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Expanding the Conversation: Multiplier Effects From a Deliberative Field Experiment

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Do formal deliberative events influence larger patterns of political discussion and public opinion? Critics argue that only a tiny number of people can participate in any given gathering and that deliberation may not remedy—and may in fact exacerbate—inequalities. We assess these criticisms with an experimental design merging a formal deliberative session with data on participants’ social networks. We conducted a field experiment in which randomly selected constituents attended an online deliberative session with their U.S. Senator. We find that attending the deliberative session dramatically increased interpersonal political discussion on topics relating to the event. Importantly, after an extensive series of moderation checks, we find that no participant/nodal characteristics, or dyadic/network characteristics, conditioned these effects; this provides reassurance that observed, positive spillovers are not limited to certain portions of the citizenry. The results of our study suggest that even relatively small-scale deliberative encounters can have a broader effect in the mass public, and that these events are equal-opportunity multipliers.

Keywords deliberation, social networks, political discussion, field experiment

John Dewey famously pointed out that majority rule is never “merely majority rule.” Appropriating the words of reformist presidential candidate Samuel J. Tilden, Dewey went on to argue that “The means by which a majority comes to be a majority is the more important thing” (quoted in Dewey, 1927, pp. 207–208). Dewey’s argument emphasizes that, unless one denies the possibility of a tyranny of the majority or believes that majority voting represents an unchanging general will, political discussion should not be construed as “mere” talk, to be contrasted with “real” political behavior. Rather, deliberation is a form of political behavior in itself, and indeed a necessary antecedent for warranting the belief that other forms of political behavior (e.g., voting) are serving their democratic function well.¹

Recent efforts to design and encourage new deliberative forums are rooted in the hope that they can improve broader political discussion and public opinion—that is, improve the means by which a majority becomes a majority (Druckman & Nelson, 2003). But,

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critics worry that any purported benefits must be limited by the relatively small number of people who can participate in a given deliberative event (Levine, Fung, & Gastil, 2005, pp. 3–4), and that if anything, deliberation only exacerbates inequalities (Sanders, 1997). Some laboratory studies of small group discussions have concluded that individuals are unlikely to share the novel information that participants acquire (Stasser & Titus, 1985, 2003; Sunstein, 2006). If these criticisms are correct, they suggest that deliberative forums can, at best, have a very limited impact.

Is the impact of deliberation fundamentally limited by individual decisions not to share the novel information participants acquire? What are the social consequences of a structured deliberative event, such as a town hall meeting between legislators and constituents? Empirical work makes a strong case that deliberative events often affect the individuals who participate in them (e.g., Barabas, 2004; Esterling, Neblo, & Lazer, 2011; Fishkin & Luskin, 2005). But formal deliberation may be of less import if its sole impact is on the immediate audience. Indeed, most theories of deliberative democracy envision a more broadly deliberative public sphere, stretching well beyond mini-publics, Deliberative Opinion Polls, and the like (Dryzek, 2011; Habermas, 1996; Mansbridge, 1999; Neblo, 2005; Warren, 2002). Likewise, if the exclusive social impact of political events is through media coverage, the audience may be reduced to mere props (Habermas, 1974). However, if participants share their experiences within their social networks, formal deliberative events could play the role of kindling, increasing the amount and quality of deliberation in the larger democratic public (Gastil, Richards, & Knobloch, 2014). These potential dynamics necessitate that scholars focus attention on whether formal deliberation ramifies out into broader patterns of informal political discussion. Despite the importance of such questions, we are among the first to examine them empirically, and to the best of our knowledge, the first to employ a field experimental design in the service of doing so.

Extant research yields few expectations about diffusion surrounding deliberative events. The “hidden profile” paradigm of group discussion laboratory experiments suggest subjects will focus on their common information, rather than sharing novel, “private” information (e.g., Strasser & Titus, 2003); Sunstein argues this is devastating for deliberation (2006, p. 83).² Of course, laboratory experiments are limited in their ability to capture the dynamics of real-world political discussion, which are usually embedded in long-standing relationships (versus with strangers), involve multiple interactions (versus one time), are on topics chosen by the participants (versus assigned by the researcher), and may not be explicitly instrumental (versus scenarios with a “correct” decision). The literature on interpersonal political discussion networks (e.g., Huckfeldt & Sprague 1995, 2004; for a review, see Sokhey & Djupe, 2011) has never really addressed this question. That said, if anything, the work on social influence in the mass public, contrary to the “hidden profile” literature, lends plausibility to the idea that citizens would share rather than withhold information they have acquired from a novel political event, whether potentially because of sociability (e.g., Eveland, Morey, & Hutchens, 2011; Lyons & Sokhey, 2014), political expertise (Huckfeldt, 2001), persuasion (e.g., Ryan, 2013), social pressure (e.g., Sinclair, 2012), or some other factor.

In short, whether formal deliberation affects broader political communication in the mass public remains an open question. We find that deliberative events can reverberate powerfully beyond the participants themselves via continued discussions within social networks. While structured deliberative events tend to be small in scale, social networks create a multiplier effect, and thus even small-scale deliberation may have relatively broad impacts on public opinion.

To test whether social networks serve as “deliberative multipliers,” we organized a formal deliberative event: We held an online “town hall” with a sitting United States Senator

(Sen. Carl Levin, D-MI) at 7 p.m., on July 28, 2008, and invited more than 450 of his constituents. Elsewhere we examine the direct impact of this meeting on those participants, which was broad and considerable (see Minozzi, Neblo, Esterling, & Lazer, 2015). Here we examine the potentially more important issue of what happened outside of the event itself. Did the internal discussion spur additional conversation outside of the virtual room? If so, what did individuals talk about, and did anything condition their propensity to discuss the event with the members of their social networks?

Early work on the flow of political information focused on the interaction between mass media and interpersonal networks. For example, the classic two-step model of diffusion proposed that information typically flows from the media to opinion leaders, and from opinion leaders to the broader population (Katz & Lazarsfeld, 1955). Informed by this and other works of the Columbia school (e.g., Berelson, Lazarsfeld, & McPhee, 1954; Lazarsfeld, Felix, Berelson, & Gaudet, 1944), contemporary researchers have examined interpersonal political discussion in the context of campaigns, linking it to—among other things—political participation (Fowler, 2005; Rolfe, 2012); vote choice (e.g., Beck, Dalton, Greene, & Huckfeldt, 2002; Huckfeldt & Sprague, 1995; Sokhey & McClurg, 2012), attitudinal strength and opinion formation (e.g., Huckfeldt, Beck, Dalton, & Levine, 1995; Levitan & Visser, 2008), partisanship (e.g., Kenny, 1994; Sinclair, 2012), and participation (e.g., Klobstad, 2011; Mutz, 2006).

Here, of course, we are interested in the flow of information outside of the mass media. That is, we focus on what discussions are induced when an individual (“ego”) has some proprietary insights—information to which her discussion partners (“alters”) have not been exposed. Unmediated political events have features that make them normatively interesting. In particular, the individual exposed to an unmediated political event has strong reasons to believe that she has unique knowledge vis-à-vis her social circle. From a discursive point of view, then, we would want to know whether this proprietary information flows beyond the participants in the event. If individuals focus discussion on information shared in common within their group (Stasser & Titus, 1985, 2003; Sunstein, 2006), then the outside repercussions of the deliberative sessions will be minimal. From a societal point of view, such hoarding of private information may be normatively undesirable, because it cannot improve “the means by which a majority comes to be a majority.”

Interpersonal Networks and Deliberative Events

We begin with three premises. First, much of the “deliberation” in democratic societies occurs among preexisting networks of friends, coworkers, family, and the like (Mansbridge, 1999; Mendelberg, 2002; Mutz, 2006; Neblo, in press). Second, political information typically follows a two-step flow (Katz & Lazarsfeld, 1955), where exposure to news spurs interpersonal discussion about that news. Third, social influence among peers shapes political attitudes (e.g., Huckfeldt & Sprague, 2000; Lazer et al., 2010; Levitan & Visser, 2008, 2009)—a process presumably driven by discussions regarding politics. These premises imply that the effects of a deliberative event on citizen discourse can be broken down into direct effects on the individuals involved and indirect effects within social networks (Nickerson, 2009, 2011). Thus, our core hypothesis is that there are substantial secondary effects to deliberative events that flow through the body politic.

H1: A deliberative political event will spur communication regarding politics through interpersonal networks.

There is not a lot of empirical work on the content of “everyday” deliberation in interpersonal networks (for discussions, see Eveland et al., 2011; Klofstad, Sokhey, & McClurg, 2013). That said, we suspect that the proportional impact of an event on the discussion of particular topics will be inversely related to the ambient volume of information and discussion. The logic here is fairly straightforward: The amount of information that someone is exposed to, for example, about detention policy, is far less than the amount they are exposed to about popular politics more generally. Exposure to information about detention policy should have a big impact on the (likely) low rate of discussion about that issue, and far smaller impact on the quantity of discussion about general electoral politics. We do not expect that exposure to a deliberative session will result in individuals becoming more prone to political discussion generally, but rather, the specific topics of inquiry and debate. We also view this as a sort of placebo test—the results will be more compelling if we can demonstrate that the event spurred particular types of discussion, and not just more discussion in general.

H2: A deliberative event will have a bigger impact on communication in networks for the specific subjects of the event than for discussion of politics more generally.

How “Democratic” Are Deliberative Benefits?

Some critiques of deliberation focus on how *individual characteristics* may influence deliberative outcomes—at base, these involve not only socioeconomic status (SES)-related questions of citizen capacity (e.g., Delli Carpini & Keeter, 1996; Verba, Scholzman, & Brady, 1995), but also characteristics like conflict avoidance (Mutz, 2002a; Mutz & Martin, 2001) that jell more with studies of personality and politics (e.g., Gerber, Huber, Doherty, & Dowling, 2012; Mondak, 2010). For example, Sunstein (2006, p. 206) notes that individuals from low-status groups have less influence in deliberative settings, and are less likely to share information they hold (c.f. Neblo, 2007); Neblo, Esterling, Kennedy, Lazer, and Sokhey (2010) examine how conflict avoidance predicts unwillingness to deliberate.

If only the resource-rich (as gauged by SES and interest), or those with certain political orientations (e.g., those ideologically aligned with a congressperson) pass along information gleaned from deliberation, then such events may actually promote inequalities (Sanders, 1997). This is particularly concerning, given the well-documented tendency for individuals to select networks of people who are similar to themselves (i.e., homophily; e.g., Marsden, 1987; McPherson, Smith-Lovin, & Cook, 2001).

Other scholars who have drawn connections between deliberative theory and interpersonal discussion networks raise concerns about disagreement. We consider this factor in terms of both respondents themselves (“egos”), and in terms of dyads/networks. If agreement with a representative (or with policy content) is a necessary condition for a person to engage in discussion with her network, an echo chamber results—citizens will help spread information, but only when they agree with it. Classic theories of social information seeking (e.g., McPhee, with Smith & Ferguson, 1963; Sprague, 1982) suggest something a bit different: Disagreement drives social information seeking.³ By this logic, individuals who participate in a forum and encounter disagreeable information should be expected to display increased levels of discussion with alters. That said, if discussing politics is more akin to a social act (versus a rational information search; Eveland et al., 2011), we might expect an individual’s experience of disagreement to squash further discussion.

From a network perspective, Mutz (2002a, 2002b, 2006) argues that exposure to disagreement in networks is a mixed blessing: While it promotes the democratic good of tolerance (and awareness of opposing rationales), it simultaneously suppresses political participation via ambivalence and social accountability pressures. If agreement with discussants is what promotes additional communication (Mutz & Martin, 2001), this only heightens concerns that subsequent discussions—the “multiplier effects” of forums—will not really be deliberative, but merely reinforcing (and potentially polarizing). Viewed in this light, debates over the extent to which networks in the mass public actually contain disagreement (Huckfeldt, Johnson, & Sprague, 2004; Mutz, 2006) become even more critical, as do considerations of network sophistication (e.g., Ahn, Huckfeldt, Mayer, & Ryan, 2013; Huckfeldt, 2001; McClurg, 2006). On the latter point, if individuals strongly prefer to talk about their deliberative experience with discussants that are already more knowledgeable about politics, we again see the potential for civic inequality rather than civic subsidy.

Last but not least, we concern ourselves with the structural characteristics of networks. We might expect “stronger ties” to result in more sharing of information; the increased “bandwidth” effect that has been noted in studies of information diffusion (Aral & Van Alstyne, 2011; Morey, Eveland, & Hutchens, 2012). “Strength” is a somewhat heterogeneous construct that captures frequency of communication, multiplexity, and affect, among other things (Carpenter, Esterling, & Lazer, 2003; Hansen, 1999). At the same time, it is often weaker ties that provide novel and potentially disagreeable information (Granovetter, 1973; Huckfeldt et al., 1995). If we see that only certain types of ties promote further dissemination—that is, those represented by strong ties (e.g., close friends, family, and relatives)—we have another reason to think that a “deliberative multiplier” may be less valuable than hoped.

In sum, after examining whether our deliberative event spurred subsequent conversation in networks, we test whether a number of individual-level (“monadic”) and network (measured both in terms of averages and dyads) characteristics condition such communication. Moderation would suggest that deliberative events are not equal-opportunity multipliers. Finding the opposite would provide further evidence that deliberative events can scale up while avoiding many normatively undesirable outcomes.

Data and Methods

Studying the flow of information within a network using observational data presents significant challenges. People are not passive instruments of their contexts. Rather, they actively construct those contexts (Fowler, Heaney, Nickerson, Padgett, & Sinclair, 2011; Lazer, Rubineau, Chetkovich, Katz, & Neblo, 2010). With observational data, evaluating the impact of a deliberative event on interpersonal communication is a causal tangle, because people with particular patterns of interpersonal communication may also have similar dispositions toward participating in a deliberative event (Esterling et al., 2011).

Yet randomized laboratory experiments are no easy substitute because of problems with external validity—that is, it is difficult to adequately simulate interpersonal relationships within a lab. However, there are a variety of field and natural experimental strategies one might employ (Soetevent, 2006). For example, one can find exogenous drivers of the network configuration, examining the extent to which the exogenous placement of individuals in the network creates subsequent changes (e.g., Festinger et al., 1950; Klofstad, 2007, 2011; Sacerdote, 2001). Alternately, one might use temporal sequence to infer causation (Christakis & Fowler, 2008; Lazer et al., 2010).

Here we follow a different strategy. We created a deliberative event and randomly invited subjects to participate, effectively introducing an experimental “treatment” into the subject’s pre-existing network. This randomly assigned group is compared against a control group that is not invited to participate. The question, then, is whether we observe subsequent communication regarding the event occurring at higher rates for those individuals who have received the treatment.

This field experimental approach is similar to Nickerson’s (2008, 2009), where randomly selected households with two voters were given a get out the vote (GOTV) pitch. The question was whether the individual in the household who did not receive the GOTV pitch was more likely to vote, relative to controls (alters of individuals who received a pitch unrelated to voting). Since the second voter in the household—who could have only received the GOTV pitch indirectly—was more likely to vote than the controls, Nickerson infers contagion within the household.

Here we combine the idea of using a field experiment to stimulate a preexisting network (see Nickerson, 2011), with traditional egocentric network methods. We recruited 900 voters residing in the state of Michigan through the online polling firm Polimetrix, who drew from their existing Michigan resident panel. Due to resource constraints, they did not match the sample to statewide population averages. Because of the method of sample recruitment, care needs to be taken in extrapolating these results elsewhere; this sample is clearly far more politically active and aware than the broader population. However, this population may be reasonably representative of the people who attend political events, which is a central focus of our effort.⁴

We then administered a baseline survey to capture egocentric measures of individuals’ pre-existing network via a political discussant “name generating” procedure (adapted from the 2000 American National Election Study [see Klofstad, McClurg, & Rolfe, 2009; Sokhey & Djupe, 2014, for discussions]). Specifically, we presented respondents with the following:

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.

We asked respondents to provide identifiers (first and last initials) for their alters so that we could ask subsequent questions regarding communication. We also asked respondents to indicate their relationship to the named individual (e.g., friend, spouse, coworker, etc.). There was very little heterogeneity in network size, with 93% of respondents naming three alters. In addition to the network battery, the baseline survey included a series of demographic and attitudinal questions that serve as pretreatment control variables (please see the Appendix for variable coding).⁵

The online town hall with Senator Levin took place in July 2008, lasting 45 minutes. Beginning with the 900 voters, we randomly assigned 462 subjects to participate in the town hall. In the end, 175 individuals who were invited to the town hall attended (i.e., “complied”); treatment subjects were also provided short background materials on the subject (national security policy regarding the detention of enemy combatants).⁶ In addition, 221 subjects were assigned to receive information only, and 217 subjects were assigned to serve as “pure” controls in that they were not exposed to the session or the reading material.

In the online session, participants were able to submit questions via a text-messaging system to Senator Levin. A moderator posted the questions sequentially, but only allowed

participants to ask one question (so no one person could monopolize the event). The senator did not have any prior knowledge of what questions his constituents would ask. He responded to each question orally, which was then channeled to the participants' computers via Voice over IP (VoIP). The text of his responses was also posted simultaneously using real-time captioning.

A week after the deliberative session, we administered a posttreatment survey in which we asked both treatment and control subjects a host of questions to measure their opinions on a variety of issues, and to gauge the content of their political discussions with the same alters that they named in the baseline name generator.⁷ Put differently, we supplied a controlled stimulus—exposure to a deliberative event—and then examined the impact of the stimulus on subject-specific discussions from egos to alters (as reported by ego).

While we have far more control over the data-generating process than in most purely observational studies, we nevertheless have less than in the ideal laboratory-based experiment. Specifically, a critical element of the process over which we did not have control is *compliance*—that is, whether the individuals we invited to the session with Senator Levin actually showed up. In other words, the study suffers from one-sided noncompliance among those assigned to the treatment. Of the 462 people we invited, only 175 chose to participate (37.9% compliance rate). If this problem was left unaddressed, we would not be able to tell whether the event produced substantial “multiplier effects,” since it is possible that people who have lots of conversations chose to participate selectively (e.g., Esterling et al., 2011; Imai, 2005).⁸

Accordingly, we focus on estimating two quantities that are robust to one-sided noncompliance. The first is the intent-to-treat (ITT) effect, which measures the impact of being assigned to the treatment group (whether the individual participated in the actual treatment or not). ITT essentially gives us the impact of the overall process, and is especially useful in this context, since it gives us an approximation of the impact of the overall program (Gerber & Green, 2012, p. 150). The other is the complier average causal effect (CACE), which is an estimate of the average treatment effect for the “compliers” (those subjects who take the treatment when assigned to the treatment group, and do not take the treatment when assigned to the control group; Gerber & Green, 2012, p. 151). This is estimated as ITT/ITT_D , where ITT_D is the proportion of subjects who are treated when assigned to the treatment group, minus those who would have been treated even if they had been assigned to the control group. To estimate this, we utilize two-stage least squared (2SLS) regression, with treatment assignment as the instrument for actual receipt of the treatment (Angrist, Imbens, & Ruben, 1996; Gerber & Green, 2012).⁹

Results

We begin by looking at our first and central hypothesis: Did the political event spur communication in interpersonal networks? Table 1 answers “yes.” In Table 1, the dependent variable is the average portion of the subject's network with whom she discusses each of the three topics. The first row of Table 1 shows the difference between the subjects who were assigned to the treatment, and those assigned to the pure control condition (with the *t*-test *p*-value in parentheses)—that is, the ITT effect. This comparison between the treatment group and the pure control condition gives us the estimated change in discussion from the population baseline. Individuals assigned to the treatment discussed detainee policy and the topic of Senator Levin with a larger portion of their network; this ITT effect is statistically significant for both topics. General discussion of “politics and public affairs” was not significantly affected by attendance at the deliberative sessions. This provides evidence for both

Table 1
Estimated intent to treat (ITT) and complier average causal effect (CACE) for the deliberative experience

	Topic of conversation		
	Detainee policy	Senator Levin	Politics and public affairs
Deliberation assignment vs. Control (ITT) ($N = 457$)	0.090 (0.007)	0.050 (0.060)	0.0001 (0.499)
Deliberation treatment vs. Control (CACE) ($N = 457$)	0.169 (0.006)	0.094 (0.055)	0.0002 (0.499)
Deliberation assignment vs. Info assignment (ITT) ($N = 470$)	0.096 (0.006)	0.066 (0.033)	-0.003 (0.922)
Deliberation treatment vs. Info assignment (CACE) ($N = 470$)	0.180 (0.005)	0.124 (0.027)	-0.006 (0.922)
Information treatment vs. Control (ITT) ($N = 287$)	-0.006 (0.562)	-0.016 (0.674)	0.003 (0.467)

Note. Values are differences in group means with p -values (2-tailed) in parentheses.

the impact of the deliberation sessions, and for the *subject-matter* hypothesis: participating in the deliberative encounter (“the treatment”) spurred network discussion concerning the more specialized topics of Levin and detainee policy, but failed to do so for the broader topic of politics and public affairs. The group assigned to the online discussions discussed detainee policy with, on average, 9% more of their network, and discussed Senator Levin with, on average, 5% more of their network. Without assignment to the treatment, individuals are likely to discuss detainee policy with 21% of their network, and discuss Senator Levin with about 18% of their network. With assignment those percentages increase to 30% and 23%, respectively.

The second row shows the estimated effect of the deliberative sessions on the compliers, or the CACE. Significant results were again found for discussion of the topics of detainee policy and Senator Levin, but not for general discussion of public affairs. The results suggest that compliers who were assigned to the treatment discussed detainee policy with almost 17% more of their network, and discussed Senator Levin with about 9% more of their network. Among compliers, participation in the session increased the percentage of the respondents’ social networks with whom they discussed detainee policy from about 21% to 38%, and with whom they discussed Senator Levin from about 18% to 27%.

The third and fourth rows show the ITT and CACE comparing the treatment group to the *information condition* group. Since the individuals assigned to the deliberation sessions were also given the information on detainee policy, this comparison separates the effect of the deliberative sessions from the effect of information provision. The results from comparing the treatment group with the pure control condition persist, and are actually marginally stronger for both the ITT and CACE.

Given this somewhat surprising finding, the final row looks at the difference between the group assigned to the *information condition* and those assigned to the pure control condition. Since compliance was unmonitored for the information-only condition, we are only able to estimate the ITT effect. Simply providing information about detainee policy to

respondents had no discernible effect on their propensity to discuss these topics. In fact, the results suggest a (very small) negative effect for two of the three topics of discussion. Since no significant difference is observed, we collapse these conditions for subsequent analyses.

Treatment Effects with Covariates

This analysis demonstrates the clear effect of the treatment on subsequent discussion about the topics of detention policy and Senator Levin. While the random assignment of participants to the treatment ensures the aforementioned estimation strategy is unbiased, there is still the possibility that our random assignment resulted in a random imbalance on a particularly important participant characteristic—one that also affects participants' propensity toward subsequent discussion of these issues (Gerber & Green, 2012, p. 109). To address this issue, Table 2 adjusts for the most likely covariates using regression. With our relatively large sample ($N > 100$), this procedure should produce results that are not appreciably different from block randomization on these same factors (Gerber & Green, 2012, p. 114; Rosenberger & Lachin, 2002).

The results confirm the conclusions from the previous section, with Table 2 displaying intent-to-treat effects (ITT) and the complier average causal effects (CACE) when a number of covariates (see the notes) are added to the model. Full tables are available in the online Supplemental Material. There does not appear to be an imbalance in the randomization process that produced the previous estimates.¹⁰ In all models, participation in the online sessions makes participants more likely to subsequently discuss the topics of detention policy and Senator Levin. And again, the treatment has no discernible effect on general political discussion (our “placebo”).

Addressing Concerns About Heterogeneous Treatment Effects

In the remainder of this article, we test whether deliberation's effects are conditioned on monadic (individual attributes) and network/dyadic characteristics (attributes of participants' relationships with their alters). As with any analysis of effect heterogeneity that is not built into the research design a priori, these results should be taken as more indicative than conclusive. They do, however, suggest a much more uniformly positive effect of deliberation than has been suggested by critics.

Earlier we discussed a number of individual characteristics that might blunt the impact of deliberation. These include (among others) the following: (high) conflict avoidance, (low) political interest, (low) political participation, (low) political knowledge, gender, (low) education, displaying a lack of exigency associated with the particular issue under consideration (i.e., detention policy), and displaying political agreement (or shared ideology) with the town hall representative (Senator Levin).

Figure 1 shows the results of interacting the treatment with these covariates, producing the conditional intent-to-treat (CITT) effect. We also calculate the conditional complier average causal effect, but found no substantive difference from the CITT (and therefore place these results in the online Supplemental Material to conserve space). Each panel shows a plot of the CITT with the p -value for the F -statistic underneath. The F -statistic is used to evaluate the differences in residuals between models, and is the primary method for identifying heterogeneous treatment effects (Gerber & Green, 2012, p. 298).¹¹

Since we are doing multiple comparisons, we encounter the multiple comparisons problem (Gerber & Green, 2012, p. 300)—that is, in testing a large number of group partitions, it is possible that at least one covariate interaction will show statistical

Table 2
Intent-to-treat (ITT) effect and complier average causal effect (CACE) of deliberative sessions controlling for confounding covariates

	Topic of conversation (ITT model)									
	Detention (ITT)			Politics (ITT)			Levin (ITT)			
	Coef.	Std. err.	$p > t$	Coef.	Std. err.	$p > t$	Coef.	Std. err.	$p > t$	
Deliberation assignment	0.112	0.029	0.000	0.003	0.028	0.928	0.082	0.026	0.002	
	$N = 521$									
	Topic of conversation (CACE Model)									
	Detention (CACE)			Politics (CACE)			Levin (CACE)			
	Coef.	Std. err.	$p > z$	Coef.	Std. err.	$p > z$	Coef.	Std. err.	$p > z$	
Participated in session	0.208	0.052	0.000	0.005	0.050	0.926	0.151	0.046	0.001	
	$N = 521$									

Notes. Controls for political interest, affect for Levin, a political participation index, political knowledge, conflict avoidance, party ID, importance of detention issue, average frequency of discussion with alters, average frequency of disagreement with alters, network sophistication, gender of respondent, income of respondent, education of respondent, age of respondent, and marital status of respondent. Full tables available in online Supplemental Material.

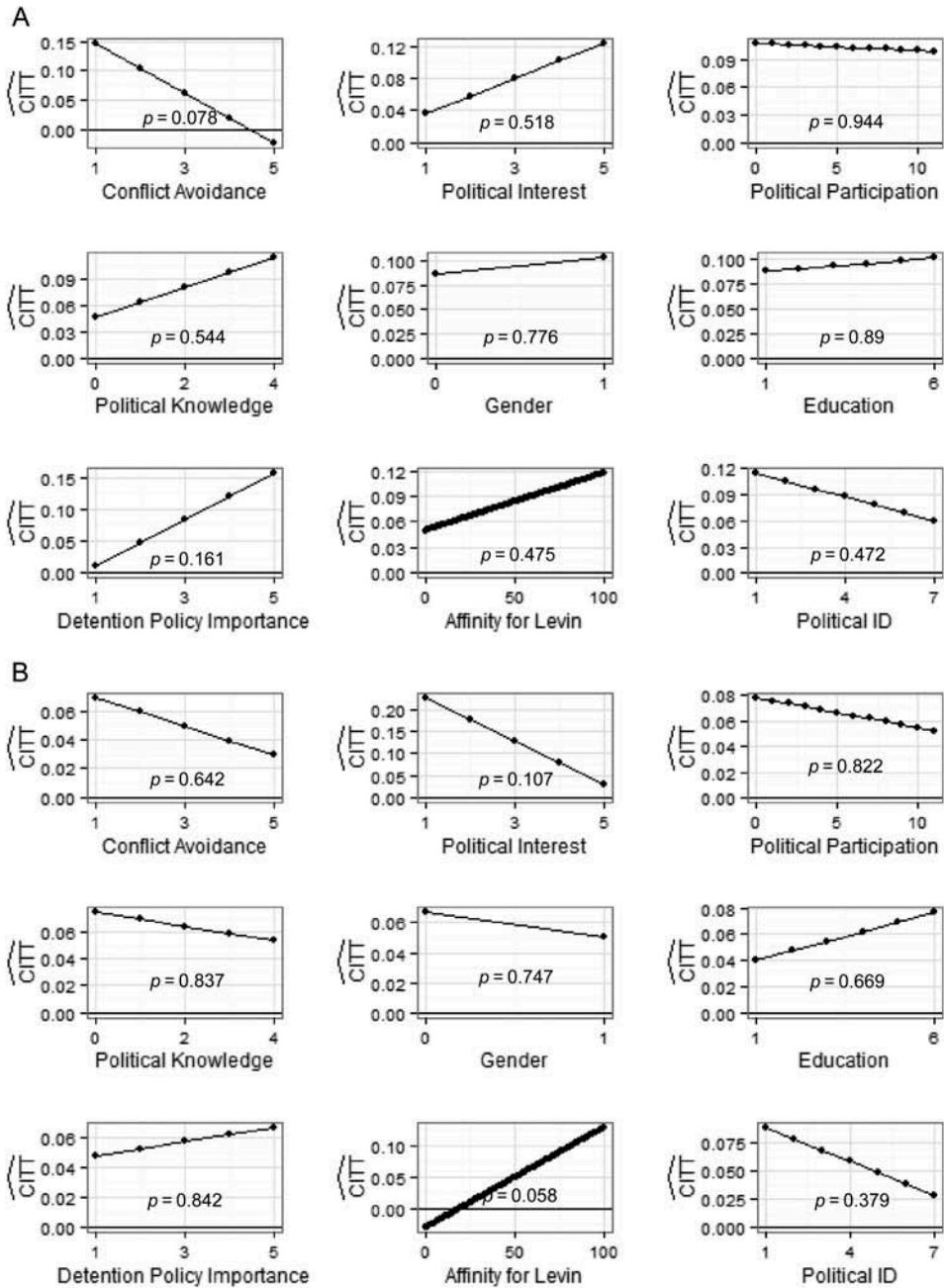


Figure 1. Conditional intent-to-treat (CITT) effects for monadic characteristics, posttreatment discussion of detention policy and Senator Levin.

significance by mere chance. To address this, the text will refer to both the significance level of the raw p -values and evaluation using the Bonferroni correction, where the target p -value for significance is divided by the number of hypothesis tests. In the case of [Figure 1](#), there

are nine hypothesis tests, so the target p -value for significance at the 0.05 level becomes 0.006. The Bonferroni correction is a conservative test, as it does not take into account the correlation between variables (i.e., it assumes the tests are independent). However, this is not too worrisome as few of the potential intervening variables approach even standard levels of statistical significance.

In the top panel of [Figure 1 \(A\)](#) we focus on the topic of detention policy, finding little evidence that any of the aforementioned covariates significantly conditions the effect of the deliberative session. Conflict avoidance does have a marginally significant (and detrimental) effect on the portion of the network with which the individual shares information ($p = 0.078$). However, the significance of this effect evaporates when we use the Bonferroni correction standard.

While these results are indicative, it is difficult to say with certainty that these effects are significant/insignificant. Perhaps most importantly, when we condition on conflict avoidance, we find that only those with the absolute highest values on the measure—which is only about 3% of our sample—fail to show an increase in the percentage of their network with whom they discussed the topic of the town hall. In other words, we find little evidence that conflict avoidance blunts the spread of information. This may not be as surprising as it may seem upon first glance, as these political discussion networks are not randomly assigned or based primarily on shared social location (e.g., workplace), but consist mainly of strong ties chosen by participants (see also [Morey et al., 2012](#)). While the correlation between conflict avoidance and average frequency of network disagreement is not strong ($r = -.053$), there are likely qualitative differences in the degree to which even conflict-avoidant individuals are comfortable with political discussion (or disagreement) that occurs within their self-chosen, core political discussion network.

The bottom panel of [Figure 1 \(B\)](#) shows a similar result for subsequent discussion of the topic of Senator Levin. Only the respondent's affinity for Senator Levin has an impact that emerges as significant (although again, not under the Bonferroni standard). Propensity to discuss Senator Levin increases with the respondent's affinity for Levin. That said, only those with the most extreme opinions of the senator (which is fewer than 10% of the sample) talked with a smaller portion of their network about the senator after the session.

Next we turn our attention to network/dyadic characteristics. These include the (strength of the) ties between individuals, the political expertise of named alters, individuals' levels of disagreement, and their frequency of discussion. There are several methods for measuring disagreement between an ego and her alter(s), including perceived level of political disagreement, shared political ideology, and shared candidate preference. Since previous studies have suggested that these different types of disagreement can produce different results ([Klofstad, Sokhey, & McClurg, 2013](#)), we utilize all three measures.

[Figure 2](#) displays the results for dyadic data, where each subject-alter pair is treated as an observation. This data structure gives us the opportunity to investigate the dyad-specific elements that affect the propensity to communicate novel information (it also introduces a violation of the stable unit treatment value assumption [SUTVA], which we discuss in a moment). To correct for the non-independence introduced by “stacking” dyads, we utilize a typical clustering correction, where the number of clusters (i.e., the number of independent observations) is used in place of the number of observations (e.g., [Gujarati & Porter, 2009](#)).

Despite our expectations from the literature on interpersonal networks—and specifically, its extensive focus on disagreement (e.g., [Huckfeldt et al., 2004](#); [Mutz, 2006](#))—only *one* of the dyadic characteristics significantly conditions the treatment effect of deliberative sessions on subsequent discussion. In the bottom panel of [Figure 2 \(B\)](#), spousal dyads reach

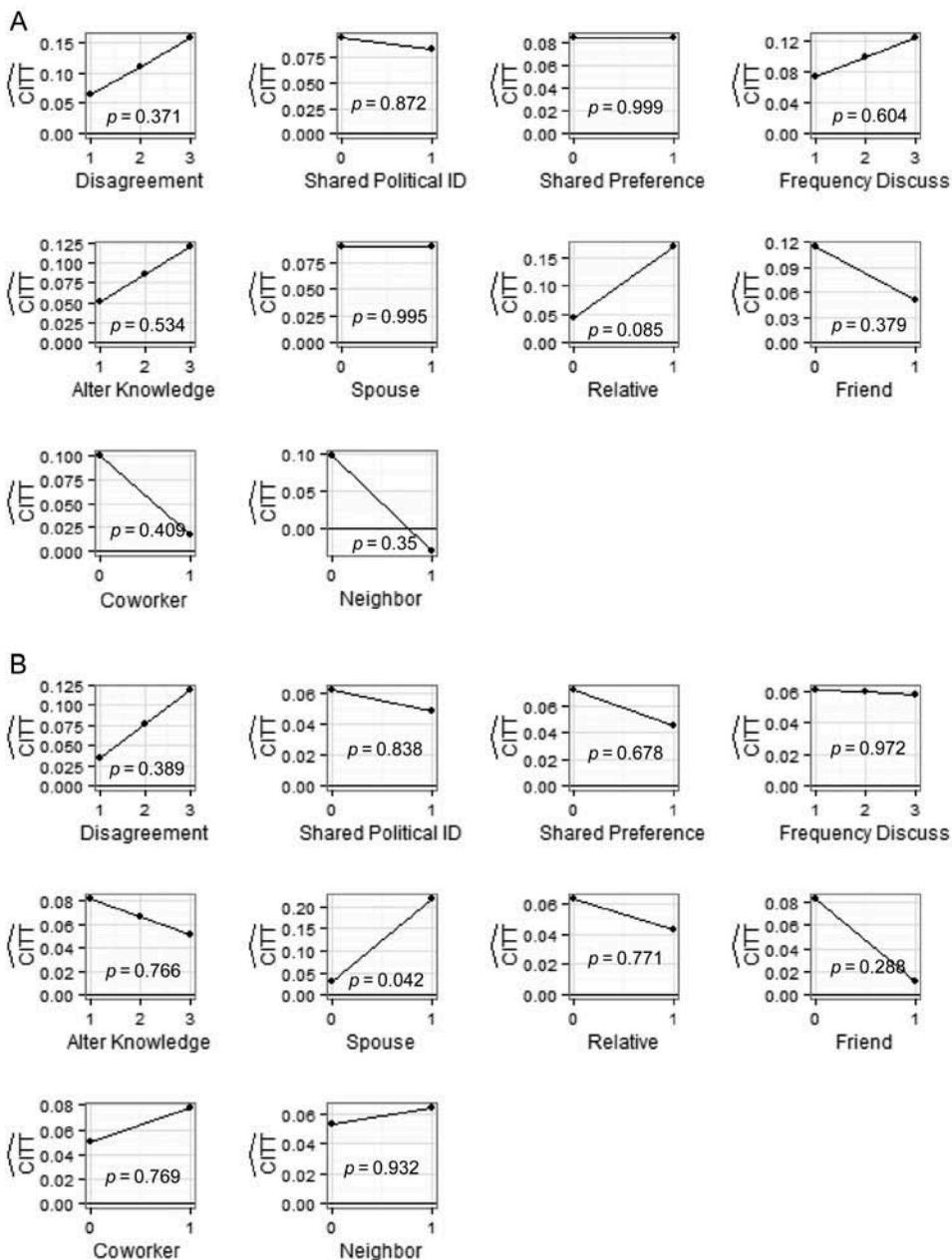


Figure 2. Conditional intent-to-treat (CITT) effects for dyadic characteristics on posttreatment discussion of detention policy and Senator Levin.

conventional levels of statistical significance for the issue of Senator Levin. In some ways, this is perhaps not surprising, and is a reflection of the intimate ties between married couples. Aside from this, virtually no other effects emerge, and interestingly, if anything the degree to which a subject reports disagreeing with an alter predicts an increase in discussion (although this effect does not approach statistical significance [see also footnote 15]).

While moving to dyads is a helpful way to condition on social characteristics, it introduces a SUTVA violation. One method we employ to handle this problem is to collapse the dyadic interactions back into a “monadic” data frame. Here the values for dyadic characteristics are turned into network averages, and the presence of a spouse, relative, friend, coworker, or neighbor is noted with dummy variables. The dependent variable—as in the first sets of estimates—again becomes the percentage of the network with whom the participant discusses the subject matter. This solves the SUTVA violation, but gives us much coarser information.

Figure 3 shows these analyses. In the detainee results, having a relative in the network passes all thresholds of significance for the CITT effect. As in the other figures, we again see that having a spouse in the network increases the probability of discussing Senator Levin. All told, with only a couple of minor exceptions, the effect of the deliberative session on subsequent discussion is insignificant (although positive) in all subgroup analyses. That said, a key exception comes in disagreement: The average level of disagreement in a network is statistically significant at traditional levels for the CITT effect on discussion of detainee policy (and almost passes the Bonferroni cutoff for the 90% significance level). *However, it is in the opposite direction of what most of the previous literature would suggest.* Subjects in networks with greater levels of disagreement are, if anything, *more* likely to discuss detainee policy after attending the session, relative to those in networks with lower levels of disagreement. This tendency is weaker, but still present, in the CITT effect for discussion of the topic of Senator Levin. While in one read this finding is a bit surprising, in another it is perhaps not so, particularly when viewed against recent work suggesting that stronger ties promote both agreeable *and* disagreeable conversation (e.g., Morey et al., 2012). For us, this finding emphasizes the difference between field tests involving self-chosen, core political discussion networks versus laboratory assigned or situational (e.g., workplace) networks. Disagreement within longstanding networks on which topics of conversation are self-chosen does not suppress communication; rather, it seems to drive respondents to discuss issues on which they have acquired unique expertise through interaction with their member of Congress.¹²

In the online Supplemental Material we perform an additional test to check the dyadic estimates (this involves a resampling procedure; we do this as another caution against potential biases caused by a violation of SUTVA). Across the checks, the conclusions remain; the impact of our deliberative session on subsequent discussion was generally positive. In addition, this “multiplier” effect seems to hold, regardless of subgroups, dyadic relationships, and network types. The effect is not conditional on certain individual (e.g., interest) and/or network characteristics (e.g., expertise), and is robust to the most fundamental of social forces, interpersonal disagreement.

Discussion and Conclusion

A frequent critique of formal deliberation is that it can only have a modest impact; few people can attend a particular session. And, even if it is possible to scale up such events, some fear that inequalities reified by deliberation may make such efforts undesirable. Here we wed work on formal and informal deliberation to demonstrate that organized events can generate subsequent political discussions that diffuse throughout the mass public. Moreover, we find that the effects are (issue) specific to our event, and are only minimally contingent on particular characteristics of participants and their networks. Specifically, we find some hints that discussion may be particularly spurred among spouses, and (somewhat surprisingly) among people who disagree with each other.

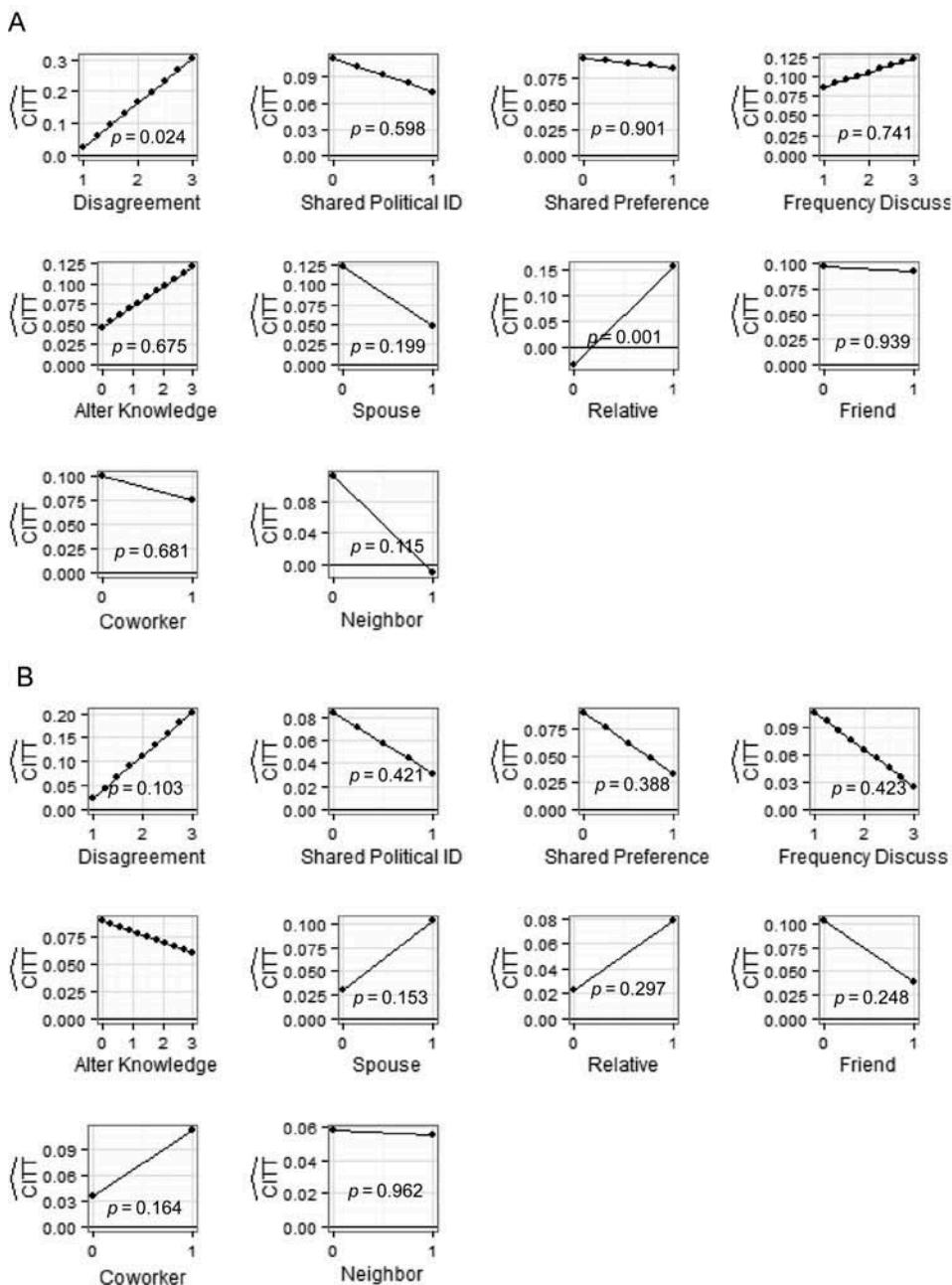


Figure 3. Conditional intent-to-treat (CITT) effects for dyadic characteristics in monadic data on posttreatment discussion of detention policy and Senator Levin.

In all, the effect was considerable: The number of people participating in the town hall was 175, and the number of alters with whom they discussed detention policy and/or Senator Levin was 254—a full 145% of the people who directly participated. Furthermore, we view this as a conservative estimate, as (a) it considers only a thin slice of the alters in the

broader networks of participants (given the nature of the name generator/data available), and (b) we are not able to estimate (potential) additional multiplier effects among alters' alters. This study builds a bridge between the research on formal deliberation and everyday deliberation, where this formal deliberative event effectively catalyzed a great number of "everyday" conversations about politics (Mansbridge, 1999).

While we are encouraged by the results of this deliberative field experiment, we are aware that it is only one study. More work is needed to affirm (and perhaps qualify) our findings. Extending the study by using other name-generator techniques—or perhaps a snowball design to measure the impact of discussion on alters' attitudes and behaviors—seems a natural next step for researchers contemplating similar work. In addition, since we can now claim with some confidence that individuals who participate in a deliberative session are likely to talk about the experience with others, researchers might consider whether deliberation affects alters' attitudes toward participation and the political system more generally (Neblo et al., 2010). Finally, it is not completely clear that our results reflect a distinctive effect of formal deliberation, versus the effect of a highly salient political event. This was a distinctive, and perhaps dramatic event; it was a chance to participate in a discussion with a sitting U.S. Senator. Would, for example, a small group discussion about the same subject have had the same multiplier effects? Would other types of exposures (to news, etc.) regarding the same subject have had similar effects? The experimental paradigm in this article allows manipulation of these types of exposures, and further research will allow examination of the question of whether it is formal deliberation per se, this particular type of a formal deliberative event, or perhaps many other types of events that provoke similar secondary effects in networks. In any case, the results herein are a powerful response to the generic criticisms of formal deliberation—they suggest that deliberation is more than "mere talk," and give us some hope that democracy itself is more than "mere majority rule."

Supplemental Material

Supplemental data for this article can be accessed on the publisher's website at <http://dx.doi.org/10.1080/10584609.2015.1017032>

Notes

1. While some prominent studies have avoided calling discussion a form of participation (Burns, Schlozman, & Verba, 2001; Verba et al., 1995), the (sizable) literature on interpersonal networks (e.g., Huckfeldt & Sprague, 1995; Mutz, 2006), studies it as both an independent and dependent variable, treating it as a central feature of democratic politics (for a related—though non-network-focused—examination of "everyday deliberation" as participation, see Jacobs, Cook, & Delli Carpini [2009]).

2. In "hidden profile" experiments, participants in small group decision-making sessions are given different levels of information to solve a common problem prior to face-to-face discussion (see Stasser & Titus, 2003, for an overview).

3. For discussions of motives as they relate to political discussion, see Eveland and colleagues (2011) and Lyons and Sokhey (2014).

4. Descriptive statistics and a cursory comparison of network characteristics between our study and several nationally representative ones appear in the online Supplemental Material.

5. We administered the baseline survey July 18–25, 2008. In line with many egocentric network studies (e.g., Mutz's Spencer Foundation Study), network size was censored at three individuals. This likely limits the number of weaker ties in our data set (see Sokhey & Djupé, 2014, for a discussion of name-generator methodology).

6. These background materials—along with data, code, and other supporting information (SI) analyses—are available on the Connecting to Congress Dataverse (thedata.org).

7. We administered the posttreatment survey August 5–8, 2008.

8. The question of “who participates” is in itself an important one, which we directly examine in another article using two distinct, yet related studies (Neblo et al., 2010). For present purposes we treat this question as a methodological problem. Compliance among the partial-control (information-only) group was not monitored.

9. As in many observational (survey-based) studies, we also have some non-response/missing data. Across all conditions in the initial sample, 70% of individuals responded to the survey one week after the session. These response rates are calculated using AAPOR RR6, which is appropriate for opt-in survey panels (Callegaro & DiSogra, 2009, p. 1022). Critically, we find no statistically significant differences in survey non-response in the post-survey based on group assignment. Our analyses use conventional list-wise deletion, though in conducting robustness checks—for example, using matching (see footnote 11)—we employed multiple imputation techniques. Table 1 results are robust to these choices.

10. We present summary statistics/balance tests for all control variables—across assignment groups—in the SI materials (available at the Dataverse). We also tested whether there were any differences in propensities to discuss detention issues, asking about the number of people—in an open-ended format—with whom respondents discussed U.S. detention policy in the pre-survey (we found no statistically significant differences). We also used matching to further improve balance and to address model dependence (Ho, Imai, King, & Stuart, 2007a); this produced the same substantive conclusions.

11. The F -statistic is calculated as:

$$F = \frac{\frac{SSR H_0 - SSR H_A}{N \text{ Parameters } H_A - N \text{ Parameters } H_0}}{\frac{SSR H_A}{N - N \text{ Parameters } H_A}}$$

where SSR stands for the sum of squared residuals, H_0 is the model without the interaction, and H_A is the model with the interaction. Error bounds on the interaction can be somewhat misleading (since an interaction can be statistically significant while only marginally decreasing (or even increasing) the sum of squared residuals).

12. The SI document presents additional results concerning disagreement (broadly conceived). Specifically, we include 3-way interactions conditioning on (a) session participation, dyad type (spouse), and dyad-disagreement, and (b) disagreement in the session versus disagreement in respondent networks. We find some evidence that disagreement with the session (affect toward Levin; respondent partisanship) conditions the aforementioned network disagreement effects.

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Appendix: Variable Coding and Descriptive Statistics

Variable Coding

Name Generator:

From time to time people discuss government, elections, and politics. Looking back over the past 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.

Respondents were then asked to answer a series of questions about each of the (up to) three named discussants. Social ties were asked about “yes/no” items; other items asked about in dyads appear next:

Dependent Variables:

- Topics of Discussion: 1 = discussed the topic in dyad; 0 = did not discuss it

Independent Variables:

- Participated (0–1): 1 = respondent attended deliberative session

Political Characteristics and Opinions:

- Political Interest (1–5): 5 = high political interest
- Participation (0–11): an additive index created by summing across a series of acts
- Political Knowledge (0–4): an additive index, created summing across correct answers to four factual questions
- Party Identification (1–7): 1 = strong Democrat
- Importance of Detainee Policy (1–5): U.S. treatment of detainees is 1 = most serious issue facing our country; 5 = not at all important
- Affect for Levin: Feeling thermometer (0–100)

Social and Dyad Characteristics:

- Conflict Avoidance: “I often feel uncomfortable when people argue about politics.” (1 = strongly disagree; 5 = strongly agree)
- Frequency of Discussion in Dyad (1–3): 3 = very often; 2 = often; 1 = rarely
- Frequency of Disagreement in Dyad (1–3): 3 = very often; 2 = often; 1 = rarely
- Expertise of Discussant in Dyad (1–3): 3 = alter knows “a great deal” about politics; 2 = alter knows “some”; 1 = alter knows “not much”

Demographics:

- Gender: 1 = male
- Income (1–14): 14 = 150,000 or more
- Education (1–6): 6 = graduate degree
- Age (in years)
- Married: 1 = married