

Close Relationships in Close Elections

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Close elections are rare, but most Americans have experienced a close election at least once in their lifetime. How does intense politicization in close elections affect our close relationships? Using four national egocentric network surveys during the 1992, 2000, 2008, and 2016 election cycles, I find that close elections are associated with a modest decrease in network isolation in Americans' political discussion networks. While Americans are more politically engaged in close elections, they also are less likely to be exposed to political dissent and more likely to deactivate their kinship ties to discuss politics. I further investigate a potential mechanism, the extent of political advertising, and show that cross-cutting exposure is more likely to disappear in states with more political ads air. To examine the behavioral consequence of close elections within American families, I revisit large-scale cell phone location data during the Thanksgiving holiday in 2016. I find that Americans are less likely to travel following close elections, and that families comprised of members with strong, opposing political views are more likely to shorten their Thanksgiving dinner. These results illuminate a process in which politicization may "close off" strong-tied relationships in the aftermath of close elections.

Introduction

Social segregation is one of the major challenges in American society. Sociologists have long noted that Americans are socially divided along the lines of race, gender, education, religion, and social class when it comes to residential segregation, school friendships, and romantic relationships (Bruch 2014; DiPrete

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et al. 2011; Marsden 1988; Moody 2001; Mouw and Entwisle 2006). Recently, political identity has emerged as a significant source of social cleavage, along with the recent rise of political polarization in the United States. While racial residential segregation has declined¹, the American electorate has become more geographically polarized over the past decades across states and across counties (Bishop 2009). Given the rising political polarization and isolation of social networks in the United States (Lee and Bearman 2020), it is critical to examine how the salience of partisanship and politicization processes shape our strong-tied social relationships.

The presence of political disagreement in discussion networks provides a relational foundation for political deliberation. Exposure to disagreement contributes to people's ability to articulate reasons why others might disagree with their own views (Price et al. 2002). It is because a successful deliberation is cultivated by mutual respects, which enable citizens to resolve social conflicts through free and equal exchanges, to invite arguments for all sides, and to make collective decisions in light of debate and discussion (Habermas 1984). Voluminous literature shows that large and politically diverse networks enhance political deliberation and engagement (Huckfeldt et al. 2004; Jang 2009; Mutz 2002a, 2002b; Pattie and Johnston 2008). This paper investigates structural conditions that enable the maintenance of large and politically diverse networks with focus on a context of heightened political participation and partisanship—close elections.

Public attention to politics escalates during times of close elections, since additional votes matter significantly (Downs 1957; Shachar and Nalebuff 1999). Contrary to the belief that close elections are rare, Fraga and Hersh (2018) find that in the four-cycle period from 2006 to 2012, approximately 90 percent of Americans had experienced a highly competitive election at least one time. The presidential campaign and mass media focus their attention on states whose election results are expected to be close (e.g., battleground states). Due to the increased amount of political information and media/campaign attention, citizens become more familiar with the candidates and salient issues in those pivotal states (Gimpel et al. 2007; Lipsitz 2009).

Heightened politicization in the battleground states enhances voters' political engagement in various ways, including increasing voter turnout (Gerber et al. 2009), enabling higher attendance at candidate meetings (Lipsitz 2009), and encouraging more Facebook status updates alluding to political issues or positions (Settle et al. 2016). This enhanced political engagement holds the possibility of enabling political communication across the partisan divide. Yet it also may lead to increases in biased perceptions and animosity against the opposition party, a phenomenon Iyengar et al. (2012) call *affective* polarization. According to their research on the 2004 and 2008 presidential elections, affective polarization was escalated in the battleground states. The intensified partisan animus may lead to deactivation of cross-cutting social ties as spaces for disclosing political views: people instead favored disclosing political views only to trusted confidants with similar political views (Gerber et al. 2013).

Yet few studies empirically examine how heightened politicization influences the structure of social networks.

How do competitive electoral environments shape our discussion of important political matters within our close relationships? On the one hand, social networks may become large and politically heterogeneous in such environments due to heightened levels of political engagement, which fuel people's need to persuade others of their own opinions. On the other hand, people may avoid potential conflicts arising from exposure to political dissent, and consequently may become politically isolated in their network environment. This paper adjudicates between these two possibilities using nationally representative survey data that contain egocentric political discussion networks during the 1992, 2000, 2008, and 2016 presidential election campaigns.

To anticipate the main findings, I show that political isolation in interpersonal networks decreases in states experiencing close elections (i.e., the two-party vote share is less than 5 percent). Yet exposure to political dissent in discussion networks also decreases during close elections. In other words, these results indicate that the elevated attention to politics may increase the pressure and opportunity for political conversation but likely only within people's interpersonal echo chamber. In addition, I find that people are less likely to discuss politics with relatives than nonrelatives during close election cycles, which suggests the possibility that political discourse can become more conflictual and intense when activated among strong ties than among weak ties.

I further examine two potential mechanisms for politicization effects. First, the salience of politics can shift the meaning people attach to political discourses (Duncan and Stewart 2007). Politics becomes personal, and the conflict arising during political discourse is likely to escalate to bigger issues, potentially harming existing relationships. In this case, political diversity in one's networks can deactivate simply due to the sheer increase of attention to politics. In contrast, the divisive content of political campaigns and advertising may change the way in which people think and talk about politics (Flores 2018; Huber and Arceneaux 2007). According to this view, it is not just exposure to politics, but the attacking tone of political ads that would increase animosity between opposing partisans and deactivate social ties between them. Combining with data on political ads aired during three election cycles since 2000, I find that cross-cutting exposure is more likely to decrease if more political ads are aired during a presidential campaign.

To investigate the implications of close election effects on personal relationships, I focus on mobility patterns during Thanksgiving holidays that immediately follow the 2016 presidential election, where family members living apart often gather together and discuss important matters in their lives. Based on smartphone location tracking data that allow them to measure the duration of Thanksgiving dinners, Chen and Rohla (2018) have reported that families gathering from across opposing-party precincts spend significantly less time dining together than do families that come from same-party precincts. I extend their findings by showing that close election effects likely drive these effects, and also that Americans are less likely to travel to see their family that resides in close

election states. These results suggest that the disruptive role of partisanship in shaping social interaction is more salient where politics are anticipated to appear as a “hot” topic at the family dinner table due to the contestation of current election outcomes.

The Activation of Political Discussion Networks in Election Contexts

Political discussion networks and core discussion networks both consist of individuals that are connected through various types of relationships, such as family, friends, and coworkers. While there is significant overlap with regard to the size and composition of networks engaging core and political topics (Klofstad et al. 2009), politics differs from everyday topics like restaurants, local events, and sports because it entails a sense of morality and one’s social identity (Campbell et al. 1960; Eliasoph 1998). Since talking about politics carries the risk of ruining a relationship, people selectively disclose their political leanings and opinions to others (Cowan and Baldassarri 2018). There is, however, an intense social moment when it is not easy to avoid talking politics: during election campaigns.

When political events are front and center, people are more likely to discuss politics. Using data from the 1995 Indianapolis-St. Louis Study, Huckfeldt et al. (2002) show that political campaigns make voters engage with the election and discuss politics with a larger number of people. In addition, they find that campaign events do not change to whom people talk about politics, although political campaigns may increase social pressures for political conformity. Their findings on the significant amount of political disagreement during politically charged periods were the prelude to a now-robust literature on the impact of cross-cutting exposure in social networks on political behaviors (Bail et al. 2018; Baldassarri and Bearman 2007; Beck 2002; Fowler et al. 2011; Lim 2008; Sinclair 2012).

How can political disagreement survive in strong-tied networks? The presence of political diversity in networks can stem from the multiplexity of social ties; people are tied to others through multiple types of relationships, which might form at different times and in different spaces (Padgett and Ansell 1993). People can hold divergent viewpoints within their multiplex networks because every opinion is filtered through every other’s opinion (Huckfeldt et al. 2004). Political conversations arise from tangible social relationships that form primarily based on people’s shared, nonpolitical characteristics such as socioeconomic status and demographics (Sinclair 2012). Partisanship is likely to be correlated with these characteristics, thus likely to be shared, but does not fully determine the formation of social networks.

The attitudinal heterogeneity in dynamic networks also depends on the distribution of discussion topics. Network segregation emerges when a single issue dominates interpersonal communication (Baldassarri and Bearman 2007). What people talk about is strongly conditioned by what motivates them to talk in the first place. Everyday political talk is a communicative action that can be guided by instrumental and/or dialogic motives (Habermas 1984;

Kim and Kim 2008). Political discussion for sophisticated citizens arises from certain political desires, such as gaining political information, expressing their positions on particular issues, and persuading others. In addition to these instrumental motives, ordinary citizens are often motivated to learn about the lives of others and to better understand different perspectives (Conover et al. 2002). Through political dialogs, people not only form their own political opinions, but also produce the concept of “we” that enables a sense of community in their daily lives (Gamson 1992).

Close Election Effects

How do these instrumental and dialogic motivations affect the structure of political discussion networks? On the one hand, as the likelihood that additional votes can swing the election increases, people can be encouraged to speak up due to the instrumental motive. The increasing salience of politics in close elections can make people with the instrumental motive more likely talk to nonpartisan voters who had not formed their opinion yet so as to enable wins by tiny margins. On the other hand, when an election is expected to be close, political debates become more intense. People driven by the dialogic motive might be more reluctant to talk to those with whom they disagree. Political discourse often involves intense moments of contention, causing emotional distress (Frimer et al. 2017). Political context gets under the skin of our social relationships. So, people become willing to talk more about politics, but only with those that share their perspectives.

In this paper, I hypothesize that close elections may influence the size as well as the heterogeneity of political discussion networks in four different ways. First, the heightened level of interest in politics during the presidential campaign seasons and voters’ perception of the “pivotalness” of their vote can facilitate political conversation in general, in turn leading to network expansion (i.e., the increase of network size). Second, larger networks will consist of a mixture of “independent” voters and opposing partisans, especially if party supporters actively persuade using their multiplex and underutilized social ties. Cross-cutting political ties in one’s close social networks then may be temporally activated in close elections (i.e., the decrease of political homophily).

Exposure to dissenting views, however, may backfire (Bail et al. 2018). A sharp increase of state-wide attention to politics may let ordinary citizens see opposing views, to which they usually would not be exposed due to selective disclosure. This exposure in turn may allow citizens to be better equipped to identify the line dividing political friends and enemies, causing them to more selectively talk to like-minded people whose political views are certain, safe, and akin to their own (Cowan and Baldassarri 2018). As the distinction between “us” and “them” grows clearer and more important, the emotional distance between partisan groups may increase (Iyengar and Krupenkin 2018; Iyengar and Westwood 2015). Consequently, maintaining social ties with those holding disparate political views may be costly. The third hypothesis, as a result, is that the deactivation of politically conflictual ties will be a natural consequence of

close elections, especially when people give up talking to others with different views (i.e., the increase of political homophily).

It is not always the case that tie deactivation leads to the decrease of network size, since conflictual ties can be replaced by mutually agreeable social ties, but some are irreplaceable. Thus, the fourth hypothesis is that political isolation in strongly tied networks will occur alongside the rise of political homophily, since people may prefer to drop existing ties without finding others to replace them (i.e., the decrease of network size). There is also a null hypothesis that close elections do not affect the patterns of social interactions, especially in strong-tied networks. The null results can arise because people might not care about politics, even in situations when their state election results are thought to be contested. Likewise, the stability of strong-tied networks could imply that people discuss politics with the same confidants even in the politically charged periods surrounding close elections.

Two Mechanisms for the Deactivation of Strong and Conflictual Ties in Close Elections

What people talk about shapes who they talk to, but what they talk about also is strongly constrained by when they talk (Baldassarri and Bearman 2007). People are less likely to disclose their opinions on sensitive issues to their neighbors compared to their families (Cowan and Baldassarri 2018). When discussing politics is not controversial, usually because of a low-stakes political environment, then it is not very difficult for individuals to disclose their political views to those in their networks. Through these conversations, people may discern political differences between themselves and their close confidants. As the salience of political identity increases in politicized environments, political conversation can become personal and perhaps even offensive. In such settings, any existing awareness of political tension with close confidants is more likely to deactivate strong ties (e.g., families) than weak ties (e.g., neighbors), considering that political conversation is more likely to take place in strongly tied networks when politics dominates casual conversations.

Election competitiveness elevates the level of public attention to politics, making it hard for the public to avoid exposure to political news and information. The increasing salience of politics in close elections changes the way in which people attach meaning to their political identity, as well as to political disagreements that they have with their significant others. Strong ties are strong since they are deeply embedded in multiple social contexts, but strong ties demand more commitment (Coleman 1988). To the extent that identity shapes and is shaped by social interactions through strong-tied relationships, identity conflicts beget relational conflicts. The increasing salience of political identity induced by electoral competition magnifies the small difference in political views with strong confidants and amplifies the role of partisanship in the activation of social ties. This political salience mechanism suggests that it is the sheer increase in the amount of political information that can deactivate potentially conflictual ties that contain opposing views.

In contrast, it can be precisely the divisive nature of political campaigns that may change the ways that people think and talk about politics and relationships (Flores 2018; Huber and Arceneaux 2007; Lau et al. 2007). Those who attach negative emotions to opposing camps because of divisive political ads may refuse to talk to significant others who disagree. According to this negative campaign mechanism, it is not just the amount of exposure to politics, but also political ads that attack other political camps, which will trigger anger between political parties. This process has downstream effects on the public, making people deactivate interpersonal social ties with opposing partisans. I will use the volume and tone of political advertisement aired across states to evaluate these two mechanisms.

Data and Method

This paper aims to investigate the impact of close elections on how people activate their intimate social ties during presidential election seasons using nationally representative survey data on egocentric networks. The egocentric network is particularly useful in identifying the complete census of strong-tied networks across different relationship types (Perry et al. 2018). I combine all publicly available national data collected during four presidential election cycles—the US data from the 1992 Cross National Election Studies (CNES-US), the 2000 American National Election Studies (ANES), the 9th wave of 2008–2009 ANES panel study and the 2016 Time Sharing Experiment for Social Sciences (TESS) data—and merge them with state-level presidential election results. The 1992, 2000, and 2008 National Election Studies have been widely used to study how Americans discuss politics, and the consequences of politics on public opinion, voting, social distance (Djupe et al. 2018; Kloffstad et al. 2009; Lyons and Sokhey 2017). The 2016 TESS data arose from a nationally representative survey experiment during the 2016 US presidential election campaign to understand who Americans talk to, and what they talk about, during election campaigns (Lee and Bearman 2020).

All data sets employ similar network name generators to collect the information about political discussion networks, though the mode of data collection² as well as some other network information is not exactly the same (see Appendix A1). Unlike other surveys using “political matters” name generators, the 1992 CNES survey collected the network information using the “important matters” name generator while simultaneously asking how much people discuss politics with their core discussion partners³. I consider the subset of core discussion networks by following Kloffstad et al. (2009)’s strategy that defines the “talk politics subnetwork” by leaving out all discussants with whom a respondent reported no political discussion. I also include additional “political” discussion partners elicited by respondents in the 1992 CNES data after providing up to four names in the important matters name generators. To address these discrepancies, I measure key network variables in a way that maximizes comparability.

Network Measures

Since the network name generator is designed to capture the “core” part of social networks, which often comprise strong and close relationships, standard name generators tend to undercount the true size of political discussion networks (Eveland et al. 2013). However, the goal of this paper is to identify the impact of close elections on the patterns of political discussion within strong and close relationships, instead of enabling the comprehensive measurement of general political discussion networks. In this context, a theoretical maximum of network size at four and six across different surveys can mask important variation at the tail of the distribution. Thus, instead of using a network size indicator, I employ a binary indicator to capture *network isolation* in political discussion networks, which takes the value of one if respondents do not name any person in network name generators⁴.

Exposure to opposing views can be measured either by the amount of cross-cutting exposure (Mutz 2006) or the presence of difference (Huckfeldt et al. 2004). Eveland et al. (2018) show that Americans are actually more likely to be exposed to opposing views than the extent captured by standard network name generators, though hidden exposure largely arises from weak ties. Using the number of alters who had different views is likely to underestimate the amount of cross-cutting exposure. To account for the potential bias, I identify the presence of exposure to political differences in ego’s strong-tied networks by comparing respondents’ and alters’ political positions. Specifically, since the 1992 CNES and 2000 ANES surveys were conducted right after the presidential election, respondents were asked to report to whom their discussion partner voted in the presidential election⁵. After classifying the response into four categories, (1) Republican candidates (e.g., Bush), (2) Democratic candidates (e.g., Clinton, Gore), (3) Third-party candidates (e.g., Perot), and (4) do not vote/ineligible to vote, *cross-cutting exposure* is identified if a respondent has at least one discussant who votes for different candidates. Since the 2008 ANES data and the 2016 TESS data were collected before the presidential election in September, I instead use party identification. In line with the previous strategy used in the 1992 and 2000 data, I group seven-scale party identification into four categories; (1) Republican (strong/weak), (2) Democrat (strong/weak), (3) something else, (4) Independent (including leaning toward), and identify *cross-cutting exposure* if a respondent has at least one discussant who has a different party identification⁶.

I measure two other network indicators to explicate mechanisms for close elections effects. The first indicator is talking to others in the middle of the political spectrum. If American voters are motivated to influence the election in some ways when it is thought to be close, they are likely to persuade nonvoters or nonpartisans who are more amenable to change. I measure *talking to the middle* by checking whether an ego’s network contains any nonvoter in 1992 and 2000 or any nonpartisan who is identified as independents in 2008 and 2016 in egocentric networks, treating the third-party candidate voting and something else in party identification as a distinctive partisan identity. In addition, I measure

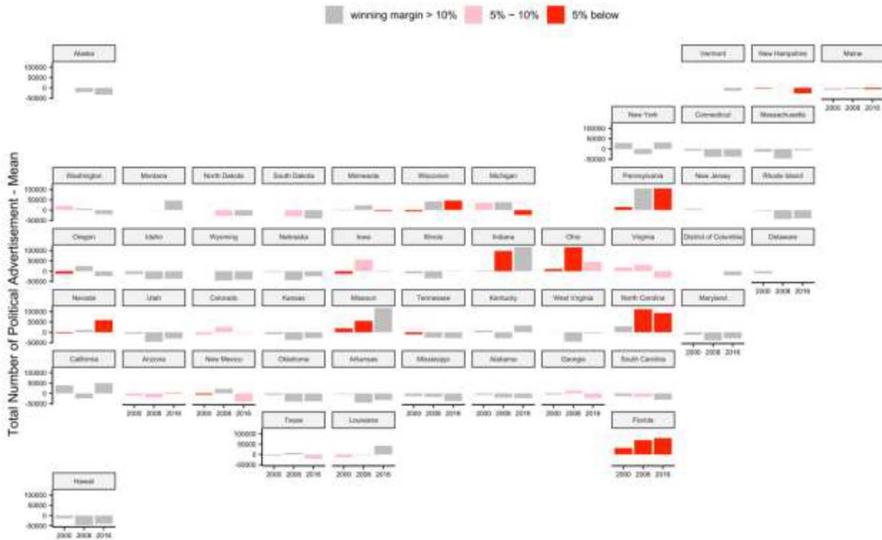
talking to relatives by identifying whether the network includes any discussant who is identified as a relative.

Close Elections and Political Advertising

Following the literature, I define a state-level close election indicator directly based on the two-party vote shares of state-level presidential elections (Fraga and Hersh 2018; Geys 2006; Iyengar et al. 2012). Using data from Dave Leip's Atlas of US presidential elections database⁷, I identify a close election if the winning margin is less than 5 percentage points⁸. I also check the sensitivity of results by employing a continuous indicator⁹. Only presidential elections are considered. Table S1 in the Appendix shows the distribution of close election states across different surveys. Among fifty-one US states, twenty-two states are classified at least once as experiencing a close election during the sample period, though the remaining twenty-nine states had never experienced close elections.

I obtain data on the frequency and tone of political advertising during 2000, 2008, and 2016 presidential campaigns, which were made available through an agreement between the Campaign Media Analysis Group (CMAG) and the Wesleyan Media and Wisconsin Advertising Projects (Fowler et al. 2019; Goldstein et al. 2002, 2011). According to CMAG, the tracking data represent the most comprehensive and systematic collection on the content and targeting of political advertisements. I used all currently available political ads aired for presidential, governor, house, and senate elections in 2000, 2008, and 2016¹⁰. The database includes exact dates, and the place of political ads aired during each campaign season, which allows me to aggregate the total number of political ads aired at state levels. I measure daily counts as well as total counts in the entire campaign period and merge them based on interview dates in the survey. I also employ the variable (*ad_tone*) to measure the tone of political advertisement coded by the Wesleyan Media project researchers using the question, "In your judgment, is the primary purpose of the ad to promote a specific candidate, attack a candidate, or contrast the candidates?" I calculate the proportion of political ads with the attack, contrast, and promote tone, respectively.

Figure 1 shows the distribution of the total volume of political ads aired during three presidential elections by close election status. Although there are some exceptions (e.g., Indiana and Missouri in 2016, and Pennsylvania in 2008), political ads were more likely to be aired in states with close election margins. Figure S1 in the Appendix shows that states with close elections defined by the 5 percent winning margin are more likely to air political ads with an attack tone in 2008 and 2016, though there are no significant differences by close election status in 2000. In sum, close elections are characterized by high levels of political exposure and information (e.g., the frequency of political ads) and simultaneously by increased animosity between opposing parties (e.g., the tone of political ads). I use both measures to examine which mechanisms are more salient for political activation or deactivation of strong-tied networks.

Figure 1. The distribution of political advertisement across states by close election status.

NOTE: The height of each bar shows the total number of political advertisement, which is mean-centered in each year, for within-year comparison. Bars are colored by close election status of states.

Analytic Strategy

This study utilizes timing differences in close elections at the state level across four different US presidential elections to identify the “close election effect”. One of the crucial reasons for my focus on state-level variations is to capture how political leaders, political pundits, and laypeople perceive and evaluate the political landscape and campaign dynamics—issues difficult to observe at the county- or neighborhood-level using zip codes (Huber and Arceneaux 2007; Krasno and Green 2008). Ideally, we would like to compare a resident living in a state with close presidential election margins in a given year (e.g., 1992) to the same resident living in a state with a landslide win at the same time. This counterfactual is not observable. Instead, by employing state and year fixed effect models, we can compare average residents living in a state that had a close election in 1992 with those who lived in the same state when an election was not close in 2000. This identification strategy has been used as a natural experiment to instrument political campaign effects on voter turnout (Gerber et al. 2009; Krasno and Green 2008).

I employ logistic regression models for network isolation, the presence of exposure to opposing views, talking to the middle, and talking to relatives. Across all models, I adjust survey weights and cluster the standard error by states. To tighten the estimates, I include several pretreatment covariates known to be associated with the choice of political discussion partners: respondents’ gender, age, race, years of education, marital status, and working status. In addition, I

control for some potentially posttreatment covariates such as the overall level of political interests and partisan strength that may affect political network size and political heterogeneity (Mason 2015). I measure partisan strength using the seven-scale party identification indicator (0 = independent, 1 = leaning toward Democrats/Republicans, 2 = Weak Democrats/Republicans, 3 = Strong Democrats/Republicans), and political interests using a similar version of “how interested would you say you personally are in politics?” (see Appendix Panel A2 for the exact wordings) I use continuous scales for both measures. I present average marginal effects (i.e., probability changes) in the main manuscript for easier and more meaningful interpretation, and attach corresponding logistic regression tables in log-odds units in the Appendix.

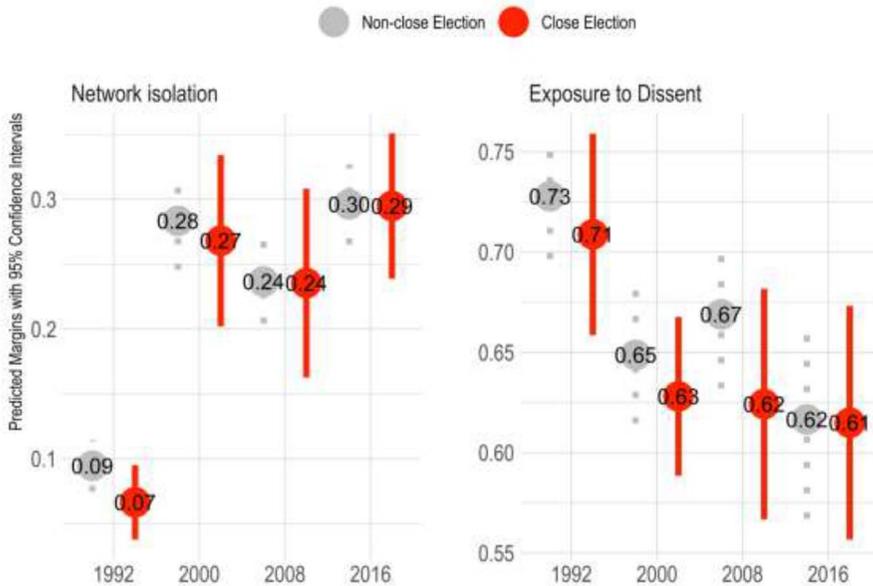
Table S2 in the Appendix shows descriptive statistics of individual demographics, network measures, and state-level measures. Demographic characteristics are roughly similar across different surveys including the level of political interest and partisan intensity. The sharp decrease in network size and cross-cutting exposure in 2016 is reported and explained by Lee and Bearman (2020). The proportion of respondents living in close election states (i.e., winning margin < 5 percent) is about 25–30 percent except for the 2008 sample. The lower probability (17 percent) in the 2008 ANES data might reflect that Obama would go on to win a decisive victory over McCain, winning both the popular vote and the electoral college. As shown in Figure 1, the frequency of political ads had been continuously rising since 2000, though the proportion of political ads with an attack tone has fluctuated over time.

Results

I first examine how election closeness across four presidential elections is associated with network isolation and cross-cutting exposure. After estimating logistic models after accounting for basic demographic characteristics, I plot the predicted margins (i.e., probability changes) with 95 percent confidence intervals for network isolation and exposure to difference across close and non-close elections (colored by red and black, respectively) from 1992 to 2016. Figure 2 shows that both network isolation and exposure to opposing views are consistently smaller in close elections than non-close elections across all surveys. Since the overlap of confidence intervals across close and non-close elections in each year may reflect lack of statistical power to detect the close election effect, I combine four surveys for all regression models.

The decrease of network isolation and exposure to difference in close elections might arise because some states like Florida have constantly played a significant role in presidential elections as the “swingiest” swing states. Then, for example, politically homogeneous networks in close elections are activated not because of close election *per se*, but because of other state-specific factors. The pattern also could be driven by some political events such as very close primary results in the spring that lead to an overall increase in political interest, which in turn may affect political discussion networks as well as the winning margins in the fall. To address these issues, I employ state-year fixed effects models that exploit the

Figure 2. Marginal effects for network isolation and cross-cutting exposure between close and non-close elections from ego-centric network surveys from 1992 to 2016.



NOTE: The predicted margins with 95% confidence intervals are computed based on logistic regression models that account for respondents' age, gender, race, years of education, marital status, and working status in each year.

temporal variation within states across elections, and further control for some potential posttreatment variables capturing political interests.

Table 1 shows the average marginal effects of close elections from estimating logit models on network isolation and exposure to opposing views. Model 1 with demographic controls indicates that close elections decrease the number of people who report that they have no one with whom to discuss politics by about 5.4 percentage points; this value shrinks in Model 2 once controls for political interest are included. Model 3 shows that there is about 5.4 percentage point decrease in cross-cutting exposure in close elections as well. To put these numbers in context, I compare them against the effect sizes of known demographic effects. I find that the magnitude of the close election effect is roughly similar to the effect of gender (i.e., men were more likely to be exposed to different views than women), and four times larger than the effects of a one-year increase in education (also see Table S3 in Appendix for the full regression table). Given that one standard deviation in variation in cross-cutting exposure across state-years is 0.19, the close election effect explains a quarter of a standard deviation change in cross-cutting exposure across states. Model 4 shows that the effect is stable against controlling for political interest and partisan strength, which suggests that the close election effect does not arise merely from increased political interest or strengthened partisanship.

Table 1. Marginal effects of close elections on network isolation and cross-cutting exposure

	Network isolation		Cross-cutting exposure	
	Model 1	Model 2	Model 3	Model 4
Close election	-0.054 ⁺	-0.047 ⁺	-0.054 [*]	-0.054 [*]
	(0.029)	(0.027)	(0.024)	(0.022)
N	5,824	5,745	4,725	4,690
State-year FE	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
Political controls	No	Yes	No	Yes

NOTE: All logistic regression models are estimated with state and year fixed-effects. A list of baseline controls includes respondents' age, gender, race, years of education, marital status, and working status. A list of political controls includes respondents' political interests and partisan strength. Standard errors clustered by states are in parenthesis ($+p < 0.1$, $*p < 0.05$). See Table S3 in Appendix for a full regression table.

Next, I investigate the likelihood of talking to the middle and talking to relatives. Models 1 and 2 in Table 2 show that people are less likely to reach out and speak to those who are in the middle of the political spectrum during close elections. While the coefficient is not statistically significant, it becomes marginally significant at $p < 0.1$ after accounting for the influence of political interests in Model 2. Finally, Model 3 shows that people discuss politics through nonkin ties more than through relative ties in close elections, and Model 4 shows again that this result is not merely due to the increase of political engagement induced by close elections. These modest decreases in talking to the middle and talking to relatives, in the order of approximately 5 percentage points, may suggest that it is likely that people discuss politics for the dialogic reason rather than the instrumental reason in close elections, though more research is needed to identify the precise mechanism.

Next, I examine two potential pathways by which close elections precipitate the evaporation of opposing views in one's social networks, the political salience mechanism and the negative campaign mechanism. Table 3 shows results from estimating logistic regression models on network isolation (Model 1 and 2) and cross-cutting exposure (Model 3 and 4) after controlling for baseline demographics as well as political interest. Both results are produced by matching political ad data with survey data by using the exact interview dates (Model 1 and 3) and considering a whole season (Model 2 and 4). Models 1 and 2 show that the effects of political advertisement on network isolation are statistically insignificant, showing opposite signs depending on the matching strategy. In contrast, Models 3 and 4 show that the standardized frequency of political ads is likely to reduce cross-cutting exposure, while the regression coefficients for the tone of ads are not statistically distinguishable from zero. Specifically, a

Table 2. Marginal effects of close elections on talking to the middle and relatives

	Talking to the middle		Talking to relatives	
	Model 1	Model 2	Model 3	Model 4
Close election	-0.049	-0.049 ⁺	-0.049 ⁺	-0.052 ⁺
	(0.031)	(0.029)	(0.028)	(0.028)
N	4,419	4,390	4,570	4,537
State-year FE	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
Political controls	No	Yes	No	Yes

NOTE: All logistic regression models are estimated with state and year fixed-effects. A list of baseline controls includes respondents' age, gender, race, years of education, marital status, and working status. A list of political controls includes respondents' political interests and partisan strength. Standard errors clustered by states are in parenthesis (+ $p < 0.1$, * $p < 0.05$). See Table S4 in Appendix for a full regression table.

Table 3. Marginal effects of political advertisement on network isolation and cross-cutting exposure

	Network isolation		Cross-cutting exposure	
	Model 1	Model 2	Model 3	Model 4
Frequency of Political Ads	0.005	-0.018	-0.037*	-0.024 ⁺
	(0.013)	(0.019)	(0.006)	(0.014)
P(Ad tone = Attack)	-0.018	0.021	0.033	0.005
	(0.036)	(0.074)	(0.041)	(0.075)
N	4,488	4,497	3,545	3,551
State-year FE	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes
Political controls	Yes	Yes	Yes	Yes
Daily matching	Yes	No	Yes	No

NOTE: All logistic regression models are estimated with state and year fixed-effects. The frequency of political Ads is standardized. A list of baseline controls includes respondents' age, gender, race, years of education, marital status, and working status. A list of political controls includes respondents' political interests and partisan strength. The data are merged with political ads database based on the actual interview dates (daily matching = Yes) or the whole period (daily matching = No). Standard errors clustered by states are in parenthesis (+ $p < 0.1$, * $p < 0.05$). See Table S5 in Appendix for a full regression table.

one standard deviation increase in the frequency of political ads leads to a 3.7 percentage point decrease in cross-cutting exposure in the daily matching sample, and a 2.4 percentage point decrease in the whole matching sample.

I further examine the robustness of findings on the decrease in network isolation and cross-cutting exposure in close elections. In [Table S6](#), Models 1 and 4 reprint the close election effects on network isolation and cross-cutting exposure as a reference. I first examine the potential endogeneity concern for close election status by controlling for the lagged close election state measure. I obtain similar estimates in Models 2 and 5. Another concern for the close election indicator is an arbitrary threshold as 5 percent. Models 3 and 6 show that the coefficients on close election effects on both cross-cutting exposure and network isolation become larger if we use the continuous indicator, though the direct comparison is not meaningful since a one-unit increase of the continuous indicator implies the maximal change (i.e., a 100 percent difference to zero difference). The close election effect on network isolation is not robust against this alternative specification. Finally, I consider leaners (i.e., leaning toward Republicans or Democrats) as partisans instead of independents, and estimate the same model. Model 7 shows that the close election effect is robust against the alternative definition of cross-cutting exposure. In sum, the core findings on cross-cutting exposure (but not network isolation) are likely robust against alternative specifications of measures on both sides of the regression equation.

Family Bonding Time during Thanksgiving, 2016

What are the implications of the finding that people discuss politics less with their relatives during close and intense elections? Presidential elections are always followed by Thanksgiving, a time when family members who may live apart gather together and spend meaningful time together. I further examine the consequences of politicization on family bonding time using publicly available large-scale smartphone location data during the 2016 Thanksgiving Day, collected by [Chen and Rohla \(2018\)](#). Although it is impossible to know precisely what people talk about and how they feel during those conversations from the location data alone, it is likely that the conversations two weeks after the historically divisive 2016 presidential election may include politics, especially during Thanksgiving dinners held in states where the election results were close¹¹.

To examine the relationship between political disagreement and time expenditure on Thanksgiving Day, [Chen and Rohla \(2018\)](#) combined anonymized smartphone location data from more than 10 million Americans with a precinct-level database for the 2016 election, capturing 77 percent of Americans who own smartphones and proving politically representative of the American electorate as a whole. The home location was identified on the basis of users' pings between 1:00 am and 4:00 am over the three weeks before Thanksgiving, and the same users' Thanksgiving locations were identified based on their modal location between 1:00 pm and 5:00 pm until two days after Thanksgiving Day. Chen and Rohla measured the duration of Thanksgiving dinner as the minutes each traveler spent in the Thanksgiving location, and political disagreement by the probability of political mismatch, that is, the difference of imputed two-party vote shares associated with home and travel precincts in 2016. One of [Chen](#)

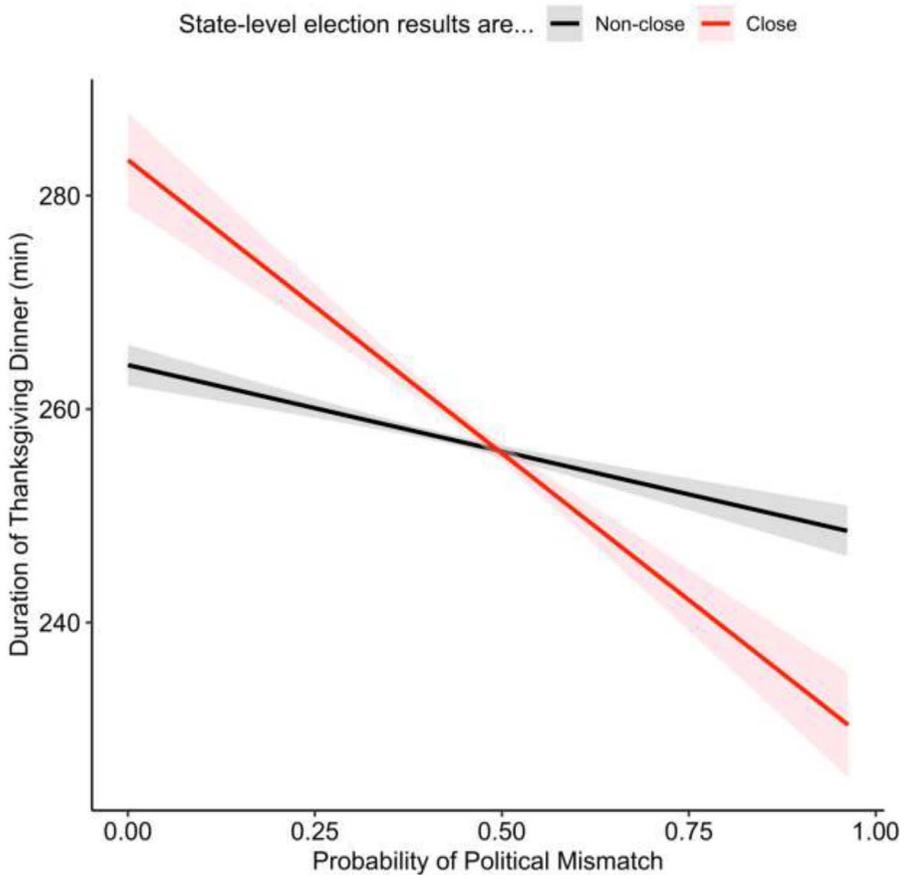
and Rohla (2018)'s key findings was that the duration of Thanksgiving dinners attended by residents from opposing-party precincts was thirty to fifty minutes shorter than same-party dinners. Results from my analysis of egocentric network data suggest the possibility that their findings were actually driven by a "close election effect."

Closely following Chen and Rohla (2018)'s analytical strategy, I restrict samples to residents who were home both in the morning and during the night of Thanksgiving, but who traveled for Thanksgiving dinner. I further exclude a small number of residents (3.7 percent) who had out-of-state travels to avoid complications arising from the mismatch of close election status in their home and travel locations. In Figure 3, the red and black lines show the predicted regression curves without any controls among residents in close election states, and non-close election states, respectively, showing the steeper slope for close election states than that for non-close election states. This pattern suggests that Chen and Rohla (2018)'s previous finding on the effect of partisan disagreement on the duration of Thanksgiving dinner is stronger in the context of close elections.

Table 4 shows results after accounting for various potential confounders (i.e., the number of political ads and other block and tract-level demographic controls in-home location). Model 1 shows that residents from completely opposing partisan precincts (from 0 percent to 100 percent) spent 17.75 fewer minutes on Thanksgiving dinners than those from the same-partisan precincts, whereas residents in states with close elections spent a little longer (1.7 minutes). Estimating the model separately in states with non-close and close elections, I find that the effect of political mismatch is smaller in non-close election states than close election states (in Model 2 and 3). Model 4 in Table 4 confirms that the difference in political mismatch effects is substantively large even after controlling for the volume of political advertisement. Specifically, families from opposing-party precincts spent about forty fewer minutes than same-partisan diners in close election states compared to the duration reduction in non-close election states. I further show that this result is robust against the unobserved home-destination heterogeneity by constructing comparison sets of smartphone users that share the same home-destination county pairs (in Model 5). Due to the nature of county fixed effects in Model 5, the main effect of dining in a close election state is not identified, but I can compare the difference between two home-destination pairs with different levels of political mismatch within the same county-pair. Under this very strict test, families who come from opposing precincts spent about fourteen fewer minutes than the same-partisan families residing in the same county.

The analysis so far has considered Americans who left home early on Thanksgiving Day and came back home later on the same day. Among about 2.8 million people in the sample who were at home early in the day, about 53 percent stayed home altogether instead of traveling on Thanksgiving Day. It is possible, of course, that some of them were ill or lacked the financial resources to visit their families; some of them also may have lived in the same neighborhood; some of them also host Thanksgiving dinner at their own houses. That said, the

Figure 3. Bivariate relationship between the probability of political mismatch between home and Thanksgiving destination precincts and the duration of Thanksgiving Dinner in 2016 by close election status.



NOTE: Each line shows the fitted line from the bivariate linear regression of the duration of Thanksgiving dinner on the probability of political mismatch with 95% interval for close election states (red) and non-close election states (black). The probability of political mismatch is defined by the imputed probability that the two-party vote shares associated with home and travel precincts differ in 2016 (i.e., $PM_{ij} = P_i(1 - P_j) + (1 - P_i)P_j$, where $P_i = (\text{dem}_i) / (\text{dem}_i + \text{rep}_i)$ for each precinct i).

findings so far imply that some of them may not have left home to see their families if they anticipated some severe political conflicts over Thanksgiving dinner. Since political conversation is more likely to be politicized and polarized in close elections than non-close elections, the context of close elections may reduce the likelihood of traveling to see one's family. I employ linear probability models and find that close elections increase the probability of staying home by about 1.6 percentage points in Model 1 in Table 5, which affects about

Table 4. The effects of political mismatch on the duration of Thanksgiving dinner in 2016 by close election status

	All	Non-close	close	All	
	Model 1	Model 2	Model 3	Model 4	Model 5
Probability of political mismatch	-17.75**	-14.03**	-40.13**	-11.61**	-24.85**
	(2.41)	(2.65)	(5.94)	(2.61)	(3.37)
Close election state	1.71**			19.72**	0.00
	(0.61)			(2.94)	(.)
Standardized number of political ads	0.11	1.90**	0.23	0.10	
	(0.25)	(0.39)	(0.36)	(0.25)	
Probability of political mismatch X close election state				-39.72**	-13.77 ⁺
				(6.22)	(7.51)
N	617,911	461,726	156,185	617,911	607,295
Block/track-level controls	Yes	Yes	Yes	Yes	Yes
Dyadic FE	No	No	No	No	County pair

NOTE: I estimate OLS regression models with standard errors clustered at the precinct-cross-precinct level in parenthesis ($+p < 0.1$, $*p < 0.05$, $**p < 0.01$). A list of block/track level controls include percent white, percent black, percent Hispanic, percent Asian, percent unemployed, average commute time at the block level and percent foreigner, median age, urbanicity, medium household income at the tract levels. Dyadic fixed effects control for an individual's home location-Thanksgiving destination county pairs.

45,000 people. The effect is robust after controlling for a host of block/track level measures (in Model 2), as well as the volume of political advertisement (in Model 3). Model 4 shows that the effect of political advertisement is significantly larger in close election states, which suggests that the close election effect and the effect of political advertisement are complementary.

Discussion

Rising political polarization has deepened not only the ideological divide between Democrats and Republicans, but also the social divide in American society (Baldassarri and Gelman 2008; Iyengar et al. 2019; Park 2018). A greater challenge arises from the decrease in exposure to opposing views in people's social surroundings, the expansion of political echo chambers, and the isolation of social networks. Combining four nationally representative egocentric network surveys with a political advertisement database, this paper shows that high politicization due to contested elections are associated with less cross-cutting exposure in strong-tied networks and less activation of kinship ties

Table 5. The effects of close elections on the decision to travel in 2016

	Staying home			
	Model 1	Model 2	Model 3	Model 4
Close election	0.016**	0.019**	0.008**	0.007**
	(0.001)	(0.001)	(0.001)	(0.001)
Standardized number of political ads			0.009**	0.006**
			(0.000)	(0.001)
Close election X standardized number of political ads				0.005**
				(0.001)
N	2,785,361	2,783,160	2,783,160	2,783,160
Block/track controls	No	Yes	Yes	Yes

NOTE: I estimate OLS regression models with standard errors clustered at the precinct-cross-precinct level in parenthesis ($+p < 0.1$, $*p < 0.05$, $**p < 0.01$). A list of block/track level controls include percent white, percent black, percent Hispanic, percent Asian, percent unemployed, average commute time at the block level and percent foreigner, median age, urbanicity, medium household income at the tract levels.

for political discussion. Using large-scale cell phone location data, I further show that close elections significantly reduce the likelihood of Thanksgiving travel for family gathering as well as the duration of Thanksgiving conversation between opposing partisan diners. Using multiple data sources, this paper illustrates a causal process through which strong-tied networks become both political and polarized.

Close elections can reduce cross-cutting exposure through at least two channels: the increasing salience of political identity and the divisive content of negative campaigns. The results show that the increased frequency of political ads is likely to reduce cross-cutting exposure, whereas the tone of political ads shows nonsignificant effects. One of the potential implications of this finding is that the politicization process can induce interpersonal echo-chambers without activating partisan animosity (c.f., Lau et al. 2007). Simultaneously, neither the tone nor the frequency of political ads aired at state levels is significantly associated with network isolation, which may suggest *airing* political advertisement per se is unlikely to make more people speak up about politics in their social networks. However, future work is needed to examine whether these findings are robust when we employ alternative measures of isolation and exposure to dissent in political discussion networks (e.g., Eveland et al. 2013) and more precise measures of *watching* political advertisement.

People's attention to the presidential election in states with close elections is greater than in other states. It is easier to observe political messages and news stories saturating social space—from the workplace, to Facebook, to the streets to the bars—in states with close elections. The heightened awareness of politics may encourage people to activate network ties to discuss politics. This paper shows that there is a modest decrease in isolation of political networks in close election contexts, which is broadly consistent with [Eveland et al. \(2013\)](#)'s finding on larger network size in battleground states. Although network size tends to correlate positively with political diversity, the findings on the simultaneous decrease of network isolation and cross-cutting exposure in close elections may imply the emergence of a new form of sociopolitical segregation in social networks.

There are several limitations of this work worth discussing. First, using network name generators cannot fully capture the whole spectrum of egocentric networks, since this method was initially designed to identify strong and intimate relationships, called “core discussion networks” ([Marsden 1987](#)). Specifically, a great deal of cross-cutting exposure is not captured by network name generators since many weak ties are unreported ([Eveland et al. 2013, 2018](#)). A similar issue potentially exists for network isolation given that some people who reported being isolated on the network name generator question said they talked about politics with their friends and families (as shown in [Figure S2](#)). If underreporting of network isolation and political diversity is systematically associated with close elections, using the network name generators may underestimate the effect of close elections. In a similar vein, although this paper focuses on the implication of close election contexts for strong relationships, future work is needed to examine the possibility that close elections increase cross-cutting exposure through weak ties.

Second, the analysis of egocentric network data rests on the experience of “egos” being in close elections, not on the experience of alters. Since the information on alters' location is not available, I cannot examine the impact of asymmetry of close election status between ego-alter pairs. If close elections have indirect effects through an alter, even if the ego does not live in a state experiencing close elections, analyses only considering an ego's location likely would generate underestimates rather than overestimates for the close election effect. Third, the analysis of cell phone data may suffer from the endogeneity problem. For example, those who have family conflicts might have moved to politically different neighborhoods (i.e., reverse causality). While I believe that close election status is relatively exogenous, we need more longitudinal research to tease out the cause and effect.

Using the cellphone location data, I show the behavioral consequence of polarization to family bonding times—the difference of time spent on Thanksgiving dinner in 2016 between the opposing partisan and the same partisan families is much larger in close election than non-close election contexts. The disruption of family bonding time due to close elections poses significant challenges to the cohesiveness of American families, as long as politics occupies a central position in our dialog and our identity ([Huber and Malhotra 2016](#)). Future

research is needed to examine whether this polarization effect on the family relationship is part of a long-term pattern of political polarization, or if it reflects the momentary disruption of the 2016 election.

Survey data and digital trace data have their own weaknesses. Survey respondents are often asked about abstract concepts and opinions without being provided information on detailed social contexts. While people's actions are strongly guided by social events before, during, or after surveys, the survey itself may be a less useful tool for capturing the role of social events in people's actions. In contrast, the digital trace data, or so-called "big data," are useful to capture how different social contexts, such as Thanksgiving, shape patterns of individual actions and interactions. That said, it is hard to know exactly how people think and express themselves in any given moment. In this paper, I utilize both nationally representative survey data and digital trace data with national coverage as supplementary sources, providing a unique opportunity to uncover the multifaceted social phenomenon.

How does the *network polarization* that I identified in this paper differ from *affective polarization*? Recall that political networks are a subset of social networks comprised of individuals that are strongly tied via multiplex relationships. These individuals are not simply the "Republicans" or "Democrats" who appear on TV, radio, and online social media, but rather are close confidants who we trust and feel connected to, and with whom we exchange social support in our everyday lives. Hating imaginary "Republicans" or "Democrats" is not the same as hating friends or neighbors who support the Republican or Democratic party, making it difficult to entirely deactivate one's social ties to opposing partisans. The deactivation of these strong ties from intimate interpersonal environments is likely to reduce the opportunity to learn from others who disagree and reinforce people's existing beliefs about imaginary opposing partisans. Network polarization deepens our perception of political polarization through this negative feedback loop (Lyons and Sokhey 2017). This paper's findings suggest that the rise of political polarization is inevitable if the current trends toward politicization continue in American society.

Notes

1. While race/ethnicity is one of the most salient and widely studied dimensions for social cohesion, recent studies show that residential segregation between different ethnic groups has continuously declined from 1970 to 2010 (Iceland and Sharp 2013; Iceland et al. 2014), partly due to the emergence of "global neighborhoods" where Hispanics and Asians are the pioneer integrators of previously all-white zones (Logan and Zhang 2010). Likewise, using data from the 1985 and 2004 General Social Surveys, Smith et al. (2014) show that cross-category contacts in race/ethnicity have increased in Americans' "important matters" discussion networks, though they attribute this as being driven by the change of population compositions.
2. The mode of data collection differs across different surveys; The 1992 CNES study employs telephone survey; the 2000 ANES study runs through

face-to-face interviews; the 2008 ANES and the 2016 TESS use internet-panel surveys.

3. As demonstrated by Lee and Bearman (2017), it is highly likely that survey respondents would frame “important matters” as political matters in the 1992 CNES data because the survey was asked about election campaign and presidential candidate’s right after the presidential election.
4. As the “important matters” name generator may generate overreporting of network isolation due to some methodological issues, such as respondent fatigues and interviewer biases (Brashears 2011; Fischer 2009; Paik and Sanchagrin 2013), it is possible that the political name generator also fails to elicit a complete list of political discussants in the strong-tied networks. I examine this possibility by contrasting a similar but more general question of political discussion frequency for those who said they had no one to discuss politics in the network name generator. Figure S2 in Appendix shows that about 85 percent of isolated individuals said that they talked about politics when they get together with their friends, relatives or fellow workers never and rarely in 2016, and 55 percent of them did not discuss politics at all with their families and friends in the past week in 2000. The pattern is exactly opposite for those who have at least one confidant. It is less dramatic in 2008, though the time frame of this survey question—“during a typical week”—may capture a general tendency for political discussion instead of invoking actual discussion partners.
5. In the 1992 CNES, respondents were asked: “Which candidate do you think [NAME] supported in the presidential election this year?” RESPONSE: < 1 > Bush < 2 > Clinton < 3 > Perot < 4 > other (specify) < 5 > Bush & Clinton < 6 > Bush & Perot < 7 > Clinton & Perot < 0 > none. In the 2000 ANES asked: “How do you think [NAME] voted in the 11/4 election? Do you think he/she voted for Al Gore, George Bush, some other candidate, or do you think [NAME] did not vote?” Response: <1> Al Gore, <3> George W Bush, <5> Some other candidate (Specify), <7> Didn’t vote, (8) Ineligible to vote.
6. Grouping “leaners” (those who lean toward Republican or Democrat) with partisans provides almost identical results (see Table S6 in the Appendix).
7. <https://uselectionatlas.org/>.
8. Technically, it is equal to $|N_{dem} - N_{rep}| / |N_{dem} + N_{rep}|$, where N_{dem} , N_{rep} are the total vote counts in a given state for Democratic and Republican candidates, respectively.
9. A reviewer raised a concern that using these cut-off points to define close election can be arbitrary and may throw away useful information above and below the threshold. Although I obtain the similar results by employing a continuous indicator (i.e., 1—the absolute difference of two-party vote shares), I mainly present results based on a binary indicator to account for the nonlinearity of close election effects.
10. The data on political ads for the 2016 Presidential election are currently not available to academic researchers, as of March 4, 2020.
11. According to the 2017 Marist Poll conducted by NPR and PBS News hour and the 2016 CNN/ORC’s international poll, about 60 percent of people said,

it is very likely or likely that politics will come up as a topic at their Thanksgiving dinner in 2016, which became slightly lower to 50 percent in 2017. If the difference between 2016 and 2017 can be explained by the heightened level of political interests due to the contentious presidential election in 2016, we could also expect that people discuss politics more in close election states where public attention to politics was more intense due to the close margin of election outcomes.

Supplementary Material

Supplementary material is available at *Social Forces* online, <http://sf.oxfordjournals.org/>.

About the Author

Byungkyu Lee is an Assistant Professor of Sociology at Indiana University—Bloomington. He studies network dynamics in the field of political sociology and medical sociology. He is currently using large-scale text and relational data from political forums on Facebook to identify structural conditions that foster cross-ideological interactions while maintaining community engagement. His other works use large-scale medical claims and other administrative data to explain the rise of “deaths of despair” in the United States.

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