Measurement and theory in legislative networks: The evolving topology of Congressional collaboration

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ABSTRACT

The examination of legislatures as social networks represents a growing area of legislative scholarship. We examine existing treatments of cosponsorship data as constituting legislative networks, with measures aggregated over entire legislative sessions. We point out ways in which the direct application of models from the social networks literature legislative networks aggregated over entire sessions could potentially obscure interesting variation at different levels of measurement. We then present an illustration of an alternative approach, in which we analyze disaggregated, dynamic networks and utilize multiple measures to guard against overly measure-dependent inferences. Our results indicate that the cosponsorship network is a highly responsive network subject to external institutional pressures that more aggregated analyses would overlook.

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1. Introduction

Political science has been slow to embrace social network analysis (SNA). While SNA is not completely absent from the pages of political science journals, over the years since Moreno's sociograms were famously introduced in 1934 (Freeman, 2004), only recently has there been a veritable explosion of interest in the discipline. Given the importance of relational concepts such as power, influence, trust, conflict, collaboration, alliance-formation and coalition-building, to name but a few, it is surprising only that it has taken this long. As others have pointed out, few observations of interest to political science may be convincingly construed as independent from one another (McClurg and Young, 2011).

Beyond the pragmatic use of network tools in addressing the interdependence of cases, a number of political scientists have been making the leap to a genuine network *perspective* in their work. In such investigations, the network becomes more than simply the sum of its parts, more than just a collection of nodes and edges. Consideration of global properties of networks opens up exciting possibilities for the study of political behavior, as we begin to ask questions based not only in the extant theory of our own sub-fields, but also tied to theories developed in the social networks literature and based on broad research programs in human and even animal behavior (Guimera et al., 2005; Faust and Skvoretz, 2002). And yet, as political scientists encountering a wide variety of measures and theoretical assumptions accumulated over decades of contributions to SNA, we face an embarrassment of riches. In sorting through different frameworks for studying social networks, it is tempting to simply adopt approaches already applied by our colleagues, without further reflection, or instead to reach reflexively for the novel, especially when the metaphors suggested by new tools (e.g., centrality and popularity, small worlds, structural holes) fit so nicely with the stories we are trying to tell.

In the current paper, we confine ourselves to an examination of one particular application of SNA, namely to the study of legislative behavior and more specifically, the use of cosponsorship data as a reflection of intra-chamber cooperation and collaboration. Nevertheless, the exercise of thinking carefully about the measurement and operationalization process has implications for any project involving the study of socio-political networks. We would be well advised to consider that even powerful network theories of political behavior ought to be grounded in the decades of theory already developed about political processes. We take the perspective that, while the implications of network theory may at times run counter to some established traditions in political science or may even allow exploration of previously inconceivable research questions, social network theory and analysis will typically supplement our existing theories and knowledge rather than replace them.

In particular, we begin by reviewing the literature analyzing the U.S. Congress and state legislatures as social networks. We finish Section 2 by briefly considering the nature of the structural construct of interest (legislative collaboration), the choices that must be made in the process of operationalizing this construct using cosponsorship data, the importance of modeling assumptions on

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subsequent measurement options, and the consequences of these decisions. Next, in Section 3, we offer our own contribution to the literature, a theory linking Congressional collaborative patterns to institutional public approval trends. In keeping with the overarching theme of the article, we discuss the reasoning behind different possible measurement and modeling options in Section 4, ascertaining the robustness of results to alternative measurements. We discuss our empirical results in Section 5, before offering some concluding thoughts.

2. Literature on the measurement of legislative networks

Analyzing the social behavior of legislators would seem to presuppose the observation of contact or interaction among them. Observations of true social interactions are often unavailable or incomplete. Legislators guard their social relationships closely, and the social behaviors we do observe (co-attendance at fundraising dinners for example) may constitute strategic choices as well as social interactions. Occasionally, scholars are able to unearth clearer data regarding these social interactions. For example, Young (1986) and Bogue and Marlaire (1975) examine the "boardinghouse effect," the effect of shared temporary lodging in Washington, on how legislators vote on the floor. Using data on legislators from 1800 to 1828, Young uncovers a positive relationship between legislators' cohabitation and their common co-voting on bills.[1]

Most work on explicitly social connections between legislators has focused on predicting social interactions rather than their implications for legislating. The earliest studies predicting legislative relationships were conducted in the 1950s and 1960s as single chamber analyses of U.S. state legislatures. Focusing on actual social contacts such as advice seeking, trust, and friendship, these in-depth examinations relied upon surveys and interviews to reconstruct legislative networks from self-identified relationships between legislators. Patterson (1959),[2] Monsma (1966), Caldeira and Patterson (1987, 1988), and Caldeira et al. (1993) all take this approach, providing both the tools for proper relational analysis and unexpected conclusions. For example, Caldeira et al. (1993) note that the predictors of mutual respect between legislators are distinct from the predictors of friendship. These contributions also uncover the importance of cross-party friendships for the spread of information and the diffusion of intra-chamber conflict. Such bridging relationships have been crucial in limiting partisan conflicts and thus avoiding what we now call legislative gridlock.

Recent efforts by scholars have taken up the question of how institutions might influence these dynamics. Particularly, Sarbaugh-Thompson et al. (2006) have found that term limits have amplified legislators' reliance on similarity in the development of many kinds of relationships and strengthened the influence of chamber leadership relative to the rank-and-file. Term limits have also dramatically weakened the tendency of legislators to form the meaningful cross-party ties that facilitate negotiation and conflict resolution. Thus, using a longitudinal analysis of the Michigan House of Representatives, the authors find that term limits may be having the unintended consequence of exacerbating intra-chamber conflict.

Studying explicit social contact between elites offers a comforting level of measurement validity, but is not without its shortcomings. Due to the great effort required to observe these sorts of networks, such studies are difficult to replicate across chambers. Additionally, self-reported relationships are known to suffer from misreporting, due to cognitive constraints, biases, and strategic considerations. This may be of little concern if one is interested in respondents' perceptions and/or self-serving selective memories; still, for objective accounting of interactions, they may be less than ideal (Bernard and Killworth, 1977; Bernard et al., 1980; Killworth and Bernard, 1980; Bernard et al., 1982). Finally, the conventional survey approach to the measurement of social networks only provides scholars with a single snapshot of the social network of interest. In reality, social and legislative relationships are dynamic phenomena, frequently changing across and even within sessions. Any attempts to generalize findings regarding the formation and evolution of legislative networks over time will be particularly difficult within a survey framework.

In order to analyze networks that are at once easier to replicate and more comparable over time, some scholars have turned to analysis of proxy measures for legislative relationships. The most common of these approaches (and the one we will focus our empirical efforts on) is the study of cosponsorship (Burkett, 1997; Fowler, 2006a,b; Tam Cho and Fowler, 2010; Gross, 2010; Kirkland, 2011).[3] While legislators make cosponsorship choices based to some extent on strategy, those choices are also reflective of the broader relational tapestry of a chamber.[4] In other words, the act of cosponsorship contains elements of both strategic and interpersonal influences. While these proxies of legislative interaction are necessarily less valid measures of social relationships than direct observation (were it possible) or even survey-based measures, unprecedented access to legislative archives provides scholars with the opportunity to compile proxies such as cosponsorship into networks across many legislatures and many points in time.

While these relational proxies have proven suggestive in uncovering the importance of relationships for legislative outcomes, they have not been employed to test theories of relational formation itself; simply put, cosponsorship has entered into analysis as explanatory rather than response variable. Fowler (2006a) indicates that there is a strong relationship between how "connected" a member of Congress is and the likelihood that he or she will see the bills and amendments he or she sponsors pass. Kirkland (2011) demonstrates that legislators who build the most diffuse network of cosponsors are much more likely to have bills succeed at veto points across eight state legislative chambers and 15 years of Congressional activity. Waugh et al. (2009) also show that the degree of polarization in the cosponsorship network is highly predictive of large changes in the party control of Congress. Finally, Tam Cho and Fowler (2010) have identified an association between what they term the "small world properties" of the Congressional Cosponsorship network and the amount of important legislation that Congress passes. In other words, they argue, the very topology of the cosponsorship network may have something to do with how successful Congress is in passing important legislation.

While operationalizing legislative cooperation or collaboration in terms of cosponsorship seems fairly natural, scholars who have done so have not excelled at convincingly justifying their measurement choices. It is not clear how exactly the cosponsorship relation should be measured; in contrast with structural variables

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[1] Bogue and Marlaire (1975) uncover a much weaker relationship than does Young (1986), after controlling for geography.

[2] Routt (1938), who wrote of Illinois assemblymen (and politicians more generally) as human relations specialists, seems to have been the first scholar to think about how legislative relationships translate into legislative politics. Patterson (1959) appears to have been the first to apply explicitly sociometric methods and to the study of a legislature.

[3] This is not, however, the only approach that has been taken. Porter et al. (2005) use common committee assignments to generate a network between legislators. Conover et al. (2011) and Sparks et al. (2011) both use the social media network Twitter to construct social networks of legislators.

[4] Koger (2003) notes that legislators cosponsor for explicitly policy-motivated reasons. However, they also cosponsor bills based on the identity of the colleague requesting support.

that persist through time (e.g., friendship, trust, admiration), cosponsorship occurs more or less in discrete time, as the act of signing onto a bill. To be fair, this is a common issue in social network analysis—structural variables are typically thought of as persisting through continuous time (though not necessarily indefinitely, in the sense that friendships end or admiration turns to disenchantment). Yet, these conceptual relations are operationalized using discrete-time observations. In the case of a survey response, as when respondents name their friends, the extrapolation from friendship at the moment of response to friendship over an extended period of interest is defensible, given that the relation is one that does not usually flicker on and off with great frequency. However, when we use *event* data to operationalize a continuous-time construct, as in the case of cosponsoring a bill together, the manner in which the activity serves as proxy for the relationship of interest is far less obvious.[5] Furthermore, the dynamic nature of collaborative choices has been lost. These scholars aggregate cosponsorship choices across many bills introduced and cosponsored on many different days into a single network. Thus, any dynamic shift in network topology that occurs within a legislative session will be missed in the aggregate network. Legislatures are adaptive bodies, constantly responding to new demands from the public and responding to varying internal and inter-chamber pressures. By aggregating this within-session responsiveness into a single network, scholars of legislatures may have overlooked many important elements of the evolving network topology.

3. A theory: Congressional network adaptation to public approval

Instead of viewing the network of cosponsorships aggregated across an entire session, we treat the cosponsorship network as an evolving series of collaborative choices that can change and adapt within sessions. These choices are responsive to both stimuli from outside Congress and interactions within the chamber. We focus our efforts on examining the influences of external stimuli on collaborative networks between legislators. Specifically, we develop and test competing hypotheses regarding the influence of Congressional approval on legislative network topology. It is possible that institutional approval by the public will promote a more collaborative institution, as legislators attempt to alter public perceptions of discord within the legislature. Alternatively, legislators may see low institutional approval coupled with their own electoral security as evidence that the public desires a more active and combative membership, for example if their own constituents support them but have a poor opinion of other MCs.

The first of our competing hypotheses builds on two key insights regarding the behavior of the House of Representatives as an institution. First, the public's approval of Congress is a function of the amount of discord in the Congressional body (Durr et al., 1997; Ramirez, 2009). When partisan conflict is high, the public has an increasingly negative view of Congress as an institution. When the two parties are more cooperative, Congressional approval is less negative (though Congressional approval is never positive, strictly speaking). In fact, Wolak (2007) provides evidence that the Congressional approval plays a key role in the total retirements from the U.S. House of Representatives. Incumbents look to easily discernible national trends to gauge their likelihood of success in re-election. Challengers consider these trends when deciding the timing of a challenge. When Congressional approval is especially low, incumbents can expect to suffer. It is in the re-election inter-

ests of incumbent legislators to maintain a high level of institutional approval, both to ward off challengers and to improve their own electoral margins.

Thus, institutional conflict influences public approval of Congress and public approval of Congress directly influences micro-level Congressional member decisions through electoral motivations. A simple extension of this logic would then imply that, when conflict is high, and re-election at higher risk, members of Congress would adapt their behavior to decrease conflict and improve their electoral odds. The majority party will have particular incentive to reduce conflict in order to prevent retirements of majority party members and risk losing majority party status.[6] However, because institutional approval has an influence on micro-level electoral chances for all members (and, thus, micro-level decisions about retirement for all members), members from both parties should face incentives to collaborate or cooperate more often in times of low institutional approval.

From this, a model emerges in which approval provides a sort of moving target for levels of partisan conflict. When institutional approval is high, parties have a sort of permission from the public to engage in conflict without electoral repercussions, and day-to-day conflict may vary around that target. When approval is low, the target level of conflict around which daily fluctuations may vary will decrease. Because this negativity influences individual members' electoral odds and decision making at the margins, it may be difficult to uncover associations in individual behavior. Instead, aggregate institutional behavior will manifest a shift in levels of cooperation as individual legislators alter their behavior slightly in response to public demands for more cooperation and less conflict. This would suggest that even small changes in individual choices could aggregate into a large change in institutional behavior.

Alternatively, Harbridge and Malhotra (2011) suggest that a bipartisan image of Congress is beneficial in the eyes of weak partisans and independents, but is actually damaging in the eyes of strong partisans. Strong partisans support bipartisanship as an abstract concept, but prefer their own copartisans to "fight the good fight" against the opposition. Their evaluations of incumbents from their own party decline when told those incumbents are engaging in bipartisan behavior.[7] Incumbents who engage in bipartisan activity during times of low Congressional approval may appease weak partisans and independents, but do so while also risking the support of their strong partisan base. Since these strong partisans are both the most likely primary and general election voters, this can be a dangerous strategy. Thus, when Congressional approval is low and incumbents face threatening electoral circumstances, conflict is likely to increase as incumbents attempt to secure the support of their most likely participants. In this way, position taking (Mayhew, 1974) and bipartisanship in Congress are targeted responses not meant for the general population, but for a subset of loyal partisans. These strongest partisans see collaborative bipartisanship as a compromise of partisan ideals. In

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[5] This is actually related to the broader issue of how (or whether) to collapse a bipartite network onto a single set of nodes; the bipartite network here consists of legislators who connect to each other via the bills that they cosponsor.

[6] McDermott and Jones (2003, 2005) and Jones (2010) indicate that Congressional approval is much more damaging to the majority party than to the minority party's electoral outcomes, which might suggest a differential response in bipartisanship between the majority and minority party. However, Volden and Bergman (2006) point out that majority parties have been significantly more cohesive as a group than minority parties. Even if minority parties gain little from improving Congressional approval, their lack of cohesion makes it simpler for the majority party to find potential cooperative partners. The parties may have different responses to low Congressional approval, but the majority party can create an image of bipartisanship much easier than the minority party can resist it.

[7] This differing response according to degree of partisanship may be an explanation for the results reported in Morris and Witting (2001). These authors report that while partisan conflict did damage Congressional approval, bipartisanship did not improve Congressional approval. This may be because bipartisanship has differing effects for strong and weak partisans.

order to avoid losing the support of loyal partisans when it is most critical (when institutional approval is low), members of Congress faced with low levels of institutional support will actually amplify the levels of conflict they project.

Thus, a competing model emerges in which institutional approval provides a moving target for internal conflict in Congress, but with a negative relationship between institutional approval and institutional conflict. When approval of Congress is high, members have little incentive to fight with one another. Increasing institutional approval allows partisan conflict to slowly dissipate. However, when institutional approval is low, members will attempt to appeal to strong partisans by increasing levels of conflict in the chamber. This increasing conflict will take time to emerge, creating long-run dynamic interplay between institutional approval and high-conflict legislating.

Previous research on the dynamic interplay of conflict and institutional approval (Ramirez, 2009) has focused on the number of partisan votes occurring in a given quarter. While this is a reasonable place to start, use of floor votes presents challenges in measuring the degree of conflict in a legislature. First, votes are a strategically chosen subset of possible issues on which conflict might emerge. Indeed, examining only floor votes as a summary of legislative behavior will obscure conflict that might occur over whether a bill is to receive a vote at all. Additionally, both conflict and collaboration are necessarily relational concepts. A legislator must have an alter with whom to collaborate or conflict. Thus, an examination of the influences of public approval on legislative collaboration and conflict is better suited to relational/network measures of organizational behavior. A network approach to the measurement of collaboration and conflict will provide us with the ability to consider all types of collaborative adaptation rather than just partisan/bi-partisan schisms. While inter-party conflict is certainly a prominent type of conflict that might influence evaluations of Congress, one need only consider the conflict between Tea Party and mainstream Republicans to observe that conflict and contentious lawmaking can be a result of both intra and inter-party divides.

The connection between institutional approval and institutional behavior is expected to manifest itself more concretely as a connection between the public's demand for—or opposition to—collaborative legislating and the network of micro-level choices about collaboration in the institution. The connection between public demand and networked behavior emerges because of the electoral influence of institutional approval. As institutional approval varies and the perceived demand for collaborative lawmaking changes in turn, the network of collaboration between legislators ought to adapt to reflect that demand. In order to see if this is indeed the case, we must start by deciding upon a suitable measurement strategy.

4. Measurement of the legislative network, operationalized through cosponsorship behavior

4.1. Measuring the collaboration network

Each act of legislative cosponsorship offers a tantalizing bit of information, both because it represents a quintessentially relational act and because of its lack of measurement error, and sensitivity to changes over time. Contrast the observation that two members of Congress have cosponsored a bill during the past week with the response of these same two individuals to a hypothetical survey, in which they are asked to identify the colleagues with whom they tend to collaborate. That an act of cosponsorship has occurred is beyond doubt, that it occurred during a particular timeframe is also clear, and while its meaning might be debated, the

event transcends individual participants' desire to frame or spin their actions.

Unfortunately, there is no shortage of ways in which to code cosponsorship. There are two levels to the operationalization; even if we make the convincing case that collaboration may be operationalized as cosponsorship, how exactly should a dyadic cosponsorship relation be measured? One possible course of action is to consider two colleagues linked if they have cosponsored even a single bill together. In research on scientific collaboration, coauthor relationships are often coded in this way. But while this may be entirely appropriate for coauthorship, typically an intensive process shared by a small number of individuals, with most pairs of scientists never collaborating on a paper, the nature of legislative cosponsorship is quite different. One criticism of research on cosponsorship is that this may be an example of cheap talk (Koger, 2003, p. 232). We are not going to rehash the arguments on both sides, but starting from the assumption that cosponsorship is not an empty act, it is at least a relatively effortless act, beyond the mental calculus of risk avoidance. As a consequence, treating a single instance of cosponsorship within a year's worth of activity as indicative of an underlying stable relation (a weak tie) yields a complete or virtually complete network, with every legislator connected directly to every other. It is also true that there is great variation in how many cosponsors appear on a piece of proposed legislation and this has implications for the degree to which we might consider cosponsors as something akin to coauthors. After all, that a bill has been cosponsored by just two representatives should give us more information about the latent relationship between them than if they had been but two among 150 cosponsors.

It has been common to consider cosponsorship as a relation from cosponsor to sponsor; the sponsor has taken the lead and by supporting his or her bill, one is implicitly supporting the sponsor as well. For a number of reasons, we will instead think of the relational nature of cosponsorship as occurring among all cosponsors. Careful consideration of the cosponsorship decision by an individual reveals it to be potentially a function of two types of object: people (other cosponsors) and the bill itself. It is possible (albeit unlikely) that legislators might sign their names to a proposal based entirely on its content, without consideration of other cosponsors. Also possible but not plausible (rare exceptions aside) is that they might cosponsor a bill purely based on the cosignatories. In practice, these choices will be based on some combination of both sources of information, along with one's own openness to using cosponsorship as a tool. Cosponsorship is thought of by both scholars and members as a means to signal positions on bills. As such, the other cosponsors on a bill matter in that they convey additional information about the bill's contents.

To construct a sequence of cosponsorship networks over many small discrete windows of time, or snapshots, we code a connection between two legislators, i and j if legislators i and j have cosponsored the same piece of legislation within some short, predetermined period of time. If we were examining 1-day windows of activity, then a connection between legislators i and j would exist if i and j cosponsored the same bill on the same day. We examine the sequence of networks constructed this way using 5-day, 10-day, and 15-day windows. Thus, using a 5-day window to measure networks, on Day t, the cosponsorship network consists of all the cosponsorship activity occurring in the 5 days leading up to and including Day t. On Day $t+1$ a new network emerges, slightly different than the network of the day before, with all of the connections between legislators who collaborated only on Day $t-5$ being removed. If legislator i cosponsors a bill five days before legislator j does, no tie will be recorded between them, *despite having cosponsored the very same piece of legislation*. This temporally disaggregated approach dramatically increases the

probability of observing genuinely coordinated behavior, as legislators who cosponsor bills in temporal proximity to one another are much more likely to be doing so for shared/similar reasons than legislators who cosponsor the same piece of legislation many days apart. This discrete window approach also allows cosponsorship relationships in the legislature to flicker on and off, rather than allowing a single act of cosponsorship at the beginning of a legislative session continue to represent a legislative relationship through an entire session.

At the other extreme, it would be a bit too strict to insist that only shared acts of cosponsorship occurring on the very same day constitute a link, since cosponsoring bills is hardly a top priority on a busy schedule, and legislators may be away from Washington on any given day. The use of multi-day windows makes sense for another reason: the construct we are attempting to capture is collaboration; more specifically, how does the distribution of collaborative relationships vary over time, and in response to level of public support for the institution? Individual instances of cosponsorship occur on a single day, but may be viewed as reflecting an unobserved relationship that persists beyond the momentary manifestation. In fact, in quite concrete terms, cosponsorship of a bill persists through time as the bill awaits further action (or, more commonly, death by natural causes once the session ends). For two legislators to be considered as maintaining a collaborative relationship, it should not be necessary for them to jointly cosponsor a bill each day, but rather that a not unusually long period of time pass between instances. Just as a friendship ought not to be declared dead if friends do not talk on a given day, a professional collaborative relationship should not be considered defunct if some time passes in which two colleagues do not participate in some joint enterprise. And, as with friendship, there is rarely a precise, agreed-upon moment in which such a relationship ends (or is suspended); once typical patterns of interaction are disrupted for a long enough period, however, we may be more bold in asserting a change in the nature of this relationship.

We make no claim that this approach to network operationalization is the "most" correct. We do believe that disaggregating the network into finer discrete time intervals can teach us things that a more aggregated approach obscures, in particular how a legislative chamber evolves in response to external stimuli. With any proximate measure of a concept of interest, a number of approaches could be employed to test hypotheses and relationships. The appropriate way to deal with this lack of validity is through multiple measures and operationalizations. We provide several global network measures for our particular approach to the network, but recognize that there are several other plausible ways to build a cosponsorship network from legislative behavior.

4.2. A model of network adaptation

In order to test our predictions regarding the adaptation of Congressional conflict to institutional approval, we make use of two data sources: the Congressional approval time series from Gallup Polls[8] and Congressional Cosponsorship networks (Fowler, 2006a,b).[9] Once we have constructed our series of networks, allowing the window of cosponsorship to vary, we extract a number of summary measures from each of these daily networks. Increased conflict or collaboration may have various consequences for the

topology of cosponsorship networks as we have measured them. Our analysis focuses on the average path length, clustering coefficient, and party modularity of cosponsorship on each day of the smoothed network series. Average path length (APL) measures the average shortest path between any two nodes, which should increase as conflict within the chamber increases. Fewer cooperative ties bridging important ideological divisions in the network should generate a longer APL. When institutional approval of Congress does temper or enflame conflict in the ways we expect, then as institutional approval declines, path length should change. When institutional approval is especially low, MCs recognize a need to alter levels of collaboration and adapt the network accordingly.

We measure the clustering coefficient as the average proportion of actors' neighbors who are themselves connected to each other (Tam Cho and Fowler, 2010; Watts and Strogatz, 1998). As conflict increases, we expect that clustering in the network will also change. If decreasing approval reduces the taste for collaboration, then as approval declines and legislators become less likely to work together, clustering should decrease. Neighbors become less likely to work together as the general level of conflict in the chamber increases and individuals become hesitant to publicly advertise their associations. Alternatively, if decreasing approval should *increase* the drive to collaborate, then as approval declines clustering should increase. Neighbors become much more likely to collaborate as collaboration becomes increasingly common throughout the chamber.

Our final reported measure of the evolving topology of Congressional Cosponsorship is modularity based on partisan divisions. Modularity (Newman, 2006) measures the probability of within group connections in a network relative to the probability of connections between any two actors in the network. The formula for modularity is:

$$Mod = \frac{1}{2m} \sum_{ij} \left[A_{ij} - \frac{k_i k_j}{2m} \right] * \delta(c_i, c_j) \tag{1}$$

where m is the number of total connections in the network, A_{ij} is the connection between actors i and j, k_i is the total number of connections actor i has, and $\delta(c_i, c_j)$ is the Kroeneker delta for actors i and j.[10] For party-based modularity, c_i represents the party affiliation of actor i. Thus, party modularity measures within party connections relative to the connections that an analyst might expect at random between two actors given the density of the network and the degree of the two actors. Taken together, these three statistics represent a useful summary of the degree of conflict in the cosponsorship network, which we expect to respond to changes in institutional approval.

Our theoretical model makes very specific predictions about the nature of the link between public opinion and network topology. Public opinion generates a sort of moving target, to which micro behavior adapts and which aggregate topology tracks over time. This moving target represents an equilibrium force around which network topology may vary in the short run, but will converge towards in the long run. Thus, we require a model capable of separating short-term covariance from long-term equilibrium forces. The error-correction model provides just such a specification.

Error-correction models (ECM) are based on the logic of co-integration in time series analysis, but as De Boef and Keele (2008) note, are perfectly appropriate for analysis of stationary data. In fact, the general error-correction model is simply a re-interpretation of the auto-regressive distributed lag model (ADL). Both the ADL model and the ECM estimate the same informa-

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[8] Polls for 2001–2007 can be found here: http://www.gallup.com/poll/28456/congress-approval-rating-matches-historical-low.aspx. Polls for 2008 were also found on Gallup's website.

[9] Cosponsorship data can be retrieved from the THOMAS database at: http://thomas.loc.gov/home/LegislativeData.php?&n=BSS, or on Fowler's website at http://jhfowler.ucsd.edu/cosponsorship.htm.

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[10] The Kroeneker delta is a mathematical function yielding a one if the two numbers upon which it operates are equal to each other, and a zero otherwise.

tion, but the ECM allows researchers to access long run effects more easily.[11] The general error-correction specification is as follows:

$$\Delta Y_t = \alpha_0 + \alpha_1 Y_{t-1} + \beta_0 \Delta X_t + \beta_1 X_{t-1} + \epsilon_t \qquad (2)$$

The model represents the short-term effects of X on Y through β_0. We expect the short-run effects of institutional approval on network topology to be indistinguishable from zero; there is no reason to suppose cosponsorship patterns will be sensitive to short-term fluctuations in opinion polls, which are themselves a noisy indicator of support. The long-run multiplier for the effect of X on Y is:

$$k = \frac{\beta_1}{-\alpha_1} \qquad (3)$$

The long-run multiplier represents the total effect of some X variable on Y. This total effect is distributed over time with the long-run effects occurring at the error-correction rate given by $-\alpha_1$. If a long-run multiplier is equal to one and has an error correction rate of 0.5, then a unit change in X eventually produces a total of a one-unit change in Y, with rate 0.5 governing how quickly this change takes place. In the first time period following the change in X, Y will change by 0.5. In the second time period, Y will change by 0.25. In the third time period following change, Y will change by 0.125 and so on as Y accumulates the full one unit change and approaches its new equilibrium. Thus, X possesses some immediate or contemporaneous effect on Y, but also possesses some effect on Y that occurs over time. As X changes, it disturbs the equilibrium of Y, which then slowly adapts to a new equilibrium.

Error-correction models are gaining popularity in political science. By allowing us to estimate both short-term covariance and long-term equilibration, they represent a powerful tool. Indeed, some of the most recent work on the relationships between public opinion and the behavior of elite political actors features error-correction specifications (Ura and Wohlfarth, 2010; Ramirez, 2009; De Boef and Keele, 2008; Jennings and John, 2009). Based on our outlined theory, we expect that Congressional approval will have no short-run effect on the measured structure of Congressional Cosponsorship networks, but will have a statistically distinguishable long-run effect on these networks. Specifically, as institutional approval declines and incumbents perceive a risk to their re-election goals, they will slowly adjust the level of cooperation and conflict in the chamber in an effort to improve their electoral fortunes.

5. Results and discussion

Before proceeding to our model results, we present the descriptive statistics from our models in Table 1. We report the means of the variables of interest in our models and the standard deviations (in parentheses). Not surprisingly, the variance in each of the network measures decreases as the measurement window we utilize increases. Lengthening the measurement window creates a smoother series in which the value on any day t is more closely related to the network summary on day $t-1$. This smoothing of the series decreases the variability in the network measures. Because the number of bills introduced on a particular day and the approval of Congress on a day are not network measures and can be assessed directly, we leave those values unsmoothed. It is also worth noting that as the size of the measurement window increases, the number of communities detectable in the cosponsorship network decreases

Table 1
Descriptive statistics in three difference measurement windows.

Variable name	5-Day model	10-Day model	15-Day model
Average path length	1.814	1.552	1.404
	(0.230)	(0.167)	(0.138)
Clustering coefficient	0.639	0.686	0.738
	(0.076)	(0.071)	(0.069)
Partisan modularity	0.146	0.125	0.100
	(0.058)	(0.051)	(0.045)
Number of communities	37.901	16.126	12.292
	(31.857)	(12.312)	(5.118)
Congressional approval	39.203	39.203	39.203
	(14.173)	(14.173)	(14.173)
Number of bills introduced	143.139	143.139	143.139
	(79.402)	(79.402)	(79.402)

Table reports the mean values of the variable in the measurement window with the standard deviation of the variable in parentheses. Congressional approval and the number of bills introduced do not vary by measurement window because we recorded their values on the day in question. They are not latent concepts in need of smoothing, but are instead measured directly.

quickly. This seems to indicate that in any brief period of time, the low-dimensional structure of legislative choices may be hard to detect, while aggregation of many choices makes that phenomenon easier to spot (Crespin and Rohde, 2010).

Below we present results from error-correction models predicting the average path length of the evolving Congressional Cosponsorship network from 2001 to 2008. As discussed in the previous section, we aggregate daily network activities into windows of different lengths of time. Thus, we include cosponsorships from several consecutive workdays in the same network. Our tables present results from windows of 5-, 10-, and 15-day periods. For example, for 5-day windows, the cosponsorship network on Day t represents the aggregation of cosponsorship activity from 5 days before Day t up to Day t. We include the number of bills introduced during each window as a covariate, along with Congressional approval as measured by Gallup. We include the former because it seems likely that the cosponsorship network is fundamentally affected by the number of opportunities for cosponsorship between legislators that might exist. The error-correction specification dictates that both the differenced and lagged values of the independent variables be included to separate out short-run and long-run effects.

Table 2 presents the results of error-correction models predicting the average path length of the cosponsorship network for three different time windows.[12] As predicted, the coefficient on the difference in Congressional approval is insignificant, which indicates that Congressional approval has no measurable short-run effects on the average path length of the cosponsorship network. However, the coefficient on the lagged value of Congressional approval is negative and statistically distinguishable from zero in each of the models presented. Coupled with a significant lagged dependent variable, this indicates that Congressional approval has a long-run, equilibrating effect on the average path length of the cosponsorship network. Additionally, the negative value on the long-run multiplier for Congressional approval indicates that when Congressional approval decreases, the average path length of the cosponsorship network increases.

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[11] These long-run equilibrium effects are perfectly captured by ADL models, but require a bit more algebra to calculate, while the ECM specification allows for the direct estimation of a long-run effect.

[12] It is common in studies of average path length and clustering coefficients to normalize these quantities by measuring the same quantity on a randomly generated graph with the same number of nodes and edges with the edges placed randomly across the network. The results we present are from non-normalized versions of these statistics, but the appendix to this article provides an analysis of the normalized network statistics.
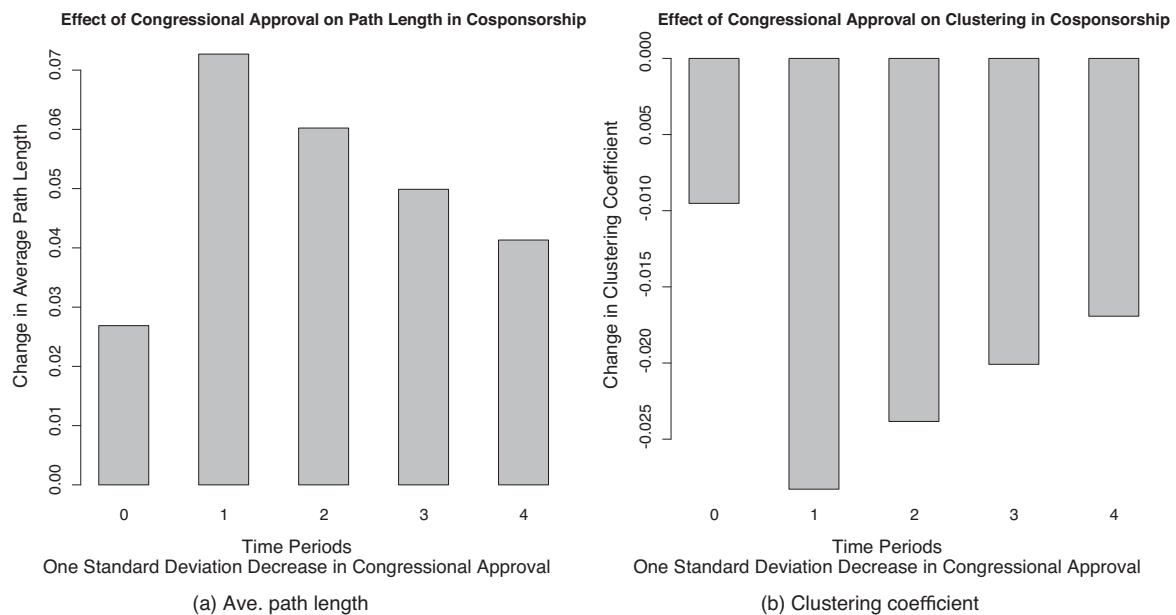
Fig. 1. Distributed effects of Congressional approval on change in average path length and clustering coefficient of Congressional Cosponsorship network in 5-day windows: (a) ave. path length and (b) clustering coefficient.

Table 2
Error correction models predicting the difference in average path length at three different time windows.

Variable name	5-Day model	10-Day model	15-Day model
APL_{t-1}	−0.172*	−0.049*	−0.032*
	(0.016)	(0.009)	(0.008)
Δ bills introduced	−0.001*	−0.0002*	−0.0002*
	(0.000)	(0.00002)	(0.00001)
Bills introduced$_{t-1}$	−0.001*	−0.003*	−0.0002*
	(0.000)	(0.000)	(0.00002)
Δ con. approval	−0.002	−0.0002	0.000
	(0.002)	(0.0008)	(0.001)
Con. approval$_{t-1}$	−0.001*	−0.0003*	−0.0002*
	(0.000)	(0.0001)	(0.00008)
Constant	0.439*	−0.134*	0.078*
	(0.035)	(0.017)	(0.012)
Long-run multiplier for approval	−0.004*	−0.006*	−0.004*
	(0.0002)	(0.0001)	(0.0001)
R^2	0.211	0.165	0.135

Table presents coefficients from error correction models predicting change in average path length of Congressional Cosponsorship networks at varying levels of aggregation. Standard errors are reported in parentheses. Long-run multipliers and standard errors are calculated using the Bewley transformation. $n = 1017$.
* $p < 0.05$.

Table 3
Error correction models predicting the difference in clustering coefficient at three different time windows.

Variable name	5-Day model	10-Day model	15-Day model
Clustering$_{t-1}$	−0.157*	−0.073*	−0.046*
	(0.017)	(0.012)	(0.008)
Δ bills introduced	0.0001*	0.0001*	0.0001*
	(0.0000)	(0.0000)	(0.0000)
Bills introduced$_{t-1}$	0.0001	0.0001*	0.0001*
	(0.0000)	(0.0000)	(0.0000)
Δ con. approval	0.0006	−0.0002	0.0000
	(0.0007)	(0.0004)	(0.0000)
Con. approval$_{t-1}$	0.0002*	0.0001*	0.0001*
	(0.0000)	(0.0000)	(0.0000)
Constant	0.075*	0.033*	0.020*
	(0.016)	(0.008)	(0.006)
Long-run multiplier for approval	0.001*	0.002*	0.002*
	(0.0001)	(0.0001)	(0.0000)
R^2	0.105	0.072	0.096

Table presents coefficients from error correction models predicting change in the clustering coefficient of Congressional Cosponsorship networks at varying levels of aggregation. Standard errors are reported in parentheses. Long-run multipliers and standard errors are calculated using the Bewley transformation. $n = 1017$.
* $p < 0.05$.

Error-correction specifications are capable of providing estimates of several types of dynamic effects all within the same model.[13] This can make the direct interpretation of coefficients somewhat difficult. As such, Fig. 1(a) plots the effects of a one standard deviation decrease in Congressional approval over 5 time points using the coefficients from the 5-day model in Table 2. This figure helps demonstrate the pronounced differences in the immediate and long-term effects of changes in Congressional approval. The contemporaneous change in average path length due to a one standard deviation decrease in Congressional approval is an indistinguishable increase in average path length.[14] The subsequent changes in average path length due to changes in Congressional approval are large individually, and are considered the result of an equilibrating process that distributes the cumulative long run multiplier over many time periods. For this model, the long-run multiplier for a one-unit change in Congressional approval is a statistically significant −0.004, twice the size of the short-run effect estimated by the model (−0.002).[15]

Table 3 presents error-correction models for the clustering coefficient of the cosponsorship network for three different lengths

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[13] See De Boef and Keele (2008) for proper calculation of long-run multipliers, interpretation of effects from error-correction models, and the creation of distributed lag effects plots. Recall that long-run multipliers are the ratio of the lagged independent variable of interest over the negative of the lagged dependent variable.

[14] For all of our subsequent interpretations we begin at the mean value of Congressional approval of 39.2 and move one standard deviation down to an approval level of 25.03.

[15] We calculated standard errors for the long-run multiplier using the Bewley Transformation. See Bewley (1979).

of temporal aggregation. Once again, the independent variables in the model include the number of bills introduced on a given day and Congressional approval. As was the case for average path length, Congressional approval has no short-run effect on the level of clustering in the cosponsorship network. There is, however, a statistically significant and positive coefficient on the long-run multiplier for Congressional approval. This indicates that Congressional approval may have a positive long-run effect on clustering in the cosponsorship network. As Congressional approval increases, clustering in the network equilibrates to a higher level over a long period. This result is robust to the length of aggregation we employ. The contemporaneous effect of a one-unit change in Congressional approval is an insignificant 0.0006, while the long-run multiplier on a one-unit change in Congressional approval is a larger and statistically significant 0.001. This supports the notion that decreasing institutional approval leads to decreasing conflict and thus decreasing clustering. As Congressional approval declines, legislators become less likely to maintain small, restrictive clusters of collaborative partners and become more likely to develop expansive, unclustered networks.

Once again, we provide distributed effects plots in Fig. 1(b). This figure plots the effect of a one standard deviation decrease in Congressional approval on clustering in the cosponsorship network across five time points using the coefficients from the 5-day model in Table 3. When Congressional approval decreases, there is an immediate decrease in the level of clustering in the network. This immediate decrease is statistically indistinguishable from zero and precedes a series of decreases in clustering in the periods following the change in Congressional approval. A one standard deviation decrease in Congressional approval corresponds to an immediate 0.009 unit decrease in clustering in the cosponsorship network at $t = 0$. The larger long-run change cumulates over time with a decrease of 0.028 occurring at $t = 1$ and a decrease of 0.023 occurring at $t = 2$. This will continue until the full long-run effect of a one standard deviation decrease from the mean of Congressional approval is cumulated, and the equilibrium level of clustering is reached. Notice that the estimated long-run multiplier is nearly twice the size of the contemporaneous change in clustering. Also, given the slow rate of equilibration, this process will take many days to manifest the total effect of the equilibrium shift indicating a lengthy long-run effect.

Each of the models we report uses network summary statistics as dependent variables. As such, the interpretation of the magnitude of effects is somewhat difficult. A decrease of 0.028 in the clustering coefficient is a not a particularly intuitive quantity. To aid in the interpretation of effects, we have generated networks of 435 actors which take on certain network characteristics, and then describe the behavior change necessary to produce the change in network statistics reported by the model estimates. For example, we report in Table 1 that the mean of the clustering coefficient for the 5-day measurement window is approximately 0.64. In a simulated network of 435 actors, with a density equal to the mean density of the cosponsorship networks in our 5-day measurement windows (0.6), this mean level of clustering is akin to saying that an actor's neighbors have a 78% chance of being connected to one another.[16] A change of −0.028 in the clustering coefficient from its mean level (the decrease in clustering produced by a one standard deviation decrease in Congressional approval), is equivalent to saying that an actor's neighbors drop to only a 62% chance of being connected to one another. This is still a high degree of clustering, but a sixteen percentage point decrease the probability

that neighbors are connected to one another seems substantively consequential.[17]

Similarly, we can interpret the increase in the average path length of a network as the change in behaviors that would result in the change in the network characteristics observed. A one standard deviation decrease in Congressional approval would increase the average path length of the cosponsorship network by $-14.17 \times -0.004 = 0.057$. Thus, this would move the cosponsorship network from its mean average path length of 1.814 to an average path length of 1.871. We again start out with a network of 435 actors and randomly distribute connections in the network until the average path length of the network reaches the mean average path length in the Congressional Cosponsorship networks. We then remove connections from the network until the path length reaches a distance of 1.871. In these randomly generated networks the vast majority of pairs of actors are only one or two steps apart. In the randomly generated network with the Congressional mean average path length only 66 pairs of actors are a full three steps apart from one another in the network. Alternatively, when we eliminate sufficiently many connections to increase the average path length in the network up to 1.871, 148 actors are three steps apart. Put another way, in a randomly generated network with a path length equal to the mean cosponsorship path length and 435 actors, 75.6% of the pairs actors are more than one step apart. After we manipulate this random network to increase its average path length to 1.871 (the change in the network's path length corresponding to a one standard deviation decrease in Congressional approval), 86.9% of the pairs of actors are more than one step apart.

Coupled with the results from the clustering coefficient analyses, these results indicate that when Congressional approval decreases, the cosponsorship network becomes somewhat less clustered and has longer typical path length. When approval is low, representatives become more likely to cosponsor with partners outside of their dense clusters of common collaborators and yet average distances between actors in the network grows. The average distance between legislators in the network is likely growing *because* clustering is breaking down, reducing the efficiency gained by bridging connections across group divisions. A compelling explanation is that the cosponsorship network is adapting to Congressional approval. We take these results as initial indications that the cosponsorship network is becoming less contentious. Clusters are breaking down and distance between actors in the network is increasing as a result. These results are in keeping with our hypothesis that low Congressional approval creates an incentive towards collaboration, but a more fine grained analysis focused specifically on partisanship can be crafted.

Our next two analyses focus on the *community structure* of cosponsorship. Table 4 presents error-correction models predicting the difference in partisan modularity of the Congressional Cosponsorship network at three different levels of aggregation. Table 5 presents error-correction specifications predicting the difference in the number of communities detected in Congressional Cosponsorship at three different levels of aggregation. We calculate the number of communities in the cosponsorship network using the "fastgreedy" algorithm of Clauset et al. (2004). We allow the "fastgreedy" algorithm to search for the optimal partition of the network and count the number of communities that emerge out of the network partition with the highest modularity. We supplement partisan modularity scores with this count of communities detected because the breakdown in clustering and cooperation may occur within parties, across parties, or in groups orthogonal

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[16] As stated earlier, clustering in a network is a measurement of how often one's neighbors are connected to one another.

[17] Code to demonstrate these simulated networks can be provided upon request.

Table 4
Error correction models predicting the difference in party modularity at three different time windows.

Variable name	5-Day model	10-Day model	15-Day model
Modularity$_{t-1}$	−0.178*	−0.056*	−0.036*
	(0.018)	(0.011)	(0.008)
Δ bills introduced	−0.00003*	−0.00003*	−0.00003*
	(0.00001)	(0.00001)	(0.00000)
Bills introduced$_{t-1}$	−0.00003	−0.00002*	−0.00002*
	(0.00002)	(0.00001)	(0.00001)
Δ con. approval	0.0002	0.0003	−0.00004
	(0.0006)	(0.0003)	(0.0002)
Con. approval$_{t-1}$	0.0001	0.00001	0.00000
	(0.0001)	(0.00004)	(0.00002)
Constant	0.027*	0.011*	0.007*
	(0.006)	(0.002)	(0.002)
Long-run multiplier for approval	0.0004*	0.0001*	0.0001*
	(0.0001)	(0.0000)	(0.0000)
R^2	0.091	0.048	0.051

Table presents coefficients from error correction models predicting change in the party modularity of Congressional Cosponsorship Networks at varying levels of aggregation. Standard Errors are reported in parentheses. Long-run multipliers and standard errors are calculated using the Bewley transformation. $n = 1017$.

* $p < 0.05$.

to parties. Thus, a modularity score based explicitly on partisan conflict may miss other types of conflict that emerge in the chamber.

While the contemporaneous coefficients for Congressional approval presented in Table 4 are themselves insignificant, the long-run multiplier for the effect of a change in Congressional approval (measured as the ratio of the coefficient on lagged Congressional approval over the negative lagged dependent variable) is a statistically significant 0.0004. This, combined with the results of the error-correction model's short-run effects indicates that a one standard deviation decrease in Congressional approval from the mean is associated with a 0.0063 long-run decrease is partisan modularity. While this effect is not substantively impressive (a drop of one-ninth of a standard deviation in modularity for a one standard deviation decrease in approval), it is distinguishable from zero and a possible indication of the role played by declining

Table 5
Error correction models predicting the difference in the number of detectable communities at three different time windows.

Variable name	5-Day model	10-Day model	15-Day model
Communities$_{t-1}$	−0.106*	0.018*	0.018
	(0.013)	(0.010)	(0.011)
Δ bills introduced	−0.077*	−0.010*	−0.003*
	(0.005)	(0.002)	(0.001)
Bills introduced$_{t-1}$	−0.084*	−0.013*	−0.004*
	(0.007)	(0.002)	(0.001)
Δ con. approval	0.050	0.058	−0.003
	(0.221)	(0.064)	(0.030)
Con. approval$_{t-1}$	−0.078*	−0.015*	−0.004
	(0.029)	(0.008)	(0.004)
Constant	19.61*	2.354*	0.607*
	(1.976)	(0.576)	(0.307)
Long-run multiplier for approval	−0.739*	0.829*	0.193*
	(0.032)	(0.012)	(0.005)
R^2	0.198	0.049	0.023

Table presents coefficients from error correction models predicting change in the number of communities detected by the "fastgreedy" algorithm for Congressional Cosponsorship Networks at varying levels of aggregation. Standard errors are reported in parentheses. Long-run multipliers and standard errors are calculated using the Bewley transformation. $n = 1017$.
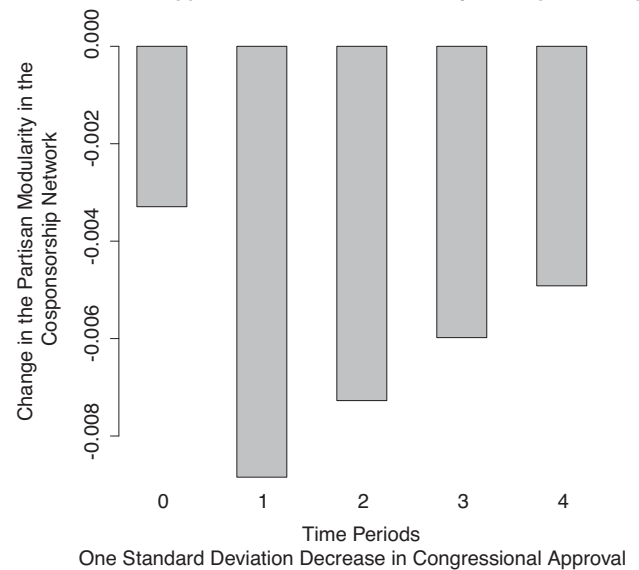
* $p < 0.05$.

Effect of Approval on Partisan Modularity in Cosponsorship

One Standard Deviation Decrease in Congressional Approval

Fig. 2. Distributed effects of Congressional approval on change in partisan modularity in the Congressional Cosponsorship network in 5-day windows.

institutional approval in creating a less contentious, more collaborative network.[18]

Fig. 2 plots the distributed effects of a one standard deviation decrease in Congressional approval on partisan modularity. Initially, the contemporaneous effect of a decrease in approval is a small decrease in partisanship, but an insignificant one. Over time, the cosponsorship network becomes less partisan in response to decreases in Congressional approval. With the total change in partisanship over many time points (0.0004) being twice the size of the contemporaneous effect (0.0002). Interpreting the magnitude of effects in the modularity model can be difficult. Long-run and short-run effects combine to produce changes in modularity, and modularity itself is a balance of within versus across party connection density. As mentioned, a one standard deviation decrease in Congressional approval corresponds to a decrease of 0.0063 in partisan modularity in the 5-day model. In a network of 435 actors with 50 within party cosponsorships and 54 cross-party cosponsorships, this change in modularity is roughly equivalent to two additional cross-party cosponsorships in a 5-day period or a decrease of two within party cosponsorships in either party.

Finally, Table 5 indicates that Congressional approval consistently exhibits a long-run relationship with the number of communities in the cosponsorship network. The analysis of the number of communities discovered by "fastgreedy" in the cosponsorship network indicates that Congressional approval lacks a discernible contemporaneous effect, but does have a long-run equilibrating effect on the community structure of the cosponsorship network. In the 5-day model, the contemporaneous effect of a one-unit change in Congressional approval is a statistically insignificant 0.05, while the long-run multiplier is a statistically significant −0.739. This indicates that over time as Congressional approval

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[18] This result also demonstrates the importance of the full interpretation of error-correction models. These models contain information on two short-run effects and a long-run effect that require some post-model calculations to properly interpret. The coefficients from lagged and differenced Congressional approval do not indicate any effects of Congressional approval on modularity, but error-correction models allow for more nuanced interpretations and effects. As is the case with interaction or conditional coefficients, the parameter estimates initially obtained are not of primary interest when taken in isolation.

decreases, the number of detectable communities in the cosponsorship network equilibrates to a larger number. However, notice that the relationship changes signs as the level of aggregation changes. It may be that the relationship between the number of detectable communities and institutional approval is unstable and sensitive to the kind of aggregation employed in the creation of the network. While this would suggest that this particular model is not especially useful for evaluating our theory, it does remind us just how critical are the choices we make in operationalizing the cosponsorship network. Network constructs are sensitive to these choices and different operationalizations can lead to dramatically different results.[19]

Taken together, these results indicate that when Congressional approval decreases, the cosponsorship network responds by becoming less clustered, more spread out, and less partisan. When popular opinion of Congress declines, cosponsorship clusters break down and representatives work outside of otherwise stable clusters, the average path length in the network grows, and most importantly, partisanship declines. We take these as evidence that 1) the cosponsorship network may in fact be adapting to changes in popular opinion of the institution and 2) when Congressional approval declines, legislators respond by becoming less contentious and more willing cooperative partners. That the partisan nature of the cosponsorship network in the U.S. House weakens in the face of declining Congressional approval would seem to be strong evidence that as Congressional approval declines, representatives adapt by working together across ideological lines more often.

6. Conclusion

We have sought to draw attention to the fact that inferences about Congressional cooperation based on records of cosponsorship may be quite sensitive to measurement choices. Compound global measures of social networks carry assumptions that should not be ignored; in particular, the timescale of aggregation may profoundly affect the conclusions we reach. Choices in this regard will be in part pragmatic ones, but should be tailored to the research questions being asked and reflect the subjective judgment of the researcher—suitably defended. Furthermore, the results of alternate specifications should be provided; these not only indicate the robustness of results to measurement choices, but may offer additional insight into the dynamics of legislative collaboration.

Rather than operationalizing collaboration based upon cosponsorship across an entire session, we disaggregate the cosponsorship network into smaller discrete time intervals. By breaking the network down by weeks of activity, we may detect subtle changes over the course of a session and avoid overemphasizing individual acts of cosponsorship. By using multiple measures, we protect ourselves somewhat against measure-dependent inferences. We then offer an examination of the dynamic nature of collaboration in the U.S. House and uncover that collaborative legislative

networks are highly responsive to external stimuli in a number of ways. As institutional approval of Congress declines, the cosponsorship network tends to become less conflict-prone, as indicated by longer average path length, lower levels of clustering, and lower party modularity. This easing of conflict may be driven, we argue, by electoral incentives. Incumbents wish to alter the perception of their institution because that perception has important influences on electoral events like the emergence of challengers and the retrospective choices of voters. Previous work on Congressional approval has recognized that a link exists between the electoral fortunes of incumbents and Congressional approval. We have simply taken the next step by examining how legislators then respond to that changing environment. These responses accumulate slowly, but are detectable nonetheless. Thus, collaborative networks in the U.S. House may be fruitfully examined as dynamic phenomena, changing over time in predictable ways.

Moving forward, we hope to see further analyses of cosponsorship networks. There is no doubt that both micro and macro patterns in cosponsorship offer valuable insights into legislative behavior and processes. Social network tools and theories can provide extremely useful ways to examine an environment highly characterized by interdependent behaviors. Nevertheless, legislative scholars must move with some caution. As with any new methodological tool, the choices and assumptions we make as analysts play a critical role in the results we observe.

Appendix A. Supplemental appendix

In Section 5, we provide error correction models predicting the average path length and clustering coefficient of the Congressional Cosponsorship network at different levels of aggregation. For these analyses, we utilize the calculated path length and clustering coefficients, but the common practice in network studies of these quantities is to normalize these statistics. Rather than simply examining the average path length from two potentially dissimilar networks, an analyst calculates the average path length in the network and divides that number by the average path length from a randomly generated network with the same number of nodes and edges, but with edges placed down randomly in the network. This ratio provides the average path length in the network relative to the expected value of average path length in a random network. Because our models focus on the way network behaviors change within the same network from one time point to the next, we thought it simpler and more appropriate to present the raw statistics, but in the interests of completeness, we have also done the equivalent analyses using the normalized statistics.

Table 6 provides the results from error correction models predicting the normalized average path length of the Congressional Cosponsorship network at three levels of aggregation. To normalize the path length from the network, we rewire the edges of the network with a probability of 0.9. This provides us with a randomly generated network against which we can compare the cosponsorship network. Each of the long-run multiplier effects remains negative and statistically significant, just as in the earlier presentation. Once again, Fig. 3(a) plots the change in average path length for a one standard deviation decrease in Congressional Approval using results from the 5-day models. The shape of the curve is nearly identical to that presented in Fig. 1(a). Thus, the normalization of the average path length statistic does not seem to have changed our inferences about the cosponsorship network's responsiveness.

Table 7 provides the same error correction models, this time using the normalized clustering coefficient as the outcome variable of interest. The normalization matters much more here than it did for the average path length. The long run multipliers for

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[19] The R^2 from our models may seem low, but as mentioned earlier, the dependent variable in a error correction model is the change in Y. Thus, the amount of variance explained by a model is not an overly useful metric for evaluating error correction model fit. However, error-correction model is a reparametrization of the auto-regressive distributed lag model (ADL), where the ADL model predicts outcome variables using a lagged dependent variable and contemporaneous and lagged independent variables. The ADL and ECM are equivalent models with different algebraic manipulations (De Boef and Keele, 2008). If we re-run our models as ADLs rather than ECMs, the R^2 from our 5-day clustering coefficient model is 0.723, the R^2 from our 5-day path length model is 0.764, the R^2 from our modularity model is 0.682, and finally the R^2 from our communities model is 0.846. The R^2 values all increase as we move from the 5-day models to the 10-day and 15-day models with 3 of the 4 15-day models having R^2 values over 0.90. Thus, our models have stronger predictive power than the R^2 from the ECMs suggest.

Table 6
Error correction models predicting the difference in normalized average path length at three different time windows.

Variable name	5-Day model	10-Day model	15-Day model
Normalized APL_{t-1}	−0.207[*]	−0.059[*]	−0.039[*]
	(0.018)	(0.009)	(0.008)
Δ bills introduced	−0.001[*]	−0.0003[*]	−0.0002[*]
	(0.000)	(0.0000)	(0.0000)
Bills introduced$_{t-1}$	−0.001[*]	−0.0003[*]	−0.0002[*]
	(0.000)	(0.0000)	(0.0000)
Δ con. approval	−0.002	−0.0003	0.000
	(0.002)	(0.001)	(0.001)
Con. approval$_{t-1}$	−0.001[*]	−0.0003[*]	−0.0002[*]
	(0.000)	(0.0001)	(0.00001)
Constant	0.764[*]	−0.239[*]	0.149[*]
	(0.061)	(0.029)	(0.022)
Long-run multiplier for approval	−0.002[*]	−0.006[*]	−0.005[*]
	(0.0002)	(0.0001)	(0.0001)
R^2	0.210	0.178	0.151

Table presents coefficients from error correction models predicting change in normalized average path length of Congressional Cosponsorship networks at varying levels of aggregation. Average path length is normalized by rewiring the edges of the Congressional Cosponsorship network with probability of 0.9. Standard errors are reported in parentheses. Long-run multipliers and standard errors are calculated using the Bewley transformation. $n = 1017$.

[*] $p < 0.05$.

Table 7
Error correction models predicting the difference in normalized clustering coefficient at three different time windows.

Variable name	5-Day model	10-Day model	15-Day model
Normalized CC_{t-1}	−0.154[*]	−0.026[*]	0.079[*]
	(0.016)	(0.010)	(0.014)
Δ bills introduced	−0.00001[*]	-0.2×10^5[*]	-0.6×10^6[*]
	(0.000)	(0.0000)	(0.0000)
Bills introduced$_{t-1}$	−0.00001[*]	-0.2×10^5[*]	-0.6×10^6[*]
	(0.000)	(0.0000)	(0.0000)
Δ con. approval	−0.00001	-0.5×10^5	-0.1×10^6
	(0.00004)	(0.0001)	(0.001)
Con. approval$_{t-1}$	−0.00001[*]	-0.3×10^5[*]	-0.1×10^5[*]
	(0.000)	(0.0000)	(0.0000)
Constant	0.004[*]	−0.0005[*]	0.00007
	(0.000)	(0.0000)	(0.00006)
Long-run multiplier for approval	−0.00007[*]	0.0001[*]	0.00001[*]
	(0.0000)	(0.000)	(0.0000)
R^2	0.163	0.088	0.076

Table presents coefficients from error correction models predicting change in normalized clustering coefficient of Congressional Cosponsorship networks at varying levels of aggregation. The clustering coefficient is normalized by rewiring the edges of the Congressional Cosponsorship network with probability of 0.9. Standard errors are reported in parentheses. Long-run multipliers and standard errors are calculated using the Bewley transformation. $n = 1017$.

[*] $p < 0.05$.

these networks switch directions between levels of aggregation, sometimes mirroring the results from the non-normalized analysis and other times reversing directions. Fig. 3(b) plots the effects of a one standard deviation decreases in Congressional Approval on the normalized clustering coefficient in the cosponsorship networks using results from the 5-day model. The shape of the curve is very different than the one appearing in Fig. 1(b), suggesting that the choice of normalizing or not is an important one in this analysis.

This inconsistency in the long run effects for the normalized clustering coefficient stands in contrast to the consistency of

effects for their non-normalized counterparts. The fluctuation in the normalized version must be due to the normalizing statistic by which the raw clustering coefficient is divided. In other words, there must be some strong correlation between the clustering coefficient from randomly generated networks based on rewiring and Congressional approval, *and that that correlation must be changing*. Fig. 4 plots the effects of a one standard deviation decrease in Congressional approval on the randomly generated values of the clustering coefficient at three different levels of aggregation. The models that generated these results are error correction models with the same independent variables as those throughout the
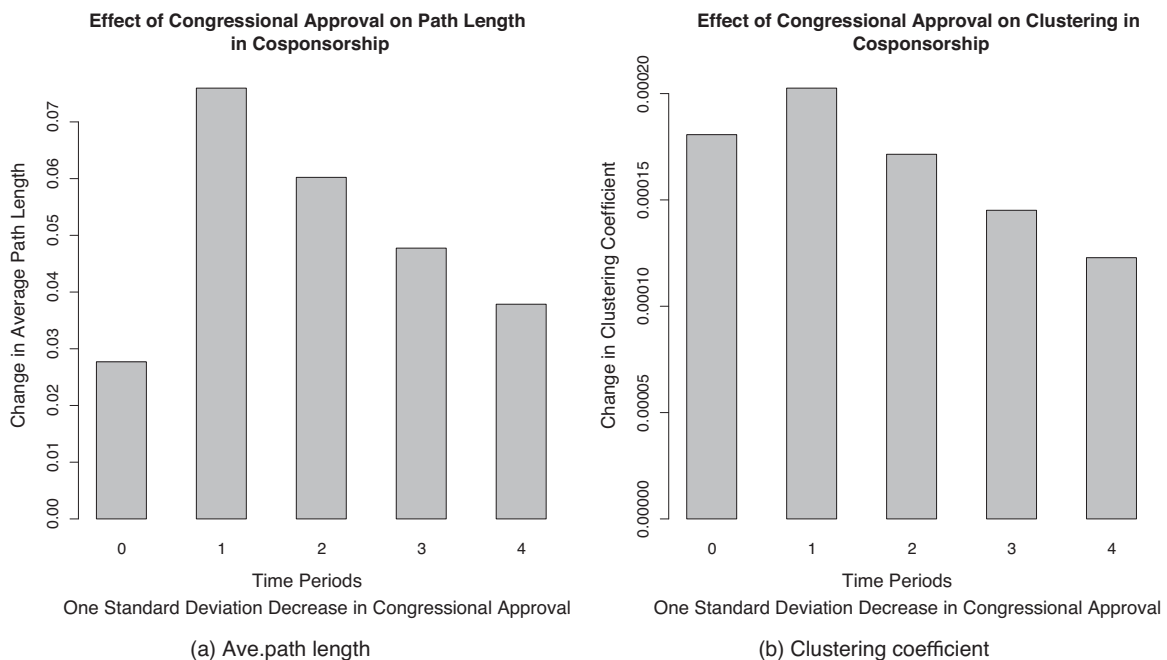

(a) Ave.path length

(b) Clustering coefficient

Fig. 3. Distributed effects of Congressional approval on change in normalized average path length and normalized clustering coefficient of Congressional Cosponsorship network in 5-day windows: (a) ave. path length and (b) clustering coefficient.
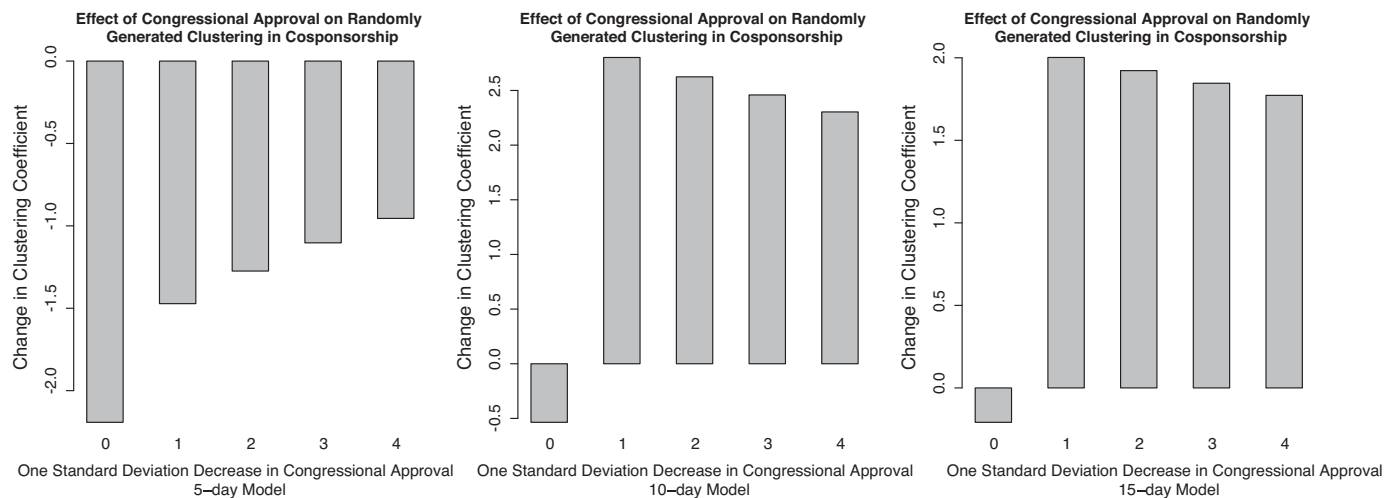
Fig. 4. The effect of Congressional approval on the randomly rewired Congressional Cosponsorship network at three different aggregation windows. Notice that the effect of Congressional approval on the clustering coefficient changes at different levels of aggregation.

paper. The key difference is that rather than predicting the values of the true network, we predict the clustering coefficient of the randomly generated networks that result from rewiring.

As the figure demonstrates, the randomly generated values of the clustering coefficient are strongly correlated with levels of Congressional approval. More importantly, the nature of this correlation hinges on the level of aggregation we employ. The 5-day aggregation windows produce a negative relationship between randomly generated values of clustering and Congressional approval, while the 10- and 15-day windows produce a positive correlation between the two. This would seem to underscore the point that in compound network measures, the choices of aggregation and normalization are extremely important.

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