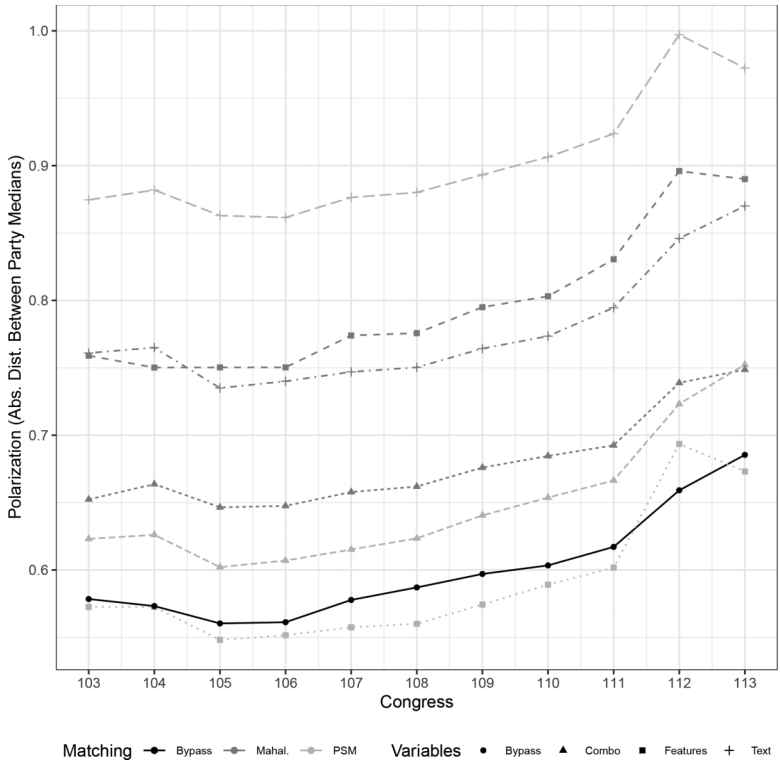

Figure 5: Polarization measures with different matching algorithms.

Consistently, we find that non-bypassed roll calls generate small differences in the overall distributions of the two parties' ideal points over time, pushing the distribution of the Democratic party's members to the left (and pushing a small percent of Republicans to the far right). The medians for each party are shown with the solid line for bypassed roll calls and the dotted line for non-bypassed.

The results are basically consistent between the two matching methods, though the Republican distributions basically do not move in the Mahalanobis distance matching results. We do not want to over-interpret the overall change in distributions between the two parties, as the specific spatial location for ideal point models suffer from some identification problems.[21] What we can say, though, is the relative ideological orientation of the parties is further apart under non-bypassed roll calls. The changes in overall polarization are

[21] See Lerner (2020) for a discussion of why this might make some absolute comparisons challenging.

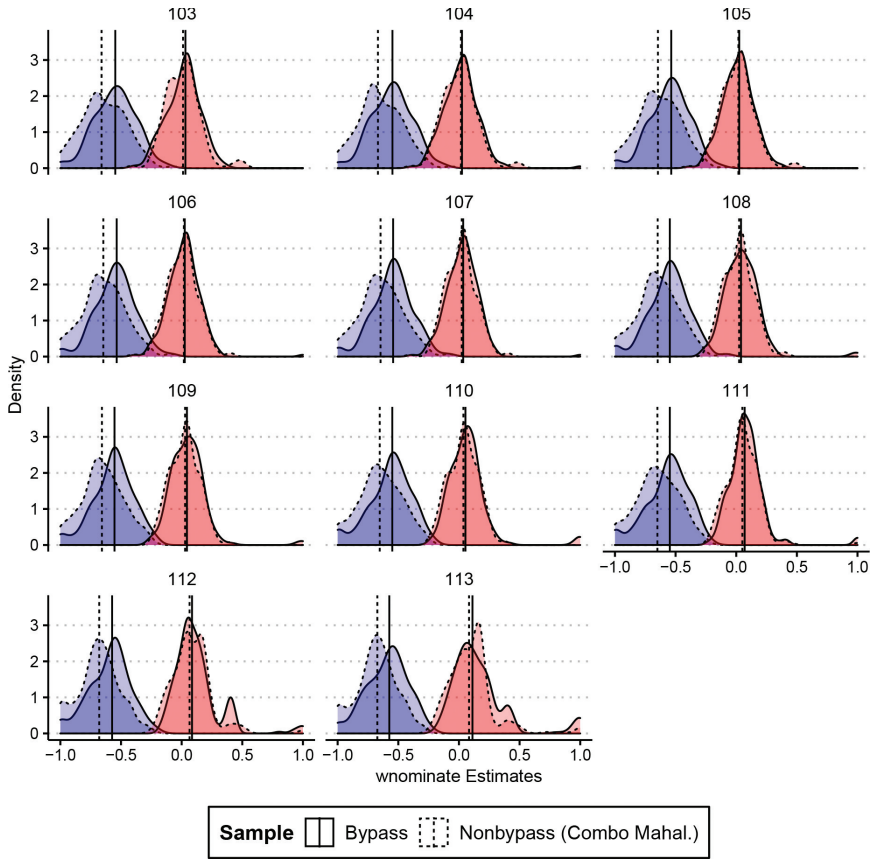

Figure 6: Nonbypass versus bypass ideological distributions using Mahalanobis matching.

consistent, and show constancy even over the eleven Congresses we study. It is clear from our results that party leaders circumventing regular order provides for a less polarized set of roll calls, and that a return to the procedures of the textbook Congress would, most likely, just serve to exacerbate polarization, not ameliorate it.

Discussion

In this paper we created matched subsets of House bills that bypass committee and those that go through the committee process in order to make inferences about how procedural context can affect our measures of ideology and partisan polarization. In general, we find that committee bypass is associated with nar-

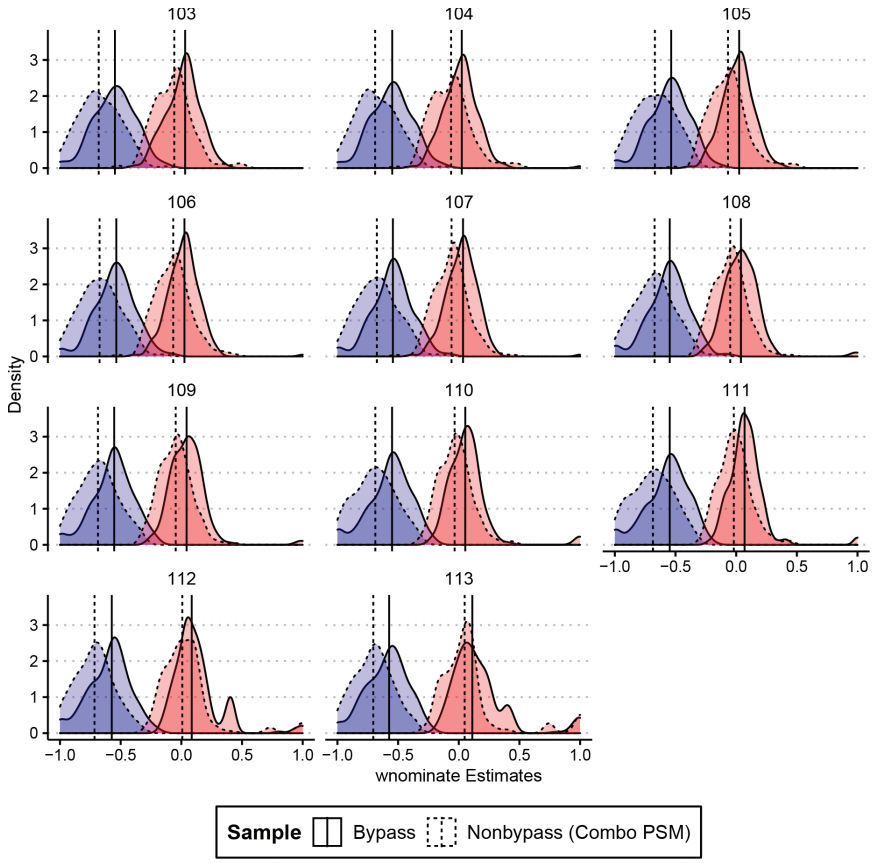

Figure 7: Nonbypass versus bypass ideological distributions using propensity score matching.

rower vote margins (compared to matched non-bypass bills), but is negatively associated with party unity votes. Additionally, we find that polarization between the two parties — measured as the distance between party medians — is considerably lower when estimated using votes cast on bypass bills compared to votes used on matched non-bypass bills. When the legislative process is more open and deliberative, as opposed to more centralized, closed off, and leadership-driven, the observable partisan polarization is exacerbated.

These results lend support to the recent work of Curry and Lee (2020b) and Curry and Lee (2020a), who argue that modern party leaders use their control over procedure primarily to craft bipartisan compromises, rather than to aggressively push for non-median policy outcomes. Committee bypass, then, may be a rational reaction on the part of majority party leaders to the increasingly divisive partisan political environment outside of Congress, which

provides incentives for rank-and-file members to loudly castigate compromise as weak and unprincipled. Rather than bypassing committee to push for ideologically extreme partisan policy outcomes, leaders are centralizing the legislative process in order to keep important bipartisan deals above the chaotic political fray. In future work, we would like to see if our result extends to other procedural tools highlighted in Curry and Lee (2020b) and Curry and Lee (2020a) and to what extent.

We believe there are additional takeaways from our analysis that are worth considering as well. Primarily, we need to think critically about ideal point estimates, and products that are a down-stream consequence of those estimates. Namely, we should not assume that the data generating process for any given roll call vote in any given Congress is always going to be the same, and we should consider how institutional and procedural changes may differentially distort ideal point estimates over time. Given also the variation in member-level ideal point estimates when the analysis is subset to different sets of roll calls, it is worth thinking about how stable these estimates are. This is not a call to abandon DW-NOMINATE or other ideal point estimating enterprises, but potentially it is a call to think more carefully about how we use and interpret those estimates.

From a congressional reform perspective, we are skeptical that reforms aimed at increasing the role of committees and committee hearings in the legislative process will lead to improved lawmaking or decreased polarization. While we cannot speak directly to the quality of bills passed, we can observe the partisanship and polarization surrounding similar bypassed bills and non-bypassed bills. Reforms aimed at changing the procedures of Congress are missing the forest for the trees, as partisan lawmaking and polarization merely reflect the underlying political realities and are unlikely themselves to be the causes of those realities. To put it more bluntly: returning to an era where committees played an outsized role in the legislative process is unlikely to bring with it a decrease in polarization, but rather would simply reflect already existing member polarization. As is often the case, institutions share the vices and virtues of their members. Forcing more legislation to go through the committee system would not produce less a polarizing legislative process. More directly, it is a less polarized Congress itself that allows the committee system to work, not the other way around.

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