Party and Promotions After the Republican Revolution

William Minozzi*   Matthew P. Hitt†

May 29, 2012

Abstract

We take advantage of a rare shift in institutional rules to provide unambiguous evidence of causal effects of party influence in Congress. After the 1994 Republican Revolution, the new majority party in the US House adopted rules that centralized committee assignments within the party leadership. We build a research design around this event to study how committee promotion affects party support. Using both regression and matching, a series of placebo tests, a sensitivity analysis, and an analysis of the dose-response relationship between the value of the promotion and party support, we find that post-Revolution Republicans who were promoted to more prestigious committees supported the party more than did unpromoted members with statistically indistinguishable preferences and past behavior. These effects are unique to post-Revolution Republicans, robust to substantial relaxation of the key identifying assumption, and specific to the set of votes on which party seems to matter most.

*Assistant Professor, Department of Political Science, 2137 Derby Hall, Ohio State University, Columbus, OH 43210 Phone: 614-247-7017, Email: william.minozzi@gmail.com

†PhD Candidate, Department of Political Science, 2140 Derby Hall, Ohio State University, Columbus, OH 43210 Phone: 720-934-3083, Email: hitt.23@osu.edu

Introduction

Do party leaders in the US House of Representatives wield the levers of institutional power to steer the actions of rank and file members? Or is legislative behavior driven solely by ideological preferences? It can be difficult to offer unambiguous answers to such inherently causal questions, especially in elite institutions. As a general matter, members' preferences almost certainly drive *both* party affiliation *and* legislative behavior (Krehbiel 1993). Thus, any apparent effect of the party on members' behavior may be confounded by the essentially unobservable preferences of those members. As a result, standard analysis of roll call data may be of limited use to identify whether party leaders use their institutional tools to direct the behavior of their members. However, without convincing and unambiguous answers to questions of how party leaders use their power, our understanding of one of the most important elite institutions in the world remains fractured and fundamentally incomplete.

In this paper, we construct and deploy a research design that overcomes the limits of standard analytic techniques. To do so, we focus on a rare instance in which the application of a key lever of party influence by party leaders can effectively be controlled via post hoc means. The resulting design is a close inferential analogue of a randomized controlled trial, and we use the design to study whether party leaders effectively manipulate their members' behavior, above and beyond the effects of preferences.

Specifically, we exploit the structural and institutional consequences of the 1994 Republican Revolution. After the Revolution, Republican Party leaders redesigned the committee assignment system with the explicit intention of rewarding party loyalty.[1] Importantly, we do not focus on the question of *who* gets improved committee assignments, which has been thoroughly studied (see, e.g., Bullock 1976; Shepsle 1978; Krehbiel 1993; Cox and McCubbins 1993; Smith and Deering 1990; Groseclose and Stewart 1998; Frisch and Kelly 2006).

[1]The committee system was also weakened initially, but it was subsequently returned to its former, more powerful role while simultaneously maintaining the loyalty-rewarding qualities of the Republican assignment mechanism.

Rather, we examine the effects of receiving a *promotion*—assignment to a more prestigious committee—on the future behavior of the members who were promoted.

We use the tools of modern causal inference and evaluation research to develop a sophisticated identification strategy. Our goal is to generate unambiguous estimates of whether, all else equal, a party member who is promoted to a better committee supports the party more than those who do not. By "all else equal" we mean that we use both regression and matching to isolate the effect of promotion. To do so, we match promoted party members with unpromoted members who are similar on many potentially meaningful criteria. In so doing, we achieve an impressive degree of balance between our matched sets of promoted and unpromoted legislators, which makes the identifying assumptions that motivate a causal interpretation of the resulting estimates as unambiguous as can reasonably be expected in such an observational study. We augment this strategy with a series of placebo tests and a sensitivity analysis to verify whether the changes in party support we document should be ascribed to promotion decisions made by party leaders. Finally, we examine the "dose"-response relationship between more valuable promotions—those that entailed a jump from a very low prestige committee to a very high prestige committee—and party support.

Using this design, we find that promotion has significant effects on party support among Republicans after the Revolution. We also find that that these effects are limited to Republicans after the Revolution, that they are strongest on the set of votes on which party seems more to matter the most, and that they are robust to considerable relaxations of the crucial assumptions in the identification strategy.

In the next section, we document the inferential problems inherent in the study of party influence. We then describe the research design we use to overcome those problems. Subsequently, we analyze the effects of promotion on party support among Republicans after the Revolution, and proceed to offer several placebo tests, a sensitivity analysis, and an analysis of the "dose"-response relationship. We conclude by arguing that these findings are clear, unambiguous evidence of party influence and cannot be explained by preferences alone.

Challenges in Measuring Party Influence

Many theories of Congress treat the role of the majority party as fundamental. Party cartel and negative agenda control theory (Cox and McCubbins 1993, 2005) depict the majority party as the primary gatekeeper that limits the legislation under consideration. Conditional party government theory (Rohde 1991; Aldrich 1995) sees a strong role for the party as a prime mover of legislation, conditional on the coherence of the individual preferences of its members. And vote buying and valence theories (Groseclose and Snyder 1996; Wiseman 2006) hold that in exceptional cases, party leaders can purchase the support of their members, in either explicitly monetary terms or via electoral support.

In contrast, a simpler theory built on the rudiments of the spatial model and the median voter theorem (Black 1958) renders parties as merely epiphenomenal (Krehbiel 1993). This alternative theory presupposes only that legislators have single-peaked preferences in a single dimensional policy space and that collective choices are made using simple majority rule. This simple idea poses a fundamental challenge to party-based theories of Congress: given that the governing rules of the institution depend on simple majority rule, how could party leaders manage to persistently enact their own minority position (cf. Krehbiel 1991)? The simpler theory is a component of most party-based theories, each of which takes the spatial model as a baseline from which to develop a more complex view of lawmaking. While the existence of parties remains a curiosity in the context of this simple theory, the spatial model retains the potential to explain many, if not most, empirically documented phenomena.

Drawing on the spatial model, Krehbiel (1993) presses this challenge, writing that: "The premise of this study is that one important legislative function of political parties is to govern by passing laws that are different from those that would be passed in the absence of parties" (255). This premise is based on a counterfactual notion of causality (Lewis 1973). As an observer, one can only see the world as it is, and not as it would be under different circumstances. To build a case that a party's role is significant, one must show that things would be different in the absence of some action taken by that party. While existing empirical evidence may be consistent with any particular party-based theory, much of it is also consistent with the simpler, preference-based spatial model. Krehbiel (1999, 2000, 2003, 2007) later sharpens this critique by studying different measures of party influence based on roll call votes. These studies illustrate that apparent party effects are also consistent with purely preference-based voting. In essence, he argues that if a phenomenon can be explained by both the spatial model and a more complicated party-based theory, then we should prefer the simpler of the two. Parsimony, on this view, is the relevant decision criterion.

Why, then, do so many scholars embrace more complicated party-based theories (Binder, Lawrence, and Maltzman 1999; Finocchiaro and Rohde 2008; Gailmard and Jenkins 2007; Jenkins 1999; Jenkins, Crespin, and Carson 2005; Roberts and Smith 2003; Smith 2007; Snyder and Groseclose 2000; Wiseman 2004)? One possibility is that such scholars are implicitly adopting a "quasi-Bayesian" approach. Parties dominate the history of the nation (Aldrich 1995). They seem to play a key role in electoral accountability, both at the individual and aggregate levels (Carson et al. 2010). Indeed, the very fact that parties are limited to mere curiosities in the spatial model may be taken as a mark against it if we expect any complete model of legislative politics to include parties. According to this view and in light of the historical and behavioral sweep of American politics, evidence that is observationally equivalent across the spatial model and more complicated party-based theories ought not count against the party-based theory. If one's "prior" belief is that parties are important, if there exist sensible theories of how and why parties could be important, and if tests of those theories corroborate the hypothesis that parties are important, then the burden ought to shift back to the skeptic. Scholars who acknowledge Krehbiel's skepticism yet persevere nonetheless may appeal to such a counterargument implicitly.

Although this counterargument may be attractive, it leaves nagging doubts. Rather than settling the question, it moves the debate from an empirical question into the realm of philosophy of science. It would be nice to conjure a test that would settle the question more definitively and comprehensively, once and for all. But such a test has not been, *and almost certainly will not be*, forthcoming. Elite political institutions are composed of powerful actors whose goal is to manipulate outcomes. Thus, there are rarely opportunities for causal inference that approach the inferential power of randomized controlled trials. Instead, the ability to provide definitive evidence of party influence is likely to be rare and case-specific. While we cannot offer evidence of party effects with both inferential clarity and historical sweep, we can trade off the latter in return for the former. Thus, we exploit a rare historical event to construct a research design that provides unambiguous causal inferences about party influence.

Party and Promotion After the Revolution

The Republican Revolution of 1994 offers a rare opportunity to search for causal evidence of party influence. For decades prior to 1994, Congress exhibited something approaching a steady state, so much so that some predicted an indefinite era of one-party dominance (Sprague 1981). However, during the early days of the 104th Congress, Newt Gingrich and the new Republican leadership changed the institutional environment of the House. For example, the party leadership expanded its control over the committee assignment process (Aldrich and Rohde 1998, 2000; Evans and Oleszek 1997; Vander Wielen and Smith 2011). Specifically, Gingrich created a powerful Steering Committee that had many responsibilities, including the nominations of Republican committee members (Owens 1997). Additionally, Gingrich tripled the number of votes accorded to the Speaker in committee assignments compared with his Democratic predecessor, Tom Foley (Aldrich and Rohde 1998). While the seniority system was largely respected with regard to committee assignments in prior Congresses, the new leadership deviated from that norm and selected more junior members considered more loyal to the party for a number of plum committee assignments and chairs (Aldrich and Rohde 2000, 15).

The new leadership also placed a greater emphasis on loyalty to the party in floor voting.

Majority Whip Tom Delay met biweekly with his team to keep strict track of the floor votes of the rank and file (Owens 1997, 254). Further, leaders demanded loyal voting records from members with plum committee assignments on the threat of removal (Owens 1997, 250). For the first time in decades, a majority party in Congress asserted central control over committee assignments, deemphasized the countervailing norm of seniority, and kept assiduous track of member behavior. Compared with both the Democratic party and the Republican party in Congresses prior to the 104th, Republicans in the post-104th House experienced a party leadership that explicitly attempted to exert more influence over roll call voting, in part through threats regarding committee assignment. Further, leaders made the threat of removal credible by using their initial assignments to show that loyalty, not seniority, would take precedence for prestigious assignments.

Not all aspects of the chamber changed, which is critical for our empirical analysis. For example, Carson, Monroe, and Robinson (2011) examine roll rates to show that Republican leaders after the Revolution exhibited a similar degree of control over the House agenda as the pre-Revolution Democratic leadership. That said, one important aspect of the institutional structure did seem to undergo a change. During the Gingrich years, committees were made dependent on the central leadership for whipping and floor assistance (Owens 1997, 252). Leaders explicitly instructed committee chairs that whenever their policy goals came into conflict with the goals of the central party leadership, the party's goals must take priority (Aldrich and Rohde 2000, 15). However, following the abrasive and combative Gingrich years, Republicans elected Dennis Hastert of Illinois Speaker. Hastert adopted a more passive and compromising style in direct contrast to Gingrich (Owens 2002, 258). Strahan and Palazzolo (2004) note that under Hastert, committees regained a more prominent role in policymaking. Given that change, prestigious committee assignments regained their pre-Revolutionary value with the advent of the Hastert speakership. The important difference, then, is that the threat of losing an assignment would have concomitantly become more real, and therefore the party could expect its committee promotion to engender increased party support.

Research Design

Our research design focuses on Republican members of the US House during the Hastert speakership, from 1999 to 2006.[2] We estimate the effects of the treatment variable *Promotion*, an indicator that equals 1 if a member's most prestigious committee assignment from a term is better than it was in the previous term, and is 0 otherwise. To measure committee prestige, we use Groseclose and Stewart's (1998) scores, which are based on the revealed preferences of members who move from one committee to another. An average of about 10 Republicans per year received promotions to better committees.[3]

The outcome variables with which we examine the effects of *Promotion* include several measures of party support. Our primary measure is *Party-significant Support*, which is defined as the percentage of the time a member votes with the majority of the party on the set of *party-significant votes*, which are identified and extensively studied in concurrent work (CITE WITHHELD). Roll call votes are categorized as either *party-significant* or *party-insignificant* using an iterative procedure. To begin the procedure, all lopsided votes (i.e., those decided by a margin of at least 65% to 35%) are categorized as "party-significant" and the complementary set of close votes is categorized as "party-significant," following Snyder and Groseclose (2000). In each iteration, ideal points are estimated using the set of votes categorized as party-insignificant. Then, for each roll call vote, the yea/nay decisions of all representatives are regressed on those ideal points and an indicator for party. If the coefficient on party is significant, the roll call vote is then re-categorized as party-significant; otherwise,

[2]We do not include data from the Gingrich speakership for several reasons. First, the party changeover of 1994 resulted in the elimination of some committees, changes in the jurisdiction of others, and, for Republicans, changes in the assignment procedures, as we document in the previous section. Thus, it is not clear that legislators had well-informed preferences over these new committees. Furthermore, because Republicans went from being the minority party to being the majority party, there were instantly a large number of newly vacant seats previously held by Democrats. Accordingly, the promotion rate was about 20% higher in the 104th Congress than in the next highest term in our dataset. A seemingly reasonable alternative would be to include only the second of the two Gingrich terms along with the Hastert speakership. Doing so, however, introduces confounding heterogeneity into the data, that is otherwise easily avoided. Although these concerns lead us to present only the Hastert speakership, including data from the second term of the Gingrich speakership along with the Hastert years yields substantively identical results.

[3]For summary statistics on this and all other variables, see appendix Table A.

the vote is re-categorized as not party-insignificant. This procedure quickly moves away from the initial lopsided/close categorization and is repeated until the vast majority of votes are stably re-categorized.[4] In (CITE WITHHELD), the authors show that the party-significant votes overlap considerably with categories that others have identified as being associated with party influence, including lopsided votes (Snyder and Groseclose 2000), procedural votes (Finocchiaro and Rohde 2008), and the standard *Congressional Quarterly* categorization of votes according to whether majorities of each party vote against each other. Moreover, they show that *Party-significant Support*, the percentage of the time that a member votes with a majority of the party on the set of party-significant votes, is positively associated with ideological extremism.

Party-significant votes comprise precisely those votes we expect party leaders to care about most. However, for completeness, we also examine the effects of *Promotion* on three additional measures of party support. First, we estimate the effects of *Promotion* on *Party-insignificant Support*, the rate at which members vote with the majority of the party on the complementary category identified alongside party-significant votes. Second, we estimate the effects of *Promotion* on *Procedural Support*, the rate at which a member votes with a majority of the party on procedural matters (Finocchiaro and Rohde 2008).[5] Finally, we estimate the effects of *Promotion* on the standard *CQ*-style measure *Party Unity*, the percentage of the time a member voted with a majority of the Republican Party on votes on which majorities of the two parties were opposed. Our expectations for the effects of *Promotion* on these rates are different than for *Party-significant Support* because the latter measure reflects precisely those votes on which party appears to matter most. That said, we certainly do not expect sign reversals on this second set of measures. Rather, we anticipate that the effects of *Promotion* on *Party-insignificant Support*, *Procedural Support*, and *Party Unity* will be positive, yet not as substantial as the effects on *Party-significant Support*. Indeed,

[4]In practice, about 5% of votes are not stably categorized in any given term. These votes are therefore not used to generate these support rates.

[5]To identify procedural votes, we relied on data from Rohde (2004). For the 109th Congress, data were collected by the authors.

significant estimates on these three other measures would necessarily temper the inference that the apparent effects on *Party-significant Support* are causally linked to *Promotion*, and reignite suspicion that unobservable preferences of the members are driving both the putative cause and the apparent effect.

To obtain credible causal estimates of promotion effects, we focus on the most homogeneous subsample of members possible. Therefore, we exclude several groups of members for whom the causal effect of promotion may represent different causal processes. For example, we exclude newly elected party leaders and committee chairs because attaining such a position of power bespeaks a large increase in the ability to direct the agenda. Party leaders exercise obvious control over the agenda, and committee chairs steer many highly important and obligatory pieces of legislation through their committees. Therefore, any apparent effect of *Promotion* on party support in these cases could merely reflect a closer relationship between that member's preferences and the resulting agenda. We also exclude women, African-Americans, and Latinos from the analysis. In this case, there are two separate reasons to exclude these members. First, because the number of members in each group is small, it is essentially impossible to find excellent matches to use as credible counterfactuals. Second, it is possible that phenomena such as tokenism (Kanthak and Krause 2010) play a complex role in the selection of these members for *Promotion*, which places an even greater premium on finding excellent counterfactuals. These exclusions remove systematic, immeasurable, and unobservable biases that would have contaminated our estimates. The estimates for the remaining subset are much more unambiguously the causal effects of *Promotion*.

Analysis

Did Republican Party members who received promotions subsequently support the party more than their unpromoted colleagues? The evidence suggests that they did. Promoted members may have felt that they had more to lose by bucking party leaders. They may therefore have increased their support for the party so as to ensure their newly acquired

Table 1: Estimated Effects of *Promotion* for Republicans, 1999-2004

	Unmatched		**Matched**	
Dependent Variable	*Bivariate*	*Multivariate*	*Bivariate*	*Multivariate*
Party-significant Support				
Promotion Effect	**1.4**	**1.0**	**1.6**	**1.6**
(Standard error)	**(0.6)**	**(0.4)**	**(0.5)**	**(0.5)**
Party-insignificant Support				
Promotion Effect	1.0	0.5	0.9	0.6
(Standard error)	(1.0)	(0.6)	(0.7)	(0.7)
Party Unity				
Promotion Effect	1.5	0.4	1.1	0.9
(Standard error)	(0.9)	(0.5)	(0.6)	(0.6)
Procedural Support				
Promotion Effect	1.1	0.6	0.8	**0.9**
(Standard error)	(0.6)	(0.4)	(0.5)	**(0.4)**
n treated	57	57	57	57
n total	545	545	114	114

Each cell presents an estimate of the effects of *Promotion*. Rows present different outcome variables, and columns present estimates from different methods. Bivariate unmatched estimates were calculated with OLS regression and heteroscedasticity-robust standard errors. Multivariate unmatched estimates use the same technique and include controls for *Previous Seniority*, *Previous Best Committee*, and their interaction; *Previous Contributions*, *Previous Extremism*, and their interaction; *Previous Party Unity*, *Previous Procedural Support*, *Previous Party-insignificant Support*, and *Previous Party-significant Support*; and *Previous DW-Nominate*, *Previous Presidential Vote*, and *Previous Vote Share*. Matching columns refer to genetic matching on these covariates. The bivariate matching column presents differences in means with appropriate standard errors. The multivariate matching column presents multivariate regression estimates on the matched data using the covariates, with heteroscedasticity-consistent standard errors and controls. Estimates in bold are significant at $p < 0.05$, two-tailed.

positions. This support would be especially valuable on the set of votes on which party seems to have mattered the most, which yields the dependent variable *Party-significant Support*. The first column of Table 1 presents simple bivariate regressions of each party support measure on *Promotion* and an intercept.[6] All four estimates are positive, and the estimate for *Party-significant Support* is statistically significant ($p < 0.05$, two-tailed). Although this effect size may seem modest, its magnitude appears more impressive when we dig in on the interpretation. Consider the effect of *Promotion* on *Party-significant Support*, which is a 1.4 percentage point increase on a scale that is already severely skewed to the right. That is, the average value of *Party-significant Support* for post-Revolution Republicans is 92.9. Given that the largest possible value is 100, an increase of 1.4 represents about 20% of the available range.

As expected, the effects of *Promotion* on the three other measures of party support are all positive and of roughly the same magnitude as the effect on *Party-significant Support*. In fact, the estimated effect of *Promotion* on *Party Unity* exceeds that on *Party-significant Support*. This finding actually suggests that we have not yet adequately identified an effect associated with party influence, since we expect the party to value *Party-significant Support* more than on *Party Unity*. Moreover, party members may have been selected for *Promotion* because of leaders' expectations about higher than average party support. Indeed, the Republican Party leadership explicitly intended to reward loyalty with prestigious committee seats. But it is also possible that other attributes explain *Promotion*. For example, more senior party members are unlikely to seek assignments to different committees and may also have cemented solid reputations with voters in their districts. If so, such members might not need the party's help to be reelected, and thus they may feel freer to stray from the party line.

Of course *Promotion* may be associated with other covariates that are in turn associated with these outcomes. Such a chain of association would bias our estimates of *Promotion*

[6] All regression estimates include heteroscedasticity-consistent standard errors, and all p-values are two-tailed.

Table 2: Propensity Score Model of *Promotion* for Republicans, 1999-2006

Covariate	Coefficient	Standard Error
Prev. Seniority	0.60	0.23
Prev. Best Committee	0.10	0.08
Prev. Seniority $\times$ Prev. Best Committee	-0.10	0.02
Prev. Contributions	0.12	0.17
Prev. Extremist	-5.03	2.84
Prev. Contributions $\times$ Prev. Extremist	0.46	0.27
Prev. Party-significant Support	0.22	0.11
Prev. Party-insignificant Support	-0.02	0.06
Prev. Party Unity	-0.10	0.11
Prev. Procedural Support	0.03	0.05
Prev. DW-Nominate	2.17	2.85
Prev. Presidential Vote	0.00	0.03
Prev. Vote Share	-0.01	0.01
Intercept	-14.92	5.54
n	545	
Residual Deviance	237	
Null Deviance	365	

Notes: This table presents a logistic regression model of the promotion decision. Note that there is substantial decrease in Deviance. This is strong evidence of non-random selection, and suggests that the unmatched bivariate estimates of promotion effects from Table 1 are biased. Cells present logistic regression estimates (heteroscedasticity-consistent standard errors).

effects, and so we apply both regression and matching to adjust for several relevant covariates. To identify these covariates, we first build a model of the probability that any particular member of the Republican Party received a *Promotion*, which is sometimes called the propensity score (Rosenbaum and Rubin 1983).[7] First, some members may not seek promotions. Once a member has secured a seat on a committee, he may begin to accumulate expertise and a specialized staff. If that committee is "good enough," he may prefer to remain on that committee rather than seek a promotion. Therefore, we include *Previous Seniority*, *Previous Best Committee*, and their interaction in our propensity score model, to adjust for the varying levels of desire for promotion. In addition to desires, members may also vary in how actively they seek promotions out. In the related context of leadership positions, Heberlig, Hetherington, and Larson (2006) and Kanthak (2007) provide evidence that members effectively pay for leadership positions via cash contributions to other party members. Similarly, members may also attempt to increase the odds of *Promotion* by making cash contributions. Therefore, we also include *Previous Contributions* (in real terms and logged) from members' leadership PACs to other Republican Party members. We further follow Heberlig, Hetherington, and Larson (2006) and include *Previous Extremism* (measured as having a DW-Nominate score more extreme than the party median), as well as the interaction of *Previous Contributions* and *Previous Extremist*, as ideological extremists may have to pay more for *Promotion.* Beyond contributions, leaders may seek to promote loyal members and therefore select on criteria that help characterize loyalty. Therefore, we include for *Previous DW-Nominate* score, and the lagged dependent variables *Previous Party Unity*, *Previous Procedural Support*, and *Previous Party-significant Support* in our propensity score model. Finally, leaders may use committee assignments to help shore up the electoral chances of vulnerable members. Thus, we include *Previous Vote Share* (measured as the fraction of the two-party vote the member won in the most recent election) and *Previous Presidential Vote* (measured as the fraction of the two-party vote the Democratic presidential candidate

[7] To avoid post-treatment bias, we measure each covariate before *Promotion* assignments were made.

won in the most recent presidential election) in the propensity score model.

The best predictors of *Promotion* are *Previous Seniority*, *Previous Best Committee*, and their interaction (see Table 2). Once members have spent a few terms in Congress and achieved relatively high quality assignments, they are far less likely to be awarded a *Promotion*. We also found that *Previous Contributions* is a significant predictor of *Promotion* for *Previous Extremist* party members, which extends the findings in Heberlig, Hetherington, and Larson (2006) to include improved committee assignments. That is, after the Revolution it appears that *Extremist* Republicans had to pay for their committee seats, just as they appear to have paid for leadership positions. Finally, the only roll call-based measure with a significant coefficient is *Previous Party-significant Support*, which indicates that promotions may have been awarded to disproportionately to those members whom the party has been able to count on in the past on precisely those votes where party seems to matter the most. These findings, and especially this last finding, suggest that further attempts to eliminate selection bias via statistical adjustment are highly necessary.

Using the covariates from Table 2, we performed three separate multivariate analyses of *Promotion* effects. Columns 2 through 4 in Table 1 the estimates that result from these methods. First, Column 2 presents a standard linear regression of each outcome variable on *Promotion* and the covariates listed in Table 2. The results of the multivariate regressions are substantively similar to those from the bivariate regression. As above, each estimate is positive, but only the estimated effect of *Promotion* on *Party-significant Support* is statistically significant.

Although regression is familiar and easy to interpret, it also entails two problematic methodological issues. First, regression is parametric; the method conditions for covariates in a strictly linear fashion. Although we have no ex ante reason to expect a nonlinear relationship among the covariates and outcome variables, we also have no ex ante reason to expect those relationships to be linear either. If in fact the relationship between, for example, *Previous Vote Share* and *Party-significant Support* is curvilinear, then the estimates of the causal effects of *Promotion* on *Party-significant Support* will be biased (Achen 2002). The second issue associated with regression is its extrapolation from the available data (Ho et al. 2007). That is, regression offers an estimate of how the average member would respond to a promotion, and when no such members are available (say for very senior members), regression extrapolates how they would respond in a linear fashion. Such extrapolation is especially implausible in this case because many members likely do not seek better committee assignments. In this case, including members who were not promoted and who are fundamentally unlike the set of promoted members will likely downwardly bias the estimates of promotion effects.

To address both of these issues, we match promoted members to unpromoted members using genetic matching (Diamond and Sekhon 2010), which both reduces bias nonparametrically and avoids extrapolation. Essentially, the idea behind matching is to isolate the subset of unpromoted members who are most similar to promoted members, and then to estimate the effect of *Promotion* as the difference between the two matched groups. In the jargon of causal inference, we estimate the average effect of the treatment on the treated (ATT), where the treatment is *Promotion* (Sekhon 2009). We measure similarity in terms of covariate balance.[8] For each covariate, balance is the degree of similarity in the empirical distributions between promoted members and matched unpromoted members. Extrapolation is avoided entirely by excluding unpromoted members who are dissimilar to every promoted member. The crucial assumption here is that we have observed all the covariates that contribute to the assignment of promotions. This assumption is in general untestable, and therefore in the next section we perform a sensitivity analysis to probe the dependence of our inferences on the assumption.

In practice, matching can be infeasible when there are substantial limits on the set of possible matches. However, because there are about 9 times as many unpromoted members as promoted members, we were able to achieve excellent covariate balance between the two

[8] Thus, we did not balance on the propensity score itself, which can hinder covariate balance (Diamond and Sekhon 2010).

groups. After genetic matching, the sets of promoted members and matched unpromoted members are very similar on all the covariates listed above. All matching was done without replacement, meaning that observations were included in the post-matching dataset at most once.[9]

Column 3 presents differences in means for the promoted and matched unpromoted party members. Here, we again find positive effect estimates, yet statistical significance only for *Party-significant Support*. Moreover, the estimated effect of *Promotion* on *Party-insignificant Support* has now dropped to less than a third the magnitude of that on *Party-significant Support*. We take this as evidence that the effects measured using the matching method are clearly attributable to party influence in the committee assignment process.

Finally, we combine these two methods and estimate a multivariate regression model of the outcome variables on the pre-processed matched dataset (Ho et al. 2007). Column 4 presents this final set of estimates and confirms the earlier findings, with one exception. Although the estimated effect of *Promotion* on *Party-significant Support* remains positive and significant, the effect of *Promotion* on *Procedural Support* is now also significant.

In general, the pattern emerging from these analyses is that the effects of *Promotion* are clearest on *Party-significant Support*. The sole case in which *Promotion* has a significant effect on another measure, it is *Procedural Support*, and the effect size is half of that on *Party-significant Support*. Once the data are matched and the focus is shifted exclusively to those members who are most likely to be promoted, the effect sizes on all three other variables decline. We take this as evidence that *Promotion* had a disproportionate impact on precisely those votes on which party seems to have mattered most.

[9]More specifically, we measure balance in terms of the p-values from t-tests and bootstrapped Kolmogorov-Smirnov tests of the covariate equivalence between promoted legislators and the matched subsample of unpromoted legislators. In particular, genetic matching maximizes the minimum such p-value, which is sometimes called the *fitness value*. Using these measures, balance is generally excellent. The worst fitness value we achieve is 0.30, meaning that no hypothesis test of covariate equivalence can be rejected at p lower than 0.30, and for many covariates the corresponding p-value is much higher. Full balance details appear in the Appendix. We used the **R** package **Matching** (Sekhon 2011) for all matching analyses. Although we did allow the genetic matching algorithm to match on the propensity score, we did *not* balance on the propensity score itself.

Placebo Tests and Sensitivity Analysis

Given the assumption that we have adequately explained selection using the observable covariates on which we condition and match, these estimates of *Promotion* effects may be interpreted as causal. However, that assumption is strong, crucial, and untestable in principle. While we cannot directly test the selection assumption, we can perform a series of complementary analyses that serve to deepen its credibility. Throughout, we concentrate on the estimated effects on *Party-significant Support*, the most robust finding from our initial analyses.

We present three placebo tests, each of which is related to an alternative explanation that would cast doubt on a causal interpretation of the estimated effects of *Promotion* on *Party-significant Support*. In matching parlance, the placebo test compares the findings of the analysis to a different stratum of data for which same purported effect cannot logically follow the same causal pathway (Sekhon 2009). First, recall that we have argued that the changes in the committee assignment process resulted in the ability of party leaders to exercise more control over their members in the wake of the Republican Revolution. In contrast, the Democratic Party did not change its committee assignment procedure after 1994. If the causal interpretation of the estimated effects is correct, we should therefore not find significant effects of *Promotion* on *Party-significant Support* for Democrats from the same time span. If we did find significant effects, it would be more likely that we had failed to control for an important covariate associated with *Promotion* during these years.

The first row of Table 3 reports estimates that correspond to the first row of Table 1, but for Democrats from 1999-2006 rather than Republicans. We find no significant effects; in fact, the unmatched estimates for Democrats do not even share the same sign as those for Republicans. We again achieve excellent balance without dropping any observations, yet the matched estimates for Democrats are also nowhere near conventional levels of statistical significance.[10] This first placebo test therefore renders it unlikely that we have failed to

[10]Post-matching, we also checked the covariate balance of an indicator variable for *South* among Democrats,

Table 3: Placebo Tests of *Promotion* Effects

	Unmatched		**Matched**		n	
Dependent Variable	*Bivariate*	*Multivariate*	*Bivariate*	*Multivariate*	Treated	Total
Party-Significant Support						
Democrats (1999-2006)						
Promotion Effect	-0.2	-0.5	0.4	0.3	37	440
(Standard error)	(1.5)	(0.7)	(0.8)	(0.6)		
Democrats (1987-1994)						
Promotion Effect	-0.7	-0.8	-1.2	-0.7	61	583
(Standard error)	(1.4)	(0.7)	(1.0)	(1.0)		
Republicans (1987-1994)						
Promotion Effect	2.1	-0.1	-0.1	-0.1	66	462
(Standard error)	(1.2)	(0.6)	(0.8)	(0.8)		

Each cell presents an estimate of the effects of Committee Promotion. Rows present different placebo test populations, and columns present estimates from different methods. Bivariate unmatched estimates were calculated with OLS regression and heteroscedasticity-robust standard errors. Multivariate unmatched estimates use the same technique and include controls for *Previous Seniority*, *Previous Best Committee*, and their interaction; *Previous Contributions*, *Previous Extremism*, and their interaction; *Previous Party Unity*, *Previous Procedural Support*, *Previous Party-insignificant Support*, and *Previous Party-significant Support*; and *Previous DW-Nominate*, *Previous Presidential Vote*, and *Previous Vote Share*. Matching columns refer to genetic matching on these covariates. The bivariate matching column presents differences in means with the appropriate standard errors. Finally, the multivariate matching column presents regression estimates on the matched data, with heteroscedasticity-consistent standard errors and controls for the covariates. No effects are significant at $p < 0.05$, two-tailed.

control for an important covariate common to the time period.

Our next two placebo tests allow us to probe whether there we may have failed to control for some attribute associated with, respectively, the majority party or the Republican Party. To do so, we again replicate our analysis first using the set of Democratic Party members from 1987-1994 and then the set of Republicans from that era. The next two rows of Table 3 present these results. Again, we find no estimates that are significant at the $p = 0.05$ level, although the unmatched bivariate regression estimate for Republicans during this earlier era grazes conventional levels. On balance, these three placebo tests indicate that it is very unlikely that we have excluded a systematically important covariate from our analyses.

Even though the placebo tests suggest that we have properly controlled for selection, it remains possible that we have missed a covariate (or set of covariates) that the post-Revolution Republican leaders used when assigning *Promotion*. Therefore, our final step is to subject the bivariate matching estimate for *Party-significant Support* to a sensitivity analysis (Rosenbaum 2002).[11] In the sensitivity analysis, we essentially hypothesize the existence of that unobserved covariate. Hypothetically, this covariate is directly associated with *Party-significant Support*, and so it may bias our estimates of *Promotion* effects away from zero in the direction of our estimate. Next, we postulate an association between this hypothetical covariate and assignment to *Promotion*. Put another way, we assume that the party members who were actually promoted were more likely to have been promoted than those who were not, and that the reason for the difference in likelihood is precisely measured by this generic unobserved covariate. Finally, we retest whether *Promotion* still has a significant effect on *Party-significant Support*, even under these adverse conditions. By repeating this analysis for different postulated levels of associations, we discover how deep such an association must have been to render the corresponding estimate statistically insignificant. The sensitivity analysis, then, is a measure of the postulated association at which the *Promotion* effect on *Party-significant Support* becomes insignificant.

and found a very high level of balance. See the Appendix for details.

[11] We used the **R** package **rbounds** (Keele 2010) for the sensitivity analysis.

Because *Promotion* is a binary variable, a useful measure of the association between the unobserved covariate and *Promotion* is the odds-ratio. In this case, the effect of *Promotion* on *Party-significant Support* becomes insignificant at an odds-ratio of 1.7. That is, the estimated effect of *Promotion* could be explained away if the party members who actually were promoted were at least 1.7 times as likely as members who were not promoted because of some variable or set of variables not included in our slate of covariates. To put this in perspective, it is helpful to consider this odds-ratio in more substantive terms. To that end, reconsider the logit model of *Promotion* in Table 2. An odds-ratio of 1.7 corresponds to a logit coefficient of about 0.5. Recall that in Table 2 we found that an extremist Republican Party member would be more likely to be promoted if he contributed more cash to his fellow party members campaign committees. In fact, the coefficient on *Previous Contribution* for *Extremists* is about 0.5. Thus, our sensitivity analysis shows that, in order for the estimated effect of *Promotion* on *Party-significant Support* to be explained by an unobserved covariate, that covariate would need to be associated with *Promotion* at about the same rate as a one-point increase in *Previous Contribution* among *Extremists.* Since *Previous Contribution* is logged, that means the estimated effect *Promotion* can only be explained away by omitting a covariate at least as effective at achieving *Promotion* as an extremist party member who *doubled* his cash contributions to other party members, which would represent a substantial omission. Moreover, this hypothetical generic covariate would need contribute unfailingly to increased *Party-significant Support.* In combination, the three placebo tests and the sensitivity analysis lead us to confidently interpret the estimated effects of *Promotion* on *Party-significant Support* as unambiguously causal.

"Dose" and Response

Finally, we further scrutinize the inference that *Promotion* specifically leads to increases in *Party-significant Support* by taking account of variation within the promotions themselves. Some promoted members moved to a committee of nearby prestige (in Groseclose-Stewart terms), for example moving from a committee of rank n to rank $n-1$. Others made much larger jumps, at times jumping as many as 15 slots. Our final analysis takes advantage of this variation to explore a "dose"-response relationship between the dose—the *Jump* in prestige associated with the *Promotion*—and the corresponding difference in party support on the various sets of votes.

We utilize the matched pairs we generated with genetic matching to generate estimates of the median dose-response relationship and confidence intervals for each measure of party support. Specifically, we utilize the technique of inverting a hypothesis test (Rosenbaum 2002) to determine which coefficient values on *Jump* we cannot reject given our data. That is, suppose that for each committee slot that a promoted member jumps, his rate of party support increases by b percentage points. We can test this hypothesis by calculating an adjusted party support score for each unpromoted member in each matched pair. To do so, we add the unpromoted member's party support score to his matched promoted member's *Jump* value times b. The resulting adjusted party support score is the counterfactual. Then, we test the null hypothesis that the party support scores for promoted members and the adjusted party support scores for unpromoted members are equal.[12] By doing this sequentially for different values of b, we form a confidence interval of values that we cannot reject for the dose-response relationship. We also report the Hodges-Lehmann point estimate for b (Rosenbaum 2002).

Table 4 presents the analysis of dose and response for Republicans after the Revolution. As is clear, the 95% confidence interval for *Party-significant Support* is the only one to exclude 0.[13] And the point estimate for *Party-significant Support* is almost three times as large the largest of the other three. This dose-response analysis helps clearly identify the effects of *Promotion* as tightly constrained to *Party-significant Support*. Furthermore, Table

[12]There is a further complication posed by the ceiling of 100 percent on party support rates. That is, for large values of b and large *Jump* values, some unpromoted members may have adjusted party support scores in excess of 100. To accommodate this issue, we use the minimum of 100 and the unpromoted member's adjusted party support score. Rosenbaum (2002) refers to the estimates that results as Tobit effects.

[13]In fact, the 90% confidence intervals also include 0 for the three measures of party support.

Table 4: Dose-Response Effects of *Promotion*

Dependent Variable	*Median Effect*	*95% Confidence Interval*
Republicans (1999-2006)		
Party-significant Support	0.23	[0.08, 0.44]
Party-insignificant Support	0.06	[-0.12, 0.31]
Party Unity	0.01	[-0.07, 0.17]
Procedural Support	0.08	[-0.05, 0.24]
Democrats (1999-2006)		
Party-significant Support	0.09	[-0.22, 0.30]
Republicans (1987-1994)		
Party-significant Support	-0.01	[-0.22, 0.19]
Democrats (1987-1994)		
Party-significant Support	-0.14	[-0.42, 0.10]

Median Effect is a Hodges-Lehmann point estimate of the dose-response relationship between the *Jump* associated with a *Promotion* and each measure of party support. Confidence intervals are calculated by inverting hypotheses based on the Wilcoxon signed-rank statistic.

4 also shows the dose-response analysis as applied to the three placebo cases of minority party Democrats from after the Republican Revolution and both Republicans and Democrats from before the Revolution. In each case, the confidence interval includes 0, and the two point estimates for the cases from before the Revolution are actually negative. This dose-response analysis of the placebo cases deepens the inferential impact of the finding for post-Revolution Republicans and *Party-significant Support*.

We can also use Table 4 to revisit the substantive impact of these effects. Given that the largest *Jump* associated with a *Promotion* is 15, the median estimate of 0.23 means that such promotions entail about a 3.5 percentage point increase in *Party-significant Support*. Indeed, the average level of *Previous Party-significant Support* for promoted members is about 94%, which means that the largest promotions are associated with increases in *Party-significant Support* that are more than half of the available slack.

Conclusion

Using modern tools of causal inference, we have shown that after the 1994 Republican Revolution, promotions to more prestigious committees caused Republicans to increase their support for the party. This increase in support was specific to the set of roll call votes on which there is evidence that party is associated with individual voting above and beyond preferences. These results are robust to method and to relaxing the key identifying assumption. Consistent with anecdotal evidence about how Republican Party leaders altered the institution, these findings are specific to Republicans of the era.

Given the specificity of this finding and the exacting nature of the research design, it is difficult to explain the difference in *Party-significant Support* based on *Promotion* among post-Revolution Republicans based on a theory of preferences alone. The primary reason is that we have matched legislators on several different measures of preferences based on their previous roll call records. The resulting matched groups are very well balanced on these measures. We have excluded newly appointed party leaders and committee chairs, and so the difference in *Party-significant Support* apparently due to *Promotion* cannot be attributed to differences in agenda control. The placebo tests using Republicans from an earlier era and Democrats further demonstrate that a preference-based theory will not suffice. The sensitivity analysis indicates that we would have had to omit a large and substantial covariate in order to explain away our findings. And the dose-response analysis indicates that members who received better promotions rewarded the party more by increasing their *Party-significant Support* rates more.

The research design we develop may serve as a model for future work on party influence in of elite institutions like the House. Such institutions present ubiquitous inferential problems that are not easily resolved, but they do not lack for interesting causal questions. For example, one could use this sort of research design to explore the causal effects of campaign contributions, legislative scheduling priority, or other desirable products of institutional power.

Moreover, such an analysis helps identify those cases in which the available evidence simply will not permit unambiguous causal inferences. That is, in the case of promotion, there happens to have been substantial overlap in the qualities of the promoted and unpromoted members, so much so, that we can confidently interpret differences among matched sets of these members as evidence of causal effects. In other cases, sufficient overlap may not exist. Without this kind of research design, it is difficult to even identify the kind of questions that can be confidently answered, much less to answer those questions unambiguously.

The constellation of evidence we present suggests that parties must influence their members, at least somewhat. It is not clear that a preference-based theory could explain these findings. Although this evidence cannot and should not be taken as "proof" of any particular party-based theory of Congress, it does suggest that the parsimonious, preference-based, spatial model is fundamentally incomplete. The parsimony of the simple spatial model is valuable only insofar as it explains the extant empirical evidence from studies of Congress. It is entirely possible that the theories of party cartels, agenda control, and conditional party government are overly simple descriptions of a complex institution. It would not be too surprising if future studies that focused on producing unambiguous causal effects of party influence found evidence that complicates or even contradicts these accounts. Thus, scholars should aggressively seek creative opportunities for research designs that can generate compelling evidence of causal effects.

References

Achen, Christopher H. 2002. "Toward a New Political Methodology: Microfoundations and ART." *Annual Review of Political Science* 5: 423–450.

Aldrich, John H. 1995. *Why Parties?: The Origin and Transformation of Political Parties in America*. Chicago: University of Chicago Press.

Aldrich, John H., and David W. Rohde. 1998. "The Transition to Republican Rule in the House: Implications for Theories of Congressional Politics." *Political Studies Quarterly* 112(4): 541–567.

Aldrich, John H., and David W. Rohde. 2000. "The Republican Revolution and the House Appropriations Committee." *Journal of Politics* 62(1): 1–33.

Binder, Sarah A., Eric D. Lawrence, and Forrest Maltzman. 1999. "Uncovering the hidden effect of party." *Journal of Politics* 61(03): 815–831.

Black, Duncan. 1958. *The Theory of Committees and Elections*. Cambridge: Cambridge University Press.

Bullock, Charles S., III. 1976. "Motivations for US Congressional Committee Preferences: Freshmen of the 92nd Congress." *Legislative Studies Quarterly* pp. 201–212.

Carson, Jamie L., Gregory Koger, Matthew J. Lebo, and Everett Young. 2010. "The Electoral Costs of Party Loyalty in Congress." *American Journal of Political Science* 54(3): 598–616.

Carson, Jamie L., Nathan Monroe, and Gregory Robinson. 2011. "Unpacking Agenda Control in Congress: Individual Roll Rates and the Republican Revolution." *Political Research Quarterly* 64: 17–30.

Cox, Gary W., and Mathew D. McCubbins. 1993. *Legislative Leviathan: Party Government in the House*. Berkeley: University of California Press.

Cox, Gary W., and Mathew D. McCubbins. 2005. *Setting the Agenda: Responsible Party Government in the U.S. House of Representatives*. New York: Cambridge University Press.

Diamond, Alexis, and Jasjeet S. Sekhon. 2010. "Genetic Matching for Estimating Causal Effects." Working paper.

Evans, C. Lawrence, and Walter J. Oleszek. 1997. *Congress Under Fire: Reform Politics and the Republican Majority*. Boston: Houghton Mifflin.

Finocchiaro, Charles J., and David W. Rohde. 2008. "War for the Floor: Agenda Control and the Relationship Between Conditional Party Government and Cartel Theory." *Legislative Studies Quarterly* XXXIII: 35–61.

Frisch, Scott A., and Sean Q Kelly. 2006. *Committee Assignment Politics in the U.S. House of Representatives*. Norman, OK: University of Oklahoma Press.

Gailmard, Sean, and Jeffery A. Jenkins. 2007. "Negative Agenda Control in the Senate and House: Fingerprints of Majority Party Power." *Journal of Politics* 69(3): 689–700.

Groseclose, Tim, and Charles Stewart, III. 1998. "The Value of Committee Seats in the House, 1947-91." *American Journal of Political Science* 42(2): 453–474.

Groseclose, Tim, and Jr. Snyder, James M. 1996. "Buying Supermajorities." *American Political Science Review* pp. 303–315.

Heberlig, Eric, Marc Hetherington, and Bruce Larson. 2006. "The Price of Leadership: Campaign Money and the Polarization of Congressional Parties." *Journal of Politics* 68(4): 992–1005.

Ho, Daniel E., Kosuke Imai, Gary King, and Elizabeth A. Stuart. 2007. "Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference." *Political Analysis* 15(3): 199–236.

Jenkins, Jeffery A. 1999. "Examining the Bonding Effects of Party: A Comparative Analysis of Roll-Call Voting in the US and Conferderate Houses." *American Journal of Political Science* 43: 1144–65.

Jenkins, Jeffery A., Michael H. Crespin, and Jamie L. Carson. 2005. "Parties as Procedural Coalitions in Congress: An Examination of Differing Career Tracks." *Legislative Studies Quarterly* 30(3): 365–389.

Kanthak, Kristin. 2007. "Crystal Elephants and Committee Chairs: Campaign Contributions and Leadership Races in the U.S. House of Representatives." *American Politics Research* 35(3): 389–406.

Kanthak, Kristin, and George A. Krause. 2010. "Valuing Diversity in Political Organizations: Gender and Token Minorities in the U.S. House of Representatives." *American Journal of Political Science* 54(4): 839–854.

Keele, Luke. 2010. "An Overview of rbounds: An R package for Rosenbaum Sensitivity Analysis with Matched Data." Working paper.

Krehbiel, Keith. 1991. *Information and Legislative Organization*. Ann Arbor, MI: University of Michigan Press.

Krehbiel, Keith. 1993. "Where's the Party?" *British Journal of Political Science* 23: 235–266.

Krehbiel, Keith. 1999. "The Party Effect from A to Z and Beyond." *Journal of Politics* 61: 832–840.

Krehbiel, Keith. 2000. "Party Discipline and Measures of Partisanship." *American Journal of Political Science* 44: 212–27.

Krehbiel, Keith. 2003. "The Coefficient of Party Influence." *Political Analysis* 11: 95–103.

Krehbiel, Keith. 2007. "Partisan Roll Rates in a Nonpartisan Legislature." *Journal of Law, Economics, and Organization* 23: 1–23.

Lewis, David. 1973. "Causation." *Journal of Philosophy* 70(17): 556–567.

Owens, John E. 1997. "The Return of Party Government in the US House of Representatives: Central Leadership - Committee Relations in the 104th Congress." *British Journal of Political Science* 27(2): 247–272.

Owens, John E. 2002. "Late Twentieth Century Congressional Leaders as Shapers of and Hostages to Political Context: Gingrich, Hastert, and Lott." *Politics and Policy* 30(2): 236–281.

Roberts, Jason M., and Steven S. Smith. 2003. "Procedural Contexts, Party Strategy, and Conditional Party Voting in the U.S. House of Representatives, 19712000." *American Journal of Political Science* 47: 305–17.

Rohde, David W. 1991. *Parties and Leaders in the Postreform House.* Chicago: University of Chicago Press.

Rohde, David W. 2004. "Roll Call Voting Data for the United States House of Representatives, 1953-2002." Dataset Compiled by the Political Institutions and Public Choice Program, Michigan State University, East Lansing.

Rosenbaum, Paul R. 2002. *Observational Studies.* 2nd ed. New York: Springer.

Rosenbaum, Paul R., and Donald B. Rubin. 1983. "The central role of the propensity score in observational studies for causal effects." *Biometrika* 70(1): 41–55.

Sekhon, Jasjeet S. 2009. "Opiates for the Matches: Matching Methods for Causal Inference." *Annual Review of Political Science* 12: 487–508.

Sekhon, Jasjeet S. 2011. "Multivariate and Propensity Score Matching Software with Automated Balance Optimization: The Matching Package for R." *Journal of Statistical Software* 42(7).

Shepsle, Kenneth A. 1978. *The Giant Jigsaw Puzzle: Democratic Committee Assignments in the Modern House.* Chicago: University of Chicago Press.

Smith, Steven S. 2007. *Party Influence in Congress.* New York: Cambridge University Press.

Smith, Steven S., and Christopher J. Deering. 1990. *Committees in Congress.* 2nd ed. Washington, D.C.: CQ Press.

Snyder, James M., Jr., and Timothy Groseclose. 2000. "Estimating Party Influence in Congressional Roll-Call Voting." *American Journal of Political Science* 44: 193–211.

Sprague, John. 1981. "One-Party Dominance in Legislatures." *Legislative Studies Quarterly* 6(2): 259–285.

Stewart, Charles III, and Jonathan Woon. 2009. "Congressional Committee Assignments, 103rd to 111th Congresses, 1993-2009: House.".

Strahan, Randall, and Daniel J. Palazzolo. 2004. "The Gingrich Effect." *Political Science Quarterly* 119(1): 89–114.

Vander Wielen, Ryan J., and Steven S. Smith. 2011. "Majority Party Bias in U.S. Congressional Conference Committees." *Congress and the Presidency* 38(3): 271–300.

Wiseman, Alan E. 2004. "Tests of Vote-Buyer Theories of Coalition Formation in Legislatures." *Political Research Quarterly* 57(3): 441–450.

Wiseman, Alan E. 2006. "A Theory of Partisan Support and Entry Deterrence in Electoral Competition." *Journal of Theoretical Politics* 18(2): 123–58.

Appendix

Table 5: Summary Statistics

	1999-2006		1987-1994	
Variables	Republicans	Democrats	Republicans	Democrats
Promotion[a]	0.1	0.1	0.1	0.1
	(0.3)	(0.3)	(0.4)	(0.3)
Party-significant Support[c]	92.9	88.7	81.5	82.9
	(4.5)	(8.9)	(9.1)	(10.6)
Party-insignificant Support[c]	86.9	84.3	80.2	88.0
	(6.9)	(7.4)	(7.8)	(6.1)
Party Unity[b]	92.0	86.6	81.0	85.9
	(6.6)	(12.0)	(14.5)	(11.3)
Procedural Support[b]	96.2	89.8	82.8	92.4
	(4.3)	(8.3)	(12.7)	(7.1)
Best Committee[a]	17.5	16.0	16.0	16.8
	(4.1)	(5.3)	(5.0)	(4.6)
Seniority[d]	5.1	6.7	5.7	5.8
	(2.9)	(4.3)	(3.2)	(3.2)
Contributions (logged)[e]	10.0	9.6	3.9	4.4
	(2.2)	(2.9)	(3.7)	(3.9)
Extremist[a]	0.5	0.4	0.5	0.5
	(0.5)	(0.5)	(0.5)	(0.5)
DW-Nominate[b]	0.4	-0.4	0.3	-0.3
	(0.2)	(0.2)	(0.2)	(0.2)
Presidential Vote[d]	57.0	57.2	60.7	48.8
	(8.0)	(10.7)	(7.9)	(9.6)
Vote Share[d]	69.5	70.2	69.7	71.7
	(12.6)	(12.9)	(13.2)	(14.2)

Notes: Sample means (sample standard deviations)
[a] Based on Stewart and Woon (2009)
[b] Constructed using data provided by Keith Poole, Rohde (2004), and collected by the authors
[c] Constructed by (CITE WITHHELD)
[d] Constructed using data provided by Craig Volden
[e] Constructed using data provided by Federal Elections Commission

Table 6: Balance Statistics for Republicans, 1999-2006

Covariate	Before matching		After matching	
	t-stat p-value	KS p-value	t-stat p-value	KS p-value
Prev. Party-significant Support	0.02	0.02	0.99	0.97
Prev. Party-insignificant Support	0.76	0.87	0.52	0.76
Prev. Party Unity	0.06	0.01	0.44	0.45
Prev. Procedural Support	0.34	0.01	0.52	0.32
Prev. Seniority	0.00	0.00	0.31	0.50
Prev. Best Committee	0.00	0.00	0.30	0.83
Prev. Seniority × Prev. Best Committee	0.18	0.18	0.72	0.74
Prev. Contributions	0.27	0.31	0.81	0.32
Prev. Extremist	0.84	—	0.32	—
Prev. Contributions × Prev. Extremist	0.00	0.00	0.84	1.00
Prev. DW-Nominate	0.55	0.29	0.30	0.59
Prev. Presidential Vote	0.31	0.72	0.31	0.89
Prev. Vote Share	0.99	0.94	0.52	0.65

Notes: Low values indicate worse balance on the relevant covariate. The KS p-values are based on bootstrapped Kolmogorov-Smirnov statistics with 5000 resamples.

Table 7: Balance Statistics for Democrats, 1999-2006

Covariate	Before matching		After matching	
	t-stat p-value	KS p-value	t-stat p-value	KS p-value
Prev. Party-significant Support	0.90	0.30	0.96	0.49
Prev. Party-insignificant Support	0.34	0.62	0.72	0.68
Prev. Party Unity	0.95	0.90	0.99	0.70
Prev. Procedural Support	0.17	0.68	0.84	0.68
Prev. Seniority	0.00	0.00	0.85	0.75
Prev. Best Committee	0.00	0.00	0.54	0.69
Prev. Seniority × Prev. Best Committee	0.19	0.31	0.77	0.67
Prev. Contributions	0.84	0.57	0.64	0.86
Prev. Extremist	0.25	–	1.00	–
Prev. Contributions × Prev. Extremist	0.00	0.00	0.88	0.93
Prev. DW-Nominate	0.30	0.36	0.56	1.00
Prev. Presidential Vote	0.78	0.34	0.69	0.49
Prev. Vote Share	0.57	0.83	0.60	0.94

Notes: Low values indicate worse balance on the relevant covariate. The KS p-values are based on bootstrapped Kolmogorov-Smirnov statistics with 5000 resamples. We further checked balance on an indicator for South, and found and after-matching t-test p value of 0.74, indicating balance well within the acceptable range.

Table 8: Balance Statistics for Democrats, 1987-1994

	Before matching		After matching	
Covariate	t-stat p-value	KS p-value	t-stat p-value	KS p-value
Prev. Party-significant Support	0.93	0.80	0.48	0.48
Prev. Party-insignificant Support	0.17	0.12	0.77	0.80
Prev. Party Unity	0.63	0.89	0.74	0.91
Prev. Procedural Support	0.33	0.04	0.37	0.26
Prev. Seniority	0.00	0.00	0.24	0.63
Prev. Best Committee	0.00	0.00	0.24	0.59
Prev. Seniority × Prev. Best Committee	0.66	0.49	0.48	0.98
Prev. Contributions	0.19	0.42	0.45	0.97
Prev. Extremist	0.74	—	0.32	—
Prev. Contributions × Prev. Extremist	0.00	0.00	0.30	0.85
Prev. DW-Nominate	0.27	0.44	0.75	1.00
Prev. Presidential Vote	0.05	0.07	0.33	0.48
Prev. Vote Share	0.69	0.39	0.61	0.31

Notes: Low values indicate worse balance on the relevant covariate. The KS p-values are based on bootstrapped Kolmogorov-Smirnov statistics with 5000 resamples. We further checked balance on an indicator for South, and found and after-matching t-test p value of 0.39, indicating balance well within the acceptable range.

Table 9: Balance Statistics for Republicans, 1987-1994

	Before matching		After matching	
Covariate	t-stat p-value	KS p-value	t-stat p-value	KS p-value
Prev. Party-significant Support	0.02	0.06	0.77	0.99
Prev. Party-insignificant Support	0.00	0.00	0.68	0.68
Prev. Party Unity	0.00	0.02	0.45	0.69
Prev. Procedural Support	0.00	0.02	0.73	0.99
Prev. Seniority	0.00	0.00	0.34	0.38
Prev. Best Committee	0.00	0.00	0.35	0.78
Prev. Seniority × Prev. Best Committee	0.13	0.43	0.68	0.93
Prev. Contributions	0.72	0.34	0.51	0.42
Prev. Extremist	0.16	—	0.53	—
Prev. Contributions × Prev. Extremist	0.00	0.00	0.38	0.60
Prev. DW-Nominate	0.31	0.39	0.37	0.48
Prev. Presidential Vote	0.08	0.24	0.39	0.82
Prev. Vote Share	0.09	0.07	0.49	0.60

Notes: Low values indicate worse balance on the relevant covariate. The KS p-values are based on bootstrapped Kolmogorov-Smirnov statistics with 5000 resamples.