Name generation in interpersonal political network data: Results from a series of experiments

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ABSTRACT

We present results from three large scale survey experiments focused on the manipulation of political name generators. Using syntax that is widely employed outside of political science, we generate interpersonal political network data by varying the roles of alters, the time horizons of relationships, and the specific political nature of social exchanges. Across varying samples and electoral environments, we look for differences in these conditions on a wide range of common interpersonal network items, assess latency data on these treatments, and employ more detailed information on named discussants than most existing political ego-centric studies. We evaluate how well the now standard “compound” political name generator captures interpersonal political networks, finding that it does quite well save a few items of significant political importance. We discuss the implications of this research agenda for theories of social influence and the study of disagreement in democratic politics.

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1. Introduction

The use of name generators to populate ego-centric networks is well established in the study of political behavior. Drawing on previous work (e.g., Laumann, 1966, 1973), the political name generator dates to the pioneering efforts of Huckfeldt and Sprague (1995) and their 1984 South Bend Study. In political science over the past twenty-five years there has been relatively little deviation from Huckfeldt and Sprague's approach, though select studies have adopted the General Social Survey's “important matters” prompt.

While existing studies have evaluated the extent to which this political name generator produces differences in the composition of networks compared to an important matters generator (e.g., Klofstad et al., 2009),\textsuperscript{1} there has as of yet been no experimentation within political name generation. That is, while slightly different political name generators have been used in political science surveys, there has been no systematic comparison of their outcomes, and there has been no controlled experimentation among political network generators. Therefore, we ask the following: How representative is the standard political name generator? Does it return a good sample of people's political networks, or does it mask systematic biases?

Our goals in this paper are twofold. First, we draw on the broader ego-centric, interpersonal network literature from across the social sciences to introduce to political science a vocabulary for discussing the components of name generators. Second, we begin the work of assessing how name generators may influence our vision and understanding of political networks, presenting the results from three large scale survey experiments manipulating name generator texts. Across the studies, we examine several key sources of variance: the roles of alters, the time horizons of relationships, and the specific political nature of the exchanges. The network components we examine as outcomes, including size, density, disagreement and expertise, are diverse, covering the multitude of variables with which researchers have long concerned themselves, as well as others that have not received wide airing.

Given these dimensions of concern, we argue that the widely used, “compound” political name generator provides a valid sample of political networks, though we note a few important exceptions. A compound name generator, a concept we develop below in detail, simply asks respondents to name networks that combine more than one relationship basis or form. We discuss the implications of these findings, and present a future research agenda for the study of political networks through name generation. In particular, we argue that investigations of social networks in political life could benefit from more precisely targeted instructions for how research participants elaborate their political networks. Theories of social influence harbor competing assumptions about the nature of the

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\textsuperscript{1} Indeed, Huckfeldt and Sprague's Indianapolis-St. Louis Election Study (1996–1997) randomly assigned respondents to either a political or an important matters generator.
ties between egos and alters; gathering particularized networks that enable explicit tests of these assumptions could facilitate theoretical advances.

2. The nature of name generation

Although there are many ways to capture a social network (see, e.g., Bernard et al., 1990; Christakis and Fowler, 2011; Fu, 2005; McCarty et al., 1997), one of the most common ways has been the name generator used to direct respondents. First employed in the social sciences by Laumann (1966, 1973), name generators have taken on four basic forms based on the nature of the social tie that is most important to the researcher. Though sometimes described with different terms (Bernard et al., 1990), Marin and Hampton (2007) characterize generators as asking for ties based on interaction, role relations, affection, and exchange (see also Milardo, 1988; Van der Poel, 1993). Perhaps the best known social network name generator is Burt’s (1984) – adopted for use in the General Social Survey – which asks for people with whom the ego discusses important matters. This is a clear example of an interaction-based approach, asking for the subset of the network with which the ego has had contact, usually over some specified period of time (Milardo, 1988). Another subset can be captured by asking for specific roles that alters may fill, such as coworkers, neighbors, friends, or family (Campbell and Lee, 1992; Feld, 1984). Other sub-networks can be characterized by some term of affection, such as closeness (Antonacci, 1986; Wellman, 1979).

Lastly, other pieces of the personal network can be generated by asking about a particular exchange, whether of goods or information. For instance, exchange-based generators trying to capture a social support network might ask for alters from whom the ego might borrow a cup of sugar or one thousand dollars (Marin and Hampton, 2007; McAllister and Fischer, 1978; Wellman and Wortley, 1990).

These efforts have been subject to challenges. Each tack is an imperfect measure of the overall social (“global”) network – or even just the full social support network – though some, especially interaction generators, perform better in representing the full support network than others (Marin and Hampton, 2007).2 In a way, this relates to an important, ongoing debate about whether name generators capture the absence of a network – the proportion of GSS respondents claiming to have zero “important matters” discussants. Comparing the 1985 and 2004 GSS studies, McPherson et al. (2006) argue that America is becoming more socially isolated (based on nearly a quarter of 2004 respondents naming no discussants, versus less than 10 percent of respondents in 1985). Fischer (2009) questions this portrait of the public, arguing that the high portion of 2004 “zeroes” is likely due to either respondent fatigue or technical error. One additional possibility, we will argue, is that specific reminders are necessary for respondents to fully elaborate their networks.

Validity is also at stake in various approaches. Many of the terms employed can have different meanings to different populations. For instance, Bearman and Parigi (2004) famously found that “important matters” included such seemingly unimportant topics as cloning headless frogs. Even the more expected examples people provide bear little obvious connection to instrumental ends with which important matters networks are often correlated. Other studies indicate that interaction-based generators are often invalid measures of actual contact since they suffer from recall problems if the time period specified is long or vague, as in “a typical day” (Marin and Hampton, 2007). Role-based name generators also can suffer in terms of concept validity, as the definition of such a common role as “friend” can vary across groups (Burt, 1983). That said, location or organization-based roles may be less problematic because they are less subjective. And some work has been more reassuring – in one study a significant variation in the wording of the important matters generator (the change to “significant people”) and the inclusion of a probe for negative encounters produced little shift in resultant networks (Strait, 2000).

Marin and Hampton (2007) argue that exchange-based generators may suffer the fewest validity and reliability problems, given that the role is often quite concrete (e.g., borrowing a cup of sugar). However, exchange networks can suffer from the same problems as the others. For one, exchange networks can be unstable as payoffs become biased over time, though this may not have to do with the name generator itself (Dogan et al., 2009). And, certain kinds of exchanges can be quite variable in their interpretation as well. For example, in survey after survey, clergy understand the term “political” to be quite narrow in definition, including only candidates, parties, and elections; these religious leaders distinguish issues like abortion, environmental protection, and gay rights – which academics would certainly define as political – preferring to give these a “social” label (e.g., Djupe and Gilbert, 2003).

3. Capturing political networks

The text of the political name generator has changed very little from that first implemented by Huckfeldt and Sprague in 1984 in their South Bend Study:

We are interested in the sort of political information and opinions people get from each other. Can you give me the first names of the three people you talk with most about the events of the past election year? These people might be from your family, from work, from the neighborhood, from church, from some other organization you belong to, or they might be from somewhere else.

Huckfeldt and Sprague’s 1996 Indianapolis-St. Louis study randomly assigned respondents to either the important matters or a political matters generator, the wording of which is as follows:

From time to time, people discuss [government, elections and politics/important matters] with other people. I’d like to know the people you talk with about these matters. These people might or might not be relatives.

That same text was also employed in the 2000 American National Election Study (and several others since). However, the wording was changed for the 2008–09 ANES Panel Study to a two-part question:

During the last six months, did you talk with anyone face-to-face, on the phone, by email, or in any other way about government or elections, or did you not do this with anyone during the last six months? [If so] What are the first names of the people who you talked with about government or elections during the past six months? Please be sure not to type the same name for two different people. If two people have the same name, please be sure to type two different names below, like “John” and “John Junior” or “older John” and “younger John.”

We focus our attention on the character of these generators, and they were quite well-designed. With limited space for social network batteries in political surveys – surveys in which questions conceptually close to government, parties, candidates, and issues are prized – Huckfeldt and Sprague designed a compound name

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2 By full social support network, we mean the set of alters that provides “emotional aid, instrumental aid, and companionship” (Marin and Hampton, 2007:168).
generator combining interaction, exchange, and role attributes.\(^3\) The generator asks about interaction (discussion), interaction concerning a particular topic (the exchange), and lists the roles that may be included. Thus, these compound generators arguably provide facially valid samples of a person’s political network, given that they ask respondents to consider most of the sources of variance for name generators. The exception among the generators described above is from the 2008–09 ANES, which deleted references to the various roles that alters may fill, though it did remind respondents about various modes of contact with discussants.

If most existing political name generators have face validity, what kinds of networks do they return? Like most users of these name generators, we presume that they return the core (as does the important matters generator [Marsden, 1987]). The networks from the generators listed above are intimate; the highest percentages come from the family and workplace, and thus have a high degree of exposure and interaction (Mutz, 2006). Frequency of discussion rates in these networks are modest, averaging “sometimes talking politics” (2000 ANES, authors’ calculation). They are mostly agreeable in the sense of sharing the same presidential vote choice. The likelihood of exposure to at least one disagreeable discussant grows with the size of the network (Huckfeldt et al., 2004). Only about 30 percent of networks have an oppositional political discussant (Mutz, 2006), though the figure rises to 60 percent if that discussant is “not in agreement” (Huckfeldt et al., 2004).

There have been a few attempts to assess whether political networks are different from important matters networks. By all accounts, the differences are quite modest (Huckfeldt and Mendez, 2008; Huckfeldt and Sprague, 1995), leading Klofstad et al. (2009) to conclude that Americans (at least) do not select core discussion partners on political factors, whether rooted in agreement or expertise (see also Huckfeldt and Sprague, 1995). Djue and Sokhey (2008) find that important matters networks are slightly less populated by political experts, and that they have more family and thus stronger ties. On the other hand, Huckfeldt et al. (1998) show quite dramatic differences in how the accessibility of preferences manifest from political versus important matters discussants.

Only one large-scale study focused on mass political behavior has implemented a “multiplex” name generator setup: the American component of the 1992 Cross-National Election Project (Beck et al., 1992).\(^4\) The 1992 CNEP used the important matters generator (Burt, 1984) for the first 4 discussants, and then an explicitly political generator for a fifth name. Investigations of the resulting networks find that even after fully stocking the important matters network, most respondents are able to provide an explicitly political discussant (Beck et al., 2002). Thus, while there may be substantial overlap between political and important matters networks, there is not complete overlap (Klofstad et al., 2009).

While the comparison with important matters networks has provided an essential point of contrast, questions remain about three key potential sources of variance. First, we are concerned with how different time periods bound informant recall, recognizing that the last few weeks of a campaign are often unlike what occurs up to that point. At issue, too, is the debate about whether short term recall is more reliable (for a summary, see Bernard et al., 1984), whether people more accurately recall patterns over a longer time frame (Freeman and Romney, 1987; Freeman et al., 1987), or whether recalling long term association is tightly linked to short term contact (Eagle et al., 2009). Without a controlled comparison, we are concerned about the potential for a different pattern of contact to be revealed from a short (two week) versus longer (six month) temporal window. However, following Eagle et al. (2009), we do not expect to find great differences between standard and shorter recall periods.

Second, a growing line of political networks research has been concerned with discussant roles, mostly because of the potential for the concentration of other salient political attributes within them. For example, coworkers are thought to be more politically diverse than church members (Mutz and Mondak, 2006).\(^5\) This research is based on the standard, compound generator (e.g., 2000 ANES). And while a diverse set of roles is present in such data,\(^6\) we do not know much about what dedicated political role networks look like, and whether they differ from the standard core network beyond simply having a higher concentration of the specified role.

Finally, and third, we suspect that the compound generator undercounts social experiences with disagreement. Experience with disagreement occupies a prominent place in the political science literature on social networks. Facing disagreement may drive down rates of political discussion (Huckfeldt and Sprague, 1995; Mutz, 2006) and political participation (McClurg, 2006; Mutz, 2006; but see Scheufele et al., 2004), though it may supply a number of public goods including higher opinion quality and greater tolerance for minorities (Mutz, 2006). That is, disagreement in social networks might be considered an example of everyday deliberation (Conover et al., 2002; MacKuen, 1990), which may work to counter elite manipulation (Druckman and Nelson, 2003).

If the standard compound generator returns the core, it is likely to be sparsely populated with disagreement, whether because of selection or social pressure that erodes the Burr. And, this is what previous research has found (see Mutz, 2006), depending on the measurement scheme (Huckfeldt et al., 2004). Surely individuals have greater exposure to disagreement, but both structural and psychological reasons undercut reporting. People may actually be surrounded by the like-minded and they may project that expectation on their discussants, but there is also motivation to forget disagreement (Huckfeldt and Sprague, 1988). With explicit prompting, citizens may be able to recall more interactions with people with whom they disagree.

4. Exchange, role, and disagreement generators

In what follows, we explore a wealth of different political name generators, each focusing on a different aspect of the compound political name generator in broad use since 1984. We present the results from a series of name generator experiments embedded in surveys administered across multiple years and locations: (1) a statewide, internet survey of Florida registered voters conducted in October, 2010; (2) a statewide, internet survey of Colorado registered voters conducted in August, 2010; and, (3) a mail survey of registered voters conducted during the 2008 presidential election in Franklin County (Columbus), Ohio. In each experiment, we conceive the control condition to be the standard compound political name generator (e.g., 2000 ANES) given our goal of testing how variations may shift the resultant networks away from a facially valid generator. The treatments are the different generators to which respondents were randomly assigned (2 in the case of the Florida

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3 To be clear, we would argue that the important matters generator is also “compound,” combining multiple attributes (interaction and exchange).

4 Following standard usage, by multiplex generator approach, we mean the use of more than one name generator for the same respondents.

5 However, church members are also more diverse in terms of their levels of political expertise, which may help to distribute expertise throughout the population (Djue and Sokhey, 2008).

6 A problem with numerous, existing social network batteries is that they do not allow multiple listings of roles. For instance, the 2000 ANES only collects coworker, church member, and neighbor statuses for non-spouses and non-relatives, which serves to drastically undercount the presence of these roles.
study; 3 in the Colorado study; and 6 in the Columbus study). Table 1 describes the treatments and their administration.

All three studies vary the time horizon for the standard political name generator, and all three include a different form of exchange than has been considered previously: disagreement. Two studies included a question specifying a particular target for discussion: the presidential candidates (or potential nominees). And, the Columbus community study was the only one to specify three different roles for respondents to consider when reporting interpersonal political networks: those discusants outside the family, those from the workplace, and those from the church.

Across all three survey-experiments we compare the networks resulting from the experimental generators to the control condition on a host of indicators concerning the nature of the exchange and interaction, the roles occupied by network members, and some new measures gauging the ego’s orientation toward her alters. Additionally, in the two internet-based studies, we compare response latencies timing discusant name generation to gain insight into the accessibility of network information based on the psychology of the survey response (see also Huckfeldt et al., 1998, 1999).

By examining discusant and network characteristics across generators, our experiments shed light on social information seeking and the nature of political discussion networks in mass publics. We consider whether core networks of strongly tied, social intimates are general purpose and look the same across prompts, or whether they are constructed differently by individuals based on particular political tasks.

5. Data and methods

For the two statewide surveys – the Florida Voter Poll (FVP) and the Colorado Voter Poll (CVP) – respondents were randomly selected from the state voter file and sent an invitation letter by mail. The letter contained a link and an access code for the online poll, which was programmed in Qualtrics. Both studies were conducted in the week before the relevant Election day – November 2, 2010 in the case of the Florida study, and August 10, 2010 in the case of the Colorado study (the survey link expired at midnight the night before the Election day). As these were distinct electoral contexts, the sampling was slightly different across the studies: the FVP was intended to predict the 2010 midterm general election in Florida (and thus to capture the general, midterm electorate), and the CVP to predict the 2010 midterm primaries in Colorado (thus, the primary electorate).

Generally, the polls performed well in predicting outcomes despite expected low-response rates; indeed a secondary goal of these studies was to test out alternative techniques for pre-election polling (Barber et al., 2010). Of course, here we are more interested in the internal validity leveraged through embedding experimental conditions. The FVP and CVP are largely replications of one another, thus comparing them provides important variance on several dimensions, notably types of electorates and the distinct, ambient information environments that come with varying electoral institutions (i.e., primaries versus general elections).

The Columbus study was a county-wide mail survey experiment conducted in October, 2008. The authors sent 8 randomly selected groups of 1000 voters drawn from the Franklin County voter registration file different types of name generators, varying the network prompt based on role, exchanges, and time horizons. The Columbus study provides tests of additional political name generator conditions and a third variant relative to the other two studies: the ambient information environment of a presidential election in a battleground state.

It is highly likely that sampling from the voter file and surveying close to elections in all three of our cases increases the estimated size of political networks and the frequency of interaction within

Table 1

Name generator conditions in three large-scale survey experimental studies.

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<td>[Control] From time to time, people discuss government, elections, and politics. Looking back over the last few months, we would like to know the people you talked with about these matters. These people might be relatives, spouses, friends, or acquaintances. Please think of the first three people that come to mind and answer each question for each person. We will not record the names of the people you list below. Note: The Columbus study was conducted by mail (as respondents could see the number of spaces for names in the survey booklet, the prompt noted this). In the cases of Colorado and Florida (internet mode), respondents were not told how many names to generate (names were solicited one at a time): “Please think of the first person that comes to mind, and enter their initials in the space below.” After listing a name, respondents were asked “Can you think of anyone else?” (this process was repeated for up to 4 names) #1: … Looking back over the last week or two, we would like to know the people you talked with about these matters… #2: … Looking back over the last few months, we would like to know about the people you disagreed with about these matters… #3: From time to time, people discuss the candidates for president. … (in the case of the Colorado study, the object is either the Republican or Democratic primary candidates, tailored to the respondent based on her previous answers on party identification) #4: … Looking back over the last few months, we would like to know the people you talked with about these matters outside of family members… #5: … Looking back over the last few months, we would like to know about the people from your work that you talked with about these matters… #6: … Looking back over the last few months, we would like to know about the people from your church that you talked with about these matters…</td>
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7 In the Colorado study, the candidate-discussion target treatment was tailored to party affiliation as reported by the respondent. For example, if an individual was randomly selected to receive this name generator and had described herself as a Republican earlier in the survey, the target of discussion was the “Republican nominee.” If a respondent did not report an affiliation, the target in this treatment became “local political matters.”

8 Probability Proportionate to Size (PPS) sampling was used to select the samples. Probabilities of voting used in the PPS samples were estimated using multivariate logistic regressions, and employed information available in the voter file to produce a probability of voting for each individual voter. More information on the methodology and accuracy of the samples is available from the authors (see also the work of the collaborators on the studies – Barber et al., 2010). The response rates across the 7 conditions (see Table 1 for the list) were as follows: last two weeks (5.9), control (6.1), church (7.7), coworkers (7.9), non-family (8.6), candidate-specific (14.4), and disagreement (15.8).
them (see Hucklefeld et al., 2004; Klofstad et al., 2009). But, this is precisely why we and others in the literature made these choices – to gain access to political networks when they are proximate to a salient political decision.

In all three studies, we checked to make sure that randomization to conditions worked effectively. In the two statewide survey experiments, there were no statistically significant differences across treatment conditions with respect to major demographics or political characteristics – this is not surprising, as respondents were not presented with a randomly generated version of the network battery until they reached that sequence in the online survey.10 For the Columbus study, because mail questionnaires were sent to respondents, there is a greater possibility for breaks in randomized assignment to condition due to non- and/ or selective response. Thus in our analyses, we control for the characteristics for which we found statistically significant, though substantively minor differences (for the list of covariates, please see the notes in the bottom of the table reported in Appendix B).11 It is also worth noting that in all three studies we include people who report having no political discussants (regardless of treatment condition) only when examining network size; otherwise results are restricted to individuals who name at least one alter.

6. Results

6.1. Florida and Colorado, 2010

In Fig. 1, we present the results of the Florida and Colorado survey experiments with respect to network size, interaction, and exchange (we first discuss these two studies together because of their similarity in design). In all graphs, the control condition (the 2000 ANES language) is represented with a white bar. All effects across conditions are network averages/proportions, and asterisks represent effects that are statistically significant from the control condition at the p < .05 level (please see Appendix B for the actual estimates from the regressions).

The Florida study tested two name generators against the control. One generator condition asked about discussion in the last two weeks, while the other asked for those with whom the respondent disagrees on political matters. The Colorado study also used these two conditions, but added a third condition to target a specific political exchange – some Coloradans were randomly assigned a generator that asked for alters with whom they had discussed their party’s primary candidates.

We begin with a discussion of the absence of networks in these studies (see Table 2). This references the ongoing debate about the extent to which Americans lack adequate social and, here, political support from other citizens. Fischer (2009) claims that the 2004 GSS over-reports the extent to which Americans lack important matters network ties – that 25 percent is much too high (see McPherson et al., 2006). Both the Florida and Colorado studies were much shorter than the GSS,12 did not ask high, highly intrusive batteries of

<table>
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<th>Table 2</th>
<th>The percent of respondents failing to name any discussants by name generator and state of study.</th>
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<td></td>
<td>All</td>
</tr>
<tr>
<td>Colorado</td>
<td>23.8</td>
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<td>Florida</td>
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Questions before the network battery, and did not prompt respondents for the specific number of discussants sought.13 The results in Table 2 are largely consistent across the two studies and come very close to the 2004 GSS estimate (as well as the 2008–09 ANES panel – conducted online – where approximately 20 percent of respondents name 0 discussants [authors’ calculation]). The samples average just under one-quarter of respondents with no political discussion partner, though the proportions shift by treatment. In Colorado, a logistic regression predicting the naming of zero discussants indicates that those administered the disagreement generator were 13 percent more likely to name zero discussants relative to the control (p < .01) (full results not reported); in the Florida study those receiving the disagreement treatment were 18 percent more likely to name zero discussants relative to the control (p < .01), while those receiving the short recall period prompt were 11 percent less likely to name zero discussants relative to the control (though at the p < .1 level) (full results not reported). We are not able to comment on whether a quarter of these two state-wide populations truly have no political discussion partners, or whether these results are more about some kind of consistency in survey item non-response. However, the general congruence of our results with the 2004 GSS and the 2008–09 ANES lends plausibility to the idea that an emerging, nationwide pattern is atfoot.

In the panels of Fig. 1 (“size, interaction and exchange”), we begin with a comparison of network size, noting that the networks gathered by the control in the Florida and Colorado studies (about 1.5 discussants, on average) are a bit smaller compared to other omnibus studies such as the 2000 ANES (in which the average network had 1.8 members [authors’ calculation]).14 All name generators produced similarly sized networks to the control except that disagreeable networks were significantly smaller by about .5 discussants in both states. It is important to note that in both states – spanning two distinct electorates, and two distinct electoral environments – respondents averaged about one discussion partner with whom they disagreed. This would seem to contradict reports that Americans are exposed to little disagreement as captured through the standard, political name generator (our control condition; Mutz, 2006).

In both states, networks generated around disagreement also show some evidence of different levels of interaction and exchange. That is, relative to the standard political name generator, disagreeable networks hosted less discussion, a smaller portion of expert discussants, and, of course, more disagreement (a manipulation check). What is perhaps most remarkable is that these differences, while statistically significant, are not larger. That is, disagreeable networks have nearly the same portion of experts and are host

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10 For more information on randomization functions in Qualtrics, please see: http://un.php.net/manual/en/function.rand.php. In the Colorado study, 113 of 1142 respondents completing the survey were assigned to one of the 4 network conditions (i.e., a control battery plus three treatment batteries; a handful of respondents were not assigned to a network condition due to a coding glitch). In the Florida study, 303 of the 433 respondents were assigned to one of three network conditions (i.e., a control battery plus two treatment batteries; 130 respondents were not involved in the survey experiment, and therefore received no battery).

11 We examined balance across conditions and found significant, if substantively small, differences on political interest (p = .06), partisanship (p = .08), age (p = .01), religious guidance (p = .01), and church attendance (p = .01). Therefore, we included these covariates as controls in the models reported in Appendix B.

12 The median length of an interview in the GSS series is about 1.5h (www.ropercenter.uconn.edu/data_access). Average completion time in the Colorado study was approximately 14 min; average completion time in the Florida study was approximately 17 min.

13 Though common in many network surveys, this can be problematic in that it primes respondents to provide the specified number of alters; we also speculate that it could potentially tip off respondents that a large number of questions will follow the naming of discussants.

14 The 2000 ANES battery (and other existing batteries) was not specifically to assess (internet) mode effects, but this is surely a point of interest in the study of political networks, as more scholars move to web-based instruments.

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to relatively frequent interactions. In other words, we find that when asked, people can identify not just someone with whom they have regular disagreeable interactions, but relatively high quality disagreeable interactions.

By the same token, one other result regarding disagreement from the Colorado study is worth mentioning. Networks formed around discussion of the primary candidates hosted significantly lower levels of average disagreement relative to the control battery. However, it is notable that the frequency of disagreement was not “never” in this condition. This finding offers comment on the oft-used strategy of measuring disagreement through a simple comparison of partisanship in the network. Given that intra-party primary contests have limited appeal beyond the party base, we cannot assume that sharing the same partisanship means that a relationship does not still experience disagreement (Sokhey, 2009).

The panels of Fig. 2 display evidence concerning whether equivalent roles populate the different network conditions (the bars represent the mean portion of the network composed of discussants holding various roles relative to the ego). The evidence varies a bit across the two states. In Florida, the three networks are quite similar – the one exception is that spouses are unlikely to be named in disagreeable networks, which fits in a long line of research on mate selection and political choices (e.g., Alford et al., 2011; Eaves and Hatemi, 2008; Martin et al., 1986). In Colorado, there is a bit more variation, primarily showcasing how disagreeable networks look different from other types. Disagreeable networks are less likely to contain a church member, more likely to contain cantankerous Uncle Joe (a relative), and less likely to contain a spouse. The other differences that emerge suggest that neighbors – who are relatively rare in these networks – are more common when the prompt asks for alters based on discussions held in the last two weeks, and for alters based on a salient political target (primary candidates). At this point we can only speculate as to why we see differences in the proportions of alters fulfilling different roles across different prompts in Colorado versus Florida. However, the Colorado findings suggest that individuals can name discussants outside of their core network when prompted and when the political environment provides fodder for engaging peripheral discussion partners. Disagreement means different things in the context of a primary election, and an electoral context with less ambient information may mean turning to other people for specific political information, a point we will return to in the discussion section below.

Fig. 1. Effects of name generator treatments in Florida and Colorado on network size, interaction, and exchange. (A) Florida (October 2010); (B) Colorado (August 2010). Note: Frequency of discussion and disagreement range 1–4; network size range 0–4; expertise range 1–3. Bars are means estimated with OLS regression coefficients (see Appendix B for estimates). The ticked axis is the network mean score for the variable on the opposing axis, for each treatment (bars). *p < .05, and denotes an effect that is statistically significant from the control condition in a regression model.
Fig. 2. Effects of name generator treatments in Florida and Colorado on discussant roles. (A) Florida (October 2010); (B) Colorado (August 2010). Note: All measures range from 0 to 1. Bars are means estimated with OLS regression coefficients (see Appendix B for estimates). The ticked axis is the network mean score for the variable on the opposing axis, for each treatment (bars). *p < .05, and denotes an effect that is statistically significant from the control condition in a regression model.

Fig. 3. Effects of name generator treatments in Florida and Colorado on discussion initiation. (A) Florida (October 2010); (B) Colorado (August 2010). Note: All measures range from 0 to 1. Bars are means estimated with OLS regression coefficients (see Appendix B for estimates). The ticked axis is the network mean score for the variable on the opposing axis, for each treatment (bars). *p < .05, and denotes an effect that is statistically significant from the control condition in a regression model.
recently reigned supreme (e.g., Huckfeldt et al., 2004; McClurg, 2006; Mutz, 2006).

6.2. Columbus, OH, 2008

Fig. 4 presents the same results for the Columbus survey-experiment. Recall from Table 1 that the Columbus study tested 6 name generator conditions compared to the control language. In addition to those used in the Colorado study (two week recall; candidate-specific; disagreement), the Columbus study also specified a set of roles for alters to fill when asking for network names: church members, coworkers, and non-family members. We tested the same set of network variables as in Figs. 1–3. The estimates come from regression models that include a set of controls, as there were some covariate differences across treatment groups (given differences in responses across treatment mailings). The bars present estimates when the controls are held at their mean values, and asterisks represent effects that were statistically significant from the control condition at the p < .05 level (please see Appendix B for the model estimates).

In panel A of Fig. 4, we see an immediate difference in network sizes when compared to the previous studies (about 2.5 discussants compared to 1.5). All of the networks are similarly sized except for the church and coworker-generated networks – this is, in part, a function of selective participation in religious institutions and the workplace. Notable in these data is that explicitly disagreeable networks are similarly sized to both the control condition and two week/recent recall period prompt. Moreover, special purpose political networks (the candidate-specific prompt) are also just as extensive as those generated by the compound (control) generator.

Though there are significant differences relative to the control condition, discussion rates, disagreement rates, and average levels of expertise, the substantive differences are not large. Church, disagreement-based, and non-family networks host statistically significantly less discussion relative to the standard name generator, though probably for different reasons. Only disagreeable networks have statistically higher rates of disagreement (again, a useful manipulation check), and expertise rates are functionally equivalent to the control condition for all networks save the church and disagreeable networks, which yield networks with a lower portion of political expertise. Thus, socially supplied political goods can be found across a wide range of role, exchange, and interaction-generated networks, though the population of certain role-based networks is limited by participation in contexts where those roles are found.

To examine this further, we also compared the pools of role-based discussants generated from the control condition to those yielded from relevant treatments (e.g., comparing coworkers from the control generator to discussants from the coworker name generator); we looked for differences on discussion rates, disagreement, and perceived expertise (results not shown). Discussants from the coworker treatment do not differ from coworkers from the control, while non-family member discussants from the treatment are talked with less often than non-family members generated in the control condition. Church members from the church treatment are talked to less often and are thought to be less expert than church members from the control (though they are equally agreeable). Thus, by and large, the compound, control name generator performs quite well in returning equivalent population samples of discussant characteristics from particular roles.

Panel B of Fig. 4 shows that there is considerable overlap in roles across the 6 treatments and the control condition. Neighbors can be found in church member networks, friends across all role-based network prompts, and coworker networks are populated with alters in multiple other roles. One important conclusion from these findings is that it would be unwise to think of roles as tightly partitioned, and surveys should allow multiple role designations for alters. On this point, most of the variation we observe is sensible: for instance, a higher portion of coworkers inhabit coworker, friend, and non-family defined networks.

What is notable is that the role composition of special purpose exchange networks (i.e., those based around the discussion of candidates), short recall period networks, and disagreeable networks are not statistically distinguishable from the control condition. This jells with the findings from the Florida study, and suggests that whether in the mass public or in select, primary electorates, special-purpose networks are located in the same social spaces as general purpose networks.

In panel C, we return to the items gauging discussion initiation and conflict avoidance. In these data, there are no differences in rates of balanced ("we both do") networks across the 7 name generators, though the same patterns appear in rates of imbalance as in the two statewide studies. It is more common for alters to initiate political discussion in disagreement-based, coworker and non-family networks; only in disagreeable networks are ego more likely to report being conflict avoidant with a statistically distinguishable portion of the network (relative to the control condition). These results reinforce the notion that networks can vary dramatically when it comes to how discussion partners actually interact.

6.3. Response latencies

Lastly, we are able to report on the accessibility of different types of political networks as measured by the length of time it took respondents to name their networks (see Bassili, 1993; Huckfeldt et al., 1998, 1999). The dependent variable is the average number of seconds it took a respondent to name a network member, the independent variables are the conditions, and predicted counts (of seconds) are presented by the bars (asterisks represent estimates that are statistically significant from the control condition at the p < .05 level; raw estimates are presented in Appendix B). The results, displayed in Fig. 5 for both the Florida and Colorado studies (these data could not be generated in the Columbus study given the mail mode), demonstrate that it was more difficult for respondents to populate certain types of networks. In Colorado, the average time respondents took to name a network member to discuss the primary candidates was a few seconds longer relative to the standard political name generator; this expanded to roughly a dozen extra

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15 While a number of causes could be responsible for this, a likely culprit is the survey mode. In the Columbus study, respondents were presented with a set of boxes, and could name all respondents at the same time before answering questions about each discussant (all on one page). In the statewide online studies, respondents had to give the initials of – and answer questions about – one alter at a time, which is more akin to the way things have been done in CATI conducted surveys (e.g., 1992 CNEP; the half split design of the 2000 ANES). The online format of presenting questions on multiple pages can seem tedious to respondents – a typical problem with network batteries more generally – and likely resulted in the undercounting of network size. While this is unfortunate, an advance of the online format was the ability to assign treatments at the time of the discussion sequence (to cut down on selective and non-response), and to include latency timers. It is notable that online surveys need not be structured in this way (just as CATI surveys can solicit all names first, before asking detailed questions about specific alters), and can resemble the paper format in which questions are asked of all discussion partners at once.

16 Again, these larger network sizes probably cannot be directly compared to those in the other two studies because of the mode. That is, given that the network battery constituted just under half of the survey instrument, those respondents with low numbers of or no discussants likely just opted for survey non-response (instead of skipping many questions – item non-response).

17 Twenty-five percent of respondents in the church member name generator condition indicated never attending church; another 29 percent indicated only attending services “a few times a year,” and, overwhelmingly, did not name a church-member discussant. In contrast, about 80 percent of the co-worker name generator condition sample indicated working full or part time in the past year.
seconds in the case of the disagreeable network treatment relative to the control condition. In the Florida study – which did not have the candidate-specific network treatment – only the disagreeable network took longer to populate than the control (and by about 10 s).

We expect that the reasons differ for the two treatment findings relative to the control condition, and thus the implications of these findings differ. Trying to recall specific discussion topics (e.g., a network with whom you engaged the topic of the candidates) is a more difficult task than recalling a broad class of

![Diagram]

**Fig. 4.** Columbus, OH (Franklin County) survey experiment, Fall 2008. Effects of name generator treatments. (A) Size, interaction, and exchange; (B) discussant roles; (C) discussion initiation and avoidance. For each grouping of bars, the first (top) bar is the control condition, the second “two weeks,” the third “church,” the fourth “disagreement,” the fifth “candidate-specific,” the sixth “non-family,” and the seventh (bottom) “coworkers.” Bars are means estimated with OLS regression coefficients (see Appendix B for estimates), holding the controls of political interest, partisanship, age, religious guidance, and church attendance at their means. The ticked axis is the network mean score for the variable on the opposing axis, for each treatment (bars). *p < .05, and denotes an effect that is statistically significant from the control condition in a regression model. Source: 2008 Columbus, Ohio Study.
conversation (i.e., “politics”). However, surely this is not why disagreeable networks take longer to list; in this case, people may have to engage a broad search of their social experience to find disagreeable partners. Though, again, it is important to note that they do find them. We turn to the implications of these patterns in the next section.

7. Discussion

We pursued these experiments suspecting that theoretical advances can be assisted with the careful articulation of political name generators. For instance, consider three common perspectives about why people engage in political discussion. Downs (1957; see also Huckfeldt, 2001) argues that people use individuals for their heuristic value to subsidize the search for costly political information (especially if those people provide agreeable, expert information). While not addressing political discussions per se, Homans (1958, 1974; see also Huckfeldt and Sprague, 1995: 108) argues that relationships are sustained by an exchange of goods, and that goods need not be the same for each partner in the dyad. McPhee et al. (1963; see also Huckfeldt and Sprague, 1995) suggests that people seek out confirmation for their tentative opinions among their social intimates, re-sampling opinions until they are affirmed.

These three perspectives involve quite different styles of social interaction, quite different priorities for politics in relationships, and distinct relationship-time horizons. In Homans’s balance theory, exchanges in a dyad are shaped by previous exchanges rather than by a one-time payoff that a person extracts from another (as in Downs’ model). In an exchange network, an ego may bring up political topics simply because her alters enjoy politics, thus gaining approval (if not a politically instrumental good). Likewise, in McPhee’s model, people test their opinions to ensure they remain in the group, though perhaps not because they are interested in supplying goods to discussion partners. For Downs, politics is central to the social encounter — social sources of information are sought because the information is instrumental to making a forthcoming decision. Homans might see politics as one among many fungible goods that is exchanged in a particular relationship, whereas McPhee might see opinions that need to

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**Fig. 4.** (Continued) **Fig. 5.** Accessibility and network conditions; latencies on name generator treatments. Bars represent the predicted counts, by condition, from negative binomial model estimates (see Appendix B). *p < .05, and denotes an effect that is statistically significant from the control condition. Source: The Colorado Voter Poll and the Florida Voter Poll.

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be tested as, in a sense, burdensome things that may generate social instability. Furthermore, time plays no major role in Downs’ conception, whereas it is central to the other two, especially to Homans’.

Our measures of network discussion initiation provide a novel look at the dynamics of discussion, as well as the opportunity to test and adjudicate between these classic models of social influence. The finding that conversational balance is the modal condition in many political networks suggests that Homans’ (1958, 1974) perspective of exchange probably has more resonance compared to individually maximizing theories of social information seeking (e.g., Downs, 1957). But even here, the fact that, on average, 40–50 percent of networks are characterized by imbalance raises doubts about the exchange framework. Perhaps individuals’ networks are balanced in a number of respects, and only unbalanced in terms of politics. Still, a finding that potentially large pluralities of political relationships are characterized by imbalance squares with two-step models of information flow (Katz and Lazarsfeld, 1955). The fact that the networks characterized by frequent discussion initiation by alters are also those in which the egos are most conflict avoidant reinforces a conception of “elite-driven” diffusion and probably not a sense that experts are sought instrumentally.

8. Conclusion

In this paper, we decompose the standard, compound political name generator to examine the assumptions about the breadth and depth of citizens’ political networks. Drawing on a generation of studies of other ego-centric networks, we offer a standardized lexicon by which to assess the kinds of relationships that may be of interest to political networks scholars (roles, interaction, and exchange), and then gather these specific networks in the mass public. Though previous investigations have looked for differences between political and important matters networks, to our knowledge none have explicitly compared different kinds of political name generators (and certainly not via random assignment). We also introduce several new measures of political network interaction – dyad-level conflict avoidance and discussion balance – while repeating the past usage of response latencies (e.g., Huckfeldt et al., 1998). We find some variation in network composition across name generators, though for most treatments that variation from the control name generator condition is not substantial. Thus, as we close we need to make sense of both the similarities and differences.

We can begin by noting what these data cannot tell us. We compare network averages generated by the name generator treatments, not the specific personas that are named. This choice obscures several important points regarding equivalent networks. It is possible that variant name generators simply return the same people who happen to be good political “utility players.” But it is also possible that citizens list different discussants across these generators who, together, happen to constitute equivalent networks. Lastly, it is possible that people can construct equivalent networks from almost anywhere they look for them. These are not inconsequential differences, since the first draws on the capacities of the average individuals in networks, the second on individual compliance behavior, and the third draws on the carrying capacity of society and the context dependence of networks. Our depiction of political networks changes dramatically – especially in terms of size – if people are listing different or similar people across network prompts. But these possibilities also highlight the potential that individuals are immune to our prompts, and repeat a core with nominal differences in how they are represented.

We cannot sort out this particular problem since we did not employ a multiplex generator design. And, this is what we recommend for future research in order to nail down the intersection of these sets of networks – providing individuals with multiple generators will allow us to compare discussants explicitly, and allow comment on the efficacy of the generators themselves. In any event, these qualifications about what these data cannot tell us should be kept in mind while reading the following conclusions.

A conclusion we support is that the compound name generator (our control) does a solid job of sampling political networks. The proportions of the various roles asked about as well as the forms and content of political interactions, are generally equivalent between the control and more specific networks. Based on this evidence, the standard name generator is a highly efficient survey tool and an unbiased way to depict interpersonal political networks. Of course, we are not comparing results from the compound generator to a measure of the full network (which would be ideal), but to networks of varying prompts. Thus, while it is possible that the control and treatments are all biased, at least they appear to be tapping the underlying network in similar ways.

This bears on a number of important points, including ongoing debates about informant recall (Bernard et al., 1984; Freeman et al., 1987) and concerns in political science for capturing information close to a particular decision. Here, the short term recall prompts generated largely equivalent networks to the standard, control generator. Though we cannot discern whether the long-term prompt is biased by short term contact (Eagle et al., 2009), or whether short term contacts mirror long term patterns, that they return equivalent results should be reassuring to researchers using these data for multiple purposes.

That being said, we could reach a different conclusion given the equivalence of the control to most of our treatments. The more specific networks we operationalize through our name generator treatments are roughly equivalent to the compound, indicating that citizens’ true political networks are larger and more varied than we typically estimate with standard practice. That is, respondents have more coworker discussants, more church member discussants, and more diverse topical discussion members than we estimate with the standard battery. In addition, since discussion and expertise rates are relatively constant across special purpose and different role networks, we might reasonably conclude that socially supplied political resources are abundant across the many contexts of adult life (see also Klofstad et al., 2009), not just in a few (Mutz and Mondak, 2006).

The consistent exceptions are disagreeable networks and disagreement in networks. In some ways, it seems obvious that the standard political name generator would do a poor job of estimating the degree of political disagreement in the mass public. Using the standard battery suggests little exposure to disagreement, while a simple reminder (in the form of the disagreement name generator) reverses that conclusion: 90 percent of respondents in the Columbus study, 67 percent of respondents in the Colorado sample, and 60 percent of respondents in the Florida survey report at least one disagreeable discussant when assigned this prompt. Given the central theoretical and empirical importance of disagreement in recent political network studies (Huckfeldt et al., 2004; Mutz, 2006), the underrate of exposure to disagreement captured by the compound generator is a potentially serious problem. From one vantage point, hand wringing over the lack of disagreement in citizens’ political networks is probably unnecessary (see also McClurg, 2006), and Americans are receiving healthy doses of everyday deliberation. However, the disagreement name generator is not just a simple methodological solution to an important democratic problem. Fig. 5 indicates that disagreeable networks are also potentially less accessible in memory than those prompted by the standard approach, suggesting that the information and norms
from disagreeable partners may factor less in citizens’ decision-making calculi. Yet at least one point is clear: Americans in these varying samples, embedded in varying electoral contexts, do have access to politically disagreeable alters as long as they are reminded to list them.

These findings also start to raise questions about what it means to disagree about politics (see also Klofstad et al., 2013). Previous investigations have used a variety of measures such as sharing the same partisanship and vote choice, in addition to subjective measures of the frequency of disagreement in political discussion (the latter of which we present in the tables and figures). Fortunately, we do have multiple measures in our studies, and the comparisons are worth noting: in the Columbus study, 66 percent of networks generated based on the disagreement treatment contained at least one discussant who shared the ego’s presidential vote preference; 28 percent of disagreeable networks in Colorado had at least one discussant who shared the ego’s partisanship; in the Florida study, 24 percent of respondents in the disagreement condition named at least one discussant who shared the ego’s partisanship.

Finally, it is worth stressing the fact that the three studies we employed in this paper were carried out in different electoral conditions – one in a primary (Colorado), one in a midterm general election (Florida), and one during the 2008 presidential election (Columbus). The choice of these cases – mostly by convenience – limits the external validity of the results, at least in how the marginals relate to those obtained from national samples (if not in how the treatments function). But, one of the strengths of using these diverse samples is the tantalizing glimpses of interactions between electoral environments and networks. For example, it seems that presidential elections, especially in battleground states (of which Ohio was one in 2008), work to constrain differences across network types. Overwhelming attention by media, candidates, and parties means that ambient political information is plentiful, and that discussion, expertise, and disagreement will vary little across network generators. Less mineering electoral environments allow networks to have greater variance in composition, but surely also reduce engagement levels with those networks (see also Mondak, 1995). Clearly, more attention is needed on what appears to be an important relationship between types of political networks and types of electoral contexts. Researchers may want to choose their network prompt carefully based on the electoral context and question of interest.

To close, we suggest that given the increasing attention in the social sciences to the specification of tight causal mechanisms, political network scholars should not shy away from providing specific directions to survey respondents that bear close concept to questions of interest (Burt, 1997). Scholars studying voting behavior should consider generating networks with specific reference to the vote decision; those interested in the consequences of disagreement may want to prompt disagreement; those looking to better understand political participation might articulate networks oriented around participatory norms or recruitment, or centered in organizations of particular interest (Burns et al., 2001; Djupe and Gilbert, 2009; Verba et al., 1995). For certain kinds of investigations, using a generic “important matters” or the political compound generators may be appropriate. But specifically targeted networks may gain in validity by avoiding WIMTY18 problems, aid in the testing and advancement of theory, and appear not to suffer losses in terms of form and content.

Acknowledgements

We thank Michael Barber, Chris Mann, Quin Monson, and Kelly Patterson for assistance in gathering the data. All errors remain our own.

Appendix A. Variable coding (see Table 1 for name generator wording)

- Items are the same across the Florida (FVP), Colorado (CVP) and Columbus (Franklin County, OH) studies unless otherwise noted:

  Size, interaction, and exchange

  Network frequency of discussion: An averaged index, ranging from 1 to 4, composed of responses for all named discussants to the network battery item asking, “How frequently do you discuss politics with this person?” (1 = never; 2 = not very often; 3 = often; 4 = very often)

  • For the Columbus study, this measure runs 1–3 (3 = very often; 2 = often; 1 = rarely)

  Network frequency of disagreement: An averaged index, ranging from 1 to 4, composed of responses for all named discussants to the network battery item asking, “How frequently do you disagree about politics with this person?” (1 = never; 2 = not very often; 3 = often; 4 = very often)

  • For the Columbus study, this measure runs 1–3 (3 = very often; 2 = often; 1 = rarely)

  Network political expertise: An averaged index, ranging from 1 to 3, composed of responses for all named discussants to the network battery item asking, “Generally speaking, how much would you say this person knows about politics?” (1 = not very much; 2 = some; 3 = a lot)

  Network size: Number of discussants named by respondent, ranging from 0 to 4.

  Discussant roles

  Church, coworkers, neighbor, friend (CO & FC studies only), relative (CO & FC studies only), spouse, female: A averaged measure running 0–1, giving the portion of the network that is composed of each.

  Discussion initiation and avoidance

  Network conflict avoidance: An averaged measure, ranging from 0 to 1, composed of responses for all named discussants to the network battery item asking, “Have you ever felt reluctant to discuss politics with this person?”

  Ego initiation across network: An averaged index, ranging from 0 to 1, indicating the portion of the network for which the main respondent (ego) reports being the one who tends to initiate political conversation. “When you discuss politics, who tends to initiate conversations?”

  Alter initiation across network: An averaged index, ranging from 0 to 1, indicating the portion of the network for which the main respondent (ego) reports that her alter tends to initiate political conversation. “When you discuss politics, who tends to initiate conversations?”

  Balanced initiation across network: An averaged index, ranging from 0 to 1, indicating the portion of the network for which the main respondent (ego) reports that she and the alter tend to initiate political conversation equally. “When you discuss politics, who tends to initiate conversations?”

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18 This acronym stands for “Whatever it means to you,” and is a phrase often used in survey interviewing (e.g., Moore, 2004).

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Appendix B.

Estimates for Florida study (separate model estimates on rows).

<table>
<thead>
<tr>
<th>Dependent vars.</th>
<th>Disagreement treatment indicator</th>
<th>Short recall treatment indicator</th>
<th>Constant</th>
<th>Number of obs.</th>
<th>Adj. $R^2$</th>
<th>$F$ (prob.)</th>
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<td></td>
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<td>Expertise</td>
<td>-.24*** (.10)</td>
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<td>2.92*** (.07)</td>
<td>225</td>
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<td>Discussant roles</td>
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<td>Avoid conflict</td>
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<td>-.04 (.05)</td>
<td>.13*** (.04)</td>
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<td>“We both do”</td>
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<td>-</td>
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Each dependent variable is a network average (save the latency model); each row presents estimates/statistics from a separate model-dependent variables are on rows, independent variables (treatment indicators) are the columns. Standard errors are in parentheses. In each model the control condition is the reference category. All rows are OLS regressions, save the “avg. # seconds” estimates (negative binomial count model).

- $p < .1$
- $p < .05$
- $p < .01$

Estimates for Colorado study (separate model estimates on rows).

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<th>Short recall treatment indicator</th>
<th>Candidates treatment</th>
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<th>Number of obs.</th>
<th>Adj. $R^2$</th>
<th>$F$ (prob.)</th>
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<td>1.16 (.32)</td>
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<td>.00 (.04)</td>
<td>-.04 (.04)</td>
<td>.42*** (.03)</td>
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<td>1.39 (.24)</td>
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<td>.01 (.03)</td>
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<td>.04 (.04)</td>
<td>.37*** (.02)</td>
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<td>23.82 (.00)</td>
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<td>-.03 (.04)</td>
<td>-.05 (.04)</td>
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<td>856</td>
<td>.00</td>
<td>.73 (.54)</td>
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<td>Avoid conflict</td>
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<td>.42*** (.06)</td>
<td>.02 (.05)</td>
<td>.11*** (.06)</td>
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<td>855</td>
<td>-</td>
<td>LR $R^2$: 66.2 (.00)</td>
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</tbody>
</table>

Each dependent variable is a network average (save the latency model); each row presents estimates/statistics from a separate model-dependent variables are on rows, independent variables (treatment indicators) are the columns. Standard errors are in parentheses. In each model the control condition is the reference category. All rows are OLS regressions, save the “avg. # seconds” estimates (negative binomial count model).

- $p < .1$
- $p < .05$
- $p < .01$

Estimates for Columbus study (separate model estimates on rows).

<table>
<thead>
<tr>
<th>Dependent variables</th>
<th>Two weeks</th>
<th>Church</th>
<th>Disagreement</th>
<th>Candidate-specific</th>
<th>Non-family</th>
<th>Coworkers</th>
<th>Compound (constant)</th>
<th>N</th>
<th>Adj R²</th>
<th>F (p)</th>
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<tbody>
<tr>
<td>Expertise</td>
<td>−11 (.07)</td>
<td>−19 (.09)**</td>
<td>−14 (.07)**</td>
<td>−01 (.06)</td>
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<td>−08 (.07)</td>
<td>2.39 (.19)**</td>
<td>597</td>
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<td>2 (.01)</td>
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<tr>
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<td>−01 (.11)</td>
<td>−36 (.08)**</td>
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<td>−00 (.08)</td>
<td>−03 (.08)</td>
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<tr>
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<td>−16 (.09)</td>
<td>−24 (.12)**</td>
<td>−29 (.08)**</td>
<td>.03 (.07)</td>
<td>−25 (.08)**</td>
<td>−16 (.09)</td>
<td>1.51 (.24)**</td>
<td>599</td>
<td>.09</td>
<td>6 (.00)</td>
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<tr>
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<td>−60 (.13)**</td>
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<tr>
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<td>−10 (.04)**</td>
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<td>572 .22 17 (.00)</td>
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<td>−31 (.05)**</td>
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<td>4 (.00)</td>
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<td>“We both do”</td>
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<td>−08 (.06)</td>
<td>−01 (.05)</td>
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<td>.48 (.15)**</td>
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<td>3 (.00)</td>
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<td>−14 (.04)**</td>
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<td>.04 (.13)</td>
<td>598</td>
<td>.01</td>
<td>2 (.05)</td>
</tr>
</tbody>
</table>

Each dependent variable is a network average; each row presents estimates/statistics from a separate model-dependent variables are on rows, independent variables (treatment indicators) are the columns. Standard errors are in parentheses. In each model the control condition is the reference category. All rows are OLS regressions controlling for political interest, partisanship, age, religious guidance, and church attendance (results for controls not shown).

p < .1
p < .05
p < .01

References


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