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ORIGINAL ARTICLE

Political isolation in America†

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Abstract
This study documents historical trends of size and political diversity in Americans' discussion networks, which are often seen as important barometers of social and political health. Contrasting findings from data drawn out of a nationally representative survey experiment of 1,055 Americans during the contentious 2016 U.S. presidential election to data arising from 11 national data sets covering nearly three decades, we find that Americans' core networks are significantly smaller and more politically homogeneous than at any other period. Several methodological artifacts seem unlikely to account for the effect. We show that in this period, more than before, "important matters" were often framed as political matters, and that this association probably accounts for the smaller networks.

Keywords: egocentric network, political polarization, core discussion network, network size, political homophily, survey experiment

1. Introduction

In 1960, fewer than 5% of Americans reported that they would be displeased if their son or daughter married outside their political party. Fifty years later, roughly one in four people would be "upset" by a "cross-party" marriage and roughly half of all Republicans and one-third of all Democrats report that they would be "unhappy" if their son or daughter wanted to marry someone whose party affiliation was different than theirs (Iyengar et al., 2012). Political polarization which used to be simply ideological appears to have seeped into the fabric of everyday social and emotional life (Baldassarri & Bearman, 2007; Iyengar & Krupenkin, 2018; Iyengar & Westwood, 2015). In this article, we consider how rising political polarization shapes our interpersonal networks with respect to our discussions with others important to us about important and political matters.

The "important matters" name generator, which asks with whom people discuss matters that are important to them, has been widely used and instrumented by numerous studies to map core discussion networks and measure the flow of information and support through social ties. Specifically, decades of research using data from the General Social Survey (GSS) and the National Social Life, Health and Aging project show that egocentric networks collected from the important matters name generators are strongly tied to various individual outcomes across broad health, cultural, and political domains (e.g., Cornwell & Laumann, 2011; Cornwell et al., 2008; Lizardo,

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2011; Mutz & Mondak, 2006). In this regard, the characteristics of egocentric networks such as network size and political diversity serve as a barometer of individual and collective social and political health.

There is a long-standing debate on what this instrument really captures. Some of the things we know about the instrument are (1) the content of the important matters which people talk about include seemingly trivial topics such as "cloning headless frogs" or "KFC changing the color of their bucket" (Bearman & Parigi, 2004; Brashears, 2014); (2) the "important matters" name generator invokes different kinds of confidants depending on what people consider as "important"; and (3) people discuss important matters with distant others who happen to be available at the right moment (Small, 2013; Small & Sukhu, 2016). Additionally, there is evidence that the content of individuals' discussions with others that is identified as "important" is shaped by the survey context (Bailey & Marsden, 1999).[1] Similar contextual effects on the content of discussions individuals report having with others are observed in studies fielded during major elections (Lee & Bearman, 2017).[2]

In this study, we use both the important matters and the political matters network name generators to measure how healthy our social networks were during the fall of the 2016 U.S. presidential election. The comparison of core and political discussion networks across various contexts is necessary for studying the implication of interpretations of "important matters" in political contexts to the activation of social networks. Our data arise from a nationally representative survey experiment of 1,055 American adults. We compare our results from the experimental study conducted in 2016 with data from 11 other studies over nearly three decades.

In the fall of 2016, amidst a contentious election, we find that the average network size ($\sim$1.4) is smaller than reported in all previous studies. The decline of important confidants to whom Americans are strongly tied and feel close to can be arguably seen as a form of social isolation (McPherson et al., 2006). What makes our results striking is that average network size is now 35% smaller than that reported in 2004 and that the decline in the number of confidants is observed for both political matters discussion networks and important matters discussion networks. Because people understood important matters to be political matters in 2016, we may observe a form of political isolation not seen previously. While we show that some of known methodological artifacts did not likely account for the smaller networks in our sample, we need more research to examine whether this observation reflects the momentary disruption of the 2016 election or is more long-standing.

We are not the first to document rising political polarization within social relationships in the U.S. context. Huber & Malhotra (2016) show that, on online dating sites, political ideology appears as strong as other traditional indicators such as race and education when predicting partner selection. Likewise, Chen & Rohla (2018), using anonymized smartphone-location data and precinct-level voting records, provide evidence that families spent significantly less time together at their Thanksgiving dinner if they were likely to vote for different candidates. These studies aside, this is the first study to identify a significant increase in the extent to which people discuss important political matters only with like-minded others in close and intimate offline networks.

In contrast to the role of online exposure to opposing views which generates "backfire effects" that solidify existing attitudes (Bail et al., 2018), individuals exposed to diverse beliefs in their offline interpersonal networks become more tolerant (Pattie & Johnston, 2008). Likewise, the benefits of a having large network are well known in the literature on political attitudes and behaviors; people with larger networks have more accurate political knowledge (Lake & Huckfeldt, 1998), are more likely to vote (Mutz, 2002), and experience greater exposure to ideological diversity (Huckfeldt et al., 2004). In this regard, smaller political and important matters discussion networks as a response to intense polarization pose significant challenges to democratic discourse, which are heightened especially if such retrenchment induces political echo chambers.

This article unfolds as follows: We first introduce our data and contrast it to all publicly available national network surveys which use comparable name generators to assess network size. This allows us to compare average network size in 2016 with the overall trends for network size arising from "important matters" and political matters name generators. We then consider the composition of the networks in which individuals are embedded and show that while historically people have discussed politics with friends, neighbors, and coworkers, these non-kin ties largely disappear in Americans' discussion networks in 2016. Across the board, we show that Americans' discussion networks have become smaller, more closed, and more homophilous with respect to political beliefs. We summarize the methodological challenges identified by prior scholarly debates on the decline of Americans' core discussion networks and show that our finding is robust against those challenges based on both out-of-sample prediction and in-sample experimental results. Finally, we discuss the broader implication of our findings as well as some limitations.

2. Data

We conducted a nationally representative survey experiment of 1,055 American adults during the fall of the 2016 U.S. presidential election with a support from Time Sharing Experiment for Social Sciences (TESS) to understand who Americans talk to, and what they talk about, during election campaigns. We conducted the survey via the GfK Group's (formerly Knowledge Networks) internet panels recruited using a dual frame method combining random digit dialing and address-based sampling. The same sampling frame as the GSS is used. Informative response rates for online panels are difficult to calculate, but of the 2,000 panelists randomly drawn from the KnowledgePanel, 1,101 responded to the invitation and 1,055 completed the survey with qualified responses, yielding a final stage completion rate of 52.8% and a qualification rate of 95.8%. The recruitment rate for this study, reported by GfK, was 13.5% and the profile rate was 64.4%, for a cumulative response rate of 4.6%.[3] We address this low response rate by employing survey weights, calibrated by the GfK Group, in our statistical analysis which adjust for biases in panel construction and retention across all analysis (see DiSogra & Callegaro, 2016).

We used a computer-assisted self-interview method which, while differing from previous studies, yields data that are of comparable quality to that obtained by other methods (Heerwegh & Loosveldt, 2008).

The name generator questions used across different surveys are similar but not precisely the same. We used the following network name generators in our TESS survey:

> *For the important matters name generator*: From time to time, most people discuss **important matters** with other people. Looking back over the last month—who are the people with whom you discussed matters **important** to you? Just tell us their first names or initials.
> *For the political matters name generator*: From time to time, most people discuss **government, elections, and politics** with other people. Looking back over the last month—who are the people with whom you discussed matters **political** to you? Just tell us their first names or initials.

We probed for up to six names and measured network size by counting the total number of names provided in the network name generators. Subjects who said "no one" were asked whether this was because they had nothing to talk about or no one to talk with. Subjects who reported discussing important or political matters with at least one person (alter) in the last month were asked about the topic of their most recent conversations and a series of questions about the characteristics of the alter with whom they had the most recent discussion (gender, age, race, party identification, discussion frequency, and relationship type). In terms of discussion topic, subjects were asked to describe what they discussed in their last conversation in a free-response format.

While previous research has established that people are able to recall what they discussed in their last conversation reasonably well (Bearman & Parigi, 2004; Brashears, 2014; Small, 2013), it makes sense that some people may not perfectly remember or even forget to report what they discussed. To address this potential underreporting issue, we use a follow-up question to ask respondents to identify whether in their last discussion they discussed one or more of the most popular Google search terms[4] for the past week as well as two major political events, "the Presidential debate" and "the Presidential election." We use this Google trend prompt for memory aid, though it may increase over-reporting of certain topics due to social desirability bias. We use both instruments with free-response format and fixed-choice format to report lower-bound and upper-bound estimates for discussion topic.

As noted earlier, the average network size in 2016 was 1.38. It is reasonable to wonder whether such isolation is new. To assess this question, we reanalyzed all publicly available nationally representative data in the U.S.A. that employed "important matters" or "political matters" name generators for comparison.

2.1 Other data sets

We use the GSS egocentric network data arising from the "important matters" name generator collected in 1985, 1987, 2004, and 2010 and the American National Election Studies (ANES) "political discussion" network data collected in 2000 and 2008. We also use the 2006 ANES pilot study that employs both important and political matters name generators. In addition, we use other nationally representative surveys: Brashears' (2011) TESS survey and the U.S. component of the 1992 Cross National Election Study. Following Klofstad et al.'s (2009) strategy, we extract political subnetworks out of core-discussion networks in the 1987 GSS and 1992 CNES data by excluding alters with whom people do not or rarely discuss politics in their response. To our knowledge, no other nationally representative network surveys that employ network name generators in the U.S.A. are publicly available.

The GSS data and the 2000 ANES data were collected mostly through face-to-face interviews. The 2006 ANES and the 1992 CNES were collected through telephone interviews, and the 2008 ANES and the 2010 TESS were collected through internet panels such as our TESS study. The 1985, 1987, 2010 GSS surveys and the 2010 TESS study were fielded in the spring and summer of non-presidential election years. The 2004 GSS and the 1992 CNES, and 2000/2008 ANES data were collected during presidential election years. Table A1 in the Appendix summarizes the characteristics of all surveys in addition to average network size reported.

3. Results

Figure 1 shows the average network size of both important matters (blue-filled dot) and political matters (red-empty dot) and 95% confidence intervals capped at three across all data sets. In general, core-discussion networks are larger than political discussion networks. The two exceptions are the 2004 GSS and 2008 ANES data sets. The 2004 GSS data reported significantly smaller networks and the 2008 ANES data produced significantly larger networks. Many network scholars have suggested that a series of methodological artifacts account for the decrease of network size in the 2004 GSS, including coding error, respondents' fatigue, training/priming effects, and malfeasant interviewers (Brashears, 2011; Fischer, 2009; 2012; McPherson et al., 2009; Paik & Sanchagrin, 2013). Similarly, the substantial increase of political network size in the 2008 is likely to have arisen from the ANES 2008 survey design, which presents eight boxes for respondents to enter the name of discussion partners.[5]

With these exceptions in mind, the network size discrepancy between the important matters and political matters name generators is clearly visible. More strikingly, it shows that the average size of both important and political discussion networks in 2016 was significantly smaller than any other period. Given that large political discussion networks provide a foundation for political tolerance (Huckfeldt et al., 2004; Huckfeldt & Sprague, 1995; Pattie & Johnston, 2008), this decrease in network size is a significant threat to democratic discourse.

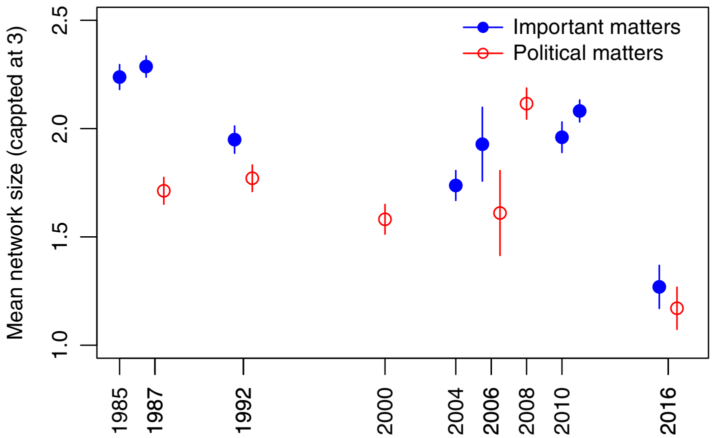

Figure 1. Average size of core discussion and political discussion networks.

Following previous research, we asked those who did not report a discussion partner whether they had nothing to discuss or had no one to discuss anything with (i.e., the relationally isolated).[6] We find that fewer people (43.4%) have nothing to talk about than no one to talk to, for the important matters name generator in 2016. This is consistent with other studies (Bearman & Parigi (2004) and Brashears (2011) reported 44% and 36%, respectively).[7] This pattern does not hold true for the political matters name generator where relationally isolated people outnumber those with nothing to talk about in 2016. When people are asked why they did not discuss government, elections, and politics with others, 70% of our respondents said that they had no one to discuss those issues with. We can see that political isolation accompanies relational isolation.

3.1 The suppression of network size due to politicization

The suppression of network size from the politicization process is a potential mechanism accounting for the small network size in 2016. Specifically, if "politics" per se is fundamentally conflictual, and people try to avoid conflicts as much as possible, then a politicization process would naturally lead to smaller core-discussion networks. To test this idea, we examine what individuals reported talking about and who they talked to.

We asked respondents what they discussed with their last conversation partner, and manually coded responses first based on 23 detailed categories, which later are combined into Bearman & Parigi (2004)'s 9 categories, Small (2013)'s 12 categories, and finally Brashears (2014)'s 14 categories. For maximal comparability, we adopt the following strategy. We additionally collected the same type of text data from the 1997 North Carolina study (Bearman & Parigi, 2004) and the 2010 TESS study (Brashears, 2014) and arranged all three text sets alphabetically. We then detached text identifiers to prevent two independent coders from identifying the source of each text.[8] Based on our coders' coding results, we decide to maintain both coding results for the maximal coverage (e.g., if the first coder categorizes a text into A and B, and the second coder into A and C, then we classify the text into A, B, and C). Note that a systematic bias in coding, if present, would affect all three text sets at the same time, which suggests that the difference in coding results between three sets would be unbiased. After coding all text into 23 categories, we transformed them into 9, 12, and 14 categories, respectively, following rules described in Table A2 in the Appendix.

Figure 2 shows that more than one-third of our 2016 respondents reported discussing politics in response to the "important matters" name generator. In contrast, only 5% of Americans discussed politics in the important matters name generator in the spring of 2010, and 13% of North Carolina residents talked about politics in the winter of 1997.[9] Our coding results are comparable to Brashears (2014)'s report of 5.1% for political discussion and Bearman & Parigi's (2004)'s 11.3%.

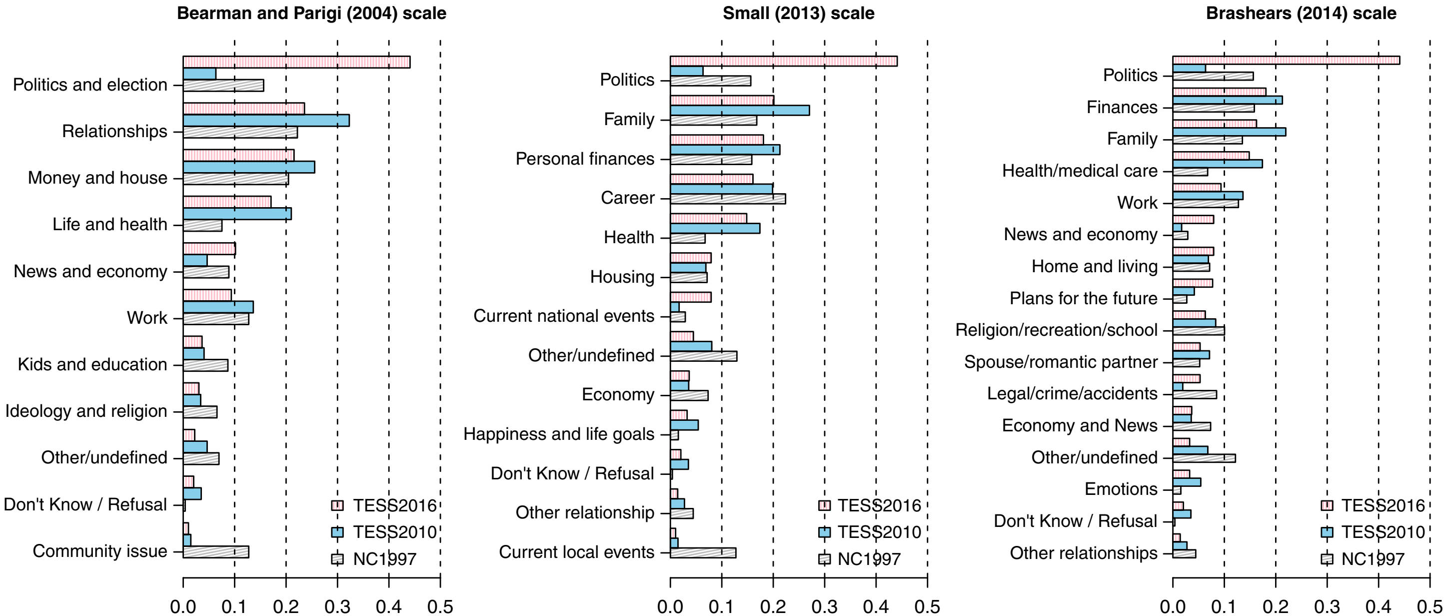

Figure 2. Coding results of discussion topics across three data sets.

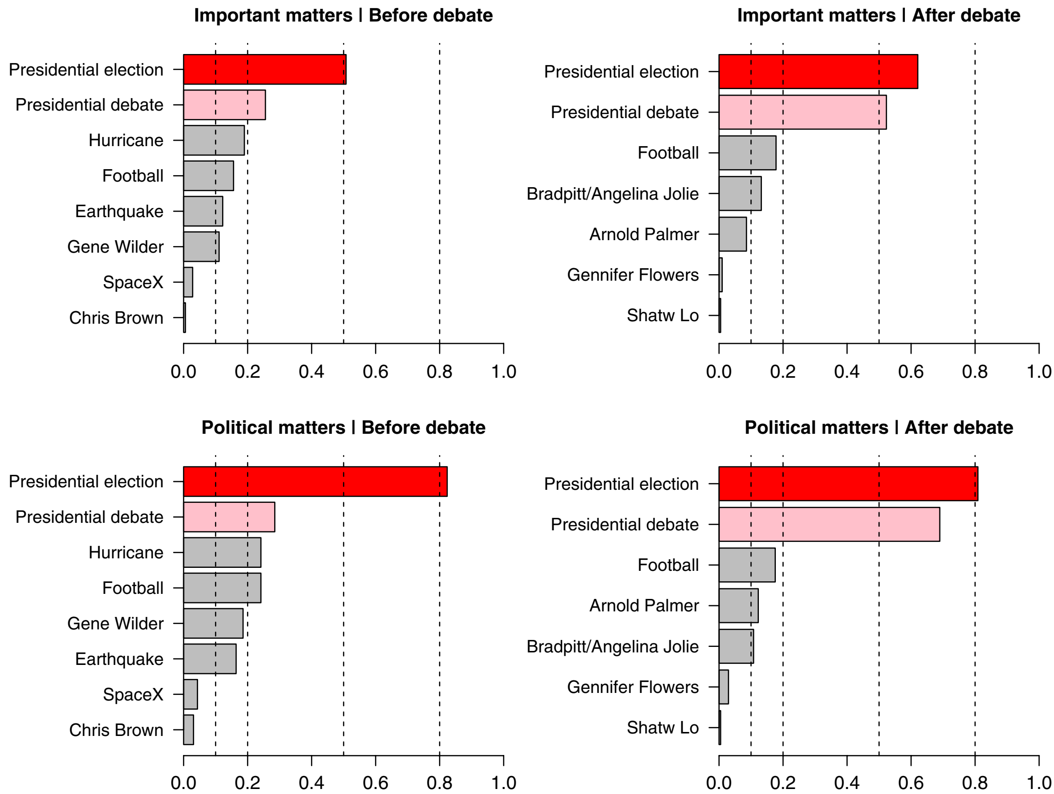

Figure 3. The frequency of discussion topics chosen among a set of hot Google searches in the past week in the 2016 TESS data (multiple choices).

Still, on first glance, two-thirds of our 2016 respondents report not talking about politics. This is a lower-bound estimate. To provide an upper-bound estimate, we use the multiple-choice option following the free-response question, which enables us to identify whether those people who did not say that they discussed politics actually discussed politics. Figure 3 shows results from the analysis on topic choices among the most searched Google news in the past week. First, more than 50% of the people responding to the important matters name generator report that they had already talked about the presidential election in early September.[10] This gives rise to the small gap in network size between the important matters and political matters name generators. After the presidential debate, about 60% respondents said they discussed the presidential election in the important matters name generator, which is slightly lower but comparable to the proportion of respondents who discussed the presidential election in the political matters condition.

Although we picked the most frequently searched topics ("Google hot trends") for the past week, these topics rarely featured as the focus of discussion. One potential reason for the mismatch between what people discussed and what people searched would be that the Google trend reports are based on national searches, rather than local searches. While this mismatch is not our main interest,[11] it is interesting to observe that Americans talked about football as one of the discussion topics to the similar extent before and after the presidential debate. The first presidential debate did not change how people interpreted important matters. Important matters to most people in early September were about the presidential elections and political matters.

3.2 Increased homophily and a narrowing of discussion partners

Given a politicization process, people will choose discussant partners whom they think they can discuss conflictual political issues with safely. This interpretation is consistent with the findings

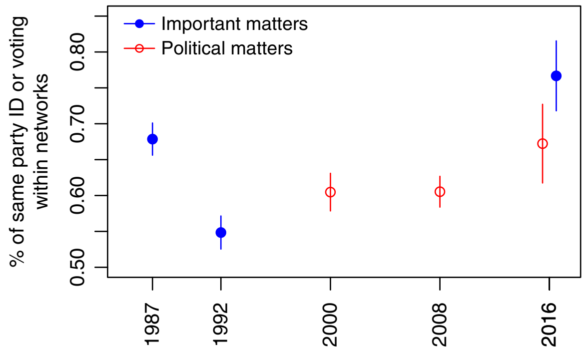

Figure 4. Political homophily in important matters and political matters discussion networks.
Note: The proportion of same voting behaviors (four categories: Republican candidate, Third party candidate, Democratic candidate, and no vote) within networks is measured in the 1992 CNES and the 2000 ANES data, and the proportion of same party ID (four categories: Republican, something else, Democrat, and Independent) within networks is measured in the 1987 GSS, the 2008 ANES, and 2016 TESS data.

from the analysis of relationship type in Table 1, which shows that compared to all other surveys, Americans in 2016 were more likely to talk about both important and political matters with close and strong-tied family members and less likely discuss them with friends, neighbors, and coworkers. For example, 75.6% and 62.4% people talked about important and political matters with close and strong-tied family members, respectively, in 2016, which is higher than any other period and consistent with the observation that the proportion of discussion partners who are friends and neighbors has decreased over time. People now talk about important and political matters mainly with "safe" others that they can trust.

Figure 4 shows trends for political homophily measured by the proportion of same voting behaviors (four categories: Republican candidate, Third party candidate, Democratic candidate, and no vote) within networks in the 1992 CNES and the 2000 ANES data, and the proportion of same party identification (four categories: Republican, something else, Democrat, and Independent) within networks in the 1987 GSS, the 2008 ANES, and 2016 TESS data. The level of political homophily is highest in 2016. Table A3 in the Appendix estimating multilevel dyadic logistic regression models for political homophily shows that the sharp increase of political homophily is statistically significant for both the political matters and important matters name generators. Here, we also confirm that political homophily is negatively correlated with network size, as measured by the order of names appearing in the network name roster. This provides support for the idea that smaller networks arise from the politicization process, which induces the activation of a limited number of close and safe social ties.

In 2016, we find that core-discussion networks exhibit significantly stronger political homophily than political discussion networks do. But, we discover that this is because people discuss important matters with their spouse and their family more than with their friends and coworkers. Kin relations tend to exhibit higher political similarity compared to friendship and coworker relationships (Mutz & Mondak, 2006). We investigate this for our data by focusing on the difference of political similarity across kin and non-kin relationships.

Figure 5 shows the level of political homophily across non-kin and kin dyads in five data sets. As expected, kin ties are more politically similar than non-kin ties across all data sets. The homophily gap between kin and non-kin is greatest in 2016 (=0.152), twice that of the second biggest gap found in the 2008 ANES (=0.087). This pattern is consistent with the idea that people talk about political issues with their close confidants who they can safely trust because they are politically more similar. Since politics as a topic of conversation is fraught with potential risk of relationship disruption, it is safe to avoid such potential conflicts within our close and strong-tie networks if those networks are characterized by political heterogeneity.

3.3 Potential challenges

Prior scholarship on the important matters name generators identifies several methodological factors influencing network size. The list includes respondent fatigue due to training effects,

Table 1. Relationship type in ego-centric discussion networks.

	gss1985 [imp]	gss1987 [imp]	cnes1992 [imp]	gss2004 [imp]	gss2010 [imp]	tess2010 [imp]	tess2016 [imp]	anes2000 [pol]	anes2008 [pol]	tess2016 [pol]
Spouse	23.6	26.9	27.2	40.6	36.5	22.6	50.4	24.6		41.9
Parent	10.6	12.6		11.5	12.1	11.9	11.2			6.4
Child	8.7	8.8		4.0	6.0	7.6	6.6			5.3
Sibling	8.0	7.8		6.0	6.7	9.0	4.1			4.8
Other family	6.3	6.1		4.1	4.4	7.1	3.3			4.0
Friend	37.3 (68.9)	34.3 (70.1)	45.3	28.2 (51.9)	27.5 (43.7)	31.4	16.0			22.7
Neighbor	7.7 (9.7)	6.3 (9.0)	10.2	3.2 (4.4)	2.0 (2.8)	1.3	0.8	15.3		2.4
Coworker	11.5 (15.3)	7.7 (13.2)	19.1	7.9 (10.3)	6.8 (8.9)	4.1	5.6	26.1		8.0
Other	1.7 (2.2)	1.5 (2.6)		1.3 (2.4)	6.8 (9.8)	5.0	2.0			4.5
Relative	57.2	62.2	52.4	66.2	65.7	58.2	75.6	48.1	51.9	62.4

Note: Since the 2016 TESS asks the relationship of the last conversation's partner and the 2010 TESS asks the relationship of one randomly chosen discussion partner, we also chose the first discussant's relationship in all other network surveys. Except for the 2010 and 2016 TESS data, all other national surveys allow "multiple choices" so that the sum of all proportions may exceed 100%. For non-relative ties, we present the proportion of ties excluding relative ties. But, we also present the raw proportion without excluding relative ties in parenthesis.

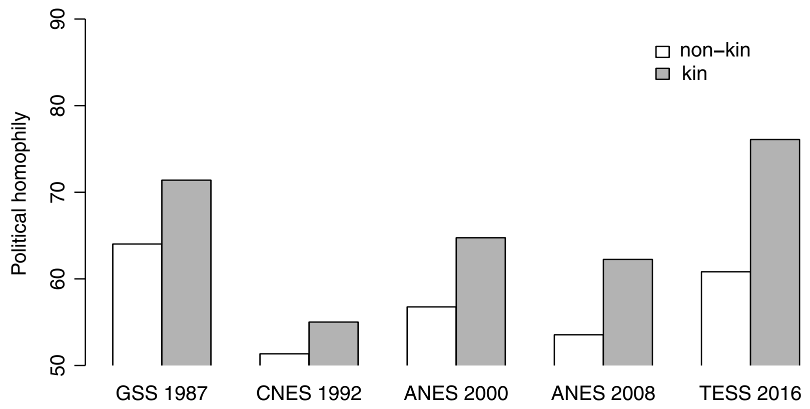

Figure 5. Political homophily by relationship type.

interviewer effects, survey mode, the role of election context, survey design, the wording of name generators, and so on (Bailey & Marsden, 1999; Brashears, 2011; Fischer, 2009; Lee & Bearman, 2017; Marsden, 1990; Matzat & Snijders, 2010; McPherson et al., 2006; McPherson et al., 2009; Paik & Sanchagrin, 2013). We consider these challenges seriously and explain how we address each challenge below.

Among these possible threats to validity, interviewer effects are not applicable to our self-administered online survey. In terms of respondent fatigue, Fisher's (2012) findings are not applicable to our study since we employed a short (median duration = 4 min, maximum duration = 15 min) stand-alone survey. No battery of questions preceded our network instrument.

It is well known that survey mode can influence how people respond to the network name generators (Bisbee & Larson, 2017; Matzat & Snijders, 2010). One concern is training effects. We employ a panel and so it is possible that our respondents might have been used to answering multiple social science surveys and so are uncommonly savvy. The idea that respondents may anticipate name generator questions so that they report fewer names is theoretically plausible. However, using the same platform, Brashears (2011) shows that training prompts—signaling that respondents would be asked about all the names they provide—actually increases, rather than decreases, reported network size. In addition, the effect of on-line surveys in other studies has been to increase rather than decrease average network size (Eagle & Proeschold-Bell, 2015). Apart from a survey-mode effect, the number of boxes presented to respondents is associated with how many names people fill in the boxes Vehovar et al. (2008). To ensure comparability, we follow Brashears's (2011) 2010 TESS design that prompts one box at a time instead of providing multiple boxes.

Our time frame was different from other surveys. Since our goal was to instrument contextual priming arising from the first presidential debate, we used the time frame "over the last month" instead of "over the last six months" in our network name generators. If respondents seriously considered this time frame in their mind when they recalled with whom they talked about important or political matters, this might reduce network size. Prior research shows, however, that this is unlikely the case. Kogovšek & Hlebec (2005) show that there is no discernable difference in network composition between the usual time frame ("over the last six month" and no time frame). Using a survey experiment, Sokhey & Djupe (2014) found that shorter recall period prompts (the last week or two versus the last few months) *decreases* the proportion of people who report being isolated but has no significant impact on the network size. To our knowledge, there is no evidence that the time frame for the important or political matters name generator is related to either network size or composition.[12] One strategy is to try to account for these potential artifacts by statistical analysis of all publicly available data sets. We turn to this in the next section.

Table 2. Prediction model for network size using all data before 2016.

	Model 1
Respondent-level factors	
Female	0.089**(0.02)
Age	−0.003**(0.00)
Black (ref: white)	−0.257**(0.03)
Other race	−0.219**(0.03)
Education	0.074**(0.00)
Currently married	0.105**(0.02)
Currently working	0.070**(0.02)
Party ID: strong democrat (ref: independent)	0.522**(0.03)
Party ID: weak dem.	0.358**(0.03)
Party ID: lean toward dem.	0.412**(0.03)
Party ID: lean toward republican	0.411**(0.04)
Party ID: weak rep.	0.378**(0.03)
Party ID: strong rep.	0.529**(0.03)
Survey mode: telephone (ref: face-to-face interview)	−0.127**(0.03)
Survey mode: online survey	0.076*(0.03)
Data-set level factors	
Probe more than one name at a time	0.285**(0.05)
Not mention "over the last six months"	−0.126**(0.04)
Survey during presidential election	−0.034 (0.04)
Important matters name generators (ref: political matters name generators)	0.432**(0.03)
Presidential election × important matters	−0.402**(0.05)
Constant	0.482**(0.06)
Observations	16987
R2: between data sets	0.7215
R2: within data sets	0.0786
R2: overall	0.1131

Note: We run a random intercept multilevel regression model on the data excluding the 2016 TESS data, based on which we run out-of-sample prediction for network size in the 2016 TESS data. We top-coded network size of all data sets at 3 since it is the maximum network size of the 1987 GSS sub-political network data.

3.4 Predicting network size after accounting for methodological challenges

Combining the 12 data sets that collect network size using the important matters or political matters name generators enables us to model all these methodological factors into one single predictive model, and we conduct an out-of-sample prediction for network size in the 2016 TESS data. Our prediction models include mode of data collection (face-to-face, telephone, and online), two variables related to survey design—one is whether more than one name is probed at the same time, another is whether "over the last six months" is specified in the name generator—and the interaction between the type of name generator (important matters versus political matters) and survey timing, in addition to individual characteristics such as respondents' gender, age, race, years of education, marital status, working status, and seven-scale party identification. Network size is capped at three since the sub-political network data from the 1987 GSS collect only up to three names. We employ random intercept regression models to account for the unbalanced size of samples across different years.

Table 2 shows results from our analysis of 16,987 individuals across 12 data sets. While the overall model R^2 is 11.31%, our predictive model explains 72.15% of the variation between data sets.

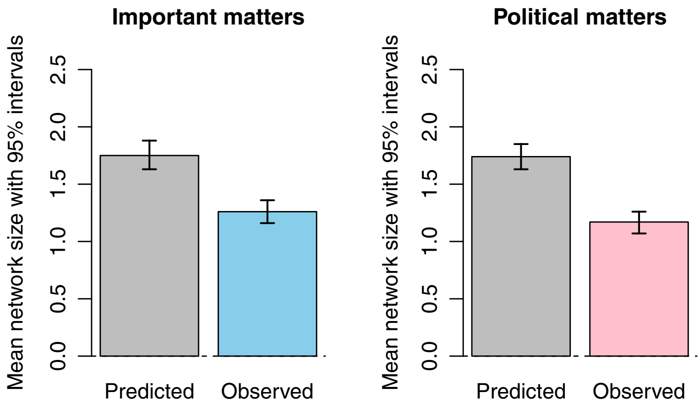

Figure 6. Predicted versus observed network size in the 2016 TESS data.
Note: The out-of-sample prediction for the 2016 TESS data is based on the prediction model in Table 2 that use all publicly available data set before 2016.

As expected, survey mode is significantly related to network size—telephone interviews decrease and online surveys increase network size. Network size is also significantly increased if more than one name at the same time is probed or "over the last six months" is mentioned. And, as expected, the important matters name generators produce larger networks than the political matters name generators during non-election periods, but the gap is closed if the survey is conducted during the Presidential election season.

Figure 6 shows that the predicted means for network size capped at three arising from the important matters and political matters name generators in 2016 are 1.75 and 1.74, respectively. Our model successfully predicts our finding—the absence of a size gap between core-discussion networks and political discussion networks in 2016, despite the fact that important matters name generators produce larger networks than political matters name generators in the pre-2016 training data set. However, the observed political discussion and core-discussion networks are significantly smaller than the predicted network size (1.26 for important matters and 1.17 for political matters).[13] These results show that some of known methodological factors we consider alone cannot account for the smaller network size we observe in 2016. Of course, they may arise from other methodological artifacts not widely studied. One of them we consider here is political priming effects.

3.5 Political priming effects

McPherson et al.s' article (2006) reported from GSS data that American's important matters networks had decreased significantly from three in 1985 to two in 2004. In addition to coding errors, training effects, respondent fatigue, or malfeasant interviewers, Lee & Bearman (2017) showed that the decline is related to the timing of the survey. The 2004 GSS was fielded in the fall of the highly polarized 2004 election period, and as a consequence, people framed "important matters" as "political matters." When political matters are primed, reported network size decreases because people talk with fewer people about politics, likely in order to avoid conflict. They show that network size declined dramatically around the first presidential debate of the 2004 election season, suggesting the presence of political priming effects which activated political ties and de-activated social ties. In this regard, a political priming effect alone might be able to explain why we observe such small networks during the 2016 presidential election season.

We assess this possibility by exploiting a feature of our experimental design which incorporates survey timing to capture contextual effects (such as politicization/polarization) and classical priming (to simulate the framing process described above) as separate treatments along with both

Table 3. Experimental conditions (2 × 2 × 2) and sample sizes per condition.

Survey before the first debate (09/08 (Thu)– 09/14 (Wed))			Survey after the first debate (09/28 (Wed)–10/5 (Wed))		
	Political priming			Political priming	
Name generator	Yes	No	Name generator	Yes	No
Important matters	Block A (127)	Block B (135)	Important matters	Block E (135)	Block F (129)
Political matters	Block C (119)	Block D (140)	Political matters	Block G (129)	Block H (141)

Note: Our target sample size per block was 117, given the power analysis we performed in advance. The first presidential debate was held on September 26, 2016.

Table 4. Experimental results on network size in the 2016 TESS data.

		Important matters (N = 526)		Political matters (N = 529)		Overall (N = 1055)	
Name generator		Mean	SE	Mean	SE	Mean	SE
Overall (N = 1055)		1.38	0.06	1.38	0.07	1.38	0.05
Psychological priming	no (N = 545)	1.47	0.08	1.42	0.09	1.44	0.06
	yes (N = 510)	1.29	0.09	1.34	0.11	1.31	0.07
Treatment effects		−0.18	0.12	−0.08	0.14	−0.13	0.09
Contextual priming	no (N = 521)	1.21	0.08	1.26	0.09	1.23	0.06
	yes (N = 534)	1.54	0.09	1.50	0.10	1.52	0.07
Treatment effects		0.33**	0.12	0.24+	0.13	0.29**	0.09

Note: SE = standard error. We run OLS regression models to conduct t-tests for the weighted mean difference of network size between two blocks using survey weights. +$p < 0.1$, **$p < 0.01$.

the important matters and political matters name generators. Using survey timing, we instrument the real-world political context, while the political priming treatment is introduced to examine the role of political issue salience. Using both different types of name generators makes it possible to measure the difference between the important matters name generator and the political matters name generator. In the experiment, subjects were randomly assigned to one of eight conditions, produced by a combination of network name generator (important versus political matters), external political priming using survey timing (before versus after the first presidential debate), and internal political priming using question order (before versus after several political questions) as described in Table 3.[14]

With respect to timing, half of our sample was surveyed 2 weeks before (09/08–09/14) the first presidential debate in 2016 (September 26), and the other half in the week immediately after the debate in late September (09/28–10/05). The timing of survey was randomized to minimize reachability bias. To assess whether political priming was salient, we varied question order. Specifically, in the priming condition, six political questions—political interest, political discussion, voting behavior, political information channel, political activity—were administered before asking the name generator. In the control condition, political priming questions were administered after the name generator. Party identification and political ideology questions preceded the network name generator in the priming condition and followed the network name generator in the control condition.

Against this background, Table 4 shows average network size across different treatment conditions in the 2016 TESS data. The two network name generators provide almost identical estimates for network size (=1.38), which suggests that people thought about "important matters" as political matters throughout the highly polarized 2016 election season. Average network size was smaller in the psychological priming condition (1.44 versus 1.31), though this effect is not statistically significant. In contrast, network size becomes significantly larger in the contextual priming condition (1.23 versus 1.52).

Table 5. Results from Poisson regression models for network size in the 2016 TESS data.

Name generators	Model 1 (all)	Model 2 (imp)	Model 3 (pol)	Model 4 (all)	Model 5 (all)
Political networks	0.01 (0.07)			−0.01 (0.09)	0.03 (0.10)
Psychological priming	−0.12+ (0.07)	−0.14 (0.09)	−0.09 (0.10)	−0.15+ (0.09)	−0.12+ (0.07)
Contextual priming	0.23** (0.07)	0.25** (0.08)	0.21* (0.10)	0.23** (0.07)	0.24** (0.09)
Psychological priming × pol. networks				0.05 (0.14)	
Contextual riming × pol. networks					−0.04 (0.13)
Observations	1055	526	529	1055	1055

Note: A list of controls include: female, age, education, race, marital status, working status, household income, household size, living in metro area. Standard errors in parentheses (+p < 0.1, *p < 0.05, **p < 0.01).

Our experimental design enables us to identify the precise role of political priming in shaping both political and social networks. Table 5 shows results from estimating Poisson regression models that test the role of political priming.[15] From Table 5, one can see that Models 1 and 2 show that there is a marginally significant effect of the psychological priming that arises from asking political questions before asking name generators when both discussion networks are assessed simultaneously, though it loses its significance when they are assessed separately. With regard to the framing effects that arise from switching conversational topic from "important matters" to "political matters," Models 4 and 5 show that they are not significant across both types of political priming because, as shown in Model 3, there is no gap between the size of the important matters and political matters networks.

The significant absence of psychological priming and framing effects in the 2016 TESS study suggests that our findings are not driven by political priming effects. Instead, it appears that "important matters" were already political so that American's *core*-discussion networks had already been deactivated, as a consequence of the polarized election season, well before our experimental window in September 2016. Simultaneously, the significant presence of contextual priming effects indicates that political events may activate parts of social networks to discuss *politics* since they can provide more contextual cues to facilitate discussion. Earlier we show that Americans in 2016 were more likely to discuss politics with their spouse and family members with similar views. Political polarization drives the shrinkage of social networks. In an already polarized context, politicization induced by political events increases the size of networks for political discussion, but only within a limit defined by sharing similar opinions.

4. Discussion

Our analysis of the 2016 TESS data show that average network size (1.38) is smaller than reported in any prior surveys, which suggests the rise of what we identify as political isolation. At the same time, the level of political homophily, which is measured by the proportion of individuals with the same political preference within our core-discussion networks, is greater than reported in all other national surveys. We find that people talk about political issues with their close relatives, more than with friends, who are on average politically more similar. Taken together, these findings suggest a rise of political isolation and the activation of interpersonal political echo chambers in 2016. Of course, as is the case in all social scientific work, our conclusions are provisional. More research is needed to identify whether they reflect the general pattern of political polarization or arise simply due to the abnormality of the 2016 election.

Both the "important matters" and "political matters" network name generators have been widely studied and implemented in various national sample surveys, most importantly the GSS and the National Election Study (NES) for some time. Although the differences are not always statistically significant, the political matters name generator consistently yields fewer names than the important matters name generator when they are collected at the same time (Huckfeldt et al., 2004; Klofstad et al., 2009; Sokhey & Djupe, 2014; Lee & Bearman, 2017). One reason the real differences may be greater than observed is that almost all studies used for comparison, such as the 1996–1997 Indianapolis–St. Louis Study and the 1992 Cross National Election Studies, are conducted during political campaigns and contain numerous political survey items that could have primed respondents to frame important matters as political matters. Our results show that it is also the case for the 2016 TESS data we collected, though we need more systematic analysis to study how broader social contexts shape how different name generators work (Mollenhorst et al., 2008, 2014).

The significant effect of contextual political priming on network size—the increase of network size after the presidential debate—is consistent with previous findings on the role of political campaign in facilitating *political* discussion (Huckfeldt et al., 2004). Initially, we anticipated that contextual priming would decrease network size for core-discussion networks but increase network size for political discussion networks. One obvious implication from our findings on the simultaneous increases of network sizes from both name generators after the presidential debate is that the impact of political events on *core*-discussion networks depends how much people frame important matters as political matters.

The second implication is more nuanced. We argue that politicization leads to the smaller size of *core*-discussion networks because people tend to avoid talking about politics more than other important topics. This does not mean that politicization always leads to withdrawal from all kinds of conversation, rather politicization per se encourages people to talk about *politics*. When on September 30, 2016 the first presidential debate took place, American society was already polarized. After the debate, people talked to more people about politics and the presidential debate in particular, and yet, even these political discussion networks were smaller than those observed in any other period precisely because of the smaller size of strong-tied networks where they can safely disclose their political beliefs.

After the election, in late November, 2016, Americans traveled and spent on average 4.2 h at Thanksgiving dinner, but families that were likely to have voted for different presidential candidates spent about 30–50 fewer minutes less than those whose candidate preferences were likely homophilous (Chen & Rohla, 2018). This result could have been anticipated. About 2 months earlier, we showed that Americans discussed politics more with their close relatives than they did with their non-kin friends and coworkers, and simultaneously that their close relatives were even more politically similar to themselves than ever before. Both results together suggest that American voters are more likely to discuss politics with their family and relatives than with friends and coworkers if and only if they share the same political beliefs.

There are several limitations of this study. First, although the 2016 TESS data are nationally representative, the TESS data structure relies on a different mode of data collection than in other studies. To address this concern, we employ the predictive modeling strategy which controls for some known methodological artifacts, showing that it is unlikely that they matters. But of course, as with all social science, it is possible that some unknown methodological artifacts may affect our results. Interestingly, though, the 2010 TESS data collected by Brashears (2011, 2014) report significantly larger core-discussion networks. Our design is similar with respect to design, survey company, target sample, sampling method/response rate, mode of data collection, and survey instrument as Brashears'. Another related concern is the low response rate of our data. We agree that a higher response rate is always better, but suspect here that more isolated individuals probably select themselves out of interactive panels.

Second, because political discussions happen online through social media and internet blogs, standard network name generators might not be able to capture these new kinds of connections. If people shift their political discussion from offline to online, Americans' online political discussion networks might have been enlarged. Although this tie replacement hypothesis is not well supported for core-discussion networks and friend/acquaintance networks,[16] political discussion networks might differ due to the conflictual nature of politics. This point along with our finding that Americans recently deactivated weak ties and instead activated strong ties for political discussion suggests an inverse U-shaped relationship between tie strength and political discussion—people discuss politics with close confidants whom they care and know well, or turn to discuss politics with unknown strangers whom they do not have to care about. What is missing in this spectrum are friends, coworkers, and neighbors, who compose most of our weak ties.

If this is the case, should we care about the rise of political isolation and political homophily in our offline interpersonal networks? Recent scholarship identifies a significant amount of political diversity in online news media, Facebook friendship, and Twitter followers (Bakshy et al., 2015; Eady et al., 2019; Gentzkow & Shapiro, 2011). However, online cross-cutting communication is more likely to be used to reinforce existing belief. For example, in a field experiment on Twitter users who self-identified as Democrats and Republicans, Bail et al. (2018) show that "backfire effects" are significant, especially for Republicans who, when they are exposed to opposing views, become more conservative.

In contrast, a large volume of literature on offline-based intimate social networks shows that the large and politically diverse networks are sources for political tolerance and drive changes in voting, political issue positions, and even party identification (Beck, 2002; Huckfeldt et al., 2004; Lim, 2008; Mutz, 2002). Since offline network ties are more likely to be embedded in multiple relational contexts, structured by their social surroundings, people might be expected to pay attention to dissenting views and learn from other perspectives, even those that they initially disagree with. Against this background, it is reasonable to think that it is even more critical now that individuals have large and politically diverse "offline" networks. Our findings suggest that this was unlikely the case in America in 2016.

Conflict of interest. Authors have nothing to disclose

Notes

1 Based on in-depth interviews of 48 people, Bailey & Marsden (1999) show that nearly half of respondents did not make a literal interpretation of name generators. Providing some contexts would help to think about what matters to them. For example, they find that those who were asked about political questions before answering important matters name generators were 17% more apt to say that they thought political questions are important matters than those who were asked about family questions.

2 Following the same logic, it would not be surprising to see that the egocentric network arising from the important matters name generator is a significant predictor for political attitudes in the GSS (e.g., Berg, 2009), whereas the egocentric network is strongly associated with health outcomes in the NSHAP (e.g., Cornwell et al., 2009), given that people are asked lots of political questions in the GSS and health-related questions in the NSHAP.

3 This response rate seems to be lower than the traditional GSS surveys (approximately 50%–60%). However, this situation is typical for online "opt-in" panel surveys, including all other TESS series. We consider the TESS 2010 which suffers from the same low response rate issue as a useful comparison set in this regard.

4 To identify the top search terms for the period in which respondents were asked to recall conversations, we monitored Google trends data by following the "daily" hot trends and collected the most popular topic for each day for the past week. The final list includes "football," "Gene Wilder," "SpaceX," "Chris Brown," "Teddy Bridgewater," "earthquake," "hurricane" for the before-debate condition (from September 8 to September 14) and "football," "police shooting," "Shawty Lo," "Gennifer Flowers," "Arnold Palmer," "Brad Pitt and Angelina Jolie" for the after-debate condition (from September 28 to October 5). The first presidential debate was held on the 26th of September.

5 Vehovar et al. (2008) show that the number of boxes has a strong influence on network size, and the distribution of network size peaks around the number of boxes presented to respondents.

6 The exact wording of the question is: "Which best describes why you haven't discussed [any important matters/government, elections and politics] with others lately? Would you say that:

(a) You haven't had any [important matters/government, elections and politics] to discuss in the last month.

(b) You haven't had a person you wanted to discuss [important matters/government, elections and politics] with in the last month."

7 This is the response from those who are isolated. Recall that the proportion of being isolated reported by the 2016 TESS (27%) is larger than those reported by Bearman & Parigi (2004) and Brashears (2011), 20% and 4%, respectively. Therefore, if we calculate the proportion of the relationally isolated unconditionally (i.e., among all respondents), in 2016, 11.9% people report that they have no one to talk about important matters, which is larger than two previous studies (8.8% and 1.8%). This number becomes even larger for those who have no one to discuss political matters (22.4% = 0.70 × 0.32 × 100) given that those who say they have no one to discuss politics with are higher (32%) than those who have no one to discuss important matters with in 2016 (27%).

8 The first author coded the entire text first as described, and responding to a reviewer's request, we hired another graduate student who did not have any clue about this paper's finding. Since we allow to classify a text into multiple categories, it is hard to calculate the standard intercoder reliability metric. Among 1,854 unique texts across three data sets, the complete agreement rate across 23 categories is 59.1%, and the partial agreement rate (i.e., they classify a text into at least one same category) is 88.9%.

9 The pattern is also consistent with Small (2013)'s finding that only 5% of "important matters" discussion topics are political in 2010 for over 2,000 Americans. Since the data set is not publicly available, we cannot code these data ourselves. To our knowledge, there is no other publicly available data about discussion topic from the "important matters" network name generators.

10 A reviewer raised a concern that "during an important political election, few people would admit to not having talked about politics recently" which may bias the estimate of discussing politics as important matters. There is obviously an important contextual effect which leads people to frame important matters as political matters. Strict social desirability bias does not seem to be able to account for the gap between talking about the presidential debate and talking about the presidential election, since both are about politics.

11 One potential way to examine the locality of these searches is to link respondents' zip-code and the city-level Google trends search results, which is not plausible for our case due to the small sample size. Instead, we examined the amount of variation of discussing Google trends topics across zip-codes by estimating random intercept logistic regression models. The intra-class correlation for each discussion topic is followed: Earthquake (0.76), Space X (0.43), Chris Brown (0.41), Hurricane (0.38), Football (0.12), Presidential Election (0.01), Presidential Debate (0), Gennifer Flower (0), Arnold Palmer (0), and Brad Pitt/Angelina Jolie (0). The ICC for discussing Shatw Lo is very high (0.99), but unreliable given that only two people reported that topic in our sample. This suggests that there was no significant local variation with regard to talking about politics in 2016, though there are some significant variations in discussing natural disasters and some events.

12 In the 1985 and 2004 GSS, among 7304 confidants, people discussed important matters less than once a month with only 292 confidants (4%). In other words, although "over the last six months" prompt is used, those whom people confide to discuss important matters are not temporarily distant from the interview.

13 If we employ network size capped at six instead of three in the prediction model, the gaps between the predicted means and observed means for network size to discuss important matters and political matters become bigger (2.41 versus 1.38 for important matters, 2.31 versus 1.39 for political matters).

14 Power calculations were conducted based on the estimates reported in the GSS 2004 and the ANES 2000 (treatment effect = 1, sample standard deviation = 1.5, alpha = 0.05), which yields the required sample size of 117 subjects to ensure 95% power. Thus, total required sample size is approximately 1,000. The final sample size is 1,055.

15 We also report the results from estimating zero-inflated Poisson regression models to account for inflated zeros, following McPherson et al. (2009), which provide the same result, in Table A4 in the Appendix.

16 Previous studies show internet and social media users were more likely to have close interpersonal relationships with respect to discussing "important matters" than non-users (Hampton & Ling, 2013; Hampton et al., 2011). Using a novel survey experiment, Bisbee & Larson (2017) show that online relationships on social media and offline relationships share similar characteristics across 17 measures including political homophily.

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Appendix A

Table A1. Comparison of average network size across all publicly available data sets.

Data set	GSS	GSS	CNES	GSS	ANES	GSS	TESS	TESS	GSS	CNES	ANES	ANES	ANES	TESS
Year	1985	1987	1992	2004	2006	2010	2010	2016	1987	1992	2000	2006	2008	2016
Name generator	imp	imp	imp	imp	imp	imp	imp	imp	subpol	subpol	pol	pol	pol	pol
% of isolation	0.10	0.06	0.10	0.22	0.28	0.13	0.04	0.27	0.06	0.10	0.28	0.38	0.25	0.32
Network size														
Max	6	6	4	6	10	6	70	6	3	5	4	10	8	18
Mean	2.91	2.49	2.25	2.11	2.97	2.39	3.27	1.38	1.71	2.05	1.78	2.31	4.28	1.41
Mean (capped at 6)	2.91	2.49	2.25	2.11	2.66	2.39	2.79	1.38	1.71	2.05	1.78	2.10	3.61	1.38
Mean (capped at 3)	2.24	2.29	1.95	1.74	1.93	1.96	2.08	1.27	1.71	1.77	1.58	1.61	2.12	1.17
N	1531	1800	1318	1426	346	1272	2061	526	1689	1318	1551	316	2658	529
Survey period	spring	spring	fall	fall	fall	summer	spring	fall	spring	fall	winter	fall	fall	fall
Presidential election	No	no	yes	yes	no	no	no	yes	no	yes	yes	no	yes	yes
Survey mode	FF/tel.	FF/tel.	tel.	FF/tel.	tel.	FF/tel.	internet	internet	FF/tel.	tel.	FF/tel.	tel.	internet	internet

Table A2. The initial coding schemes and transformation rules

	Initial category	Bearman & Parigi (2004)	Small (2013)	Brashears (2014)
1	Local community issue and current local events	Community issue	Current local events	Other/undefined
2	Current national events	News and economy	Current national events	News and economy
3	News and economy	News and economy	Economy	Economy and news
4	Personal finances	Money and house	Personal finances	Finances
5	Home and living	Money and house	Housing	Home and living
6	Car	Money and house	Personal finances	Finances
7	Work and career	Work	Career	Work
8	Plans for the future	Other/undefined	Career	Plans for the future
9	Education and school	Kids and education	Career	Religion/recreation/school
10	Politics and election	Politics and election	Politics	Politics
11	Health and medical care	Life and health	Health	Health/medical care
12	Happiness and life goals	Life and health	Happiness and life goals	Emotions
13	Emotion	Other/undefined	Happiness and life goals	Emotions
14	Recreational activity	Other/undefined	Other/undefined	Religion/recreation/school
15	Religion	Ideology and religion	Other/undefined	Other/undefined
16	Legal/crime/accidents	Other/undefined	Other/undefined	Legal/crime/accidents
17	Family	Relationships	Family	Family
18	Spouse/romantic partner	Relationships	Family	Spouse/romantic partner
19	Kid	Kids and education	Family	Family
20	Non-family (friends, neighbors, and acquaintances)	Relationships	Other relationship	Other relationships
21	Relationship issue	Relationships	Other/undefined	Other/undefined
22	Don't know/refusal	Don't know/refusal	Don't know/refusal	Don't know/refusal
23	Undefined	Other/undefined	Other/undefined	Other/undefined

Note: Initially, coders are allowed to code each text into *others/undefined* category if they think it does not exactly match with one of 23 coding scheme categories. After transforming coding results into three other coding schemes, we redefine the others/undefined category only if the text is solely classified by it.

Table A3. Results from random intercept multilevel regression models for political homophily.

Dependent variable	Political homophily	
Network name generators	Important matters	Political matters
	Model 1	Model 2
Year 2016 (=ref)	0(.)	0(.)
Year 2000	−0.233 + (0.126)	
Year 2008	−0.490*(0.116)	
Year 1987		−0.241 + (0.125)
Year 1992		−0.631*(0.126)
Age	−0.00325 + (0.00171)	0.0113*(0.00175)
Education	0.00317(0.00981)	0.00874(0.00964)
Female	0.0982*(0.0473)	0.0301(0.0533)
White (=ref)	0(.)	0(.)
Black	0.471*(0.100)	0.554*(0.0834)
Other race	−0.224*(0.0998)	−0.0664(0.151)
Race missing	0.0154(0.0853)	−0.263(0.172)
Currently married	0.260*(0.0497)	0.210*(0.0538)
Job status: working	−0.0645(0.0540)	0.0895(0.0601)
Party ID: strong democrat	0.887*(0.0943)	1.002*(0.114)
Party ID: weak dem.	0.179 + (0.0990)	0.754*(0.119)
Party ID: lean toward dem.	−0.386*(0.101)	−0.240*(0.117)
Party ID: independent (=ref)	0(.)	0(.)
Party ID: lean toward republican	−0.335*(0.101)	−0.0661(0.124)
Party ID: weak rep.	0.253*(0.0987)	0.298*(0.119)
Party ID: strong rep.	0.898*(0.0970)	0.845*(0.116)
Order in network rosters	−0.0480 + (0.0266)	−0.0487*(0.0204)
Constant	0.488 ∗ (0.216)	−0.356(0.228)
Observations	8512	6993

Note: Logit coefficients are reported. Standard errors in parentheses (+p < 0.1, *p < 0.05).

Table A4. Results from zero-inflated Poisson regression for network size in the 2016 TESS data.

Name generators	Model 1 (all)	Model 2 (imp)	Model 3 (pol)	Model 4 (all)	Model 5 (all)
Count model coefficients					
Political networks	0.07(0.07)			0.04(0.08)	0.09(0.10)
Psychological priming	0.02(0.07)	−0.01(0.08)	0.06(0.12)	−0.02(0.09)	0.02(0.07)
Contextual priming	0.16*(0.07)	0.19 * (0.08)	0.14(0.12)	0.16*(0.07)	0.19*(0.08)
Psychological priming × political networks				0.08(0.13)	
Contextual priming × political networks					−0.05(0.13)
Inflation model coefficients					
Political networks	1.66**(0.52)			14.38**(4.36)	1.15(0.77)
Psychological priming	2.40**(0.70)	7.10(4.79)	1.53(1.24)	14.93**(4.22)	2.49**(0.71)
Contextual priming	−1.22(0.78)	−4.61 + (2.45)	−0.60(1.08)	−1.17 + (0.68)	−1.98*(0.95)
Psychological priming × political networks				−13.15**(4.47)	
Contextual priming × political networks					1.16(1.10)
Observations	1055	526	529	1055	1055

Note: A list of controls include female, age, education, race, marital status, working status, household income, household size, and living in metro area. Standard errors in parentheses (+p < 0.1, *p < 0.05, **p < 0.01).

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