The Incidental Pundit
Who Talks Politics with Whom, and Why?

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April 24, 2019

Word Count: 9513 (8126 in text, 1387 in references)

Abstract. Informal discussion plays a crucial role in democracy, yet much of its value depends on diversity. We describe two models of political discussion. The purposive model holds that people typically select discussants who are knowledgeable and politically similar to them. The incidental model suggests that people talk politics for mostly idiosyncratic reasons, as byproducts of non-political social processes. To adjudicate between these accounts, we draw on a unique, multi-site, panel dataset of whole networks, with information about many social relationships, attitudes, and demographics. This evidence permits a stronger foundation for inferences than more common egocentric methods. We find that incidental processes shape discussion networks much more powerfully than purposive ones. Respondents tended to report discussants with whom they share other relationships and characteristics, rather than based on expertise or political similarity, suggesting that stimulating discussion outside of echo chambers may be easier than previously thought.

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Introduction

Informal political discussion plays a fundamental role in modern democracy (Delli Carpini, Cook and Jacobs 2004), with its own set of normative criteria. Deliberative democratic theorists, in particular, emphasize the importance of "deliberativeness" in everyday political talk (Mansbridge 1999; Conover, Searing and Crewe 2002). Political conversation need not have the same qualities as formal, rule-bound deliberation to count as "deliberative" (Schudson 1997; Neblo 2015), but theorists suggest minimal criteria, including considering a variety of perspectives and alternatives. Therefore, if political conversation is to play its full, deliberative part in democracy, people must engage across differences (Mutz 2006).

If people choose discussion partners based on political similarities, however, they forgo those benefits of discussion across difference (Huckfeldt, Mendez and Osborn 2004). Some scholars suggest that political talk may be more likely between those with partisan or ideological similarities (Bello and Rolfe 2014); others that most people expose themselves to disagreement in everyday interactions (Huckfeldt and Sprague 1995; Mutz and Mondak 2006). But the etiology of this diversity—or its absence—remains unclear.

Political talk is not necessarily driven by an intentional process. We describe two models from previous scholarship: a purposive model and an incidental model. The first conceptualizes political conversation as goal-oriented. Citizens may be motivated to discuss politics to gather information that will help solve a problem, such as learning which candidate is best. In contrast, the second model posits that political discussion typically occurs as a byproduct of other social interactions. Consequently, whatever forces structure a citizen's social environment also shape political conversations—if one's social context is homogeneous, one's political discussions will be too. Both processes generate homogeneity in political conversation. The motivations matter because they suggest different prospects for fostering cross-cutting political discussion for example, by encouraging participation in deliberative forums.

We seek to identify whether political talk is more consistent with a purposive process or an incidental one. Though debate persists, the balance of evidence presented heretofore offer

more support for the incidental model. Assessing the relative importance of each, however, is challenging. Much prior work relies on egocentric surveys based on name-generator and name-interpreter techniques (Klofstad, McClurg and Rolfe 2009). Such designs cannot easily account for the opportunities for conversation, thus confounding choice with opportunity. Recent work has yielded improved inferences by using more expansive name generators (Eveland, Appiah and Beck 2018) and even cross-sectional, whole-network data (Song 2015; Pietryka et al. 2018). However, inferences based on cross-sectional data or even a single, whole network have difficulty ruling out alternative explanations that might produce similar patterns (Fowler et al. 2011).

We overcome these inferential problems by analyzing a unique dataset: the Friends for Life Study. This project presents a whole-network, multiplex, panel dataset consisting of more than 100 networks from fourteen sites across the U.S. between 2008 and 2016. We surveyed these whole networks multiple times per year, with roster-style network batteries measuring several social relationships, including political discussion.

Based on the Friends for Life Study, we find strong, clear evidence that political talk is more consistent with an incidental process than a purposive one. Using out-of-sample predictive accuracy, exponential random graph models, and Bayesian hierarchical models to pool results from many networks, we show that political attitudes and identities are poor predictors of who talks politics with whom. We find no evidence that individuals are more likely to engage with more interested or knowledgeable peers. Rather, in keeping with the incidental model, social relationships such as friendship predict discussion with much more accuracy. Furthermore, we find no evidence of more subtle purposive motivations, e.g., that friends might be more more sensitive to differences in political identities than acquaintances, avoiding politics to maintain relationships. Study participants appear neither to aggressively seek like-minded interlocutors, nor actively resist discussion across difference. Consequently, we conclude that people may not cultivate homogeneous political discussion networks so much as happen into them incidentally.

Who Talks Politics with Whom, and Why?

People may purposively seek others to engage in political conversation. They may also talk politics incidentally, as a byproduct of other social relationships. Some may do both, but much depends on which of the two predominates. The motivations for talk are likely to affect the amount of heterogeneity that we find ourselves exposed to under the status quo, especially insofar as individuals seek to limit or deepen the diversity of views to which they are exposed. Further, these motivations affect how much improvement could be achieved with efforts towards broader civic engagement.

Whatever motivates people to talk politics must explain the tendency to interact with those who are similar to them. Social scientists have consistently found that social relationships are more common between individuals with similar attributes, attitudes, and actions (McPherson, Smith-Lovin and Cook 2001). Political behavior is no exception (Huckfeldt and Sprague 1995; Mutz and Mondak 2006; Pattie and Johnston 2009; Bello and Rolfe 2014). One of the principal explanations for such homogeneity is homophily, the idea that individuals purposively seek others out based on similarity. Although often elided, homogeneity and homophily are distinct concepts. Homogeneity refers to the outcome, and homophily to a potential cause of that outcome. Alternative causes of homogeneity are also possible. For example, demographic (rather than political) homophily might cause political homogeneity in discussion, or demographic homogeneity might even be a byproduct itself.

The motivations for political talk matter because they affect its value for democracy. The most familiar normative axis governing political talk captures diversity. In egocentric networks, exposure to diversity is associated with more information and better decisions (Huckfeldt, Mendez and Osborn 2004), and greater tolerance of and respect for a legitimate opposition (Mutz 2006). To the extent that political talk is homogeneous, it lacks such diversity. But different motivations are prone to different levels of homogeneity. Some may purposively seek less diversity, while others seek more, and there may be more who do the former than the latter. Incidental discussion, in contrast, is characterized by individuals who do not actively seek or avoid difference; they simply

happen into it (or do not) as a byproduct of other social activity. If purposive discussion tends to be more homogeneous than heterogeneous, then the incidental variety is likely to be, all else equal, somewhat more diverse.

These motivations also suggest different prospects for efforts to improve diversity in discussion. Deliberative democrats argue that politics can be improved by fostering discussion across lines of difference. Similarly, schools use exchange programs to expose students to people from outside of their neighborhoods, and religious organizations bring together otherwise segregated religious congregations. Deliberative forums, such as deliberative opinion polls (Fishkin 2011) or online townhalls connecting officeholders and their constituents (Neblo, Esterling and Lazer 2018), bring small groups of people together to talk about important issues, policy solutions, and their potential drawbacks. The motivations underlying the formation of social networks shape the prospects of such efforts. To the extent that social networks are homogeneous because of preferences to be with similar others, people are likely to resist attempts to diversify their exposure. To the extent that the political homogeneity of our networks is incidental to other factors, they may be less resistant, or even welcome such opportunities. Much, therefore, depends on whether most political talk is purposive or incidental.

The Purposive Model of Political Talk

Political talk may be driven by many goals, including the rational exchange of information. For instance, Schudson (1997) argues that political conversation is (and should be) goal-oriented, focused on arguments, and guided by rules to resolve conflict and make collective decisions. Such discussion requires cognitive engagement, efficacy, interest, and knowledge, and risks confrontation. Consequently, people may purposely avoid political conversation (Conover, Searing and Crewe 2002; Eliasoph 1998), limiting their interactions to those who are like-minded (Mutz 2006) or have certain personality traits (Gerber et al. 2012), or seeking consonant peers (Finifter 1974). Although some people may seek exposure to different points of view, purposive talk is more often characterized as seeking politically similar partners and avoiding the politically different.

People seek discussion partners whom they perceive as experts, relying on opinion leaders (Katz and Lazarsfeld 1955) who they believe to be highly interested in politics, perhaps inferring that such discussants actively seek out relevant information and have accurate political knowledge (McClurg 2006). Evidence from surveys (Huckfeldt 2001) and experiments (Huckfeldt, Pietryka and Reilly 2014) supports this perspective, suggesting that peers' perceived expertise affects ones' political attitudes. And it can even be rational to seek such advice from biased experts who share one's predispositions, rather than neutral or balanced sources (Calvert 1985).

Political conversation has the potential for conflict, and so the purposive model includes intentional avoidance. Individuals may gravitate toward others with similar attitudes to avoid unpleasant interactions (Mutz 2006; Ulbig and Funk 1999), preferring attitudinally congruent information (Garrett, Carnahan and Lynch 2013; Stroud 2010). Purposive motivations may also be conditional. Friends with different political identities might avoid politics to maintain relationships. Anticipation of disagreement might motivate more conflict avoidant people to less readily reciprocate discussion of political topics. Whether out of information-seeking, ego-protection, relationship management, or conflict avoidance, the purposive model suggests that homogeneity is due to homophily.

The Incidental Model of Political Talk

In contrast, political talk may be mainly incidental. In general, association depends on the opportunity to create ties (Feld 1982). This idea applies to political discussion, *inter alia.* For example, Kim and Kim (2008, 53) characterize political discussion as "non-purposive, informal, casual, and spontaneous political conversation voluntarily carried out by free citizens." Incidental political talk occurs between people as they interact in their daily lives (Marsden 1987; Small 2013), driven by motivations other than politics (Lazer et al. 2010), such as relational needs (Eveland, Morey and Hutchens 2011), perhaps even approaching the "purely expressive" (Mansbridge 1999, 212).

The incidental model emphasizes context in social networks and physical space. While citizens may construct their social networks "based on shared characteristics[, t]hese characteristics

are seldom political" (Sinclair 2012, xii). Similarly, schools, workplaces, and voluntary organizations are often characterized by separation on gender, race, age, and religion (McPherson, Smith-Lovin and Cook 2001). Such settings can impose constraints on individuals, who may be expected to interact regardless of political dissimilarities (Mutz and Mondak 2006). These contexts may therefore foster demographic homogeneity in political talk, though not necessarily political homogeneity, except insofar as the two are correlated.

Political discussion is also often concentrated among people who interact frequently (Marsden 1987). Everyday interactions facilitate sharing political attitudes (Eveland, Morey and Hutchens 2011). Moreover, people may find it easier to openly disagree with their strongest social ties (Morey, Eveland and Hutchens 2012), although they may also interact with emotionally distant others who are available when important issues arise (Small 2013). Regardless, the incidental model identifies political discussion as a byproduct, rather than as a primal motive.

Hypotheses

The purposive and incidental models both predict homogeneous political discussion, but via different mechanisms. The purposive model suggests individuals seek discussion with politically similar, but more interested and knowledgeable, peers.

Political Homophily Hypothesis: All else equal, individuals are more likely to talk politics with others with similar party identification and political ideology.

Opinion Leader Hypothesis: All else equal, individuals are more likely to talk politics with others with higher levels of political interest and knowledge.

Talk can also be purposively avoided; the conflict avoidant may be particularly likely to do so. In particular, conflict avoidant individuals ought to avoid conflict acceptant peers.

Asymmetric Avoidance Hypothesis: All else equal, discussion is more likely within conflict acceptant pairs and less so when either or both individuals tend to avoid conflict.

Conversely, the incidental model suggests that political talk occurs as a byproduct of other interactions, and thus predicts spillover.

Spillover Hypothesis: *All else equal, individuals are more likely to talk politics with friends.*

The incidental model holds that activities structure opportunities. If activities are organized based on demographic features—e.g., gender, race, ethnicity, religion, or age—talk should follow suit.

Demographic Homophily Hypothesis: *All else equal, individuals are more likely to talk politics with others with whom they share characteristics, including gender, race/ethnicity, religion, and age.*

Finally, the incidental model suggests that political variables should be poor predictors of talk. Predictions should not be substantially improved by conditioning on political attitudes, interest, and knowledge. Information about relationships, however, should improve predictiveness.

Predictability Hypothesis: *Political variables add less predictive power to models of discussion than variables about social ties.*

The Friends for Life Study

To test these hypotheses, we analyze a unique, whole-network, panel dataset from fourteen sites between 2008 to 2016 (excluding 2009), for a total of 112 networks. Subjects were university student recipients of a scholarship awarded by a nationwide program.[1] Students live together in a shared "chapter house" throughout college as a requirement of the program.[2] This program

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[1] The sites include large, public universities (including research flagships), an elite private university, and a large Midwestern Catholic university.

[2] Students are assigned to chapters based on four factors, in roughly decreasing importance. First, student achievement dictates which universities admit them. Second, students stay within state if it saves on tuition. Third, student preferences matter and vary, from a desire for proximity to family, higher academic prestige, or novel geography. Fourth, availability of rooms within chapters limits options; the organization breaks ties in favor of underpopulated locations. One

provides an ideal setting to observe the evolution of political discussion networks in a context substantially segregated from the broader social environment.[3] The universe of possible participants includes 2,521 individuals and 6,248 observations.

The comprehensiveness and longitudinal nature of the dataset help to separate opportunity structure from choice. Specifically, these data allow us to rule out alternative explanations that plague analyses based on egocentric surveys and even cross-sectional, whole networks. The chapter house also provides an intense, enclosed setting at a key time in political and social development. Much of the U.S. population lives in dorms for part of their lives, and their time there molds persistent identities and behaviors (Newcomb 1943; Alwin, Cohen and Newcomb 1991; Mendelberg, McCabe and Thal 2017).

There is a tradeoff in the insights yielded by egocentric and whole network data. At their best, national surveys yield insights that easily generalize, while datasets like ours provide inferential leverage by tracking specific, whole populations over extended periods of time. Whole network data can also be placed more precisely in a context—an organizational and cultural milieu—for each dyad in a dataset, which is impractical for egocentric data collected on national surveys. That said, neither dominates the other. In our case, by focusing on a college-age population, who

chapter has no women because it lacks adequate facilities. Given this process, there is variation in demographic diversity. The cost and logistical factors push against self-selection.

[3] Of course, students may also engage in political conversation outside of their chapters, and thus from some students' perspective, we do not have their complete discussion networks. From the institutional perspective, however, we do have complete networks, and there is evidence that these networks are dominant in many students' political lives. In three waves of the survey (2008, 2010, and 2011), we asked respondents, "Roughly what percentage of the people you discuss politics with are [other chapter members]?" Among respondents who named at least one political discussant, the median and modal response in this group was "50%," although the fraction varied from 0 to 100, and averages varied across chapters (see Online Appendix Table A1). Thus, for most members of the sample, we capture a majority of their political networks.

won means-tested scholarships, we sacrifice some generalizability for inferential plausibility.[4]

We measured attitudes, demographics, and social ties with a questionnaire. Respondents received email invitations and up to twelve follow-ups per wave. There were two waves per year: one at the beginning of the fall semester ("August surveys") and another near the end ("November surveys"). August surveys asked about attitudes and demographics, and November surveys probed social ties.[5] Our November survey was fielded immediately after a questionnaire fielded by the fellowship program, but all waves were explicitly optional. Every year, approximately a quarter of respondents were new entrants, including first year students and a small number of transfers, and about a quarter exited the networks, mostly due to graduation.

We measured social relationships by providing respondents with complete chapter rosters. Our goal is to capture discussion that is informal, engaged, and explicitly political, as differentiated from occasional references to political topics in the context of a broader relationship. Therefore, to measure *Political Discussion*, we asked respondents to select their peers for whom the following statement applied: "I frequently discuss politics, social issues, or current events with this person."[6] Figure 1 reveals wide variation in topology across chapters and years.

To account for the incidental opportunities for discussion, we also measured several other relationships: *Friend* ("This person is a close friend"), *Time* ("I spend a lot of time around this

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[4] That said, our dataset exhibits characteristics that mollify some potential criticisms. For example, there was more variation in political attitudes than one might expect from undergraduate populations, as we detail below.

[5] Response rates—the percentage of all possible respondents who completed at least part of the questionnaire—were 83.0% for August surveys and 93.8% for November.

[6] Our use of the modifier "frequently" might have encouraged under-reporting by respondents who engaged in limited political discussion, biasing our results against the incidental model. That said, 95% of possible respondents were named as discussants, indicating that most individuals engaged in frequent discussion according to at least one respondent's judgment. Unfortunately, our surveys included no items that measured dyad-level variation in frequency of discussion.
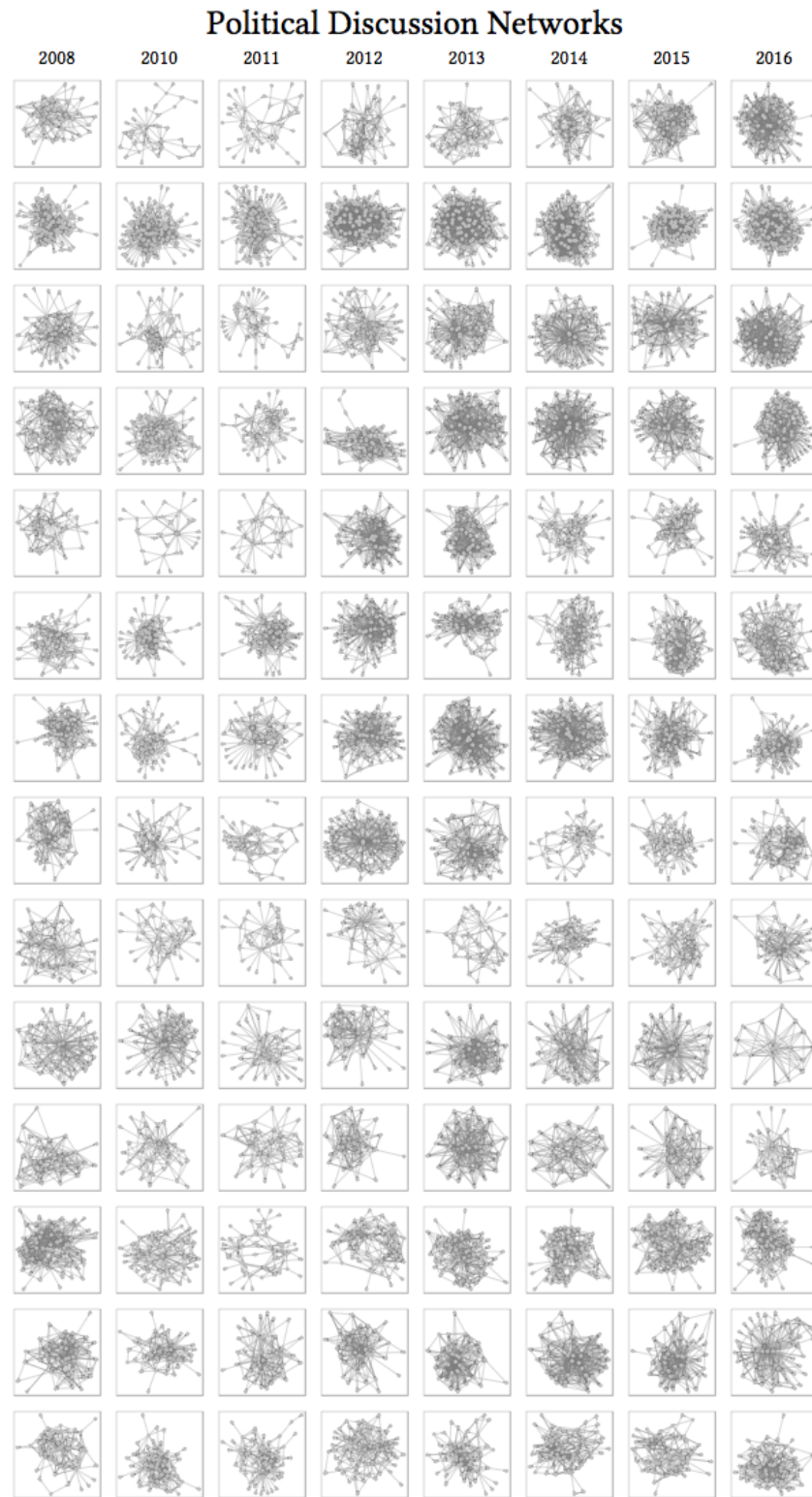
Political Discussion Networks

Figure 1: The figure depicts political discussion networks. Each row shows a different chapter, with years in columns.

person"), *Esteem* ("I hold this person in especially high esteem"), and *Negative* ("Sometimes I do not find it easy to get along with this person"), all using a similar interface. The result is a panel of directed, multiplex, whole networks, in which ties were measured dichotomously, with a 1 if an individual (or *Sender*) reports a relation with another (*Receiver*), and 0 otherwise. Response rates to network batteries were very high. Respondents named at least one peer in at least one network in about 86% cases; about 99% of possible ties were named by at least one peer.

Respondents' political, psychological, and demographic characteristics were measured on the August surveys. We measured party ID with a standard branching question and 7 point scale, folded to measure *Strength of Partisanship*. Our focus is on partisan selection of discussants, so we coded *Democrat* and *Republican*, excluding leaners.[7] That we have self-reported party ID is an advantage over egocentric surveys that rely on respondents' reports about discussion partners, and may suffer from projection bias (Goel, Mason and Watts 2010). We also measured *Political Interest*, *Political Knowledge*, and *Ideology*, which we folded to yield *Strength of Ideology*.[8] We measured *Conflict Avoidance* with subsets of eight questions that varied year-to-year. In each year except 2013, we fielded at least three items. There was one common item across waves. To put these on a common scale, we estimated a latent single dimension with an item-response theory model.[9] Demographics included *Female* and *Evangelical* Christians, and categorical variables for *Race/Ethnicity* ("Asian," "Black," "Latino," "White," and "Other", with multiple responses coded as "Other") and *School Year* (based on entrance year into the program, ranging from first year to fourth year and beyond), which is highly correlated with age.[10]

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[7] For robustness, we reran analyses with variables including leaners; results were virtually identical.

[8] The Online Appendix reports question wording (pp. A1–A3).

[9] Results are robust to using the single common item.

[10] To cope with missingness, we carried previous observations forward to impute *Female*, *Evangelical*, and *Race/Ethnicity*. For other variables, we imputed ten datasets using the R package Amelia II (Honaker, King and Blackwell 2011). We did not impute network items. See Online

Party Homogeneity in Political Discussion

Our dataset exhibits more partisan variation than typical undergraduate samples. About 32% of observations identified as Democrats, 36% as Republicans, and the remaining 31% as independents.[11] Average *Ideology* was near the scale's midpoint. These averages mask considerable variation between networks, however. Across locales, the proportion identifying as Democrats ranged from 13% to 53%; Republicans ranged from 11% to 62%. Similarly, chapter-level average *Ideology* ranged from 0.44 to 0.67. In political terms, chapters tended to resemble the states in which they are located.[12] Our sample is therefore more Republican and conservative than typical undergraduate populations, perhaps because of the nature and mission of the scholarship organization, or because men in the sample outnumbered women.[13] This political heterogeneity entails strengths and weaknesses, which we discuss below.

As expected, political discussion ties exhibit party homogeneity. Combining chapters, 27% of ties involve pairs who identified with the same major party.[14] More than half included at least one individual who did not identify with a major party. Limiting attention to pairs of major party identifiers, 57% of ties were homogeneous. This rate is similar to findings from other studies. Huckfeldt, Mendez and Osborn (2004) report that 60% of political discussion dyads shared

Appendix for details (p. A4).

[11] Accounting for imputation, all averages reported in this section have standard errors less than 1%. Estimates based on imputed datasets are calculated with Rubin's rules.

[12] There was a correlation of 0.61 between the proportion of Democratic party identifiers in a chapter-year and the most recent state-level Democratic presidential vote share.

[13] Throughout this time period, Democratic Party identifiers have outnumbered Republican identifiers by about 2 to 1 among 18- to 29-year-olds, according to the semiannual national Youth Poll of Harvard's Institute of Politics.

[14] Results are similar when we include leaners as major party identifiers. See the Online Appendix (pp. A4–A5).

Egocentric Analysis Biases
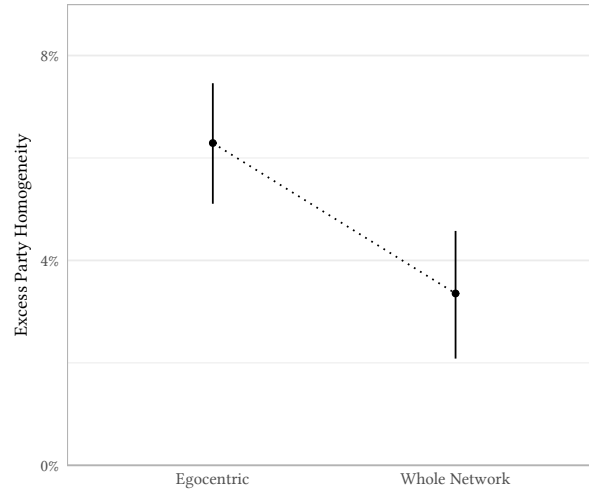## Party Homogeneity Estimates

Figure 2: Excess party homogeneity is the difference between observed levels and those that would occur by chance, calculated by permuting party labels. The estimate on the left permutes across all chapters in a year, as with egocentric data alone. Such an approach cannot leverage information about local opportunity structures. The estimate on the right permutes at the chapter-year level, leveraging such whole network information. Ignoring the additional information afforded by whole network data will typically bias estimates.

political preferences, and Klofstad, McClurg and Rolfe (2009, Table 3) find a rate of about 50%.

However, homogeneity depends on opportunity structure, and thus may not necessarily reflect purposive homophily processes. As a first cut, we estimated *excess party homogeneity* by simulating networks constructed to have no homophily, using a technique similar to the quadratic assignment procedure (Krackardt 1987). Specifically, we permuted observed party IDs across chapters in each year 100 times for each of our 10 imputed datasets, for a total of 1000 permutations. Doing so holds network topology constant while breaking any connection with copartisanship. By comparing actual homogeneity to that in the simulated networks, we estimate how much more homogeneity we observe than would be expected if homophily played no role. When we permute party labels across chapters, it appears that actual party homogeneity was about 6% greater than would be expected by chance, as shown in the left of Figure 2. Thus, the rate of political homogeneity would seem to be considerably higher than it would be without political homophily.

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Different chapters, however, yielded different opportunities. As with political composition, political homogeneity also varied across locales. At the chapter-year level, party homogeneity ranged from 42% to 77% of ties between major party identifiers, reflecting different opportunities for political discussion. In an egocentric survey, we would not have information about the local distributions of party IDs. Consequently, if we were to estimate excess party homogeneity, we would need to use the global distribution of partisanship, as we did above. Doing so risks overstating homophily for the same reason that ignoring opportunity structure does more generally.

We therefore reanalyzed the data, permuting within chapter-years. Taking opportunity structure into consideration reveals that excess party homogeneity is substantially smaller than we inferred using only egocentric information, as seen in right of Figure 2. Not accounting for this information incorrectly doubles estimates of excess party homogeneity.

Modeling Political Discussion Networks

The diversity across locales underscores the importance of the multi-site nature of our dataset. Most work on political discussion relies on cross-sectional, egocentric surveys, which do not permit tests of plausible alternative explanations (Fowler et al. 2011). Whereas naïve analysis might reveal homogeneity and therefore prompt inferences about homophily, the richness of the Friends for Life Study permits more sophisticated modeling that can rule out homogeneity due to incidental processes.

To leverage this richness, we model political discussion networks with the temporal exponential random graph model (TERGM), a time-series extension of exponential random graph models (Hanneke, Fu and Xing 2010). This approach is appropriate for modeling longitudinal network data because it allows us to directly account for dynamics like transitive closure (i.e., if A talks to B and C, then B likely talks to C) rather than mistaking such processes for homophily. The TERGM also allows us to explain ties with data from previous waves of the panel, including friendship, individuals' attributes, similarities across dyads, and network-level structural terms.

Throughout, we focus on a dyad: a *sender* who may (or may not) name a *receiver* as discussant.

At the respondent level, we include the measures described above: gender, race, religion, political attitudes, etc. Our unit of analysis is the dyad, so we include these measures for both senders and receivers.

At the dyad level, we include other relationships: *Friend*, *Time*, *Esteem*, and *Negative* using lagged terms to avoid bias from reverse causality. Further, we include terms to capture the dynamics of discussion over time (Leifeld, Cranmer and Desmarais 2017). *Delayed Reciprocity* measures the tendency for relationships to be reciprocated over time, and *Dyadic Stability* captures whether existing discussion ties remain active while new ones tend not to form. We also used similarity-based measures. We tap whether both sender and receiver were coded as the *Same Gender*, *Same Race/Ethnicity* (excluding the case when both were coded "Other"), *Same Cohort* (i.e., school year), or were *Both Evangelical*. We also include the multiplicative interaction of sender's and receiver's *Conflict Avoidance*. Positive values of this interaction term indicate similar values of the constitutive terms, while negative values indicate that one member is avoidant and the other acceptant. To probe for political homophily, we include *Same Party* (coding independents and leaners as members of neither party[15]) and *Ideological Proximity*, 1 minus the absolute value of the pairs' *Ideology* scores. Finally, to capture information seeking, we created indicators of asymmetric interest and knowledge: *Sender More Interested*, *Receiver More Knowledgeable*, etc.

ERGM-based approaches allow inclusion of endogenous terms that track density, centralization, closure, and other network tendencies. To capture network density, we include *Edges*, which is like the intercept in a regression. Similarly, we include an indicator for presidential *Election Year*, which additively modifies the *Edges* term in 2012 and 2016, when political discussion may have been more prevalent. We also include *Mutual* to assess the tendency for immediate reciprocity. To account for network centralization, we include *Activity Spread* (geometrically weighted outdegree) and *Popularity Spread* (geometrically weighted indegree).[16] We also include

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[15] Including leaners yields very similar results.

[16] Geometrically weighted terms depend on a decay parameter, which we set to 1.5 for *Activity*

Sinks, a count of nodes that receive ties but do not send them. To gauge both clustering and the potential for clustering, we include several edgewise shared partner terms—*Transitive Closure*, *Cyclic Closure*, *Activity Closure*, and *Popularity Closure*—along with their corresponding dyad-wise shared partner terms—*Multiple 2-paths*, *Shared Activity* and *Shared Popularity*.[17]

Plan of Analysis

Our analysis proceeds in three steps. First, we assess out-of-sample predictive accuracy of several models, to select a model and test the Predictability Hypothesis—that political variables will not independently add accuracy to predictions of political discussion. Out-of-sample predictive accuracy is a simple way to demonstrate which models anticipate reality (Cranmer and Desmarais 2017). At the dyadic level, political discussion is a rare event; only about 10% of directed dyads indicated such a tie. Therefore, we focus on the area under the precision-recall curve (AUC-PR), which can be interpreted as the percentage of discussion ties accurately predicted by a model, which is appropriate for rare events.[18] For each model, chapter, imputation, and held-out year, we estimated the model on the remaining years and measured AUC-PR for the held-out network.

After choosing the best predictive model, we use bootstrapped maximum pseudolikelihood

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Spread and *Popularity Spread*.

[17] These terms go by many names in literature. For example, *Transitive Closure* is called AT-T by Lusher, Koskinen and Robins (2013), and directed geometrically weighted edgewise-shared partners (DGWESP) of type OTP (outgoing transitive partners) by Hunter et al. (2013). Similarly, *Cyclic Closure* is AT-C or DGWESP-ITP; *Activity Closure* is AT-U or DGWESP-OSP; *Popularity Closure* is AT-D or DGWESP-ISP; *Multiple 2-paths* is A2P-T or DGWDSP-OTP, *Shared Activity* is A2P-U or DGWDSP-OSP; and *Shared Popularity* is A2P-D or DGWDSP-ISP. For these terms, we set the decay parameter to 0.5; our results are robust to alternative choices.

[18] The PR curve plots the precision (# true positives / # true positives + # false positives) against the true positive rate, or recall (# true positives / # true positives + # false negatives).

for statistical inference (Desmarais and Cranmer 2012; Leifeld, Cranmer and Desmarais 2017),[19] bootstrapping over years within each chapter, with 100 resamples for each of 10 imputed datasets, yielding 1000 resamples.

Finally, we summarize our findings with a hierarchical model. Our theoretical expectations apply to all chapters, but we have also documented important differences across locales. Therefore, we estimated a second-level model for each coefficient, using chapter-level coefficients and standard errors as data.[20] For each coefficient, these models yielded both an across-chapter average and "partially pooled" chapter-level estimates, which shrink the chapter-level estimates toward the across-chapter average, based on the ratio of within- and between-chapter variances in coefficients. The result characterizes both overall tendencies and heterogeneity across networks. Ultimately, we report posterior means and 95% intervals for across-chapter average coefficients, and partially pooled estimates at the chapter-level.

Out-of-Sample Predictive Accuracy

We first assess out-of-sample predictive accuracy of several models, both for model selection and to test the Predictability Hypothesis. The *Base* model includes only *Edges*, *Mutual*, and *Election Year.* From there, we built models modularly, adding terms associated with *Demographics* (gender, race/ethnicity, etc.) and *Politics* (party, ideology, interest, etc.) and their homogeneity measures, *Memory* (lagged networks), *Degree* (*Sinks*, *Activity Spread*, *Popularity Spread*), and *Shared Partners* (*Closure*, *Multiple 2-paths*, etc.). The *Full* model includes all components. Figure 3 illustrates the results, with boxplots summarizing performance over the 84 chapter-year networks.

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[19] Like other ERGMs, the TERGM is intractable to estimate directly. Maximum pseudolikelihood replaces this intractable problem with a simpler conditional logit, in which network ties are regressed on change statistics—differences in network statistics—associated with toggling each tie on and off (Hunter et al. 2013).

[20] We estimated pooling models with RStan 2.17.3 (Stan Development Team 2018).

Out of Sample Predictive Accuracy of
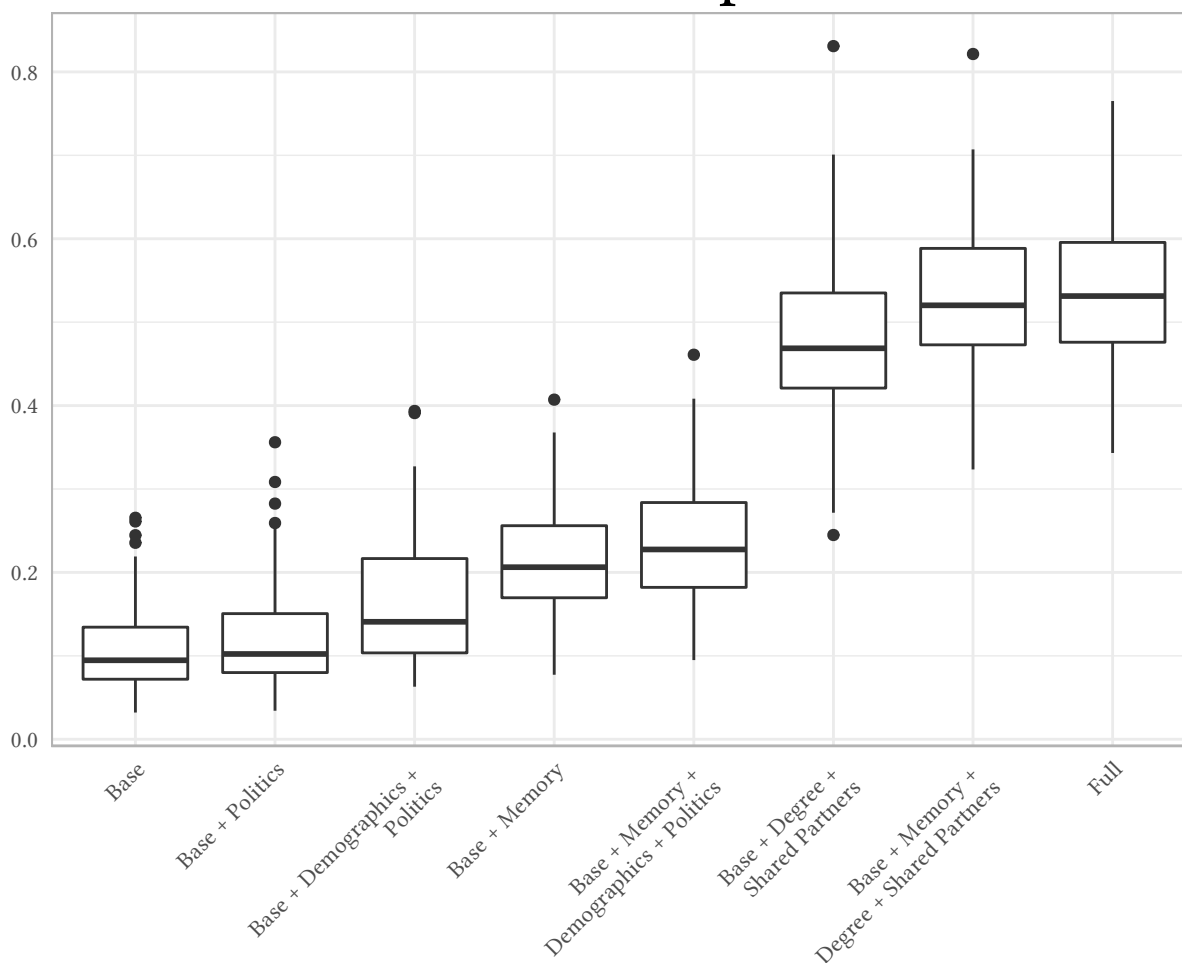# Alternative Model Specifications

Figure 3: Out-of-Sample Predictive Accuracy. Each boxplot displays variation in the area under the PR curve for all chapters in the dataset.

The *Full* model is the most accurate, with a chapter-level average AUC-PR of 0.54, a substantial improvement over the *Base* model, which averaged 0.11. In substantive terms, this means that the *Full* model accurately predicts more than half of discussion ties, out-of-sample. Comparatively, the average AUC-PR for the model *Politics* was 0.12, meaning that the *Politics* terms offers negligible improvement over the *Base* model.[21]

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[21] Area under ROC curves yield similar results.

These results strongly support the Predictability Hypothesis. When combined, *Politics*, *Demographics*, and *Memory* improved accuracy relative to the *Base* model, although almost none of this improvement comes from political information. According to AUC-PR, the *Memory* terms were substantially more predictive than *Politics*, even when coupled with *Demographics*. This support is striking, since the *Memory* terms are based on responses from the previous year. Notably, new entrants to college are structurally incapable of being included in *Memory* terms, as they were not members of the previous year's networks. Regardless, all three sets of terms yield little improvement once the endogenous variables were included. The predictive value of the *Full* model rests on the *Degree* and *Shared Partners* terms.

Predictors of Discussion

Based on predictive accuracy, we selected the *Full* model and re-estimated it on the entire dataset.[22] To summarize the results, we pooled the chapter-level models with a Bayesian hierarchical Normal model, and Figure 4 displays the resulting coefficient estimates. The posterior distributions of the across-chapter average coefficients are depicted with large dashes for means and bars for the 95% intervals. Small dashes illustrate chapter-level, partially pooled estimates.[23]

Figure 4 displays remarkable consistency across chapters for many key variables. The consistency reflects similarity in dynamics across locales. In general, coefficients shift closer to the across-chapter average when within-chapter variance is large relative to between-chapter variance, i.e., when chapters' individual confidence intervals overlap substantially. For example, the partially pooled coefficients on *Same Gender* (top left corner) range from 0.50 to 0.72, with an across-chapter average of 0.62. In the unpooled models, the range is larger, from 0.37 to 0.84.

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[22] Overall, the models yielded satisfactory goodness-of-fit. See the Online Appendix, pp. A8–A22.

[23] Some chapters lack variation on some variables (e.g., one chapter was all male), meaning that the variable was dropped.

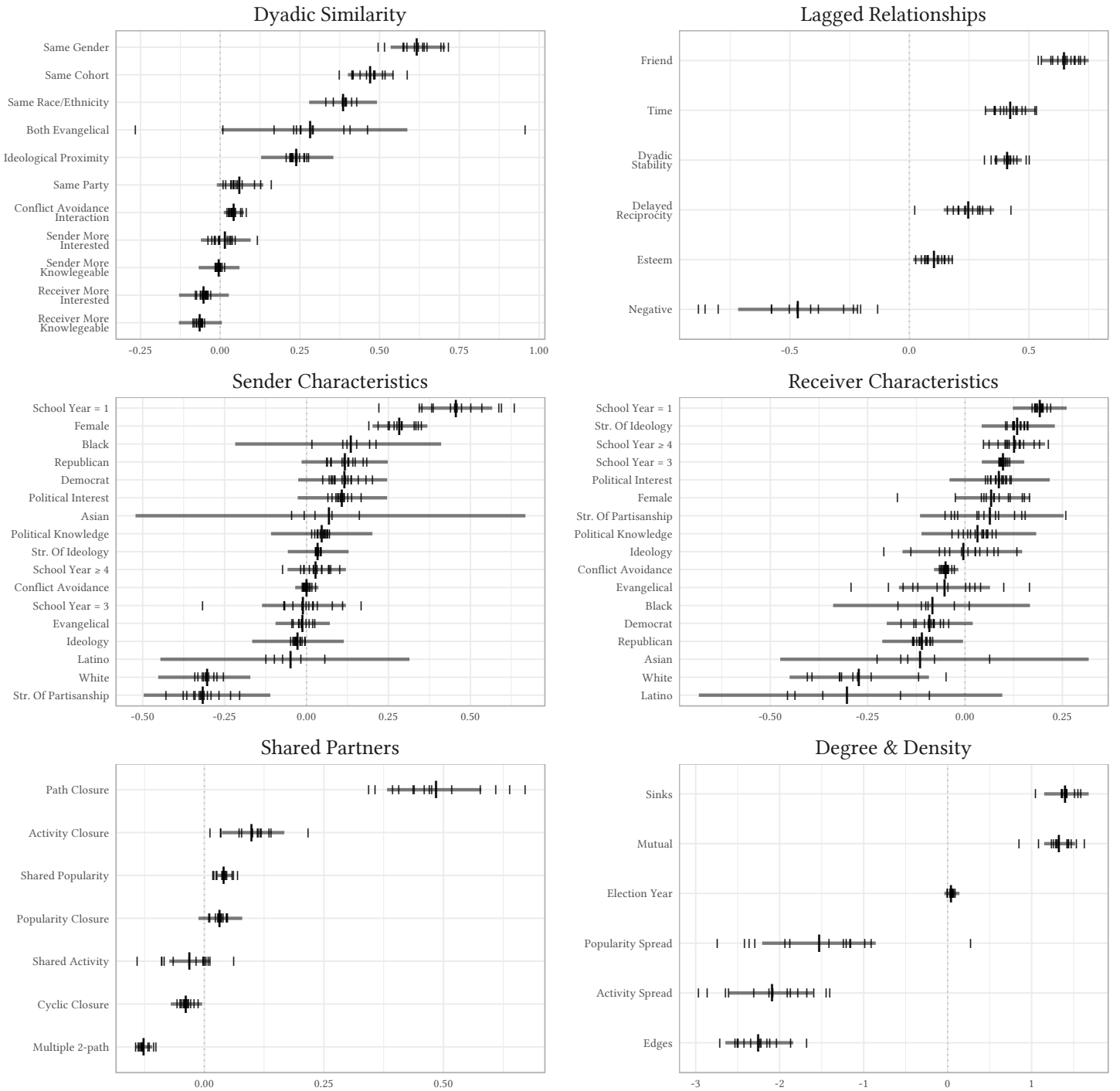# Across-Chapter and Chapter-level TERGM Coefficients

Figure 4: Across-Chapter Average and Chapter-level Coefficients. Posterior distributions for across-chapter averages are displayed with larger dashes at means and bars for 95% intervals; smaller vertical dashes illustrate chapter-level estimates.

Yet all chapter-level confidence intervals overlap, even for these two outliers, and so the pooling model shrinks chapters toward the grand mean, borrowing information across locales. A few variables retain clear heterogeneity. For instance, even after partial pooling, the chapter-level coefficients on *Both Evangelical* straddle zero. Thus, the consistencies illustrated in Figure 4 suggest similar processes at work across these sites, despite heterogeneity across networks.

There is weak support for the Political Homophily Hypothesis, but strong support for the Demographic Homophily Hypothesis. For example, compare political and demographic homogeneity. The upper left pane of Figure 4 shows the coefficients for similarity-based terms. ERGMs can be expressed as conditional logit models (Hunter et al. 2013), so these estimates can be interpreted as logit coefficients. The across-chapter average coefficient on *Same Party* was 0.06, a tenth of that for *Same Gender*. Although we provide a substantive interpretation below, the implication is clear even on the logit probability scale: copartisanship played an exceptionally small role in political discussion. Comparatively, the coefficient for *Ideology Proximity*—which was also measured on a 0–1 scale—was larger, 0.24, and constitutes the best evidence we found for the Political Homophily Hypothesis. But even that was smaller than all four estimates for measures of demographic homogeneity. Notably, all estimates were precisely estimated at the across-chapter level. The 95% posterior intervals all exclude zero, except for *Same Party*, for which zero is near the lower bound. On balance, the results indicate that political discussion is driven much more by incidental processes than purposive ones.

In contrast, there is no evidence to support the Opinion Leader Hypothesis. Not only are individuals no more likely to name more interested peers as discussants, but there is also weak evidence that they are *less* likely to name a peer if she is more politically knowledgeable. The coefficients on *Receiver More Interested* and *More Knowledgeable* are both negative, and the latter is significant at the 95% level. These coefficients are even smaller than that for *Same Party*, and so we hesitate to make strong inferences based on this evidence. One explanation for this finding is that opinion leaders also identify their "followers" as discussants. Regardless, we found no support for the prediction that people disproportionately identify their better-informed peers as

conversation partners.

There is limited evidence for the Asymmetric Avoidance Hypothesis. The coefficients on the interaction term are positive, so political discussion is less common between the conflict avoidant and the acceptant. However, interaction terms are difficult to interpret based only on coefficients. We return to this hypothesis in our discussion of substantive interpretations below.

The Spillover Hypothesis, however, is strongly supported. The three lagged positive networks—*Friend*, *Time*, and *Esteem*—are all associated with political discussion, while *Negative* ties make talk less likely. These coefficients do not merely capture the persistence of past political discussion relationships, as the model also adjusts for lagged discussion with the *Dyadic Stability* and *Delayed Reciprocity* terms. Instead, these large estimates indicate the tendency for political conversation to occur within already existing social relationships, regardless of political characteristics. The combination of positive coefficients for positive networks and a negative finding for the *Negative* network suggests a coherent pattern: individuals tend to talk politics with those who happen to be in their social neighborhoods.

To cast the results of this predictive model in substantive terms, we calculated predicted probabilities and differences in those predictions (see Figure 5).[24] For each variable, we used the unpooled chapter-level models to generate estimates of tie probabilities in both the presence and absence of a social relationship, holding other variables at observed values. For example, for every dyad in our sample, we calculated the predicted probability of a tie if sender and receiver both identified with the *Same Party*, and if they were different. We then calculated the difference and generated averages and standard deviations for each chapter-year. Finally, we applied the same pooling model as we did for the coefficients, yielding chapter-level, partially pooled differences in predicted probabilities. Figure 5 illustrates the results.

The differences associated with social relationships and demographic homogeneity dominate those associated with purposive political talk. textitSame Party yields almost no change

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[24] In the Online Appendix (pp. A6–A7), we articulate assumptions that warrant causal identification and probe sensitivity to these assumptions. Our main findings are robust to these analyses.

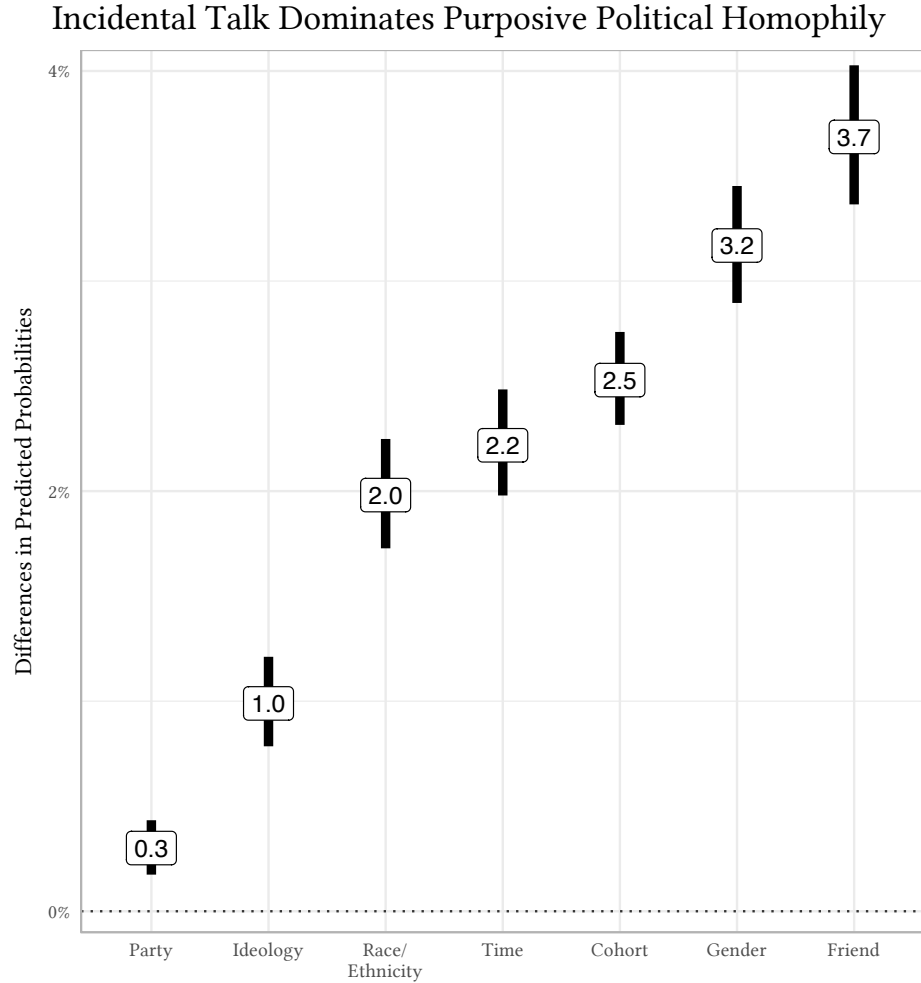Incidental Talk Dominates Purposive Political Homophily

Figure 5: Differences in predicted probabilities for incidental and purposive predictors. In each case, we estimated the average difference in predicted probabilities, switching the values of the indicated variable from 1 to 0. The figure displays estimates based on partial pooling models of differences in predicted probabilities, along with 95% intervals.

in predicted probabilities: after pooling, the estimated difference was 0.3%. And the result for *Ideological Proximity* was not much larger, at 1%. Comparatively, differences associated with demographic homophily were more than twice as large, ranging from 2% for *Race/Ethnicity* up to 3% for *Gender*. Given that the base rate for political discussion was 10%, this last estimate is substantial.

In contrast to these small differences, the estimate for *Friend* is many times that for the political homogeneity variables. After pooling, lagged *Friend* is associated with an almost 4% increase

in the probability of discussion, more than twelve times the size for copartisanship. The estimate for the lagged *Time* network is smaller, but still larger than that *Same Race/Ethnicity*. Importantly, *Friend* and *Time* are correlated at 0.6. The combined estimates for *Friend* and *Time* is about 6%, more than three-fifths the base rate.

Analysis of *Conflict Avoidance* is more complicated than for other similarity-based terms. Rather than a single difference between two values (e.g., both identify with the same party vs. the opposite), there are now four possible events to consider—talk when (1) only the sender is avoidant, (2) only the receiver is, (3) both are, and (4) neither is—and thus $\binom{4}{2} = six$ possible quantities of interest. Moreover, *Conflict Avoidance* is continuous. We therefore use its 10th percentile for the acceptant and the 90th percentile for the avoidant. Using these values, we derived predicted probabilities and differences as above. The largest contrast occurs between dyads with two conflict acceptant members, and those with a conflict acceptant sender and conflict avoidant receiver, for a difference of about 1%. Yet the 95% posterior intervals for these quantities exclude zero for all but one of the contrasts. Thus, while conflict avoidance plays a measurable role in discussion, it does not appear to be central.

Discussion Hierarchy, Opinion Leadership, and Enthusiasm

More subtly, the *Degree* and *Shared Partners* terms reveal hierarchy in political networks, in which a few highly active individuals dominate. *Activity Spread* and *Popularity Spread* reflect persistently skewed degree distributions, net of other variables, meaning that relatively few individuals send and receive most ties. For *Shared Partners*, the combination of a positive coefficient on a closure term and a negative coefficient on its related dyadwise term implies a tendency toward closure (Hunter et al. 2013). There is a large positive coefficient on *Transitive Closure*, a small negative one on *Cyclic Closure*, and a large negative coefficient on the related, dyadwise *Multiple 2-paths* term. Thus, paths tend to be closed in a transitive, acyclic fashion, suggesting that conversations might flow in one direction.

Two mechanisms could explain this apparent hierarchy. First, consistent with the Opinion

Leader Hypothesis, less informed individuals may seek out more informed peers; the more informed need not reciprocate. Or, discussion may be driven by enthusiasts—very active political discussants who generate most discussion—while others may regard political conversation as socially undesirable, and under-report their activity. On balance, our findings argue for the latter interpretation. The combination of the positive coefficient on *Activity Closure* and the negative coefficient on *Shared Activity* means that people who list the same discussants are also more likely to report talking politics with each other. These findings do not cohere well with the information-seeking variant of the purposive model, as any information that the two would receive from each other would be redundant. Taken in tandem with the absence of dyad-level evidence for the Opinion Leader Hypothesis, we conclude that political discussion hierarchies are driven more by enthusiasm for talk within existing social networks than by a purposive search for information.

Conditional Political Homophily

So far, we have focused on individual characteristics and social relationships in isolation. Yet homophily might also be depend on interactions of characteristics. Here we consider two such possibilities. Evidence for such effects would suggest that the purposive model depends on complex sets of factors that might yet be pervasive. Absence of such evidence would strengthen the case that political homogeneity is largely driven by non-political forces.

First, politics might affect discussion differently for friends and acquaintances. For example, friends with different identities might avoid politics to maintain the relationship; politics should be less consequential among non-friends. In that case, the interaction between *Friend* and political homogeneity—*Same Party* or *Ideological Proximity*—should have a positive coefficient. We estimated two models, the *Full* specification plus each interaction term. Yet we found no evidence of such effects. Both interaction terms have positive coefficients, but their 95% intervals include zero.[25] There is little evidence that political similarity matters more for friends.

Second, conflict might require some minimum level of identity strength. In that case, there

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[25] The pooled coefficient for *Friend* $\times$ *Same Party* is 0.02 $[-0.11, 0.15]$; that for *Friend* $\times$ *Ideo-*

should be a negative interaction between *Conflict Avoidance* and either *Strength of Partisanship* or *Strength of Ideology*, where higher levels of both diminish the probability of discussion. We again estimated one model for each interaction. Although both coefficients are negative, their 95% intervals include zero.[26] Thus, conflict avoidance does not seem to function differently depending on the strength of political identity. Taken together, these analyses further suggest that informal political talk is driven by non-political factors.

Conclusion

Drawing on the Friends for Life Study, we found that political talk is predicted predominantly by incidental processes. We have done so by employing an innovative methodological strategy, leveraging the study's multiplicity of whole networks observed over time. Because we track many networks over almost a decade, we can assay out-of-sample predictive accuracy, capture the dynamics of social relationships, and pool results across many locales to reveal variation in these processes. We are unaware of any study of political discussion that provides evidence as comprehensive as that presented here.

Ultimately, this evidence reveals that political discussion is almost entirely incidental. Political information is a poor predictor of who talks politics with whom; social context and network structure vastly improve out-of-sample predictive accuracy. We do observe that political homophily predicts small, positive increases in the probability of political discussion. These differences are statistically significant, yet substantively modest. In contrast, the differences associated with friendship and shared demographic traits are considerably larger, reaching almost *half* of the base rate. We found no evidence that individuals seek more interested or knowledgeable peers for discussion, and no evidence of conditional political homophily. Interactions between

logical Proximity is 0.03 $[-0.22, 0.28]$.

[26] The pooled coefficient for *Conflict Avoidance* $\times$ *Strength of Partisanship* is -0.002 $[-0.107, 0.101]$; that for *Conflict Avoidance* $\times$ *Strength of Ideology* is -0.007 $[-0.084, 0.073]$.

friendship and similar identities, and between conflict avoidance and strength of identity, were both null. On balance, political talk appears to be driven more by opportunity than intent.

Our sample is more politically heterogeneous than most college samples. Consequently, our results may be less representative of college students, yet more so of the general population. Further, the scholarship organization that forms the basis for the study has no explicitly political mission, meaning that participants are likely better representative of the general population than samples of students from political science classes. The organization is, however, means-tested, and so, if motivations for political discussion vary by socioeconomic status, our results may not generalize—though we have no *ex ante* expectations about such a relationship.

At a dyadic level, our sample also improves on most studies of college-based networks. One drawback of studying political homophily among college students is that they are predominantly liberal, with little variation in homogeneity at the dyadic level, making it difficult to distinguish homophily from other factors. In contrast, our study includes much more dyad-level variation, making political homophily easier to detect. The low levels we find may therefore constitute a high-water mark, since they emerge from an "easy" test.

This study also has important limitations. We lack evidence on variation in the frequency of political discussion across dyads. Motivations for discussion may differ for more frequent interlocutors than more intermittent partners. Some individuals might purposively seek frequent "safe" or "dangerous" discussions, which are associated with different political behaviors and outcomes (Eveland and Hively 2009). That said, because we explicitly ask for ties with whom respondents "often discuss" politics, we have likely selected the heaviest political ties, who are also likely to be the most purposive. We also lack direct evidence of motivations, although this is not a unique limitation of this study. While it is unclear whether asking participants to state motivations for talking (or avoiding) politics would yield more credible evidence, we nevertheless rely on implied indicators of what drives discussion.

Notwithstanding these limitations, our findings have important implications. First, if discussion is more incidental than purposive, it may also be more deliberative than previously thought,

since incidental political discussions have more deliberative qualities than intentional ones (Wojcieszak and Mutz 2009). Our results also suggest that people do not aggressively curate their political networks, which has implications for encouraging discussion across difference. The most plausible ways to do so require willingness to participate in discussion outside of existing networks. For example, deliberative forums—from mini-publics (Fishkin 2011) to directly representative democracy (Neblo, Esterling and Lazer 2018)—can succeed only so long as individual citizens agree to participate. There remains reason for caution, as participants may anticipate disagreement, fostering defensiveness rather than open-mindedness (Taber and Lodge 2006; Bail et al. 2018), and the lack of social closeness in such groups could result in diminished civility. Yet, it seems plausible to attract citizens as long as they do not actively police their political horizons.

The incidental model also dovetails well with evidence about how individuals respond to deliberative forums. For example, Neblo et al. (2010) find that most individuals want to participate in forums with their member of Congress and fellow constituents; little predicts unwillingness to participate or failure to attend. The effects of these events were similarly broad-based. Members were equally effective at persuading both copartisans and non-copartisans (Minozzi et al. 2015). And attendance sharply increased political discussion on topics related to the event (Lazer et al. 2015). These findings would be surprising if we expected individuals to talk politics purposively, limiting contact to those of a similar political stripe. The incidental model offers a more coherent explanation of both the persistence of homogeneity in informal political conversations and this pattern of response to encounters with the political opposition.

More broadly, the incidental model suggests that the everyday political talk, a vital component of the health of democracy, does not lack deliberativeness because people dislike cross-cutting exposure. Instead, deficiencies in heterogeneity—which remain substantial—are a byproduct of the broader social fabric that citizens weave around themselves.

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Online Appendix for

The Incidental Pundit
Who Talks Politics with Whom, and Why?

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Contents

The Friends for Life Study

The Friends for Life Study is a dormitory-based, multiplex, multi-site, whole network, panel dataset that spans fifteen locations ("chapters"), from 2008 to 2016 (except 2009), for a total of 113 chapter-years. Participants were recipients of a scholarship that required residence in the chapter house throughout the recipient's time in college. One site appears only in 2016, and so we exclude it from the analysis, since we lack longitudinal information about its networks and members. Table A1 reports descriptive statistics on these chapters and their host universities.

Table A1: Chapter Descriptive Statistics

Chapter	Avg. Size	Average Frac. Dem.	Average Frac. Rep.	Average Frac. White	Average Frac. Male	Average Disc. Frac.	University Size (undergrad)
1	44	0.26	0.25	0.81	0.78	0.41	27k
2	112	0.43	0.21	0.61	0.77	0.43	34k
3	56	0.25	0.41	0.96	0.73	0.46	33k
4	62	0.39	0.30	0.68	0.61	0.31	8.4k*
5	48	0.19	0.41	0.92	0.87	0.41	17k
6	60	0.26	0.35	0.87	0.84	0.63	30k
7	66	0.27	0.32	0.84	0.79	0.33	39k
8	53	0.26	0.43	0.93	0.87	0.50	32k
9	41	0.24	0.39	0.94	0.78	0.39	24k
10	37	0.30	0.27	0.65	0.99	0.42	15k
11	34	0.34	0.14	0.72	0.71	0.55	8.4k*
12	66	0.19	0.38	0.93	0.88	0.45	47k
13	42	0.25	0.42	0.80	0.69	0.50	33k
14	62	0.32	0.32	0.94	0.79	0.52	30k

Descriptive statistics are averaged over all survey waves and calculated based on non-imputed values. "Average Disc. Frac." refers to the across-wave average of the percentages of political discussants that respondents reported were members of the chapter. * Private. All others are public universities.

The core of the dataset consists of two pieces: an individual-level panel and a dyadic-level panel. At the individual level, we derived information on the *Chapter* and *Cohort* of each respondent based on information provided by the scholarship organization. We used *Cohort* to identify *School Year*, based on the number of years since an individual entered college. To collect other individual-level variables, we fielded a longitudinal survey with waves every August, during the first week of classes, and November, post-election. Table A2 presents summary statistics on the resulting individual-year dataset. For this paper, we used responses to the following items:

Gender
Item: What is your gender?
Responses: Male, Female
Note: We used these responses to code an indicator for *Female*.

Race/Ethnicity
Item: What racial or ethnic group best describes you? (Check all that apply.)
Responses: White, Black, Hispanic, Asian, Native American, Middle Eastern, Mixed, Other
Note: Multiple responses were accepted. Due to low frequencies, we recoded all multiple responses and all responses in the categories "Native American," "Middle Eastern," and "Mixed" as "Other." We then coded indicators for *Asian*, *Black*, *Latino*, and *White*.

Religion

Item: What is your religion? (Check all that apply.)

Responses: Baptist–any denomination, Protestant (e.g. Methodist, Lutheran, Presbyterian, Episcopal), Catholic, Mormon, Jewish, Muslim, Hindu, Buddhist, Pentecostal, Eastern Orthodox, Other Christian, Other non-Christian, None

Note: Multiple responses were accepted. We recoded all multiple responses that included "Baptist," "Pentecostal," and "Other Christian" as "Multiple Responses (Evangelical)," and all other multiple responses as "Multiple Responses." We then coded *Evangelical* as an indicator that was 1 for responses including "Baptist," "Pentecostal," "Other Christian," and "Multiple Responses (Evangelical)," and 0 otherwise.

Ideology

Item: In general, do you think of yourself as...

Responses: Extremely liberal, Liberal, Slightly liberal, Moderate, Slightly conservative, Conservative, Extremely conservative

Note: We recoded this variable to run from 0 to 1, with higher values indicating more Conservative answers.

Party ID

Item: Generally speaking, do you think of yourself as a...

Responses: Republican, Democrat, Independent, Another party

[If Republican:] Would you call yourself a...

Responses: Strong Republican, Not very strong Republican

[If Democrat:] Would you call yourself a...

Responses: Strong Democrat, Not very strong Democrat

[If Independent or Another Party:] Do you think of yourself as closer to the...

Responses: Republican Party, Democratic Party, Neither

Note: Based on these responses, we coded indicators for *Democrat* and *Republican*, excluding leaners, and *Democrat w/ Leaners* and *Republican w/ Leaners*, including them.

Political Interest

Item: In general, how interested are you in politics and public affairs?

Responses: Very interested, Somewhat interested, Slightly interested, Not at all interested

Note: We rescaled this variable to run from 0 to 1, with higher values indicating more interest.

Political Knowledge

Item: Do you happen to know what job or political office is now held by [Dick Cheney/Joe Biden]?

Responses: [open text box]

Item: Whose responsibility is it to determine whether a law is constitutional or not?

Responses: Congress, The President, The Supreme Court, Don't Know

Item: How much of a majority is required for the US House and Senate to override a Presidential Veto?

Responses: 50% + 1 (simple majority), 60% (three fifths), 66.7% (two thirds), 75% (three quarters), 100% (unanimity), Don't Know

Item: Which party currently holds a majority of seats in the House of Representatives?

Responses: Democratic Party, Republican Party, Neither, Don't Know

Item: Which party currently holds a majority of seats in the Senate?

Responses: Democratic Party, Republican Party, Neither, Don't Know

Note: Correct answers depend on when the survey was fielded. We coded correct answers as "1,"

and incorrect answers and "Don't Knows" as "0." If an observation contained any non-missing answers to these items, we coded the missing answers as "0." For respondents who answered none of these items, we coded all items as missing. Finally, we summed the correct answers to get a score for *Political Knowledge* ranging from 0 to 5, then rescaled to run from 0 to 1.

Conflict Avoidance

Item: When people argue about politics, I often feel uncomfortable.

Item: When I'm in a group, I stand my ground even if everyone else disagrees with me.

Item: I usually find it easy to see political issues from other people's point of view.

Item: If I'm sure I'm right about a political issue, I don't waste time listening to other people's arguments.

Item: I have no problem revealing my political beliefs, even to someone who would disagree with me.

Item: I would rather not justify my political beliefs to someone who disagrees with me.

Item: I do not take it personally when someone disagrees with my political views.

Item: When I'm in a group, I often go along with what the majority decides is best, even if it is not what I want personally.

Responses: All items were five-point, Likert-type scales with responses ranging from "Strongly agree" (5) to "Strongly disagree" (1).

Note: Different waves of the survey included subsets of these items. We fit an Bayesian latent variable measurement model to estimate an underlying scale for all individuals.

Table A2: Summary Statistics

Variable	Mean	SD	Min	Max	% Missing
Female	0.24	0.42	0	1	4%
Asian	0.02	0.13	0	1	4%
Black	0.04	0.21	0	1	4%
Latino	0.07	0.25	0	1	4%
White	0.82	0.39	0	1	4%
School Year = 1	0.28	0.45	0	1	0%
School Year = 2	0.25	0.43	0	1	0%
School Year = 3	0.23	0.42	0	1	0%
School Year $\geq$ 4	0.23	0.42	0	1	0%
Evangelical	0.15	0.36	0	1	7%
Democrat	0.34	0.47	0	1	16%
Democrat (w/ leaners)	0.44	0.49	0	1	16%
Republican	0.37	0.48	0	1	16%
Republican (w/ leaners)	0.45	0.50	0	1	16%
Ideology	0.50	0.26	0	1	15%
Political Interest	0.56	0.32	0	1	15%
Political Knowledge	0.73	0.31	0	1	35%
Conflict Avoidance	0.01	0.82	−3.01	3.39	11%

n observations $= 6,248$, and n individuals $= 2,521$.

At the dyadic level, we collected information on the networks within each chapter. In addition to the five networks discussed in the paper (Political, Friend, Time, Esteem, Negative), we also asked about Academic networks in a subset of years. We omitted this network from the paper

because we lack data on it in 2008, 2010, and 2016. For each network, each respondent was shown a roster of all individuals in the chapter, and asked to select the ones for which each of the following statements was true:

Political
"I frequently discuss politics, social issues, or current events with this person"

Esteem
"I hold this person in especially high esteem."

Friend
"This person is a close friend."

Time
"I spend a lot of time around this person."

Negative
"Sometimes I do not find it easy to get along with this person."

Academic
"This person has assisted me with my academics."

Missing Data Imputation

Nonresponse—at the item, observation, and individual levels–was an issue, with varying response rates across different waves of the panel. List-wise deletion based on nonresponse would result in substantial loss of information (King et al. 2001), which is especially problematic with network data (Kossinets 2006). Therefore, we imputed missing responses to our non-network items using Amelia II (Honaker, King and Blackwell 2011).

Specifically, we identified the set of variables that form the basis for our eventual analysis. For imputation, we used information from both the August surveys and the November surveys, across all available waves of the survey. These included the categorical variables *Gender*, *Race/Ethnicity*, *Religion*, *Chapter*, and *Cohort*; the ordered variables *Political Interest*, *Political Knowledge*, *Party ID*, and *Ideology*; and the continuous variable *Conflict Avoidance*, estimated using the method described in the previous section. Because *Chapter* and *Cohort* were provided by the organization, there is no missingness on these variables. However, once a respondent had provided an answer to the demographic items, they were not asked the same item in every subsequent survey. In particular, *Gender*, *Race/Ethnicity*, and *Religion* would be asked at most once a year, and in some cases less often. Therefore, our first step was to use last observation carried forward and next observation carried back in succession on these three variables. Finally, we used the amelia function from the Amelia package in R to impute ten complete datasets, taking advantage of the panel nature of the dataset by including time- and respondent-specific information.

Permutation-based Simulations Including Leaners

The results in the main paper code leaners (i.e., those who answered our first party ID branching question with either "Independent," "Other Party," etc., but who then responded to the follow-up party lean question by selecting either "Democratic Party" or "Republican Party") as neither Democrats nor Republicans. We chose this coding convention because we are interested purposive homophily based on party, and individuals who attempt to mask their party ID would seem to thwart such intentional selection criteria. Nevertheless, there is ample evidence that these leaners are not independents, but identifiers with major parties (Keith et al. 1986; Petrocik 2009).

Egocentric Analysis Biases
Party Homogeneity Estimates
(Including Leaners as
Party Identifiers)
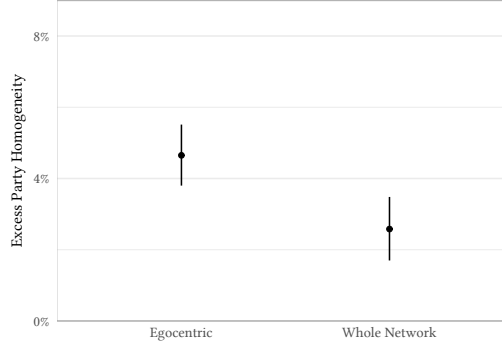
Figure 1: Excess party homogeneity is the difference between observed levels and those that would occur by chance, calculated by permuting party labels. The estimate on the left permutes across all chapters in a year, as with egocentric data alone. Such an approach cannot leverage information about local opportunity structures. The estimate on the right permutes at the chapter-year level, leveraging such whole network information. Ignoring the additional information afforded by whole network data will typically bias estimates. Both estimates include leaners as major party identifiers. Including leaners as major party identifiers does little to change the findings.

Therefore, we re-ran all analysis in the paper coding leaners as major party identifiers. As we note in the paper, very little changes based on this coding decision.

In this section, we present the results of the permutation-based simulations to estimate excess party homogeneity when we include leaners as major party identifiers. Including leaners changes the levels of observed homogeneity, but does little to alter the estimated levels of excess homogeneity, defined as the difference between observed levels and those based on permutations of party labels. For example, whereas 27% of all ties were between explicit major party identifiers, when we include leaners, this proportion rises to 42%. However, the middle 95% ranges of the two are similarly displaced from these observed levels. For explicit major party identifiers, and permuting party labels globally, the middle 95% range is $[24\%, 26\%]$, so the range of estimated excess party homogeneity is 2% to 4%. Including leaners, the middle 95% range is $[39\%, 41\%]$, so the range of estimated excess party homogeneity falls slightly to 1% to 3%.

Figure 1 presents the complete results in an analogous form to that in Figure 3 from the paper. This figure underscores the two important points from the paper. First, if we focus on all ties rather than those between major party identifiers, we find lower rates of excess party homogeneity. Second, if we permute party labels locally rather than globally, we also find lower rates of excess party homogeneity. Both of these findings emerge whether we include leaners as major party identifiers or not. Moreover, there is substantial overlap in the distributions of estimated excess party homogeneity based on both coding decisions.

Identifying Assumptions for Causal Inference

Our focus in the main paper is on prediction, but we can also use our model to yield causal inferences—conditional on appropriate identifying assumptions. Any model identifies causal estimates under the (implausible) assumption that one has correctly specified the data generating process. A more plausible strategy is to assume mean conditional ignorability: for example, the counterfactual expected value of a tie conditional on *Same Party* taking a particular value is equal to our model's estimate. This approach is similar to any observational design that yields causal inferences by adjusting for observed covariates, such as matching or synthetic case control.

This identification strategy requires us to assume both that there are no unobservables correlated with discussion and the causal variable, and that we have not introduced post-treatment bias by including such terms in our model. We have included a large slate of covariates to cope with the first requirement. But the second requirement is likely violated by the inclusion of terms like *Dyadic Stability*. For example, *Same Party* might have caused a discussion tie to form in a previous year, thus changing the value of *Dyadic Stability*. Adjusting for *Dyadic Stability* can therefore introduce bias into our estimates of marginal effects. Here, the goal of predictive accuracy is at odds with that of unbiased causal inference.

Similar issues plague other variables. Structural variables like the closure terms and the reciprocity term are measured contemporaneously with the outcome, and are therefore also susceptible to such bias. Further, the lagged social relationships might also be considered post-treatment, as two people might become friends because of political similarity.

There are no foolproof fixes for post-treatment bias. We can, however, perform a sensitivity analysis with a model that excludes post-treatment variables. Therefore, we re-estimated demographic and political homophily terms using a model that includes only the Demographic and Political terms.

Our key quantity of interest is the average marginal effect. To calculate this, for each dyad we first estimated difference between (1) the probability of discussion if a dyad identifies with the same political group, and (2) that in which the dyad identifies with different groups. Next, we take the mean over all dyads in a chapter to get chapter-level average marginal effects. Finally, we pooled across chapters using the Bayesian model.

We denominate these quantities in percentage point terms, and consider *Same Party* and *Ideological Proximity* in sequence.

First, there are only small changes in the estimates for *Same Party*. As a reminder, according to the Full model, the estimated marginal effect of *Same Party* as reported in the manuscript is 0.3% with 95% interval $[0.2\%, 0.4\%]$. Based on the model that includes only Demographic and Political terms, this value is 0.6% $[0.3\%, 0.8\%]$. Thus, there is little change in the estimates for party-based homophily, which is consistent with at most small levels of post-treatment bias.

The differences across models are a bit larger for *Ideological Proximity*. As a reminder, according to the Full model, the estimated marginal effect of *Ideological Proximity* is 1.0% $[0.8\%, 1.2\%]$. In contrast, the model including only Demographic and Political terms yields an estimate of 2.0% $[1.7\%, 2.4\%]$. This analysis suggests that the estimated marginal effect based on the Full model may have been contaminated by post-treatment bias.

While these auxiliary analyses reveal sensitivity of our Full model, at least for *Ideological Proximity*, they also help reinforce our main claim: there is evidence of limited purposive political homophily, and that it is generally smaller than incidental political talk based on other

characteristics. With the Demographic and Political model, we also see increased estimates for *Same Race/Ethnicity* (2.9% [2.6%, 3.3%]), *Same Gender* (5.2% [4.9%, 5.5%]), and *Same Cohort* (6.8% [6.4%, 7.2%]).

Based on this analysis, we draw two conclusions. First, estimates of causal effects based on our predictive model may be underestimates, but generally have the correct sign. Thus, causal interpretations of the quantities presented in the main paper may be conservative, which is why we make no such claims there. Second, the main finding—that political homophily effects are positive and precisely measurable, yet are also generally smaller than demographic homophily effects—is robust to different sets of identifying assumptions.

Goodness-of-fit Assessment

In this section, we present an assessment of goodness-of-fit (GOF) for the *Full* TERGM model from the main paper. To assess GOF, we calculated a set of network statistics for the observed networks and compared them with statistics for simulated networks. For each chapter and imputation, we simulated 100 networks, thus yielding 1000 simulations for each chapter. Networks were simulated with the gof function from the btergm package (Leifeld, Cranmer and Desmarais 2018), using the default values for the number of burn-in iterations (10000) and thinning interval (1000). The statistics we used to assess GOF include the distributions of the seven directed shared partners counts, indegree and outdegree, geodesic distance, walktrap modularity, and directed triads. The figures presented in the next fourteen pages display these results, with one chapter per figure. In each figure, the light grey lines represent the 95% intervals for simulated networks, and the bold black lines represent the statistics for observed networks. This assessment shows that the axillary statistics are within the acceptable range of the simulated distribution in most cases, but with some exceptions. Based on this assessment, we conclude that the *Full* model yields satisfactory goodness-of-fit.

Figure 2: Goodness-of-fit for Chapter 1
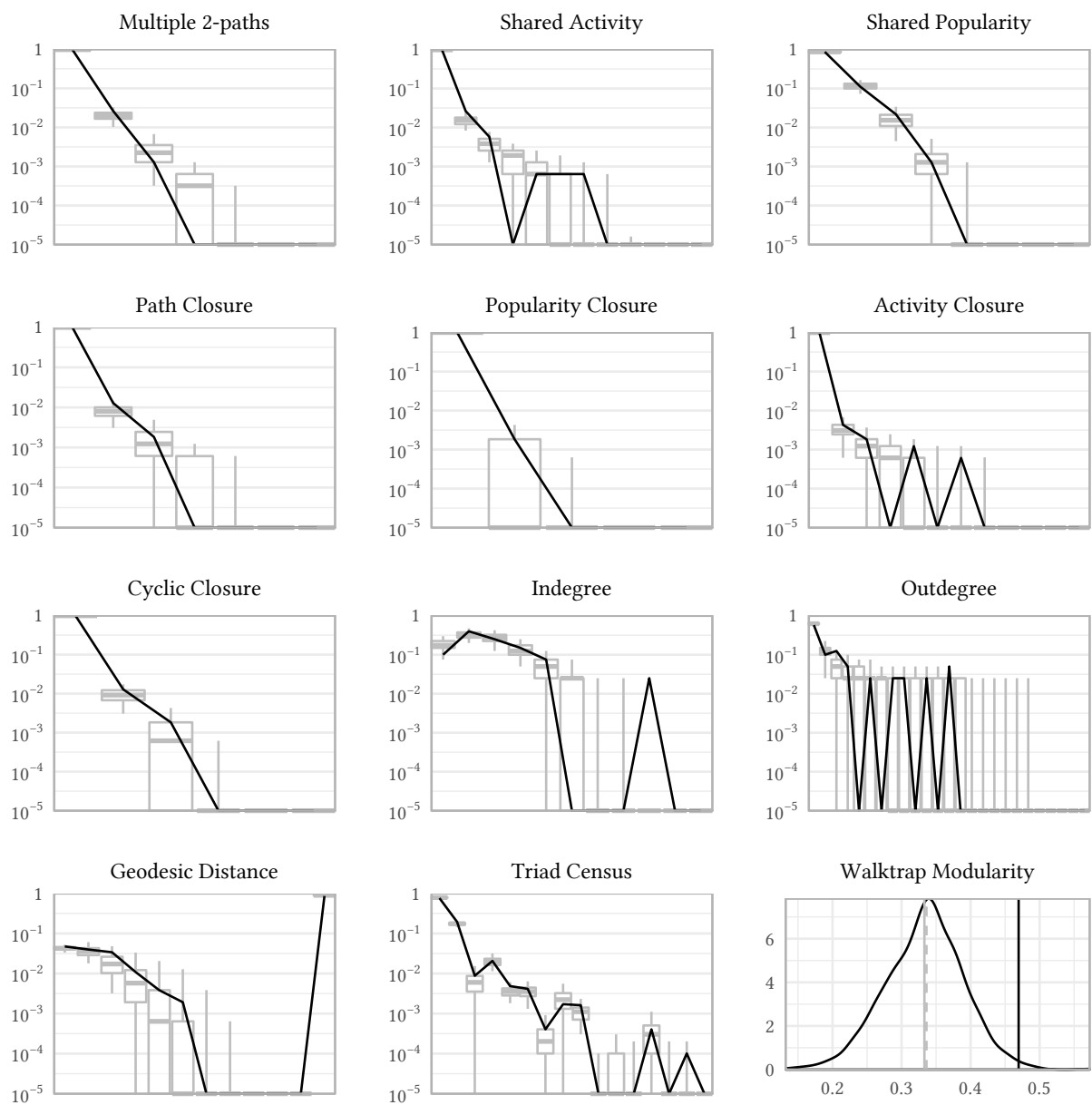
Figure 3: Goodness-of-fit for Chapter 2
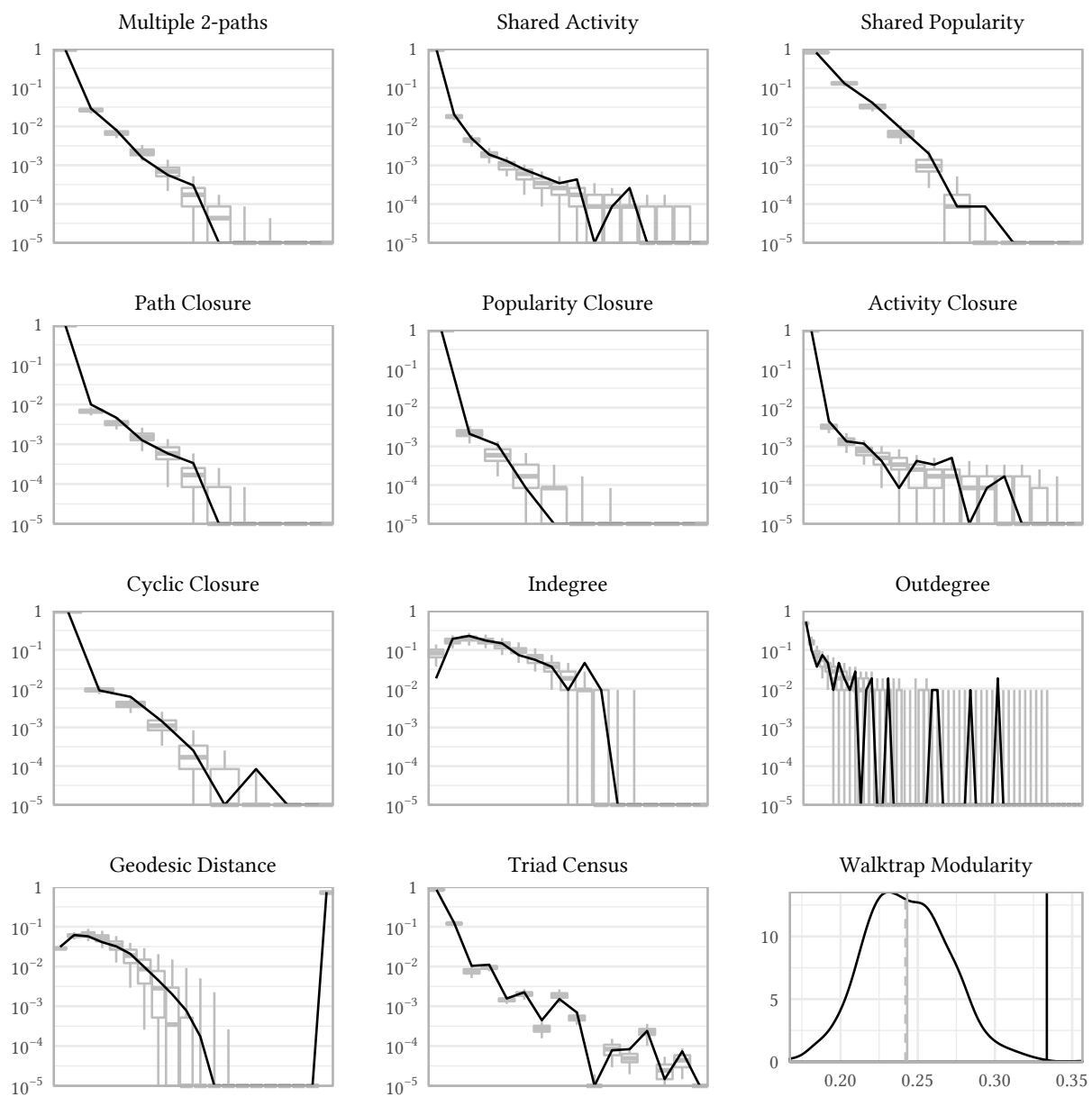
Figure 4: Goodness-of-fit for Chapter 3
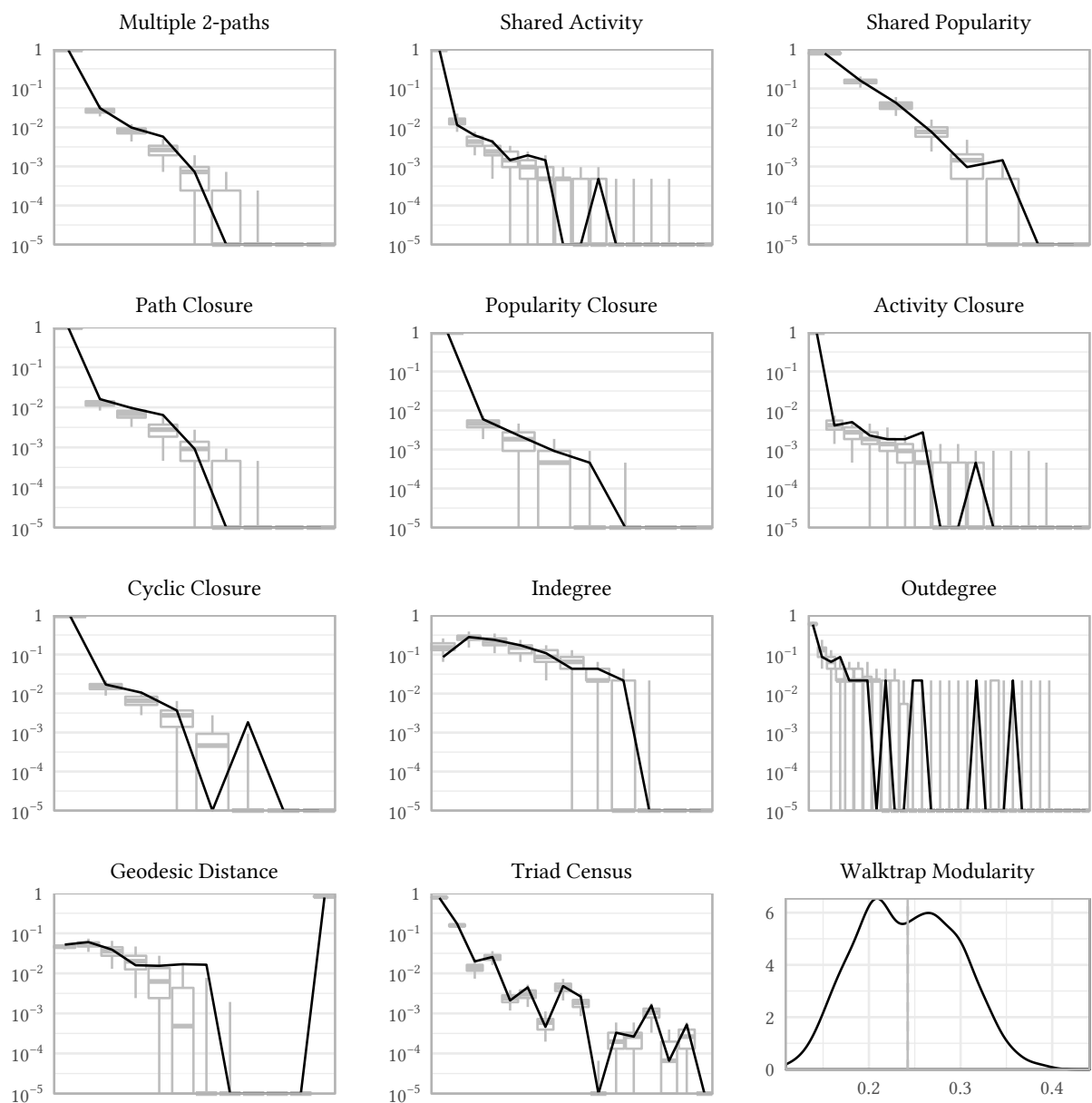
Figure 5: Goodness-of-fit for Chapter 4
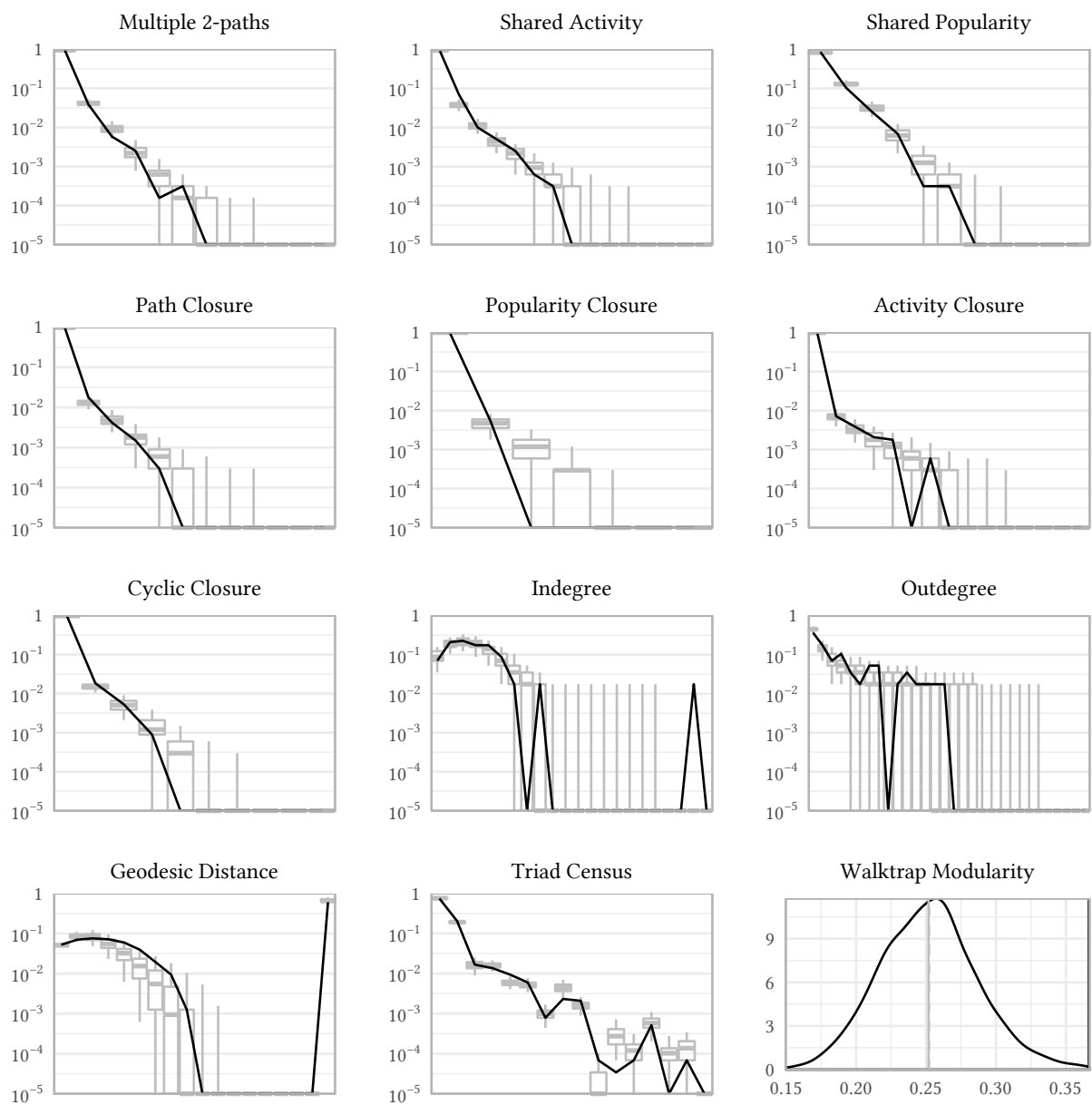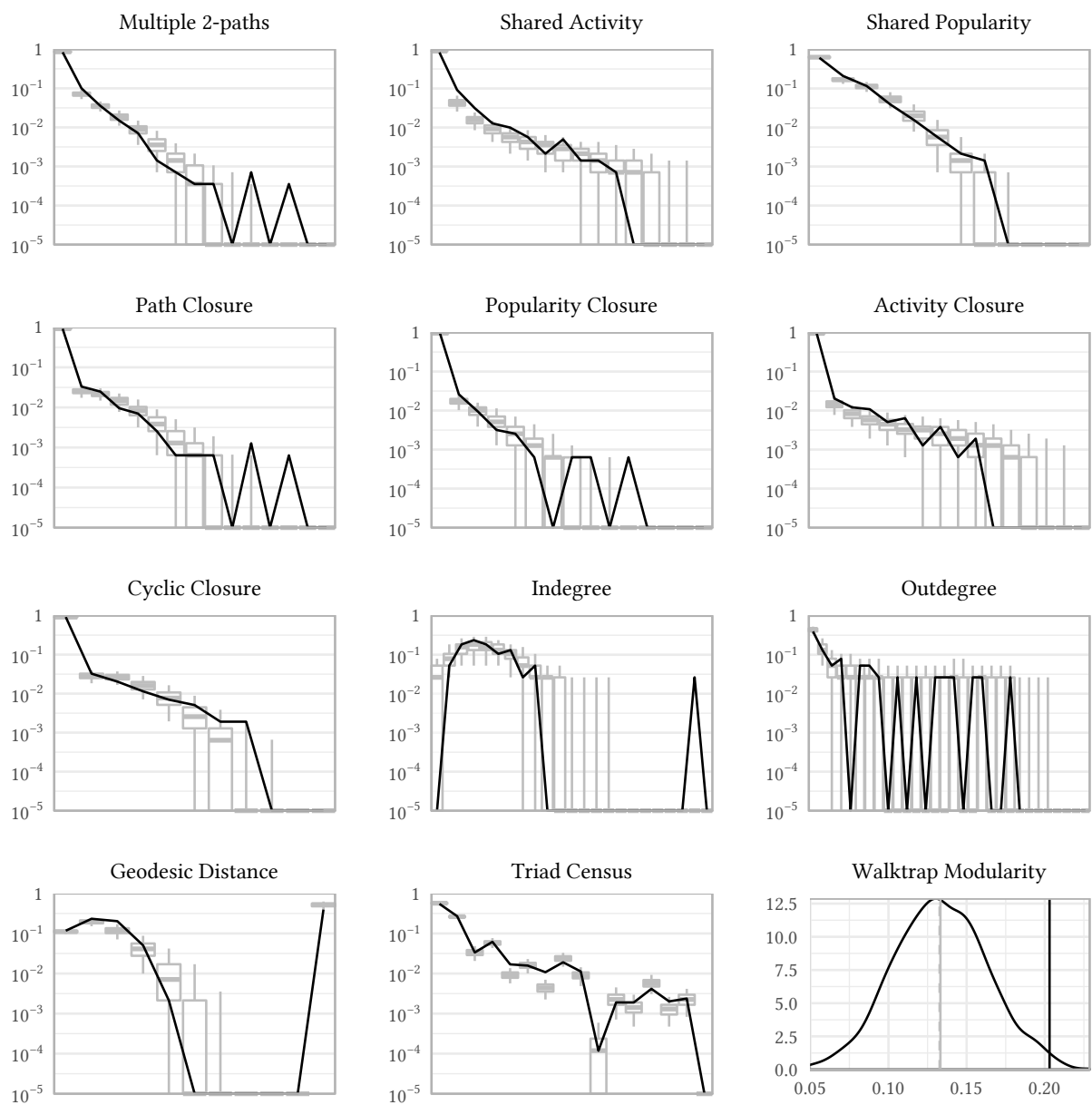
Figure 6: Goodness-of-fit for Chapter 5
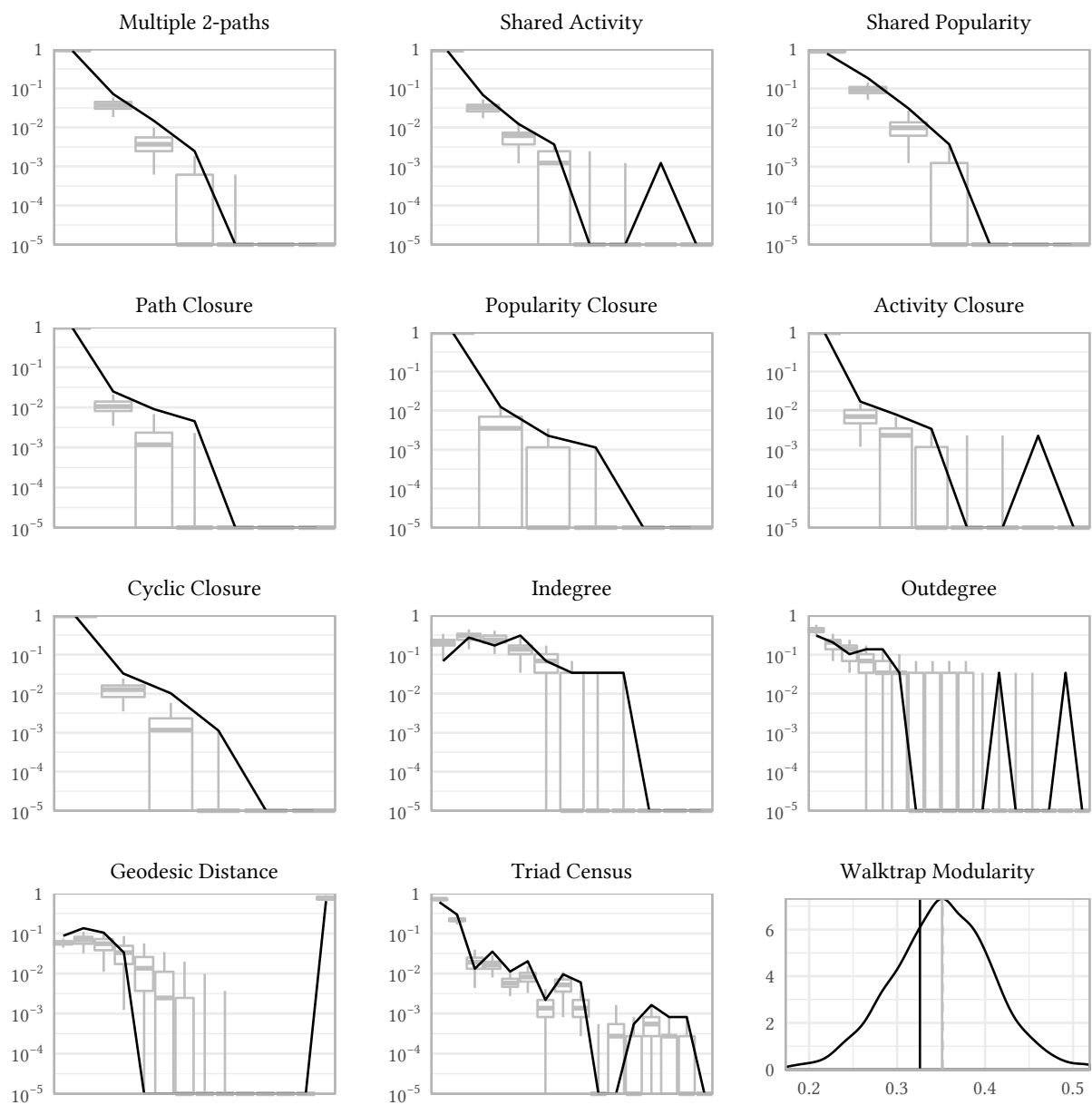
Figure 7: Goodness-of-fit for Chapter 6
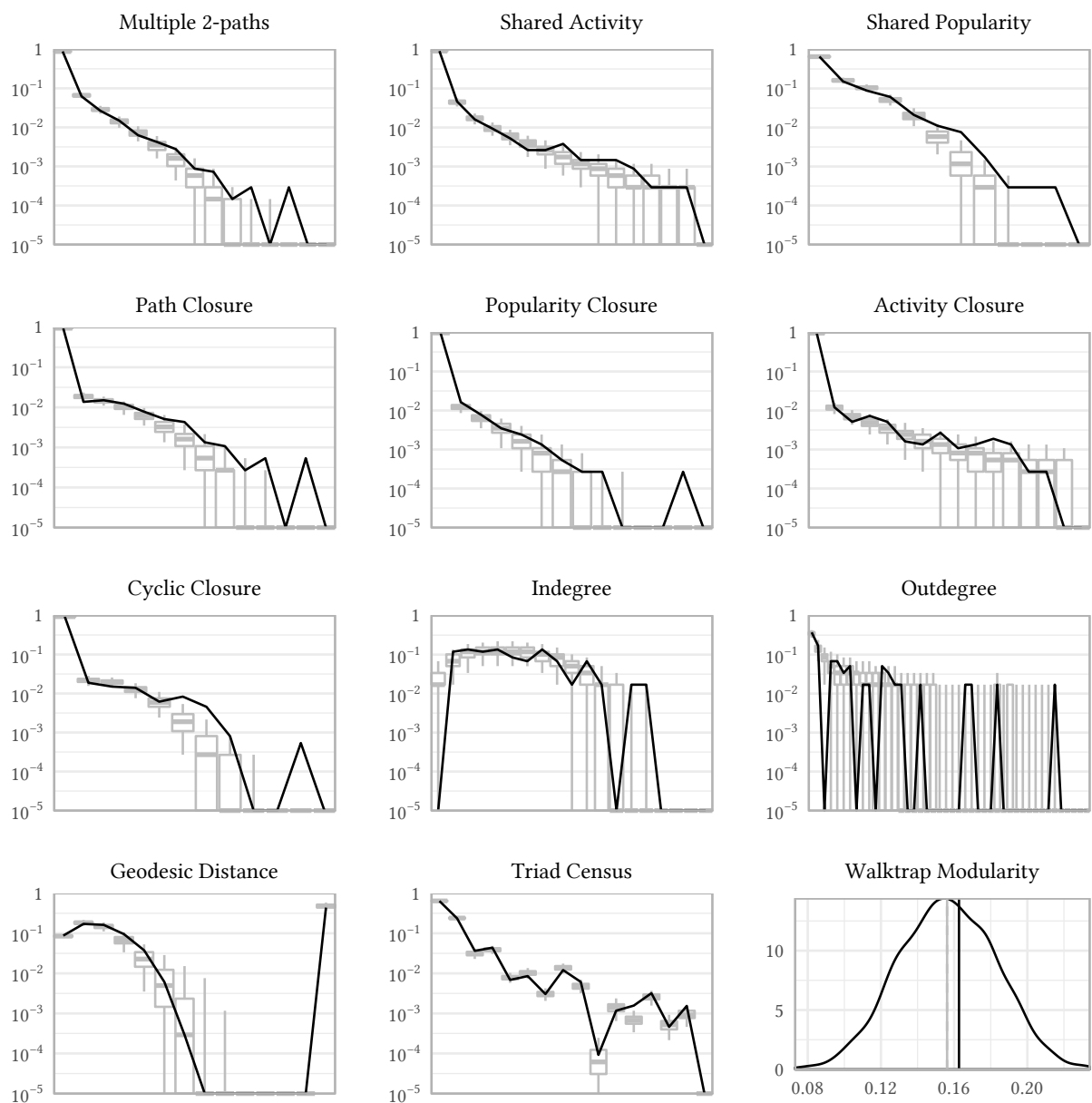
Figure 8: Goodness-of-fit for Chapter 7
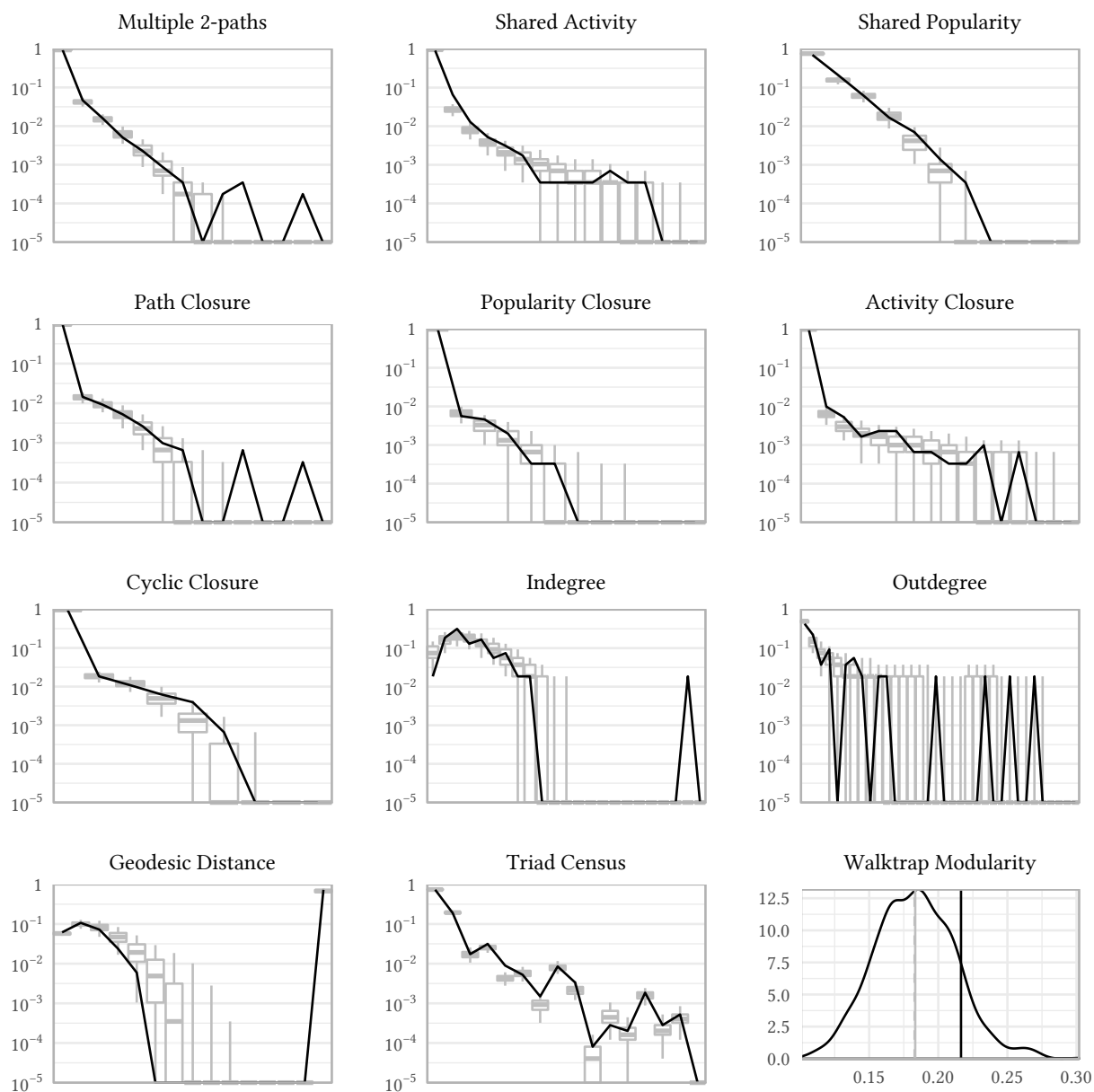
Figure 9: Goodness-of-fit for Chapter 8
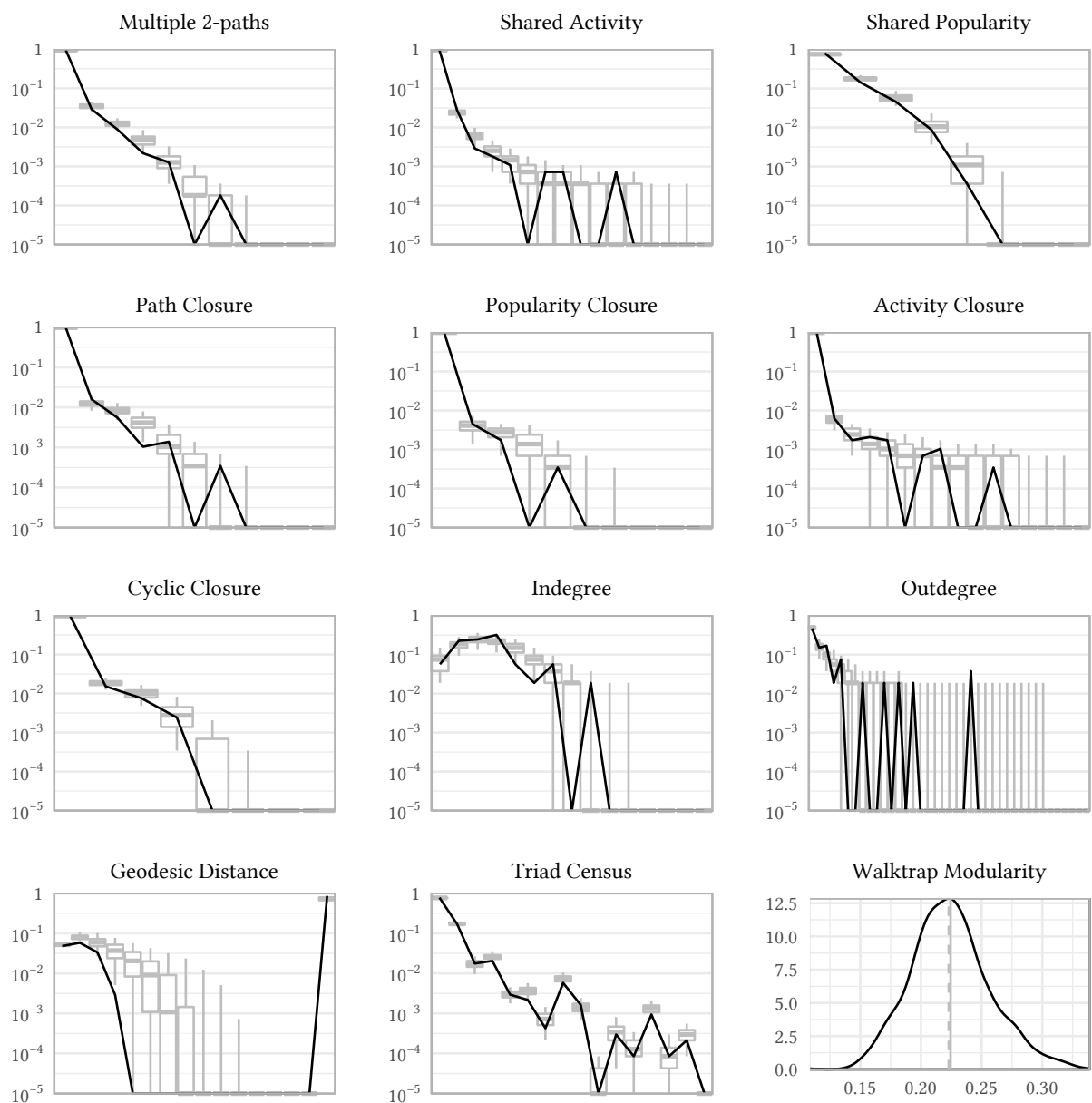
Figure 10: Goodness-of-fit for Chapter 9
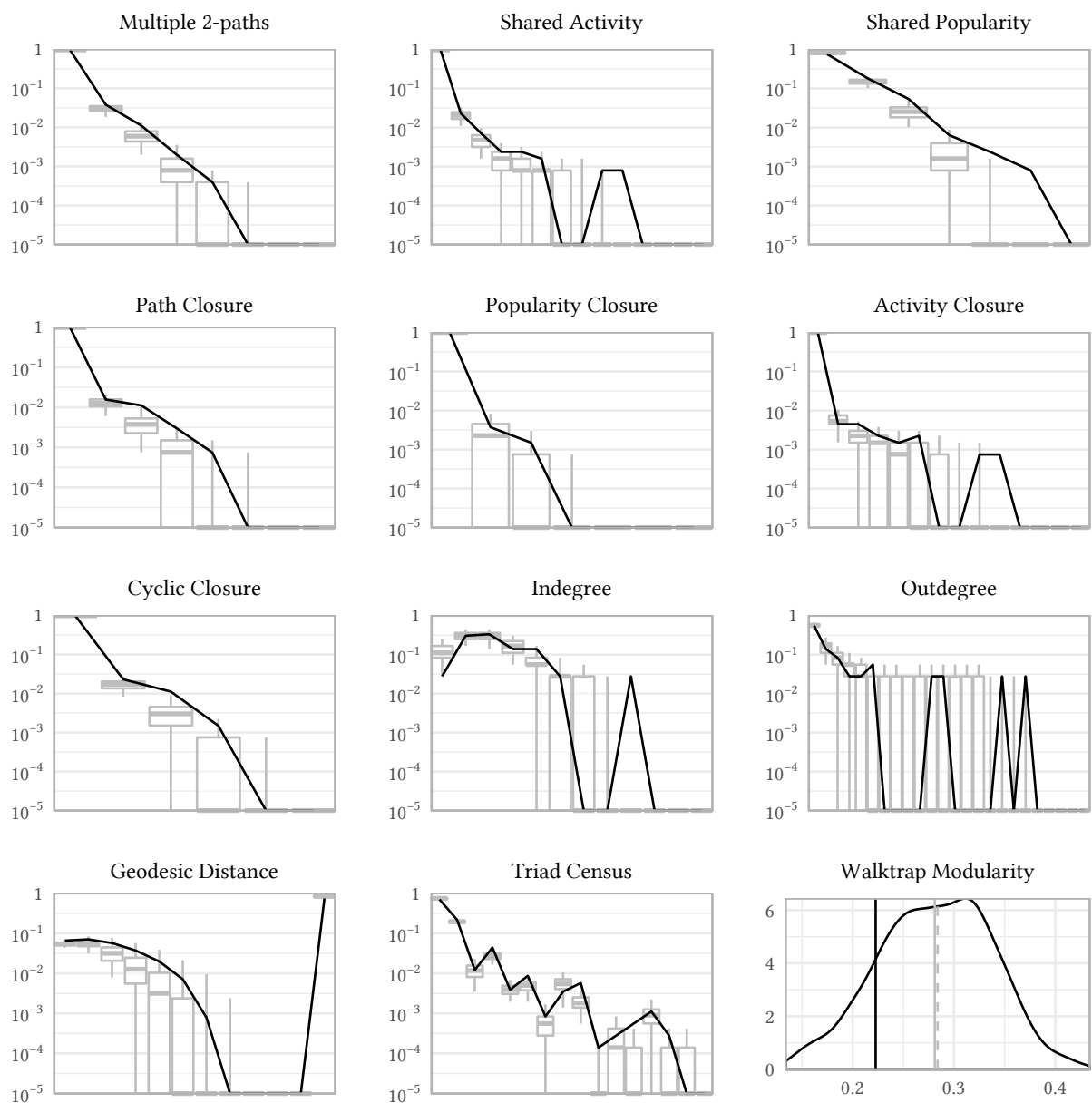
Figure 11: Goodness-of-fit for Chapter 10
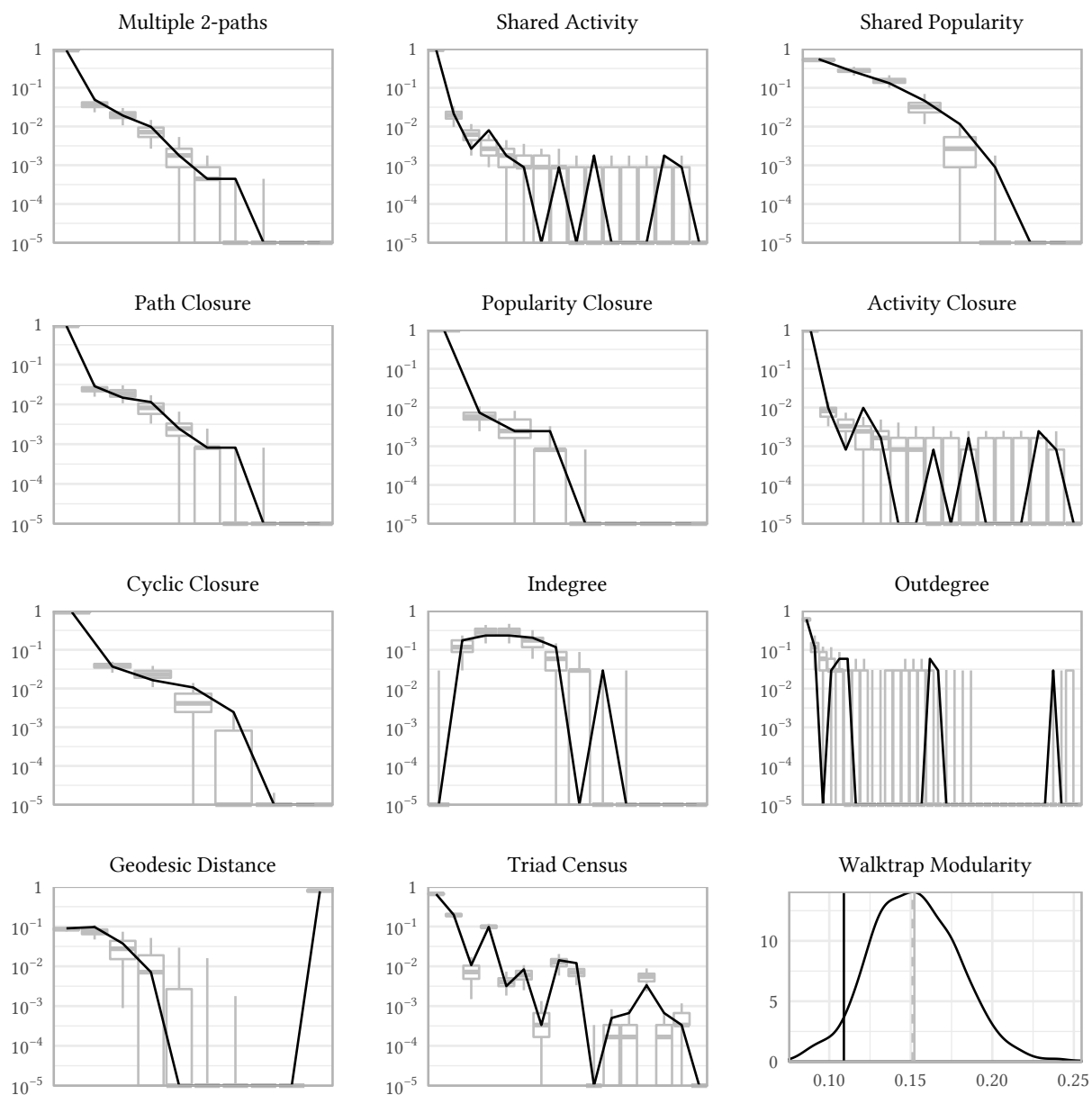
Figure 12: Goodness-of-fit for Chapter 11

Multiple 2-paths

Shared Activity

Shared Popularity

Path Closure

Popularity Closure

Activity Closure

Cyclic Closure

Indegree

Outdegree

Geodesic Distance

Triad Census

Walktrap Modularity
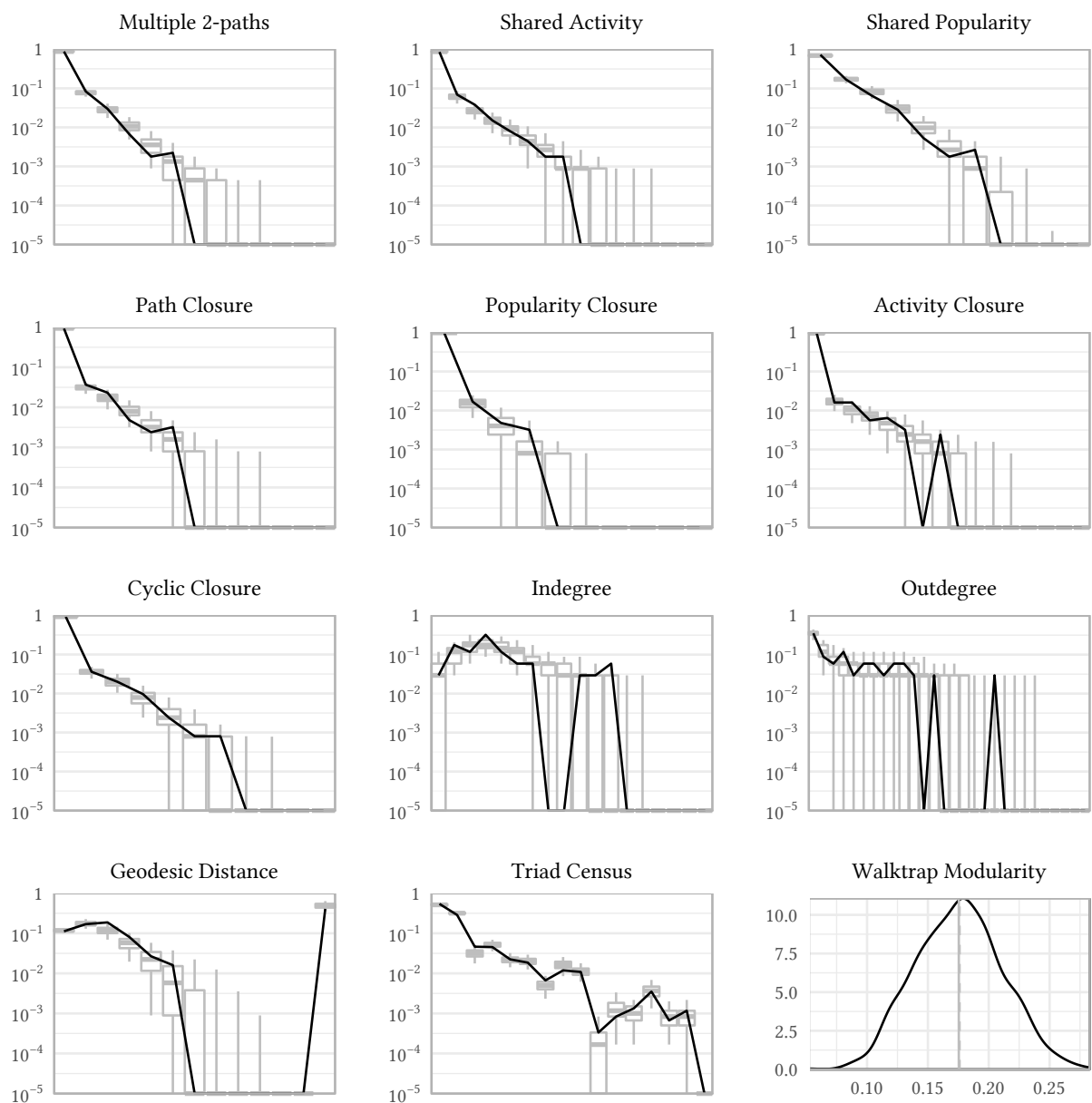
Figure 13: Goodness-of-fit for Chapter 12
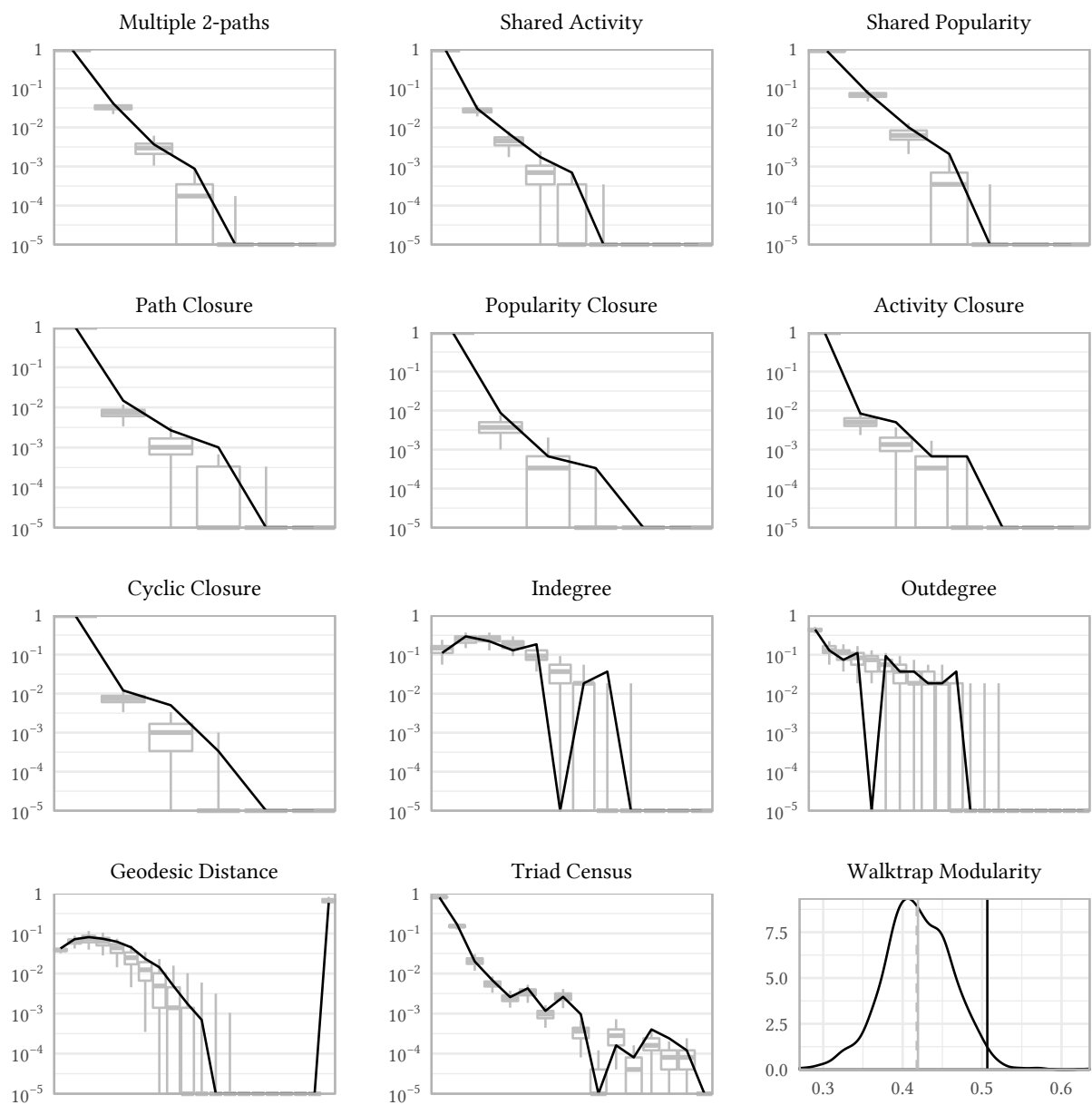
Figure 14: Goodness-of-fit for Chapter 13

Figure 15: Goodness-of-fit for Chapter 14
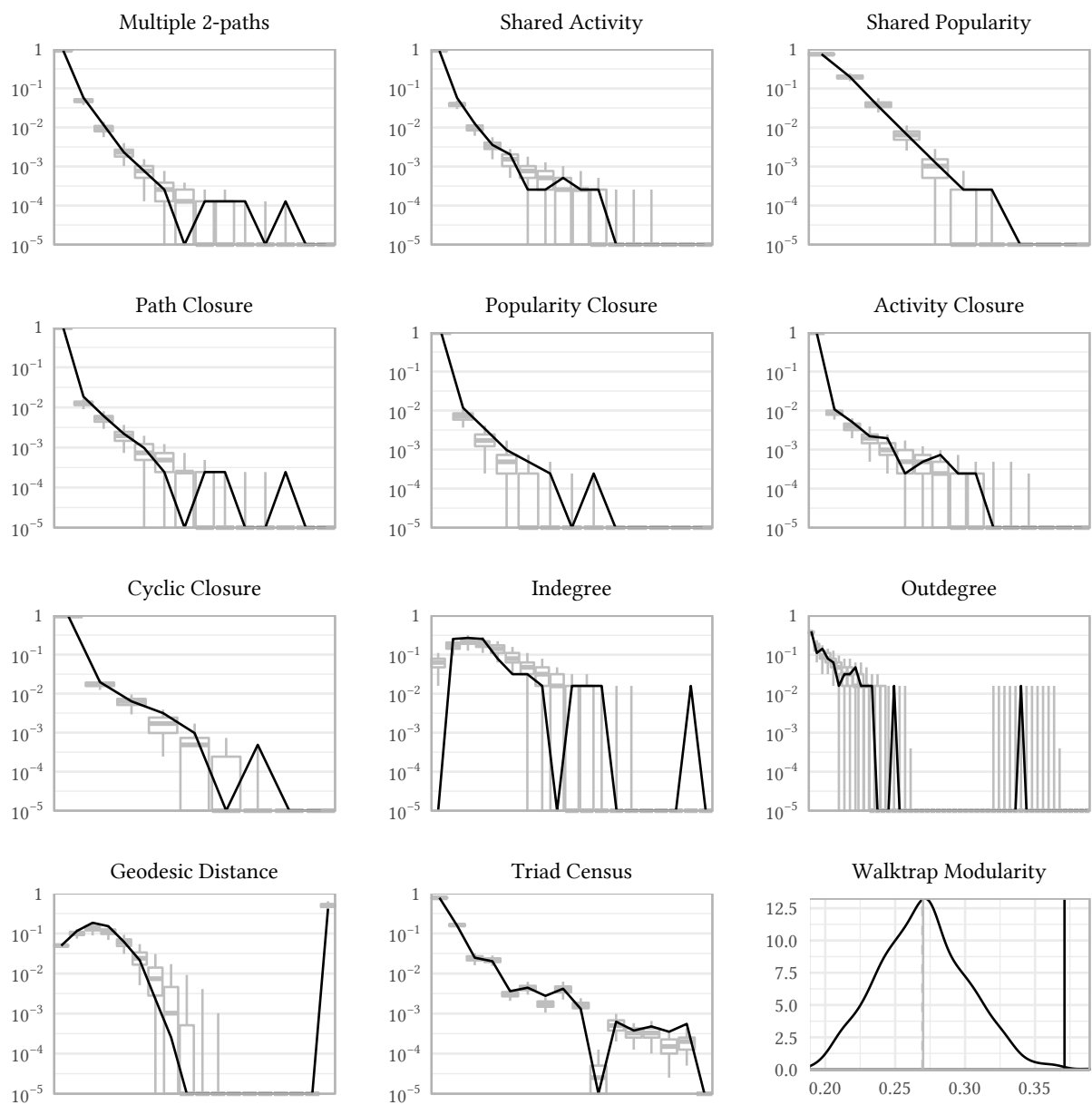
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King, Gary, James Honaker, Anne Joseph and Kenneth Scheve. 2001. "Analyzing Incomplete Political Science Data: An Alternative Algorithm for Multiple Imputation." *American Political Science Review* 95:49–69.

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URL: *https://CRAN.R-project.org/package=btergm*

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Figure 1

Political Discussion Networks
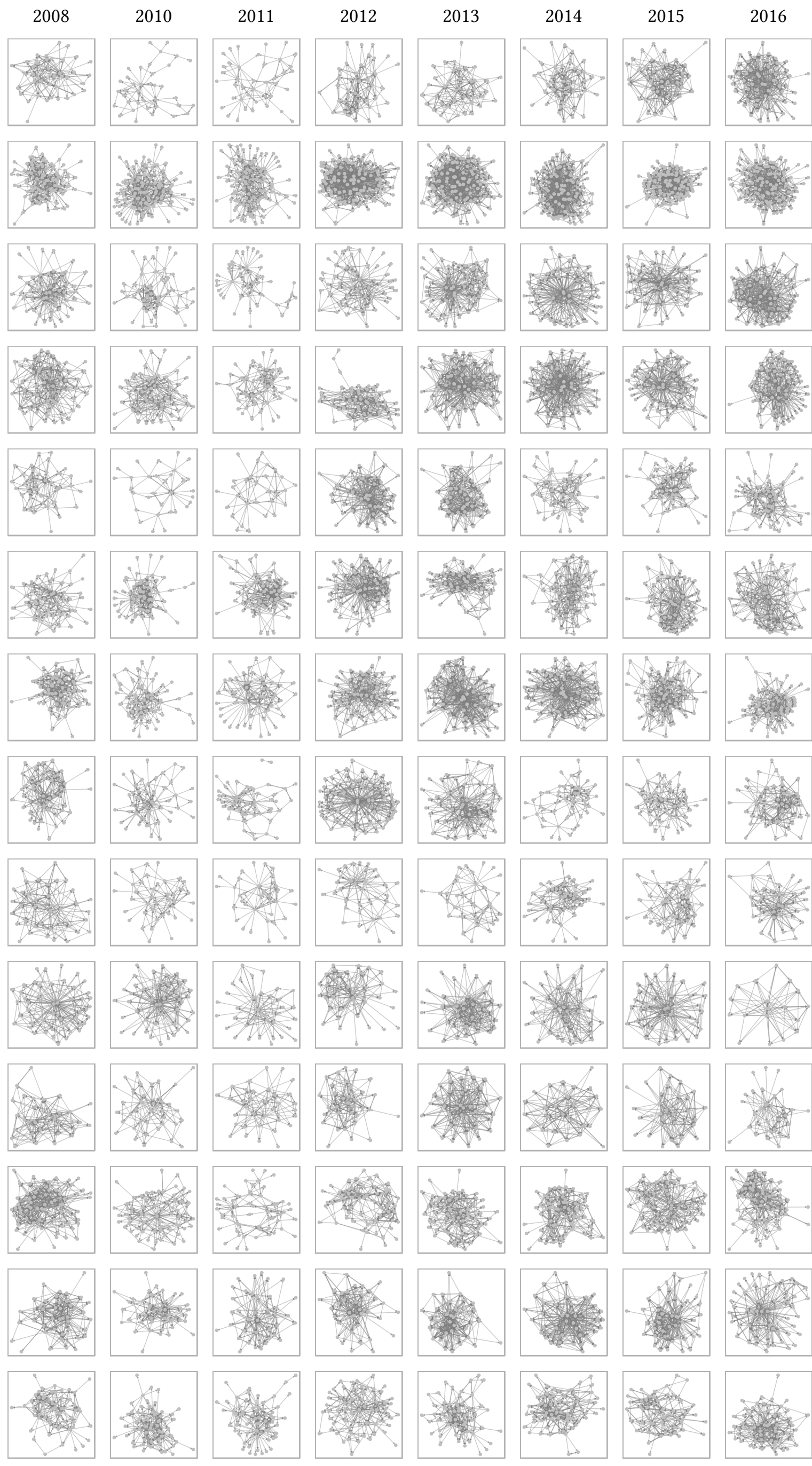
Figure 1

Figure 2

Egocentric Analysis Biases Party Homogeneity Estimates
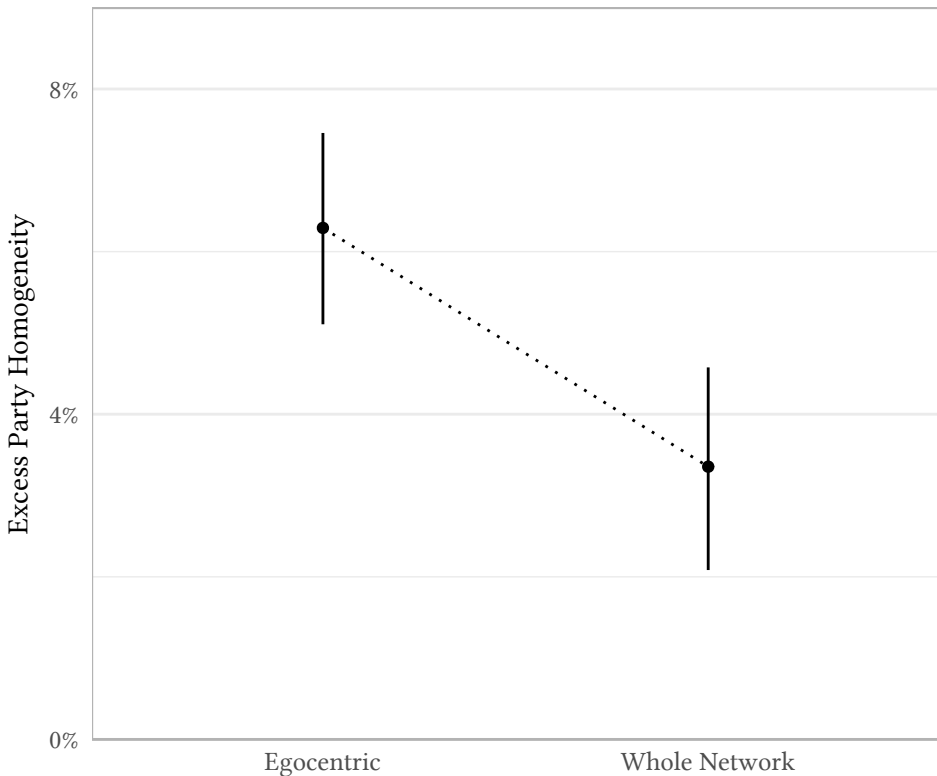
Figure 3

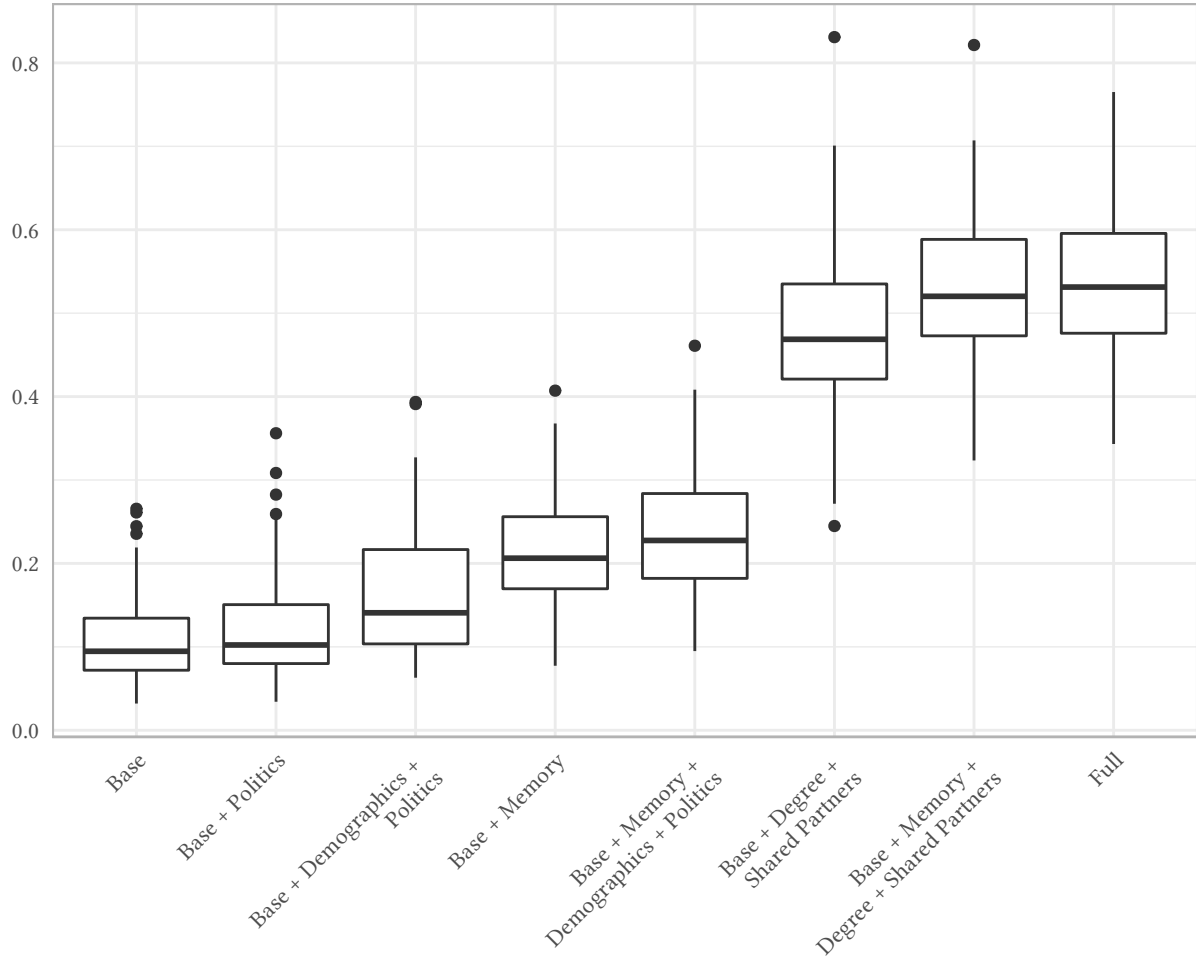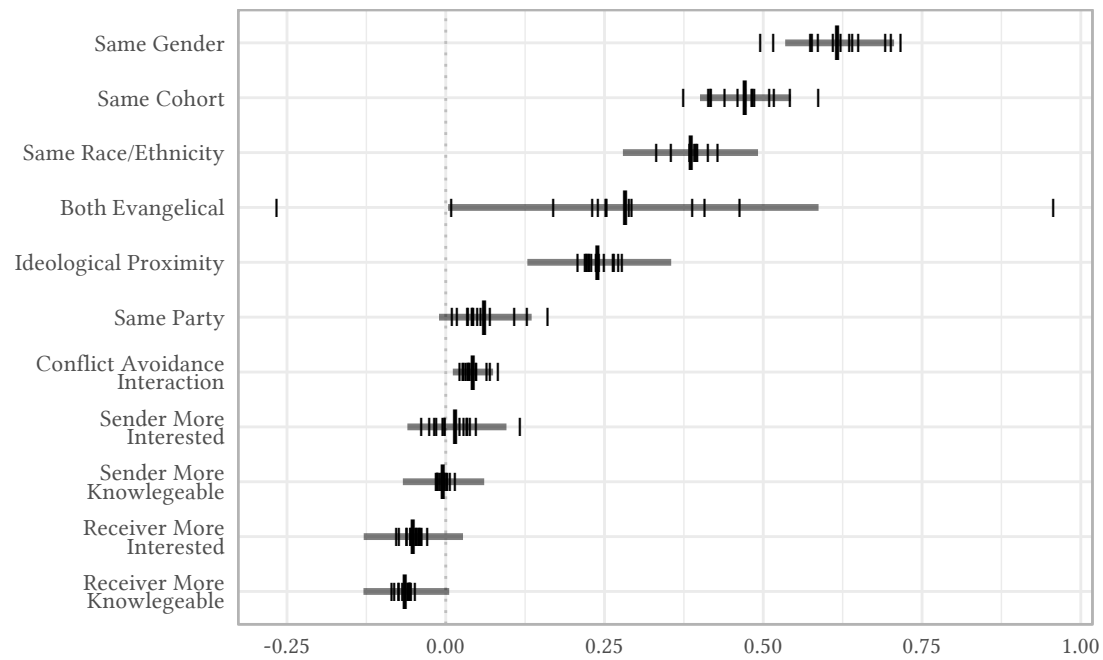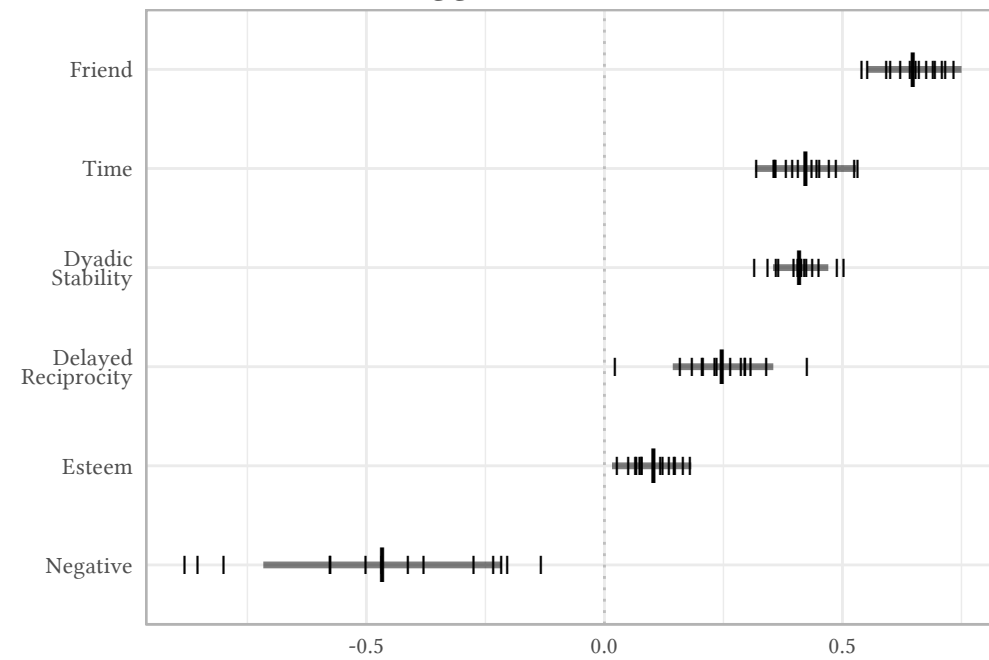# Out of Sample Predictive Accuracy of Alternative Model Specifications

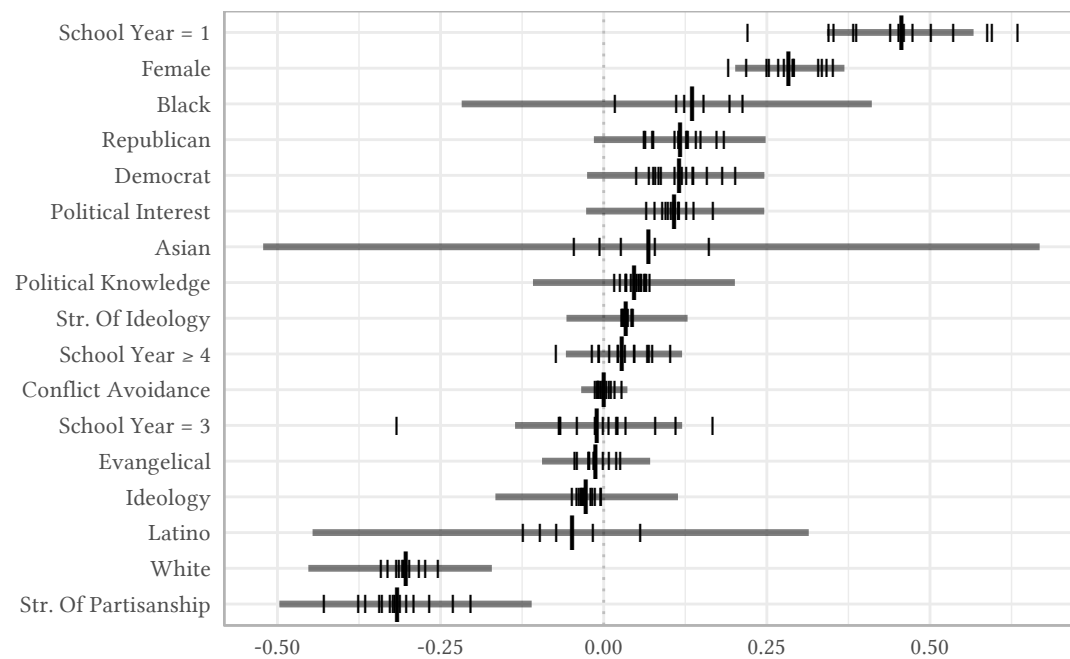Figure 4

Across-Chapter and Chapter-level TERGM Coefficients
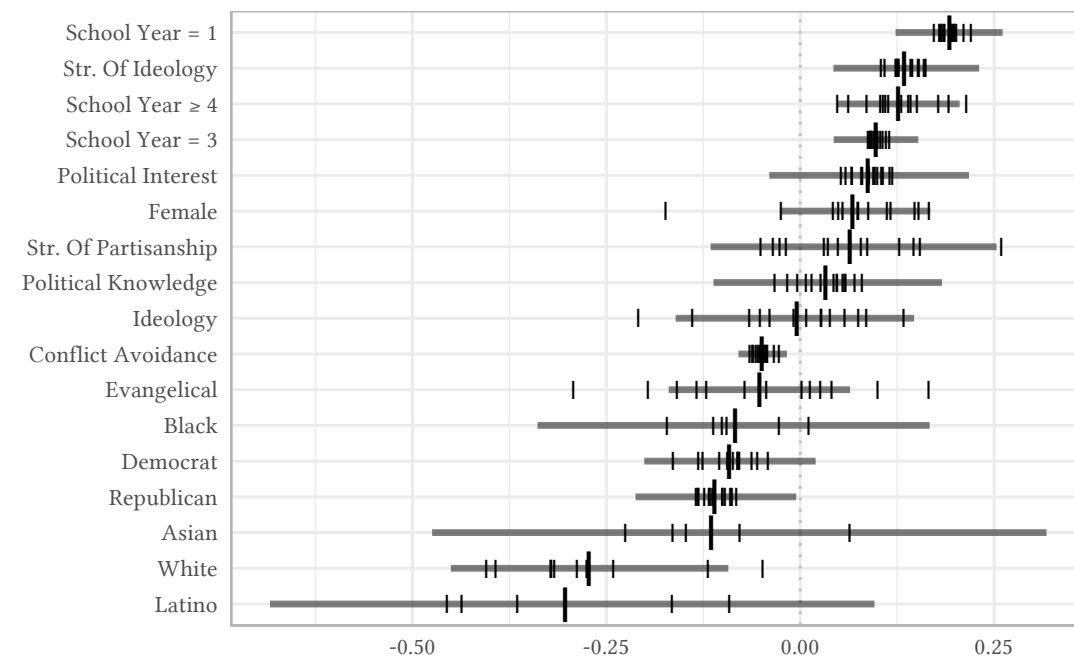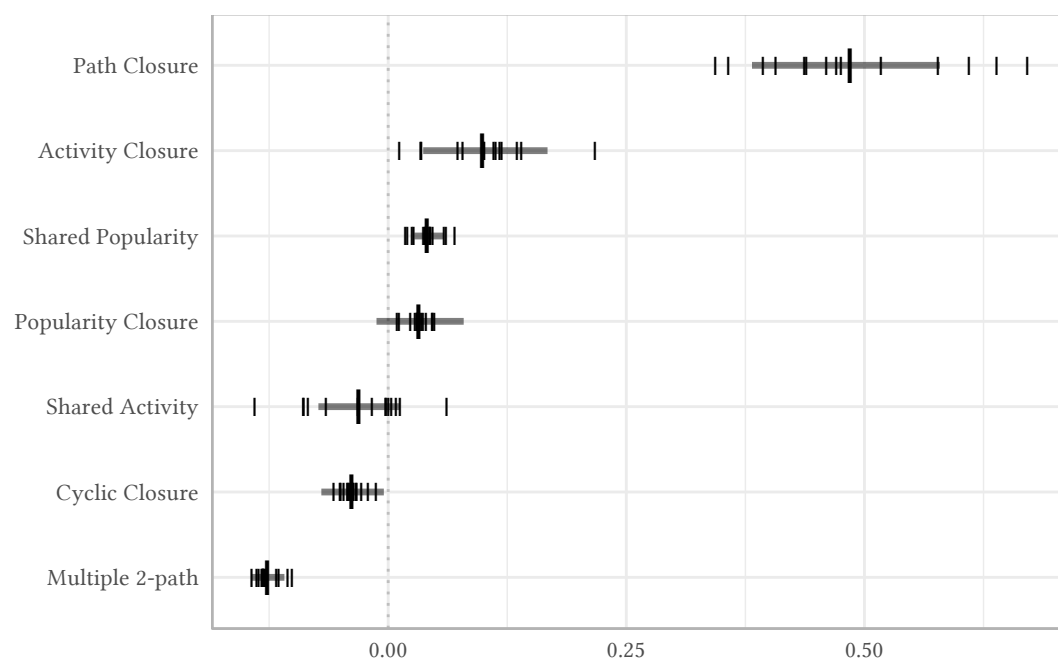
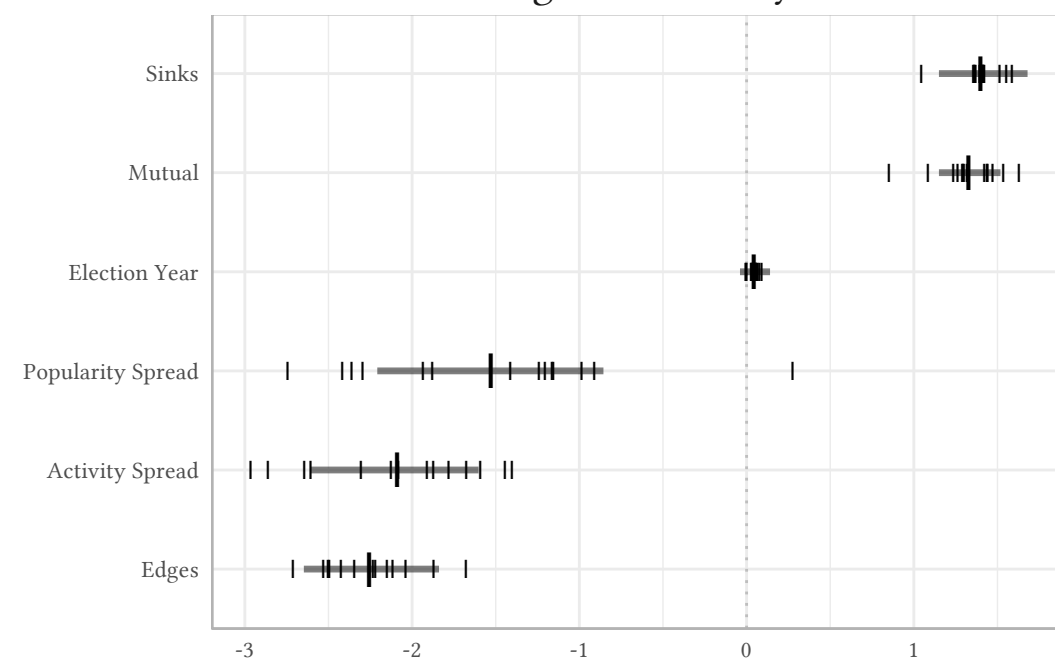Figure 5 Incidental Talk Dominates Purposive Political Homophily