From Goals to Actions: 
The Dynamics of Cosponsorship Reconsidered*

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Abstract

Scholars generally agree that a legislator’s primary goal is reelection, but they lack consensus over how members go about accomplishing this objective. While some posit that legislators appeal to their constituents for support, others maintain that they strategically interact with their colleagues. Different theories of cosponsorship are a prime example of where such competing external and internal perspectives have been proposed. One view, matching, deems cosponsorship to be position-taking for the constituency, so that legislators tend to cosponsor proposals close to their ideologies. An alternative, signaling, considers cosponsorship to be an opportunity for legislators to communicate with their colleagues, so that early cosponsors are more likely to be extreme ideologically. However, to date, we lack compelling evidence as to which perspective holds up empirically. To provide a more definitive test of these alternative perspectives, we estimate a temporal model of cosponsorship behavior for the 106th House. While similar to Kessler and Krehbiel (1996), we employ a more expansive approach and improved measurement and research design. In dramatic contrast to earlier findings that support signaling, we uncover strong and robust evidence for matching. Members ideologically predisposed to a proposal cosponsor more and, if anything, earlier, as their incentives produce externally-oriented behavior. While strategic information transmission to enhance electoral prospects may occur in the U.S. Congress, cosponsorship is not the mechanism.
Legislators lead complicated lives with myriad obligations and considerations. While representatives may be primarily reelection oriented, it is uncertain whether some actions—particularly those that constituents might find difficult to observe directly—are motivated by overtly electoral or by institutional considerations that may indirectly affect their electoral fortunes. Put differently, specifying how legislator objectives, even when agreed upon, translate into observed behaviors is frequently problematic.

Cosponsorship is an excellent example of a choice where the relationship between goals and actions is murky. Although a fair amount of work specifies the covariates of cosponsorship, there is no consensus regarding what inferences should be drawn from such results.

For our purposes, we distinguish between two views—matching and signaling—each positing a different relationship between reelection desires and behavior. Analogous to random utility models of roll-call voting that are essentially decision-theoretic (e.g., Poole and Rosenthal 1997, Rothenberg and Sanders 1999), the key prediction from the matching perspective is that cosponsorship is a function of the correspondence between the proposal and member preferences and of the costs and benefits of the participatory act (e.g., Campbell 1982; Wilson and Young 1997; Balla and Nemachek 2000; Martin and Wolbrecht 2000; Koger 2003; Crisp, Kanthak, and Leijonhufvud 2004). Consequently, externally-oriented, reelection-minded legislators will support proposals close to their ideal points as conditioned by factors influencing their costs and benefits of participating.

The principal intuitive reason to think that cosponsorship might be externally-driven is that, even though the average citizen may be unaware of her legislator’s cosponsorship choices,
elites may know (Ragsdale and Cook 1987). As a result, not only might campaign contributors be influenced by such decisions, but supporters or opponents could cast cosponsorship choices as political issues in electoral campaigns. Indeed, there are many instances of legislators and their opponents either trumpeting or deriding cosponsorship choices. Consider the 1996 and 1998 races for the Pennsylvania 13th District seat. The 13th was a divided district that appeared to be moving in a Democratic direction but had a Republican incumbent in Representative Joe Fox. In 1996, Fox prevailed over Democrat Joe Hoeffel by a mere 84 votes. Two years later, when the same two candidates were rematched in another apparent toss-up election, the American Medical Association Political Action Committee spent $450,000 on a television campaign praising Fox’s cosponsorship of the Patient Bill of Rights in an ultimately unsuccessful attempt–Fox lost by less than 100,000 votes–to salvage his reelection (Colodny, Suarez, and Rodrigues 1999).

By contrast, the signaling perspective is rooted in game-theoretic models of signaling with imperfect information, particularly as applied to legislatures (e.g., Crawford and Sobel 1982; Gilligan and Krehbiel 1989).1 Thus, scholars such as Kessler and Krehbiel (1996; see also Krehbiel 1995, Gilligan and Krehbiel 1997, Wawro 2000) maintain that, rather than position-taking for external consumption, cosponsorship choices involve strategical information transmission to fellow legislators. The most notable predictions stemming from this perspective are temporal: Extremists are more likely to cosponsor early in a proposal’s evolution, as their

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1While some scholars describe any cosponsorship that might provide information as signaling, for conceptual clarity we use signaling’s game-theoretic definition which, to reiterate, also distinguishes institutional from electoral explanations. For instance, while Wilson and Young (1997) place three alternative determinants of cosponsorship under the signaling rubric—bandwagon, ideological, and expertise—for our purposes bandwagon and ideological determinants are subsumed by matching (we will discuss bandwagon effects in our empirical analysis) while expertise is consistent with signaling.
actions provide more informational content than those of moderates, and extremist-moderate differences diminish over the proposal consideration period (matching implying no such relationship).

Like matching, there is a *prima facie* case for assuming that cosponsoring is internally-driven. The flip side of what was discussed above is that, by and large, cosponsorship decisions fly under the electoral radar and are principally observed by legislative insiders. Despite the occasional exception, only a very few of perhaps 120,000 affirmative cosponsorships in a given Congress ever go public. Additionally, it is descriptively the case that proposals with more cosponsors are more likely to be passed and, as such, members who wish to be successful have reason to think cosponsorship is important (e.g., Browne 1985).

Given these two competing views and the intuitive cases for both, our analysis reexamines the matching versus signaling debate—and, by extension, how legislative goals translate into action—by specifying and estimating a temporal model of cosponsorship for the 106th Congress (1999-2000). While similar in important ways to Kessler and Krehbiel (1996), our research design is substantially different and improved in terms of scope, measurement,

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2This claim stems from the idea of confirmatory signaling, by which members have greater credibility when providing information supporting proposals far from their ideal point (e.g., Krehbiel 1991). Regarding cosponsorship, this application seemingly involves two logical steps: (1) analogous to why creating heterogeneous rather than homogenous committees better provides information about and generates support for proposals, extremist cosponsorship furnishes more information than centrist cosponsorship; and (2) as extremists derive greater benefit from cosponsoring, they will more readily incur its costs generally and endure them earlier specifically, inducing House members to cosponsor in greater numbers and especially to be initial cosponsors, producing a temporal pattern in which moderates are relative latecomers. These empirical predictions associated with signaling are not directly derived from a model of cosponsorship behavior per se, no less from a dynamic model (Kessler and Krehbiel 1996), and are best considered as coming from a heuristic, albeit one that has had a powerful hold on contemporary scholarship.
specification, and estimation. Hence, the quality of the tests of competing hypotheses is superior to previous work. For example, rather than examining a small subset of proposals, we study the universe of proposals for an entire Congress and differentiate between the types of proposals that would be expected to be more and less subject to internal information transmission (e.g., proposal complexity). Similarly, our model specification incorporates a broader array of factors that analyses of legislative decision-making suggest are potentially important determinants of member behavior—committee memberships, retirement status, and partisanship among them—and our measurement instruments are significantly more precise, most dramatically because we directly include where proposals fit in the policy space rather than implicitly assume that all are located at the legislative median. Finally, in contrast to previous work, we utilize a flexible estimation technique that allows us to examine the determinants of cosponsorship in a continuous fashion, without restrictive assumptions, over a proposal’s lifespan.

Our results provide strong, unambiguous, and robust evidence that legislators engage in external matching rather than in internal signaling. Although there is a liberal bias toward cosponsoring, everything being equal, those whose ideal points roughly correspond to the proposal in question dominate cosponsorship activity. This is especially true early in the legislative process: Later, as momentum grows, there is somewhat of a bandwagon effect by which some of those less naturally supportive, again particularly liberals, come on board, presumably largely to curry constituent favor.

Our analysis proceeds in four steps. First, in lieu of a standard literature survey, we briefly review Kessler and Krehbiel (1996), whose temporal dimension is unique and whose attempt to delineate clear, testable, competing hypotheses corresponds to our analysis, and
expound on how the hypotheses associated with matching and signaling may be directly tested. After we specify an empirical model to test these alternative perspectives, we estimate event history models of cosponsorship to distinguish which view best characterizes the underlying dynamic process. Finally, we present various robustness tests to determine if our initial results continue to receive support.

**Improving the Model**

While the matching and signaling perspectives, or at least the hypotheses associated with each, may seem intuitive and straightforward, distinguishing between them empirically is not so simple. As such, rather than reviewing the cosponsorship literature generally (which has grown considerably in recent years), we detail the Kessler and Krehbiel (1996) approach because it most clearly contrasts the two perspectives and most closely corresponds to our analysis.³

Specifically, Kessler and Krehbiel collected data on the 51 proposals in the 103rd Congress (1993-1994) with at least 50 cosponsors. Proposal consideration is divided into three temporal periods, with each member-proposal combination categorized according to whether there was (0) no action over all three periods; (1) early period action; (2) intermediate period action; or (3) late action, with periods determined partially by the length of time between the proposal’s introduction and either its passage or the end of the Congress.⁴ Proposals are further broken up into those passing (which should more accurately reflect the specified pattern) and

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³While Kessler and Krehbiel talk in terms of position-taking rather than matching, their conceptual distinction is the same as ours. Also, as the Appendix illustrates, their original analysis had substantial data problems, producing flawed empirical results. However, their general approach remains valuable.

⁴There was some mismeasurement of cosponsorship’s beginning and ending dates (see Appendix).
binding bills with the force of law (i.e., excluding resolutions, which may be less relevant because they lack the force of law if approved). Members are classified according to ideological extremity (by quartile, using ADA scores), seniority (in years), and electoral margin (natural log of percentage). Results from two semi-parametric event history models—an unconstrained model allowing for different behavior by liberals and conservatives in case liberals may have more activist dispositions, and a constrained model imposing symmetry—find that extremists are both more likely to cosponsor and to do it earlier, with effects more forceful for liberals. It is concluded that, in accordance with signaling but not matching, “Legislators do not use bill sponsorship as a mechanism of position taking . . . but rather adopt it as an internal signaling device” (Kessler and Krehbiel 1996, 563). Cosponsorship is internally and not externally driven.

As implied, although Kessler and Krehbiel offer an innovative analysis, there are a number of related reasons to take another look at cosponsorship dynamics:

*Data censoring.* Although focusing on proposals with many cosponsors is understandable (especially when compiling cosponsorship data was more difficult than it is currently), such a selection criteria may be unrepresentative. Minimally, one obvious check of any results supporting the signaling perspective would be finding that proposals where informational signaling should be less important (e.g., few cosponsors) exhibit different patterns.

*Specification.* There are obvious features related both to the propensity of members to cosponsor generally and to the utility of cosponsoring a given proposal specifically that should logically be incorporated in the specification. Concerning the former, there are several determinants of legislative behavior discussed in the literature that are natural candidates for inclusion. For example, whether a representative is seeking reelection or is leaving the chamber
has long been recognized as a determinant of the effort level that members expend, implying that endgaming can induce declining cosponsorship (e.g., Poole and Rosenthal 1997; Rothenberg and Sanders 2000, 2001). Furthermore, claims that party membership may be important, net of ideology, albeit controversial, suggest that being in the majority party may have a greater influence than being in the minority (for a review, see Aldrich and Rohde 2000; see also Krehbiel 1998; Binder, Lawrence, and Maltzman 1999). In a similar vein, parties may influence whether one supports specific proposals, as members of the same party may have more to gain in cosponsoring a fellow party member’s proposal. Also, being on the same committee as that reporting a proposal may make one more prone to cosponsor, and to cosponsor early, for a variety of reasons—greater credibility in cosponsoring, more knowledge of the proposal, and greater interest in the issue area.

**Measurement.** There are alternative ways of measuring key concepts that would provide a potentially significant improvement. For example, it is by now recognized that measures of ideology based on sophisticated scaling techniques, such as those developed by Poole and Rosenthal (1997), are superior to interest group ratings; also, employing these measures continuously rather than truncating them would represent a considerable improvement (and, as we will show when we discuss model estimation, this is possible). Similarly, measuring behavior in continuous time throughout the proposal consideration period rather than in delineated time periods would provide a fuller picture of temporal dynamics. 

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5 Even findings uncovering no shirking on induced preferences (e.g., as reflected by vote choice) typically find effort reduction (e.g., substantial reductions in roll-call participation).

6 Although not commonly recognized, a further point regarding the overall impact of security and tenure on cosponsorship levels is that expectations are indeterminate as moral hazard and selection effects should work at cross-purposes. Members may be secure because they are good
However, the most notable measurement issue that should be improved involves proxying where the proposal lies in the policy space. The inference drawn that extremists jump in first and that moderates come in later is predicated on implicitly assuming that all proposals are at the median. But are conservatives merely supporting conservative proposals, liberals supporting liberal proposals, and moderates supporting moderate proposals? Alternatively, it makes more sense to assume that, even with the constraints posed by the need to generate support for eventual passage, relatively extreme members propose relatively extreme proposals, some of which may be modified with time in anticipation or hope of eventual passage.\(^7\) This implies that if conservatives jump in early for proposals to the right of center, with perhaps some moderation toward the center as time elapses (perhaps induced by some modification of the proposal), we are witnessing matching rather than signaling.\(^8\) While Kessler and Krehbiel formulate some indirect tests of these propositions, they do not directly integrate information on where a proposal fits in the policy space.

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representatives and, as one manifestation of their high quality, engage in much cosponsorship activity; alternatively, security may be a function of reasons exogenous to representative quality and, as such, secure members may generally work less and specifically cosponsor less. Long tenured members may have survived because they are good representatives and, therefore, cosponsor more; conversely, senior members, getting closer to the end of their legislative careers, may have less incentive to engage in costly activity and cosponsor less.

\(^7\)Content analysis suggests that there are some proposals in our sample which are modified toward the center but, even for initiatives with many cosponsors, this is a small percentage (which is roughly consistent with the Poole and Smith’s (1994) analysis of Senate proposals).

\(^8\)As we will discuss in greater depth later, we would ideally know the status quo policy and calculate the Euclidean distance for each member and proposal as a function of her ideal point, the proposal, and the status quo. Unfortunately, the status quo is measurable for only a few proposals and we need to assume for most of our analysis that, everything being equal, legislators will support proposals closer to their own ideal points. We later test whether our results hold for those few proposals where the status quo is measurable.
Thus, in our analysis, we proxy the policy position at which a proposal is located by employing the proposer’s (i.e., the sponsor’s) ideological position (e.g., an approach used by Poole and Smith 1994, and Pellegrini and Grant 1999). Assuming that a sponsor’s ideology reflects where the introduced proposal falls in the policy space allows us to incorporate the distance between a member and the initial policy proposal into our event history analysis. Put differently, we define an extremist as a representative who is ideologically distant from the initial sponsor and not merely as a legislator who is extreme from the median member.

Estimation. Finally, there is room for econometric improvement in the use of event history (or duration or hazard) models to estimate cosponsorship dynamics. While a duration approach is appropriate for estimating whether and when a member will cosponsor, there are flexible variants that will give us more confidence in the estimated relationships. For example, it is possible to estimate a model with a more complete notion of time: While dividing time into small numbers of periods requires assuming that hazard rates for cosponsorship are constant for the long stretches that each period includes, we can measure time day-by-day and allow hazard rates to vary continuously. Similarly, by stratifying estimated models on member/proposal ideological correspondence, we can relax the assumption that the effects of other independent variables are constant regardless of this correspondence, allowing both the hazard curve’s shape and the impacts of covariates to fluctuate.

With such research design concerns in mind, we now turn to estimating a model of how

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9Our content analysis indicates that, although not perfect, this is a rather good predictor of where the proposal falls on the ideological space. The biggest discrepancy appears to be for certain high profile initiatives where there is de facto joint sponsorship across the policy spectrum but where, according to House rules, only one legislator is the technical sponsor. As a check for robustness, we also operationalized the proposal position as the average ideological position of the sponsor and the initial cosponsors. The results are qualitatively similar.
Incentives translate into cosponsorship behavior.

**Match or Signal?**

As we want to avoid selection bias, we code all cosponsorship activity for the 106th Congress by proposal and House member for all House Bills, Concurrent Resolutions, and Joint Resolutions introduced by virtually all [421] legislators.\(^{10}\) Of the 5,859 qualifying proposals, 90.3 percent are Bills, 7.4 percent are Continuing Resolutions, and 2.3 percent are Joint Resolutions.\(^{11}\) The average number of cosponsors by proposal is 19.3, with a standard deviation of 38.9; 30.1 percent of proposals had no cosponsors, 1.6 percent only had cosponsors upon initial introduction, and 68.3 percent had cosponsors after initial introduction. Cosponsorships on the day of introduction accounted for 35.2 percent of the total, and the average cosponsor signed on 71.4 days later (with a standard deviation of 111.1 days).\(^ {12}\)

Our data set thus consists of each member/proposal combination (2,460,780 observations) for whether and when a proposal is cosponsored by members not serving as sponsor. For each member and each proposal, the member can cosponsor during the eligible period for cosponsorship or can abstain. When a member cosponsors, that date is coded. As proposal introduction and reporting can take place throughout a Congress, the precise timing of

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\(^{10}\)After our initial coding, we exclude some members (and the proposals they sponsor): the Speaker, those who did not serve for the full Congress, those switching parties or who were independents during the Congress, those not voting often enough to receive W-NOMINATE scores, the Louisiana delegation (because of that state’s unique electoral system), and nonvoting delegates. Also, there are 420 potential cosponsors per proposal, as we exclude each proposal’s sponsor from the roster of potential cosponsors.

\(^{11}\)Private bills and bills introduced by request are excluded as they are unlikely to be characterized by the processes of interest.

\(^{12}\)Not surprisingly, cosponsorship’s temporal distribution is skewed: The median time to cosponsorship was 23 days, 77.3 percent of all cosponsorship occurred within 100 days of introduction, and 89.3 percent occurred within 200 days.
these activities varies. To account for this, we rescale each proposal so that its introduction day is coded as day 1, and a member’s cosponsorship date, Days Since Introduction, is the number of days from when the introduction occurs. Regardless of when a proposal is introduced, initial cosponsors are coded as acting on \( t = 1 \), second-day cosponsors on \( t = 2 \), etc. Consistent with standard application of duration models, we treat observations for non-cosponsoring members as censored, with the observation period ending either when floor action is taken on the proposal (ending the opportunity to cosponsor)\(^{13}\) or, given no action, when the 106\(^{th}\) Congress concludes.

As foreshadowed, for independent variables, we measure features associated with the incentive to cosponsor generally and on proposals specifically. Controlling for these factors allows us to test whether matching or signaling is taking place by investigating the relationship between proposal-specific ideological extremity and temporal cosponsorship.

Concerning the general propensity of legislators to cosponsor, we specify four variables to capture how costs and benefits vary across members:

(1) *Republican*, coded as one for Republicans and as zero for Democrats;

(2) *Lame Duck*, measured as whether, at the time of the proposal’s introduction, the member planned to retire at the end of the 106\(^{th}\) Congress;\(^{14}\)

\[^{13}\text{We will return to this censoring assumption later. Technically, cosponsorship opportunities end on the day when the proposal is reported by a referring committee.}\]

\[^{14}\text{Measuring lame-duck status is a bit tricky for two reasons. First, assuming that retirees behave as lame ducks throughout the whole two-year congressional session and that those seeking reelection are certain that they will continue introduces at least some measurement error (Rothenberg and Sanders 2000). We avoid much of this problem by looking at whether the member decided to retire by the date of a proposal’s introduction. This raises the second issue, when to code members as retiring. We code lame-duck status by conducting for each exiting member an intensive investigation of published sources, both national and local newspapers and magazines, to ascertain when she had apparently made her retirement decision. Some members, for example those who had previously announced that they would be term-limited and stuck to}\]

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(3) Seniority, measured as the number of years of member service in the House; and

(4) Electoral Security, measured as the percentage of the two-party vote that the member received in the 1998 election.¹⁵

Expectations regarding security and seniority for overall cosponsorship are ambiguous, depending upon whether selection or moral hazard effects are stronger on net. Analogously, expectations for party are ambiguous: Democrats may be more prone to cosponsor, everything being equal, but Republicans might cosponsor more given their potential agenda-setting advantage as the majority party. Lame duck members should be less likely to cosponsor, particularly once we control for seniority, regardless of whether one adopts the signaling or matching perspective.

As for issue-specific factors, we incorporate two:

(1) On Considering Committee, scored one if the member sits on at least one committee considering the proposal and zero otherwise; and

(2) Same Party Sponsor, scored one if a member is of the same party as the initial sponsor and zero otherwise.

In both instances, expectations for the effect of these factors on overall sponsorship are positive, as being on the considering committee indicates that a member has more at stake in a proposal than other legislators, while sponsor partisanship essentially serves as a proposal-
specific measure of partisan influence. We also expect that committee membership will be
associated with early cosponsorship as, in a sense, the member is confronted earlier in the
legislative process than most members with the cosponsorship choice.

As previously mentioned, the relationship between ideology and where the proposal lies
in the policy space is key given our theoretical interest. In addition, we wish to ascertain whether
there is a liberal or conservative bias for cosponsorship. As such, we integrate ideology into our
analyses in two ways: (1) directly as independent variables; and (2) as a stratification variable so
that the hazard rate function’s shape and covariate effects can vary with member/proposal
correspondence.\footnote{For example, this circumvents Kessler and Krehbiel’s concern that including preferences as an independent variable will stretch or compress baseline hazards without allowing the basic shape of these hazards to change.}

Thus, we specify two direct measures: Ideological Distance and Conservatism Relative to
the Proposal (hereafter simply referred to as Conservatism). To produce these measures, we
initially specify two variables, Member Ideology and Sponsor Ideology, using first dimension
W-NOMINATE scores for the member and the sponsor respectively.\footnote{Our results are qualitatively similar if we substitute inflation-adjusted (Turbo) ADA scores (Groseclose, Levitt, and Snyder 1999).} (As is well-known, these
scores lie roughly in the interval \([-1,1]\), with negative and positive values representing liberal
and conservative predispositions.) Ideological Distance then equals the absolute value of
Member Ideology – Sponsor Ideology for each member/proposal combination, producing a
measure of ideological correspondence with mean 0.657 and standard deviation 0.493.
Conservatism is the actual signed difference of Member Ideology – Sponsor Ideology, creating a
variable with mean –0.053 and standard deviation 0.820.
This measure results in considerably different placement of legislators relative to the proposal than simply using the median. For illustration, if we take our 421 legislators and categorize them as liberals (the most liberal 25 percent as defined by their NOMINATE scores), moderates (the middle 50 percent on the NOMINATE scale), and conservative (the most conservative 25 percent), and look how often they fall into different quintiles, we find that liberals are in the most liberal quintile 54.8 percent of the time, the next most liberal quintile 21.4 percent of the time, the middle quintile 17.1 percent of the time, the fourth quintile 6.7 percent of the time, and the most conservative quintile 0.0 percent of the time. Analogous percentages by quintile for moderate legislators are 12.5, 28.4, 19.2, 19.8, and 20.1; and for conservatives they are 0.0, 1.8, 24.5, 33.8, and 40.0. Similarly, while on average, the proportion of legislators falling into each quintile is 20 percent per proposal, there is considerable variance by proposal.

Thus, some proposals will be characterized by few members being in some categories (e.g., those proposed by an extreme member) and others will be characterized by a relatively even distribution (e.g., those proposed by a moderate).

For exploratory purposes, we take these five groups (labeling them from Very Conservative to Very Liberal) and produce non-parametric Kaplan-Meier cumulative probability estimates: the probability, without accounting for independent variables, that a member will be a cosponsor given Days Since Introduction. We find patterns consistent with matching rather than signaling and with a liberal bias (Figure 1).

Stratification then involves dropping Conservatism as an independent variable and, instead, dividing our sample according to the measure’s quintiles (the boundaries between the quintiles being −0.909, −0.186, 0.120, and 0.688). The first and fifth quintiles represent member/proposal combinations where the member is much more liberal and much more conservative than the proposal, the second and fourth represent members being somewhat more liberal and conservative, and the third represents members having a close ideological correspondence with the proposal. Thus, some proposals will be characterized by few members being in some categories (e.g., those proposed by an extreme member) and others will be characterized by a relatively even distribution (e.g., those proposed by a moderate).

Centrists, who are close to the proposal

\textsuperscript{18}This measure results in considerably different placement of legislators relative to the proposal than simply using the median. For illustration, if we take our 421 legislators and categorize them as liberals (the most liberal 25 percent as defined by their NOMINATE scores), moderates (the middle 50 percent on the NOMINATE scale), and conservative (the most conservative 25 percent), and look how often they fall into different quintiles, we find that liberals are in the most liberal quintile 54.8 percent of the time, the next most liberal quintile 21.4 percent of the time, the middle quintile 17.1 percent of the time, the fourth quintile 6.7 percent of the time, and the most conservative quintile 0.0 percent of the time. Analogous percentages by quintile for moderate legislators are 12.5, 28.4, 19.2, 19.8, and 20.1; and for conservatives they are 0.0, 1.8, 24.5, 33.8, and 40.0. Similarly, while on average, the proportion of legislators falling into each quintile is 20 percent per proposal, there is considerable variance by proposal.

\textsuperscript{19}The cumulative probability is equal to the complement of the Kaplan-Meier survival function (i.e., 1–survival probability).

\textsuperscript{20}In all Figures we show the entire range of a proposal’s possible lifespan, but we should reiterate that most cosponsorship occurs within three to six months of introduction.
location, are more likely to cosponsor, while extremists are less prone to do so. Furthermore, there is a left-leaning bias, as those who are Very Conservative are less likely to act than those who are Very Liberal; and Conservatives are less likely to cosponsor than Liberals after some time elapses.

(Figure 1 about here)

To see if these results hold up in a full-fledged model, we employ the semi-parametric Cox proportional hazard specification for the entire sample with Conservatism incorporated directly, and for Conservatism quintiles with this variable dropped.\textsuperscript{21} The hazard rate (the probability at time $t$ that a member will cosponsor a proposal when she has not yet done so) for each observation at $t$ is given by:

$$h(t) = h_0(t) \exp(b_1 X_1 + b_2 X_2 + \ldots + b_k X_k).$$

That is, the hazard rate is determined by the baseline hazard $h_0(t)$ and the observation-specific effects measured by $X_1, X_2, \ldots, X_k$. As such, while the baseline hazard rate is not directly estimated, we can recover the survival functions and hazard rates for the whole model (or as they vary for each stratum of Conservatism) to examine the probabilities of cosponsorship over time and to identify any temporal/ideological patterns. Thus, we can parametrically specify the effects of our variables on the hazard rate without committing to any particular functional form for the baseline hazard. In addition, through stratification, we can have different baseline hazards by ideological group.\textsuperscript{22}

\textsuperscript{21}See Box-Steppensmeier and Jones (1997, 2004) for an introduction to event history models.

\textsuperscript{22}Application of Royston and Parmar’s (2002) flexible parametric model, which employs cubic splines to directly estimate the hazard rate, produces qualitative similar coefficients for the independent variables and hazard rates with analogous shapes and relative magnitudes.
This approach allows us to test the matching and signaling hypotheses with great confidence. All else equal, evidence that extremist members are more likely to cosponsor early supports the signaling perspective. Failure to find this temporal pattern, and finding that legislators tend to support proposals close to their ideal points as conditioned by individual- and proposal-specific factors, is evidence of matching.

**Determinants of Cosponsorship**

Table 1 shows maximum likelihood estimates and z-scores generated by our proportional hazards model for the full sample.\(^{23}\) Coefficients measure the change in the hazard rate accruing from a one unit change for the independent variable; those less than zero indicate that an increase in that variable decreases the cosponsorship hazard. These coefficients measure both whether and when a member will cosponsor. In general, when we state that a member is less likely to cosponsor, we also mean that, among those members who do cosponsor, they will cosponsor later.

(Table 1 about here)

Overall, this model does well statistically, as reflected by the strong chi-squared score, and has important substantive ramifications. Of greatest relevance for our analysis is that the result for Ideological Distance reaffirms our non-parametric finding: Members further away from a proposal’s location are less likely to cosponsor and, among those who do cosponsor, members further away will cosponsor later. This is a substantively strong effect: Holding other factors constant, increasing ideological distance by one unit (about half the total possible change)

\(^{23}\)We use the Breslow method to resolve ties, with alternative methods—the Efron and “exact marginal likelihood” methods—yielding qualitatively similar results. In any case, our analysis is not likely to be plagued by problems of ties, as ties are 3.5 percent for our biggest failure date \((t = 1)\), well below the threshold of 5 percent commonly cited (Prentice and Farewell 1986).
decreases the cosponsorship hazard rate by 72 percent. In direct contradiction to Kessler and Krehbiel’s findings, matching is supported and signaling is not. As we explain in the Appendix, this discrepancy is a function of (1) our operationalization of ideology as the distance from the proposal rather than median; and (2) coding mistakes in the original analysis.

Most of the other variables also affect the likelihood of cosponsorship. Similar to previous studies, the negative coefficient on Conservatism indicates that right-leaning members are less likely to cosponsor. A one unit increase (out of a maximum of roughly four units) in Conservatism decreases the hazard rate of cosponsoring by 21 percent. Consistent with expectations, a member on the considering committee is 74 percent more likely to cosponsor than others. While being in the same party as the sponsor is neither substantively nor statistically significant, Republicans are 36 percent less likely to cosponsor legislation than Democrats, everything else equal, indicating that Democratic activism overcomes any agenda-setting impacts.

Consistent with previous findings that lame duck members shirk in their activities, a member who is planning to retire is 19 percent less likely to cosponsor than one intending to continue. Findings for seniority and marginality, where moral hazard and selection effects may work in different directions, are statistically significant but substantively modest: Increasing seniority by 10 years decreases the likelihood of cosponsorship by about 4 percent and increasing the two-party vote percentage of the member by 10 percent decreases the likelihood of cosponsorship by less than 0.5 percent.

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24We calculate this and other changes by using the hazard ratio which, for Ideological Distance, is $0.283 = e^{-1.262}$. Thus, increasing Ideological Distance by one unit multiplies the hazard rate by 0.283, which is a 71.7 percent reduction.
Despite some differences across ideological groups, results for our five strata, which we again label from Very Liberal to Very Conservative, are generally similar to the aggregated analysis, with analogous strong substantive effects (Table 2).^{25}

(Table 2 about here)

Most notably, even stratifying for ideological extremity, members that are further from the sponsor cosponsor less. That this effect is strongest for the most extreme members further supports the matching hypothesis.

Also similar to the aggregated analysis, being on a considering committee has a substantial positive effect, which is strongest for the most liberal members. As for partisanship, for all but the first stratum (for which a coefficient cannot be estimated as virtually all members included are Democrats), Democrats are again more likely to cosponsor than Republicans, and more conservative Republicans are even less likely to cosponsor. Lame-duck status is again a deterrent to cosponsorship, with departing members less likely to cosponsor across all strata, although these effects are weakest for the most conservative members.

There are factors that differ qualitatively across strata. Whereas Very Liberal and Very Conservative members are less likely to cosponsor with a sponsor that is in the same party, Liberals and Centrists are more likely to cosponsor. Aggregating the different strata, as we did in Table 1, cancelled out these effects. Also, there are differences for those factors, seniority and

\^{25}A Chow test shows that there are statistical differences between the strata (p < 0.0001) but, as seen in Table 2, the substantive results are largely similar to the aggregated analysis. Also, a stratified analysis where the coefficients are constrained to be the same across strata but the baseline hazard shapes are allowed to vary produces results analogous to those in Table 1. Finally, if we try to include Conservatism in the analysis, it is dropped due to collinearity in all but one stratum (Centrist), where a comparable result to that found for the aggregated analysis is generated.
electoral security, that may be influenced both by moral hazard and selection. Seniority has little impact for those who are Very Conservative or Very Liberal relative to the proposal, while it has a negative effect for Centrists, Liberals, and Conservatives. By contrast, electoral security is insignificant for Liberals and Centrists, leading to greater cosponsorship activity for Very Liberal members, but produces less cosponsorship when members are Conservative or Very Conservative.

Although these results are interesting, of even greater relevance for our stratified analysis are the different shapes of the baseline probabilities of cosponsorship for the five quintiles of member/proposal extremism. Do those Very Liberal and Very Conservative members cosponsor more often and earlier and Centrists less often and later, or is a pattern more consistent with matching observed? Figure 2 displays the hazard rate for each quintile of member/proposal extremism holding all the independent variables at the corresponding quintile’s sample mean. The results indicate a pattern more consistent with matching. While cosponsorship’s probability is, obviously, always low (given these are daily hazard rates), Centrists relative to the proposal are most likely to cosponsor over its lifespan, followed by Liberals and Conservatives and, finally, by Very Liberals and Very Conservatives. The differences in cosponsorship rates are most pronounced right after the proposal is introduced, becoming much smaller after six months.

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26Following Box-Steppensmeier and Jones (2004, 64-65), we obtain the hazard rates using the method of Kalbfleisch and Prentice (2002, 114-118), to which we apply a kernel smoother for the graph. We obtain similar results when we calculate the hazard rate through (again, using kernel smoothing) a weighted average of the increments in the cumulative baseline hazard (Klein and Moeschberger 2003, 166-177); by numerically differentiating the baseline cumulative hazard and smoothing it (Royston 2001); and by using cubic splines (Royston and Parmar 2002).

27Indeed, despite the inclusion of independent variables and the use of the Cox model, graphing cumulative probabilities shows results qualitatively similar to Figure 1’s Kaplan-Meier estimates.
Using Kessler and Krehbiel’s terminology (with respect to their Figure 2), cosponsorship exhibits an inside-out pattern where more extreme members come on board later (consistent with matching), rather than an outside-in pattern (consistent with signaling).

(Figure 2 about here)

In brief, we can draw three inferences from Figure 2: (1) ideological matching is key throughout, and is strongest immediately after the proposal is introduced; (2) cosponsorship exhibits a liberal bias; and, most importantly, (3) there is no pattern by which extremists can be construed as entering early but not late (or by which centrists can be construed as entering late but not early).

As such, extremists do not dominate in either a relative or an absolute sense at a proposal’s early stages. If anything, there is a bandwagon by which initial centrist support brings others with ideal points further from where the proposal is in the policy space on board. To illustrate, consider Figure 3, which shows the expected composition of cosponsors over time with respect to distance from the proposal. Put differently, for each quintile at each point in time we calculate the expected proportion of a proposal’s cosponsors, e.g., the expected probability of cosponsoring at $t = 1$ is 0.0075 for Very Liberals (10.3 percent of cosponsors), 0.0179 for Liberals (24.4 percent), 0.0272 for Centrists (36.2 percent), 0.0162 for Conservatives (22.1 percent), and 0.0045 for Very Conservatives (6.1 percent). When we do this, we again find evidence that is inconsistent with signaling but consistent with matching where there is a bandwagon by which members not obviously ideologically disposed are more prone to get on board as support grows. Specifically, the proportion of Centrists decreases over time, the proportion of Liberals grows slightly, the proportion of Very Liberals increases, and the
proportion of both cohorts of Conservatives remains roughly constant. To reiterate, if, as the signaling perspective suggests, extremists are early cosponsors, then we would expect exactly the opposite: Early in a proposal’s lifespan the proportion of extremists would be highest and the proportion of centrists would be lowest and, with time, the former would decrease and the latter would increase. While one might argue that this is some odd sort of reverse signaling where centrists signal extremists that the proposal is worthwhile, this is refuted by the fact that the growth of extremist cosponsors is almost exclusively from those more liberal relative to proposals. This asymmetry is consistent only with a matching process in which legislators from liberal districts disposed toward government intervention join in when enactment’s probability grows.\textsuperscript{28}

(Figure 3 about here)

In sum, while ideological extremism of members relative to proposals conditions the effects of other variables on the hazard rate of cosponsorship, it does so consistent with matching and not signaling. There is no evidence that internal information transmission is key for understanding cosponsorship. Rather, matching with constituencies appears to be of paramount importance.

Robustness

Given the dramatic difference between our analysis and Kessler and Krehbiel’s, we want to assess the robustness of our findings in as many ways as possible. Specifically, we check robustness for subsamples of our data, including on a proposal-by-proposal basis, and with

\textsuperscript{28}Kessler and Krehbiel suggest that inside-out cosponsorship might reflect entrepreneurial behavior on the part of moderate party leaders (but not signaling). However, the asymmetry between liberal and conservative extremist cosponsorship seems to support more straightforward matching.
alternative specifications. Throughout, we find results supporting matching but not signaling.

**Data Subsamples**

One obvious robustness test is to see if evidence exists for signaling with respect to subsets of proposals. We take two general approaches in doing this. First, we focus on subsets where there is a *prima facie* reason to believe that information transmission may have taken place, e.g., because information is at a premium, the proposal being debated is of great consequence (providing an incentive for information transmission), or the proposal was successful (perhaps reflecting information transmission). Second, we examine our data on a case-by-case basis and try to inductively see if there is evidence for signaling.

We adopt a variety of tacks in pursuing the first approach. To look at instances where there is reason to think that information transmission is key, and corresponding to Kessler and Krehbiel’s analysis, we rerun our analysis for the 11.2 percent of proposals (comprising 61.9 percent of cosponsorships) with 50 or more cosponsors (Table 3). [Although we use the non-stratified sample for the remainder of our analysis, the inferences drawn are substantially unchanged if we stratify by Conservatism.] Consistent with matching, the finding for Ideological Distance remains negative (and is even larger than for the full sample), with extremists less likely to cosponsor than centrists throughout the cosponsorship period. For similar reasons, we examine proposals by their degree of complexity, specifically by looking at the 1 percent of most complex proposals (1.1 percent of cosponsorships), as measured by their size.\(^{29}\) Again, extreme

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\(^{29}\)We operationalize complexity by the length of a proposal (Takeda 2000). We measure length by the bytes of memory the “Printer Friendly Display” version of the proposal on THOMAS uses. The median number of bytes is 4408, the mean is 14,210, and the standard deviation is 44,138. For the 1 percent of proposals with the greatest complexity, size is greater than or equal to 187,533.
members are less likely to cosponsor than centrists. Interestingly, for these highly complex proposals, conservative members are more likely to cosponsor than liberal members. In addition, members of the same party as the sponsor are more likely to cosponsor.\(^{30}\)

(Table 3 about here)

To look at subsets where proposals were successful at least in reaching a roll call, we also run our model for the 17.2 and 14.7 percent of our proposals which came to a final vote and were passed (“Winning Proposals”), respectively (24.2 and 20.9 percent of cosponsorships). In both instances more extreme members are again less likely to cosponsor, although the substantive effect is somewhat smaller, though still strong. There are some other differences relative to the full sample: Liberals are no more likely to cosponsor than conservatives, the effect of seniority disappears, and members of the same party as the sponsor are now more likely to cosponsor.

Also, as they may be more important, we reran our model examining only the 90.3 percent of proposals (88.5 percent of cosponsorships) that are binding. Results are essentially the same as our full sample (and thus not shown).\(^{31}\)

As a final check, we examine whether there are any proposals whatsoever for which we can uncover behavior consistent with signaling (randomness will presumably produce a few) by running our basic model on each of the 5,859 proposals individually and seeing when Ideological Distance is statistically significant. While the coefficient on Ideological Distance is negative and

\(^{30}\)Comparable results are found for the top 10 percent of complex proposals (except that liberal members are more likely to cosponsor than conservatives); also, we find no differences in the effect of Ideological Distance across strata if we stratify all proposals by complexity (using quintiles). Nor do we find a substantive or statistical effect for an interaction of complexity and Ideological Distance (and the sign is in the wrong direction for signaling) if we estimate for our full sample a model where we interact complexity with the other independent variables.

\(^{31}\)Findings for non-binding resolutions are also qualitatively similar to those for the full sample.
When we restrict the proposals to those that have 9 or more cosponsors (so that there are more failures than covariates), 517 were matching proposals, and 25 were signaling. This greater than 10 to 1 ratio of significant cases, and the fact that less than 1 percent of cases—no more than is suggested by randomness—are consistent with signaling, again shows strong support for matching.

Alternative Specifications

In assessing robustness we consider several alternative specifications of our models, notably by integrating the status quo and time, and by reconceptualizing what not cosponsoring means. All confirm our basic inference about matching and signaling.

First, we examine only proposals for which a status quo point can be incorporated, as this is the ideal way to incorporate member utility. While NOMINATE produces no status quo measure for most of our 5,859 proposals, there is one for the 141 cases in our sample where a vote is taken on (a version of) the proposal vs. the status quo, although these estimates (along

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32 When we restrict the proposals to those that have 9 or more cosponsors (so that there are more failures than covariates), 517 were matching proposals, and 25 were signaling.

33 Nor is there much difference between the signaling proposals and the rest of the proposals in our data set. There is no statistical difference in the number of cosponsors, complexity, or whether there was a roll call vote or passage. Signaling proposals are made somewhat earlier in the congressional session and are sponsored by more conservative legislators. However, when we combine all proposals together, these characteristics do not substantively affect cosponsorship matching patterns. Specifically, when we interact Ideological Distance with introduction date, it makes a statistical, but not a substantive, difference in our basic model; when we interact Ideological Distance with sponsor ideology, the relationship actually reverses, as extreme members are less likely to cosponsor proposals of conservatives.

34 The robustness tests presented here are those we deem most important and are not inclusive. Other robustness tests that we conducted included incorporating possible length of consideration (days left in the session); substituting parametric duration models (exponential, Weibull, Gompertz, log-normal, and log-logistic) for the semi-parametric Cox model; using robust errors and robust error clustering on legislators or proposals for significance testing; and accounting for unobserved heterogeneity (frailty models) associated with legislators or proposals. Despite all these attempts (results available from authors), our findings are qualitatively similar.
with that of the alternative) are less precise than the placement of legislator in ideological space (Poole and Rosenthal 1997, 235). For these cases, we calculate *Member Utility* by taking the absolute distance between the status quo point and the member’s ideal point and subtracting it from *Ideological Distance* (the absolute distance between member and sponsor; results are qualitatively the same if we substitute squared distances). *Member Utility* is positive when the proposal (proxied by sponsor) is closer to the member than the status quo and negative when the member is closer to the status quo than the proposal. As seen in the last column of Table 3, when we substitute *Member Utility* for *Ideological Distance* we find that a member is, as expected, more likely to cosponsor when *Utility* is positive, that is, when the proposal is closer to her than the status quo. Additionally, the substantive findings supporting matching do not change: The further the member is from the proposal relative to the status quo, the less likely she is to cosponsor.

Another possibility is that covariate effects violate the assumption of being proportional

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35The description of these votes contained text such as, “Question: On Passage,” “Question: On Motion to Suspend the Rules and Pass,” or other similar language. We did not include any votes where one alternative was a proposal and the other an amendment to that proposal.

36Putting the proposals and members in the same ideological space dictates using DW-NOMINATE scores instead of the W-NOMINATE scores used in the rest of our analysis (our overall results would not change if we used DW-NOMINATE throughout).

37While we exclude *Ideological Distance*, as it is incorporated in *Member Utility*, our results for still hold if we include both measures.

38As DW-NOMINATE also estimates a proposal’s location on the final vote, this could be used instead of the sponsor’s ideal point for those proposals voted on unamended from committee (as the former measure ignores any committee amendments, whether it is an improvement is debatable). When we do this for a slightly smaller sample (120, as we drop final votes that include the term “As Amended”) the results do not change. Members further from the proposal position are less likely to cosponsor and those closer to the proposal than to the status quo are more likely to do so.
with time and, rather, a more flexible approach is required. Indeed, tests based on Schoenfeld residuals (Grambsch and Therneau 1994) suggest that the proportional hazards assumption may be problematic for all variables except Ideological Distance and Lame Duck and that allowing variable effects over time may be preferable. As such, we deviate from the Cox specification by using a non-proportional hazards (non-PH) model allowing covariate effects to vary temporally by interacting them with the logarithm of Days Since Introduction (Box-Steffensmeier and Zorn 2001) so that the hazard rate at $t$ is:

$$h(t) = h_0(t) \exp(b_1 X_1 + b_2 X_2 + \cdots + b_k X_k + b_{k+1} X_1 \times \ln(t) + b_{k+2} X_2 \times \ln(t) + \cdots + b_{2k} X_k \times \ln(t)).$$

Coefficients for the baseline variables (not interacted with $\ln(t)$) represent the effect on the first day of the cosponsorship period and those for the interacted variables estimate how this effect changes with time (Table 4). For example, the effect of the sponsor being in the same party as a potential cosponsor is initially negative but, after about two weeks, this relationship reverses and becomes positive (this is the most dramatic switch in effects in the coefficients and helps explain why there is no apparent effect in the proportional hazards model).

(Table 4 about here)

Yet, while such differences do exist, many results remain similar to those for the proportional hazards model and, most importantly, our inferences about matching and signaling are unchanged. Most notably, results for Ideological Distance remain comparable and substantively very strong. Extremists relative to sponsors are the least likely to cosponsor initially. While this relationship weakens slightly as time passes, this effect is statistically

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39 We follow Kalbfleisch and Prentice (2002) in choosing $\ln(t)$ as the functional form of the non-proportionality. Re-estimating this model substituting $t$, $t^2$, or $(1 - 1/t)$ for $\ln(t)$ produces the same substantive results with slightly worse fit.
insignificant—and this weakening is in the opposite direction of signaling’s prediction that extremists are more likely to cosponsor early.

Another possible critique of our initial analysis is that we have misconceptualized what not cosponsoring means: While a duration model framework implicitly assumes that every member will cosponsor if Congress never ends, this seems implausible as there are certainly members disposed never to sponsor a given proposal. Hence, we might like to purge those who would never cosponsor and re-estimate our model for those remaining. To do so, we employ a split-population duration model that simultaneously estimates which members in the population will eventually cosponsor and, like a standard duration model, who will cosponsor earlier (Schmidt and Witte 1989). Specifically, as there is no agreement on how to handle baseline hazards in a split-population Cox model (Chang and Wang 2004), we use a logit model and a parametric log-logistic duration model that allows some flexibility in the hazard shape.\footnote{We use the Stata program written by Forster and Jones (2001), obtained via Box-Steffensmeier, Radcliffe and Bartels (forthcoming).} Thus, a positive coefficient for the logit analysis implies that increasing that coefficient’s variable increases the likelihood of being in the cosponsorship group. A positive coefficient for the log-logistic analysis indicates that increasing that variable decreases the time until cosponsorship (or increases the hazard rate of cosponsorship).

As Table 4 shows, results again confirm the matching hypothesis. The negative logit coefficient on Ideological Distance indicates that extreme legislators (in relation to the sponsor) are less likely to be in the cosponsorship group, while the negative duration coefficient for Distance demonstrates that, among those in the cosponsorship group, extreme legislators are less
likely to cosponsor early.\footnote{The split-population model produces some other interesting findings that are less germane to our analysis, e.g., lame ducks are more likely to cosponsor but, given that they are cosponsors, are less likely to cosponsor early than members running for reelection.}

**Discussion and Conclusions**

Understanding how basic member incentives translate into behavior has often proven problematic generally and with respect to cosponsorship specifically. Although no analysis is definitive, our results lend strong credence to the belief that such puzzles can be solved, at least for cosponsorship. Specifically, by utilizing earlier insights but remedying research design deficiencies, we show that there is no support for internal dynamics being crucial but that there is consistent and undeniable evidence that members use cosponsorship to communicate with external constituencies. While we do not wish to suggest that internal signaling does not occur in the United States Congress, our results indicate that cosponsorship is not the mechanism.

There would seem to be good, intuitive reasons why cosponsorship would not be the informational vehicle of choice. First, the cost of cosponsorship may be extremely low and, therefore, signing on as a cosponsor may provide little informational content as it can be construed as cheap talk. Given this low cost, it may often be difficult for fellow cosponsors to distinguish between informational cosponsoring and cosponsorship undertaken for other reasons, discouraging attempts to use cosponsorship for internal communication altogether. Second, while many voters might not view cosponsorship directly, it is easily observable by electorally-relevant elites, be they interest groups or political candidates—for example, an extremist member’s cosponsorship of a proposal that proves quite unpopular to her supporters could be easily exploited by a challenger while the same member can trumpet her cosponsorship of an initiative that is consistent with district or interest group preferences.
References


Browne, William P. 1985. “Multiple Sponsorship and Bill Success in U. S. State Legislatures.”


Grambsch, Patricia M., and Terry M. Therneau. 1994. “Proportional Hazards Tests and
Diagnostics Based on Weighted Residuals.” *Biometrika* 81: 515-526.


### Table 1: Effects on Cosponsorship Hazards

<table>
<thead>
<tr>
<th>Variable</th>
<th>All Proposals</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ideological Distance</td>
<td>−1.262*</td>
</tr>
<tr>
<td></td>
<td>(−89.92)</td>
</tr>
<tr>
<td>Conservatism</td>
<td>−0.238*</td>
</tr>
<tr>
<td></td>
<td>(−37.62)</td>
</tr>
<tr>
<td>On Considering Committee</td>
<td>0.553*</td>
</tr>
<tr>
<td></td>
<td>(73.00)</td>
</tr>
<tr>
<td>Same Party Sponsor</td>
<td>−0.021</td>
</tr>
<tr>
<td></td>
<td>(−1.66)</td>
</tr>
<tr>
<td>Republican</td>
<td>−0.451*</td>
</tr>
<tr>
<td></td>
<td>(−64.04)</td>
</tr>
<tr>
<td>Lame Duck</td>
<td>−0.209*</td>
</tr>
<tr>
<td></td>
<td>(−13.59)</td>
</tr>
<tr>
<td>Seniority</td>
<td>−0.004*</td>
</tr>
<tr>
<td></td>
<td>(−9.91)</td>
</tr>
<tr>
<td>Electoral Security</td>
<td>−0.000*</td>
</tr>
<tr>
<td></td>
<td>(−2.23)</td>
</tr>
<tr>
<td>Number of Cases</td>
<td>2,460,780</td>
</tr>
<tr>
<td>Chi-Squared</td>
<td>39,016*</td>
</tr>
</tbody>
</table>

*Notes:* Dependent variable is cosponsoring a proposal. Coefficients are maximum likelihood estimates of the Cox proportional hazard model, with *z*-scores in parentheses (*p < 0.05, two-tailed test).
<table>
<thead>
<tr>
<th>Variable</th>
<th>Very Liberal</th>
<th>Liberal</th>
<th>Centrist</th>
<th>Conservative</th>
<th>Very Conservative</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ideological Distance</td>
<td>−1.411*</td>
<td>−0.738*</td>
<td>−0.556*</td>
<td>−1.458*</td>
<td>−1.857*</td>
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<td>(−5.00)</td>
<td>(−29.47)</td>
<td>(−34.09)</td>
</tr>
<tr>
<td>On Considering Committee</td>
<td>0.696*</td>
<td>0.601*</td>
<td>0.507*</td>
<td>0.544*</td>
<td>0.341*</td>
</tr>
<tr>
<td></td>
<td>(34.41)</td>
<td>(39.20)</td>
<td>(38.12)</td>
<td>(34.27)</td>
<td>(11.26)</td>
</tr>
<tr>
<td>Same Party Sponsor</td>
<td>−0.339*</td>
<td>0.091*</td>
<td>0.400*</td>
<td>−0.022</td>
<td>−0.431*</td>
</tr>
<tr>
<td></td>
<td>(−2.61)</td>
<td>(4.55)</td>
<td>(4.10)</td>
<td>(−0.64)</td>
<td>(−2.06)</td>
</tr>
<tr>
<td>Republican</td>
<td>−2</td>
<td>−0.477*</td>
<td>−0.363*</td>
<td>−0.571*</td>
<td>−0.734*</td>
</tr>
<tr>
<td></td>
<td>(−29.08)</td>
<td>(−33.43)</td>
<td>(−42.79)</td>
<td>(−3.43)</td>
<td></td>
</tr>
<tr>
<td>Lame Duck</td>
<td>−0.386*</td>
<td>−0.165*</td>
<td>−0.178*</td>
<td>−0.265*</td>
<td>−0.081</td>
</tr>
<tr>
<td></td>
<td>(−6.59)</td>
<td>(−4.96)</td>
<td>(−6.86)</td>
<td>(−8.90)</td>
<td>(−1.65)</td>
</tr>
<tr>
<td>Seniority</td>
<td>0.001</td>
<td>−0.002*</td>
<td>−0.009*</td>
<td>−0.005*</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.53)</td>
<td>(−2.15)</td>
<td>(−11.98)</td>
<td>(−4.74)</td>
<td>(1.26)</td>
</tr>
<tr>
<td>Electoral Security</td>
<td>0.005*</td>
<td>−0.001</td>
<td>0.001</td>
<td>−0.003*</td>
<td>−0.004*</td>
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<tr>
<td></td>
<td>(8.50)</td>
<td>(−1.69)</td>
<td>(1.92)</td>
<td>(−6.30)</td>
<td>(−5.65)</td>
</tr>
<tr>
<td>Number of Cases</td>
<td>492,213</td>
<td>492,111</td>
<td>492,169</td>
<td>492,133</td>
<td>492,154</td>
</tr>
<tr>
<td>Chi-Squared</td>
<td>2078*</td>
<td>2512*</td>
<td>2647*</td>
<td>4092*</td>
<td>1685*</td>
</tr>
</tbody>
</table>

Notes: Dependent variable is cosponsoring a proposal. Coefficients are maximum likelihood estimates of the Cox proportional hazard model, with z-scores in parentheses (*p < 0.05, two-tailed test).

*Coefficient not estimated due to collinearity.
<table>
<thead>
<tr>
<th>Variable</th>
<th>50 or More Cosponsors</th>
<th>Complex Proposals</th>
<th>Final Vote</th>
<th>Winning Proposals</th>
<th>Status Quo Included</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ideological Distance</td>
<td>-1.374* (-79.32)</td>
<td>-1.434* (-11.02)</td>
<td>-0.756* (-27.71)</td>
<td>-0.655* (-22.68)</td>
<td>–</td>
</tr>
<tr>
<td>Conservatism</td>
<td>-0.370* (-46.24)</td>
<td>0.393* (5.99)</td>
<td>0.028* (2.08)</td>
<td>0.010</td>
<td>-0.168* (3.66)</td>
</tr>
<tr>
<td>On Considering Committee</td>
<td>0.305* (29.61)</td>
<td>0.919* (15.80)</td>
<td>0.784* (53.85)</td>
<td>0.762* (48.04)</td>
<td>0.661* (22.02)</td>
</tr>
<tr>
<td>Same Party Sponsor</td>
<td>-0.104* (-6.51)</td>
<td>0.338* (2.54)</td>
<td>0.049* (1.88)</td>
<td>0.057* (2.07)</td>
<td>0.356* (5.96)</td>
</tr>
<tr>
<td>Republican</td>
<td>-0.435* (-47.83)</td>
<td>-1.538* (-23.70)</td>
<td>-0.348* (-20.13)</td>
<td>-0.315* (-16.74)</td>
<td>0.186* (3.08)</td>
</tr>
<tr>
<td>Lame Duck</td>
<td>-0.083* (-4.19)</td>
<td>-0.162* (-1.15)</td>
<td>-0.152* (-5.25)</td>
<td>-0.160* (-5.14)</td>
<td>-0.105</td>
</tr>
<tr>
<td>Seniority</td>
<td>-0.008* (-14.15)</td>
<td>-0.003* (-0.66)</td>
<td>-0.001* (-0.68)</td>
<td>0.000</td>
<td>-0.007* (-3.47)</td>
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<tr>
<td>Electoral Security</td>
<td>-0.000* (-1.86)</td>
<td>0.002* (0.96)</td>
<td>-0.001* (-1.82)</td>
<td>-0.001* (-2.65)</td>
<td>-0.003* (-3.62)</td>
</tr>
<tr>
<td>Member Utility</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.364* (18.39)</td>
</tr>
<tr>
<td>Number of Cases</td>
<td>274,680</td>
<td>24,360</td>
<td>422,940</td>
<td>362,880</td>
<td>59,221</td>
</tr>
<tr>
<td>Chi-Squared</td>
<td>25,676*</td>
<td>1251*</td>
<td>5213*</td>
<td>3821*</td>
<td>1572*</td>
</tr>
</tbody>
</table>

**Notes:** Dependent variable is cosponsoring a proposal. Coefficients are maximum likelihood estimates of the Cox proportional hazard model, with z-scores in parentheses (*p < 0.05, two-tailed test).
<table>
<thead>
<tr>
<th>Variable</th>
<th>Non-PH</th>
<th>Split Population</th>
<th></th>
<th></th>
<th></th>
</tr>
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<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Logit</td>
<td>Duration</td>
<td></td>
</tr>
<tr>
<td>Ideological Distance</td>
<td>-1.291*</td>
<td>-1.176*</td>
<td>-0.264*</td>
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<tr>
<td></td>
<td>(-56.65)</td>
<td>(-27.21)</td>
<td>(-4.59)</td>
<td></td>
<td></td>
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<tr>
<td>Conservatism</td>
<td>-0.123*</td>
<td>-0.425*</td>
<td>0.283*</td>
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<tr>
<td></td>
<td>(-11.91)</td>
<td>(-25.79)</td>
<td>(12.96)</td>
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<tr>
<td>On Considering Committee</td>
<td>0.837*</td>
<td>0.568*</td>
<td>0.082*</td>
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<td></td>
</tr>
<tr>
<td></td>
<td>(75.45)</td>
<td>(31.03)</td>
<td>(3.47)</td>
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<tr>
<td>Same Party as Sponsor</td>
<td>0.216*</td>
<td>-0.111*</td>
<td>0.128*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(10.34)</td>
<td>(-2.93)</td>
<td>(2.53)</td>
<td></td>
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<tr>
<td>Republican</td>
<td>-0.557*</td>
<td>-0.416*</td>
<td>-0.157*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-51.86)</td>
<td>(-25.38)</td>
<td>(-6.99)</td>
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</tr>
<tr>
<td>Lame Duck</td>
<td>-0.214*</td>
<td>0.377*</td>
<td>-0.868*</td>
<td></td>
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</tr>
<tr>
<td></td>
<td>(-9.11)</td>
<td>(5.05)</td>
<td>(-11.20)</td>
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<tr>
<td>Years in Office</td>
<td>0.003*</td>
<td>-0.012*</td>
<td>0.012*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(4.89)</td>
<td>(-12.62)</td>
<td>(8.85)</td>
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<tr>
<td>Two-Party Vote</td>
<td>0.000</td>
<td>-0.002*</td>
<td>0.002*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.90)</td>
<td>(-3.52)</td>
<td>(2.63)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ideological Distance × ln(time)</td>
<td>0.009</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>(1.37)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Conservatism × ln(time)</td>
<td>-0.043*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-14.58)</td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>On Considering Committee × ln(time)</td>
<td>-0.116*</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>(-32.92)</td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Same Party as Sponsor × ln(time)</td>
<td>-0.088*</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(-14.96)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Republican × ln(time)</td>
<td>0.041*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(12.78)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lame Duck × ln(time)</td>
<td>0.002</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.26)</td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Years in Office × ln(time)</td>
<td>-0.003*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-14.57)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Two-Party Vote × ln(time)</td>
<td>-0.000*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-3.00)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>constant</td>
<td>–</td>
<td>-1.213*</td>
<td>-6.075*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(-19.89)</td>
<td>(-82.52)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>sigma</td>
<td>–</td>
<td></td>
<td>0.909*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(205.99)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Cases</td>
<td>2,460,780</td>
<td>2,460,780</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Chi-Squared</td>
<td>41,871*</td>
<td>682*</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Dependent variable is cosponsoring a proposal. Coefficients are maximum likelihood estimates, with z-scores in parentheses (*p < 0.05, two-tailed test).
Note: Very Liberal, Liberal, Centrist, Conservative, and Very Conservative are defined by the first through fifth quintiles of *Conservatism Relative to the Proposal*, respectively.
Note: Very Liberal, Liberal, Centrist, Conservative, and Very Conservative are defined by the first through fifth quintiles of Conservatism Relative to the Proposal, respectively.
Figure 3: Change in Cosponsorship Proportions
(Semi-parametric Cox estimates)

Note: Very Liberal, Liberal, Centrist, Conservative, and Very Conservative are defined by the first through fifth quintiles of Conservatism Relative to the Proposal, respectively.
Appendix: Analysis of the 103rd Congress

As the closest analogue to our analysis is Kessler and Krehbiel’s (1996), a natural question that requires addressing is why our results differ so dramatically from these earlier findings—research design, measurement, estimation, or something else?

To explore these questions, we obtained the 103rd Congress data and programs used for the 1996 analysis and reproduced the unconstrained model (Table 4 in Kessler and Krehbiel) of the original analysis (“All Bills,” labeled “KK Original” in Table A-1). We then estimated three additional models: (1) the unconstrained model after fixing significant problems with the data set (“Correcting Cosponsors”); (2) a variant of this model in which we change the measurement of ideology to be the difference between the sponsor’s ideology and the potential cosponsor’s ideology (“Placing Proposal in the Policy Space”); and (3) an analogous specification to that used for our duration model of the 106th Congress (“Basic Model”).

(Table A-1 about here)

As Table A-1 shows, Kessler and Krehbiel’s original analysis finds that extreme liberal members (in the top quartile of ADA scores) are more likely to cosponsor at all three stages of

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42 We use the unconstrained model, which allows liberal extreme members to differ from conservative extreme members, as it clearly shows the effects of miscoding.

43 Three data problems required correction: (1) a mismatch between the member and the data on his/her cosponsorship such that the data were “off” by one member, e.g., cosponsorship for the representative of the Alabama first district was inadvertently assigned to the second district representative, and so on (illustrative tables are available from the authors); (2) in roughly a dozen instances, cosponsorship was incorrectly assigned to members with the same names and the like (a list of these corrections is also available from the authors); and (3) cosponsorship timing was mismeasured, as the initial cosponsorship date rather than the introduction date was used to start the timing measure and the date of a vote rather than the proposal’s reporting date was used to mark when cosponsorship was no longer possible. While correcting the latter two issues does not qualitatively affect results, correcting the first does.

44 In addition to the coefficients reported in Table A-1, the model also estimates three parameters representing the hazard rate at three points in time which, following Kessler and Krehbiel, we do not report.
the cosponsorship period while results for extreme conservatives (in the bottom quartile), though mostly positive, are statistically insignificant (Kessler and Krehbiel still see this as supporting their signaling hypothesis, though not as strongly as for liberals). For both liberals and conservatives, the strongest effect is in the early stage, corresponding to the outside-in (signaling) hypothesis.

Merely correcting for coding mistakes produces markedly different results: Extreme liberals are much more likely to cosponsor than implied by the original analysis and extreme conservatives are much less likely to do so, seemingly suggesting a liberal/conservative distinction rather than signaling per se. These patterns become even more understandable if we note that the sponsor’s median ADA score for the 51 proposals utilized in the analysis is 90, i.e., in this rather liberal and Democratically-controlled Congress, proposals generating lots of cosponsors were from rather extreme liberals. Thus, evidence that extreme liberals, defined as those with ADA scores greater than 85, cosponsor more is consistent with a finding that members support the proposals of those with whom they are ideologically most compatible—an inside-out rather than an outside-in cosponsorship pattern.

When we assume that proposals fall in the policy space in a manner consistent with the ideology of their sponsors, as measured by ADA scores, we find that being an extreme liberal member makes no difference for cosponsorship, either statistically or substantively, while being an extreme conservative makes one less likely to cosponsor, a difference which is strong substantively and statistically. The reason for this discrepancy between liberals and conservatives is that, while liberals are more likely to cosponsor than conservatives and

---

45Results are qualitatively similar when we substitute NOMINATE for ADA scores. We continue to follow Kessler and Krehbiel by separating the ideological difference into quartiles, with liberal extreme members (relative to sponsors) in the top quartile and conservative extreme members (relative to sponsors) in the bottom quartile.
extremists are less likely to cosponsor than centrists, these effects largely cancel out for liberal extremists but reinforce each other for their conservative counterparts.

Finally, when we apply our basic model to the 103rd Congress data set, the results are very comparable to those for the 106th Congress.6 Ideologically extreme members are much less likely to cosponsor than centrists, and liberal members are more likely to cosponsor. The effect of extremity in the 103rd is substantively the same as for the 106th but the effect of liberalism is weaker. For measures that correspond with the analysis of the 103rd Congress—seniority and margin (which Kessler and Krehbiel log)—our results are substantively similar. As for other measures, like our original model, being on the considering committee increases the likelihood of cosponsorship, while other coefficients for which there are expectations are signed in the posited direction but statistically insignificant. Regardless, the results of this analysis supports matching with somewhat of a liberal bias, providing no evidence of an outside-in temporal relationship between ideology and timing, and showing that extremists jump on the bandwagon later.

In short, the initial discrepancies between the analysis of the 103rd and 106th Congresses are partly a function of coding error and, once these errors are corrected, findings support matching more than signaling. Furthermore, after we use our improved measure of capturing the distance between the member and proposal, the support for matching becomes even stronger. Interestingly, many of the improvements in research design, measurement, and estimation choices which we apply to our analysis of the 106th Congress are not crucial for uncovering the basic relationships guiding cosponsorship and arriving at the inference that matching is key.

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6We use NOMINATE scores to facilitate comparison with our model in the 106th Congress. The results are qualitatively similar when we use ADA scores.
### Table A-1: Replication and Correction of Kessler and Krehbiel (1996)

<table>
<thead>
<tr>
<th>Variable</th>
<th>KK Original</th>
<th>Correcting Cosponsors</th>
<th>Placing Proposal in Policy Space</th>
<th>Basic Model</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Liberals</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Early Stage</td>
<td>0.442*</td>
<td>0.923*</td>
<td>0.006 (0.10)</td>
<td>–</td>
</tr>
<tr>
<td>Intermediate Stage</td>
<td>0.116*</td>
<td>0.457*</td>
<td>0.015 (0.31)</td>
<td>–</td>
</tr>
<tr>
<td>Last Stage</td>
<td>0.137*</td>
<td>0.495*</td>
<td>0.025 (0.51)</td>
<td>–</td>
</tr>
<tr>
<td><strong>Conservatives</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Early Stage</td>
<td>0.117</td>
<td>–0.344*</td>
<td>–1.483* (–13.99)</td>
<td>–</td>
</tr>
<tr>
<td>Intermediate Stage</td>
<td>–0.033</td>
<td>–0.315*</td>
<td>–1.209* (–15.54)</td>
<td>–</td>
</tr>
<tr>
<td>Last Stage</td>
<td>0.013</td>
<td>–0.471*</td>
<td>–1.153* (–15.52)</td>
<td>–</td>
</tr>
<tr>
<td><strong>ln(Electoral Security)</strong></td>
<td>0.006</td>
<td>–0.010</td>
<td>0.032* (2.35)</td>
<td>–</td>
</tr>
<tr>
<td><strong>Seniority</strong></td>
<td>–0.004*</td>
<td>–0.010*</td>
<td>–0.011* (–6.30)</td>
<td>–0.011* (–7.02)</td>
</tr>
<tr>
<td><strong>Ideological Distance</strong></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–1.201* (–17.79)</td>
</tr>
<tr>
<td><strong>Conservatism</strong></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–0.084* (–2.39)</td>
</tr>
<tr>
<td><strong>On Considering Committee</strong></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.486* (14.32)</td>
</tr>
<tr>
<td><strong>Same Party Sponsor</strong></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.016 (0.29)</td>
</tr>
<tr>
<td><strong>Republican</strong></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–0.043 (–0.98)</td>
</tr>
<tr>
<td><strong>Lame Duck</strong></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–0.141 (–1.48)</td>
</tr>
<tr>
<td><strong>Electoral Security</strong></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.318* (2.88)</td>
</tr>
<tr>
<td><strong>Number of Cases</strong></td>
<td>22,185</td>
<td>22,185</td>
<td>21,750</td>
<td>21,149</td>
</tr>
</tbody>
</table>

**Notes:** Dependent variable is cosponsoring a proposal. Coefficients are maximum likelihood estimates, with z-scores in parentheses (*p < 0.05, two-tailed test).