Expanding the Conversation:
Ripple Effects from a Deliberative Field Experiment

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Can formal deliberation improve the quality of public opinion? Critics point out that only a tiny number of people can participate in any given gathering. Here we consider the question of whether formal deliberation has the potential to cause additional discussion, or “ripple effects” external to an event. To address this, 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 discussion of the session topic, and of the senator himself, within attendees’ social networks. No participant/nodal characteristics moderated the treatment effects, though spouses were especially likely to be indirectly affected by the event. In the end, we conclude that even relatively small scale deliberative encounters can have a broader impact on politics and public opinion.

"Majority rule, just as majority rule, is as foolish as its critics charge it with being. But it never is *merely* majority rule. As a practical politician, Samuel J. Tilden, said a long time ago: 'The means by which a majority comes to be a majority is the more important thing.'" (Dewey, p. 365) Dewey's oft quoted argument emphasizes that unless one wants to deny the possibility of a tyranny of the majority, political discussion cannot 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.

Recent efforts to design and encourage new deliberative forums are rooted in the hope that they can improve public opinion – i.e., improve the means by which a majority becomes a majority (Druckman and Nelson 2003). But critics worry that any purported benefits must be limited by the relatively small number of people who can participate in a given deliberative event. Most theories of deliberative democracy envision a more broadly deliberative public sphere, stretching well beyond mini-publics, Deliberative Opinion Polls, and the like (Dryzek 2010; Habermas 1996; Mansbridge 1999; Warren 2002). Formal deliberative events, however, might still play the role of kindling or catalysts to increase the amount and quality of deliberation in the "wilds" of the larger democratic public. Yet, heretofore, no one has systematically studied the way that formal deliberation ramifies out into broader political discussion.

Do deliberative encounters reverberate through interpersonal networks? For example, what are the social consequences of a structured deliberative event, such as a town hall meeting between legislators and constituents? Clearly, deliberative events often affect the individuals who participate, but does that event have an impact beyond those immediate participants? Formal deliberation would be of less import if its sole impact was on the immediate audience.

And if the exclusive social impact of political events was through (non-deliberative) media coverage, the audience would be reduced to mere props (Habermas, 1974).

We argue 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 potentially large multiplier effect, and even small scale deliberation may have a relatively broad impact on politics and public opinion.

To test for deliberative multipliers within social networks, we organized a formal deliberative event: an online "town-hall" pairing a sitting United States Senator (Sen. Carl Levin, D-MI) at 7pm, July 28, 2008, inviting over 450 of his constituents. Elsewhere we examine the direct impact of this meeting on those participants, which was broad and considerable (references omitted). Here we examine what happened outside of the event. Did the internal discussion spur additional conversation outside of the virtual room? If so, what did individuals talk about, and with whom did they have conversations?

Much of the research on the flow of political information has focused on the interaction between mass media and inter-personal networks. For example, the classic two step model of diffusion proposes that information typically flows from the media to opinion leaders, and from opinion leaders to the broader population (Katz and Lazarsfeld 1955). In the context of political campaigns, for example, campaign events spur discussion among citizens, which can be inferred by the increasing conformity within networks over the course of a campaign (Berelson et al. 1954; Huckfeldt and Sprague 1995; reference omitted).

Here we are interested in the flow of information outside of the mass media. That is, what discussions are induced when an individual ("ego") has some *proprietary* insights—information that their discussion partners ("alters") have not been exposed to. 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. Laboratory research suggests that individuals have a strong tendency to focus on discussing information shared in common ex ante (Stasser and Titus 2003 and 1985; Sunstein 2006). 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."

Hypotheses

Communication in Dyads

We begin with the premise that much of the deliberation in democratic societies occurs among pre-existing networks of friends, coworkers, family, and the like (Mansbridge 1999; Mendelberg 2002; Mutz 2006). Following this logic, the effects of a deliberative event on citizen discourse can be broken down into direct effects on the individuals involved, and subsequently, into indirect or "ripple" effects within social networks. Our core hypothesis is that there are substantial spillover effects to deliberative events that ripple through the body politic.

> **Hypothesis 1:** *A deliberative political event will spur communication regarding politics through interpersonal networks.*

Of course, not all ties are created equal. Part of the core conceptual vocabulary of social network analysis at least since Granovetter (1973) is the distinction between strong and weak ties. "Strength" is a somewhat heterogeneous construct that captures frequency of communication, multiplexity, and affect, among other things. Strength of ties is especially important for quickly conveying information that is sensitive or complex (Carpenter, Esterling, and Lazer 2003; Hansen 1999).

We suspect that information regarding political events will flow especially through strong ties – even controlling for the overall frequency of political communication – because the strength of the tie may (1) create a sense of shared ownership of the experience, and (2) predict interaction within a short period after the event, when the probability of raising the event as a discussion topic is at its peak.

> **Hypothesis 2:** A *deliberative event will spur more communication with strong ties than weak ties.*

Much of the literature on communication networks focuses on the likelihood of hearing contrary points of view. The main assertions of this literature are, first, that it is desirable for those with differing points of view to talk to each other, and second, that there is a strong tendency for people with differing points of view *not* to talk to each other (Huckfeldt, Johnson, and Sprague, 2004; Mutz 2002; 2006); this latter observation fits into a much larger literature on homophily (e.g., McPherson, Miller, & Cook 2001; Marsden 1987). The question we address here is given tendencies to communicate with others like us, is there an additional tendency to share *novel* information with likeminded individuals? The prior literature does not offer a definitive answer, but the underlying logic (that people seek agreement in their political discussions) would suggest so:

> **Hypothesis 3:** *Pre-existing political discussion networks will be strongly biased toward agreement on policy issues, and deliberative events will have a bigger impact on discussions among people who already agree.*

Subject Matter: The Content of Communication

We suspect that the proportional impact of an event on discussion of particular topics will be inversely related to the ambient volume of information and discussions. The logic here is fairly straightforward: one would guess that the amount of information that someone is exposed to, for example, about food safety is far less than the amount they are exposed to about policy and politics more generally. Exposure to information about food safety should have a big impact on the (likely) low rate of discussion about food safety, and far less impact on the quantity of discussion about public policy more generally.

> **Hypothesis 4:** 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.*

Individual-Level Characteristics

Since the classic Columbia studies (e.g., Katz and Lazarsfeld 1955), received wisdom has been that opinion leaders play a key role in conveying information from the mass media. Of course, in the present study we are examining direct communication between politicians and the public. Nevertheless, the logic of the role played by opinion leaders – one informed by more recent efforts focusing on the perception and consequences of political expertise in networks (e.g., Huckfeldt 2001; McClurg 2006; Ryan 2011) – would seem to map pretty neatly onto information stemming from our event. Thus, we expect that individuals with the classic characteristics of opinion leaders (e.g., high education, political expertise) will be more likely to convey information regarding a deliberative event than other individuals.

> **Hypothesis 5:** *Individuals who are potential opinion leaders, as measured by education and political expertise, will be relatively more likely to communicate as a result of a deliberative event.*

DATA AND METHODS[1]

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 (Lazer et al., 2010; Fowler et al., 2011). 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, Neblo and Lazer, 2011).

Yet randomized laboratory experiments are no easy substitute because of problems with external validity – i.e., it is typically 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. Festinger and colleagues (1950) followed this strategy, as have a host of recent roommate studies (e.g., Sacerdote 2001; Klofstad 2007). Alternately, one might collect longitudinal data, using the temporal sequence to infer causation (Lazer et al., 2010; Fowler and Christakis, 2008).

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. 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). In Nickerson's research, 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). The (reasonable) methodological assumption was that two voters living in a household are likely to have a strong tie. Since the second voter in the household who could have only received the GOTV pitch indirectly was nevertheless significantly 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 pre-existing network with traditional egocentric methods. We recruited 900 voters residing in the state of Michigan through the online polling firm *Polimetrix*.[2] We then administered a baseline survey to capture egocentric measures of their pre-existing network via a political discussant "name generating" procedure adapted from the 2000 American National Election Study (see Klofstad, McClurg, and Rolfe, 2009 for a discussion). 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 their communication with these individuals. We also asked them to indicate their relationship to the individual (e.g., friend, spouse, coworker, etc.). 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).[3]

The online town-hall with Sen. Levin took place in July, 2008, starting at 7 pm and lasting 45 minutes. Beginning with the 900 voters, we randomly assigned 462 subjects to participate in the townhall. In the end, 175 individuals who were invited to the townhall attended (i.e., "complied"); treatment subjects were also provided short background materials on the subject (national security policy regarding the detention of enemy combatants).[4] In addition, 221 subjects were assigned to receive information only, and 217 subjects were assigned to serve as "pure" controls – 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 Sen. 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. The text of his responses was posted simultaneously using real-time captioning.

A week after the deliberative session, we administered a post-treatment 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.[5]

[Insert Figure 1 about here]

Figure 1 captures the essential idea behind our research design. Like Nickerson (2008), we supplied a controlled stimulus – exposure to a deliberative event – and then examine the impact of the stimulus on subject specific discussions from ego to alter (as reported by ego). We expect to see the strength of the tie moderate the amount of discussion, with strong ties experiencing more discussion than weak ties.

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, there are two important elements of the process over which we did not have control:

(1) compliance: whether the individuals we invited to the session with Senator Levin actually showed up. Of the 462 people we invited, only 175 chose to participate (37.9% percent compliance rate).

(2) non-response: whether, subsequent to the initial recruitment, individuals responded to the survey. Across *all* conditions of the initial sample, 70% responded to the survey one week after the session, and approximately 85% to the post election survey.[6]

Endogeneity is therefore a significant concern. Left unaddressed, we could not tell whether the event produced substantial "ripple effects," or whether people who have lots of conversations chose to participate selectively.[7] In other words, because randomization is partly broken, we have to account for this brokenness to make reliable causal inferences – something well-documented by scholars working on field experiments (Esterling et al. 2011; Imai 2005).

To address these threats, we focus on comparing those who were confirmed as having attended (the treated who complied; 175), versus the pure controls (152). We "stack" the data to conduct a dyad-level analysis (meaning that each main respondent-discussant pair appears in the data set; standard errors are adjusted to account for this clustering on the main discussant – please see the endnotes for specifics). We then performed multiple imputation for any missing data (due to item non-response), followed by matching on the imputed data; imputation prior to matching is recommended in the literature (Ho et al. 2007a; 2007b).[8] We created five datasets using the multiple imputation, implemented in *R* using *Amelia II* (Honaker and King, 2010); a ridge prior of 9 was selected (due to the number of dyads),[9] and tolerance was set to .0001.

Matching

We then matched control subjects to complying (treated) subjects using a genetic matching algorithm (Sekhon 2006) for *each* of the five imputed data sets. Genetic matching uses a genetic search algorithm to find a set of weights that achieves/improves balance across the set of potentially confounding covariates – i.e., the factors that might distinguish compliers, in the break with randomization. The procedure was performed in R using *MatchIt* (Ho et al., 2007a; 2007b), and the compliers (treated) and controls were matched on the following variables (please see appendix A for details):

Demographics: Gender, income, education, age, marital status

Political Characteristics: political interest, participation (an index of acts), political knowledge (an index), party identification, individual importance of detainee policy

Social Characteristics: conflict avoidance, church attendance

Visual summaries of the matching analysis are presented in Figure 2, which displays histograms for the propensity scores. One can see the distribution of subjects' propensity scores, for treated and controls, for each data set, pre-matching (left column in each box) and post-matching (right column in each box). The treatment and control distributions are markedly more similar post-matching. Table A1 (in the appendix) provides balance statistics for the covariates used in the matching procedure, which are well balanced in the matched data.

[Insert Figure 2 about here]

Finally, with this "pre-processing" (Ho et al. 2007a) complete, we estimated models on each matched data set (i.e., we use weights from the matching procedure), and averaged across the five sets of estimates. Below, we present the averaged results from these logistic regressions (note that each regression, in each matched data set, is estimated with robust-clustered standard errors to account for the stacking of dyads).

RESULTS

We begin by looking at our first and central hypothesis: did the political event spur communication in interpersonal networks? Table 1 answers "yes," displaying the model-based estimates – on the matched data – for the effect of our event on communication in dyads across the three separate topics of discussion: detainee policy, Senator Levin, and politics and public affairs. We also find evidence supporting the *subject-matter* hypothesis, as participating in the deliberative encounter ("the treatment") spurred dyad-level 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. *Those who attended the online discussion were almost twice as likely to discuss detainee policy and Senator Levin with an alter*—jumping from 17% to 33%, and from 16% to 30%, respectively.[10]

[Insert Table 1 about here]

Few individual factors structure communication in these dyads, though in the case of detainee policy, the more politically interested remain more likely to communicate. Looking at dyad characteristics, we see that frequent discussion emerges (perhaps not unexpectedly) as a significant predictor for all three topics. To a certain extent, both of these findings fit with the "nodal" hypothesis concerning opinion leadership, though we fail to find any direct effects for education or political knowledge.

For the broader topic of "politics and public affairs," frequency of dyadic disagreement emerges as a significant predictor, decreasing the probability of discussion. This makes sense, given our expectations regarding individuals' tendencies to communicate with like-minded individuals (the coefficient is in the same direction for the more partisan and specific topic of Levin, though it fails to achieve significance). It is worth noting that our sample contains a considerable amount of disagreement, with about 40% of dyads reporting that they disagree about politics “often” or “very often.” Cast in terms of network averages, the sample contains more political disagreement than two national studies containing egocentric network batteries: the 2000 National Election Study, and the American component of the 1992 Cross-National Election Project. It also contains higher levels of political expertise (please see Appendix Tables A.2-A.4, Network Characteristics).

Moderating Effects

Having demonstrated overall ripple effects in networks, we now move to examine whether any factors serve as moderators. Do certain types of discussants promote greater communication? Tables 2 and 3 present consider the moderating effects of dyad type, for a variety of ties: friends, relatives, coworkers, neighbors, and spouses (we also examine moderation by shared party identification, and frequency of discussion). The full specification is presented in table 2 for spousal dyads; table 3 considers other specifications, and presents only the parameters for the main treatment effect, for dyad type, and for the interaction.[11] We would interpret spousal and friendship ties as being “strong,” and view frequency of discussion –at least in some ways – as an additional metric of strength.

As in table 1, we find support for the subject matter hypothesis – there are significant effects for the topics of detainee policy and Senator Levin (models estimated on “politics and public affairs” yielded consistently null results, thus we do not present them here).

[Insert Tables 2 and 3 about here]

Interestingly, we find strong evidence of moderation, but for only one dyad type: spouses. While this interaction is only marginally significant for the topic of detainee policy, it is highly significant when it comes to discussing Levin. In both cases, the effect of the online discussion "treatment" on the probability of discussion in a dyad is heightened when the alter is a spouse. As we show in Table 4, these are *enormous* effects, more than tripling the probability of discussion of Levin, and almost tripling the probability of discussion of detainee policy.

[Insert Table 4 about here]

We also expected partisan agreement in dyads to increase the probability of communication, but find little evidence that this is the case. The interaction effects estimates are correctly signed for both topics – with agreement promoting discussion – but are small and non-statistically significant (bottom Table 3). Likewise, we find no support for the nodal hypotheses, as the parameter estimates capturing the interactions between the ego's levels of education and the treatment, and between the ego's level of political knowledge and the treatment, respectively, are all remain statistically insignificant and negatively signed.

[Insert Table 5 about here]

DISCUSSION AND CONCLUSION

Our results suggest that an unmediated deliberative political event, such as the one we constructed, can have major multiplier effects in the mass public. For example, where 175 people directly participated in our deliberative event, a very conservative estimate is that at least 164 extra people who were not in the session had discussions about Sen. Levin, and that 187 extra people discussions about detainee policy, all *because* they knew someone who *was* in the session. We note that we say "very conservative" because: (1) we only allowed subjects to name up to three discussants (i.e., the name generator was right-censored at 3), and (2) the estimates do not capture any increase in second and higher order effects through the network – i.e., in the number of discussions that the initial discussants might have had with yet other people, and so on. Existing work on contagion in social networks suggest that these secondary effects could be considerable (Christakis & Fowler 2007; Fowler 2005) ; it is therefore quite plausible that the number of individuals affected was much higher. If we envision 21st century communication to be a constant heaving of pebbles of information into the communal pool of public and private discourse, then the striking thing about this event was that its ripples were substantial enough to be detected.

Just as interesting as the main treatment effect is that the effect is so much larger for spouses. This result hints at the likely importance of strong ties in fueling the spread of political information. It might be that, at least in contemporary American society, that key sites for political discourse are not salons and cafés, but kitchen tables. Alternatively, it is possible that this large spousal effect reflects the fact that while the event took place "everywhere" (because it was virtual), for most participants the experience really took place in the household, and thus was more likely to spur intra-household political discussion. It may well be that if the event took place within other focal points for social relationships (such as the workplace, religious institutions, or other secondary associations), that the dyads that would be most activated would be ones based in those settings. It was also notable that we do not find that our treatment interacts with any nodal level characteristics – i.e., we find no evidence that political knowledge and education accentuate the secondary effects of participating in these sessions.

Of course, such results beg as many questions as they answer. This was a particularly compelling deliberative event—one that involved a sitting US Senator. We structured this event with a Senator because it fits into a larger vision of studying deliberation that encompasses elites and masses (references omitted). As the more standard construction of deliberation and public opinion has been one that does not incorporate elites (Fishkin 2009), the ripple effects for events that do not incorporate elites warrant examination. Similarly it is unclear what kind of ripples would be seen in events incorporating less prominent public officials. Would such gatherings have a greater or lesser impact — less because a discussion with a mayor may be less worthy of reporting than one with a US Senator, or more because local policy may be more salient, and robust systemic sources sparser?

Similarly, our deliberative experiment was an event that took place through a particular medium—the Internet—and was structured in a fashion that maximized openness (e.g., with a neutral moderator). There are, of course, a variety of media through which elites can communicate with the public (e.g., traditional townhalls, "tele-townhalls," and other types of online interactive media, such as blogs). How does the medium interplay with the potential ripple effects of a deliberative event? How do the rules governing the event affect its impact? Our testable intuition is that the apparent neutrality of the moderation actually amplified its effectiveness. But what would be the impact of any event that was more obviously controlled by the relevant politician/official?

Finally, we close by noting that our results also have implications for democratic practices in the 21st century. The Internet, as evidenced in our experiment, offers a tool with which politicians may reach many constituents – literally into their homes – thereby skipping intermediaries. Politicians have always conducted many in person events, but what is notable about this event was how low cost it was (by many measures), most importantly with respect to the politician's time. In the present case, Senator Levin reached nearly 200 constituents at the cost of 45 minutes. Compare this to a district event of the same size that might take 45 minutes, but that requires some presence before and after, as well as transportation (and different security measures). One could imagine, for example, a politician conducting many dozens of events like ours each year, reaching many thousands of individuals, and, indirectly, tens of thousands of individuals (or more). Put differently, these results hint at the possibility of the transformational effects of the Internet on political discourse – what enables elite to citizen communication may, in turn, trigger deliberative ripples through the broader society.

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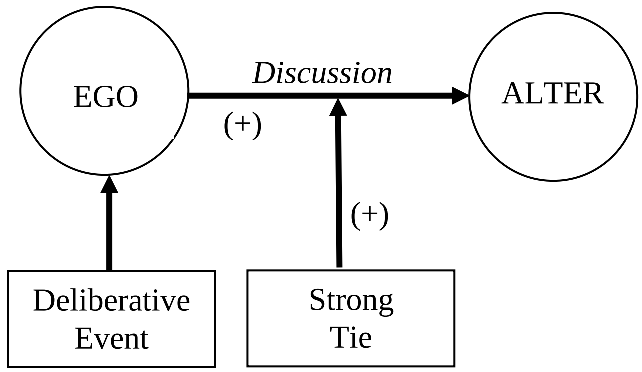

Figure 1: Summary of research design

Data Set 1 Data Set 2 Data Set 3 Data Set 4 Data Set 5

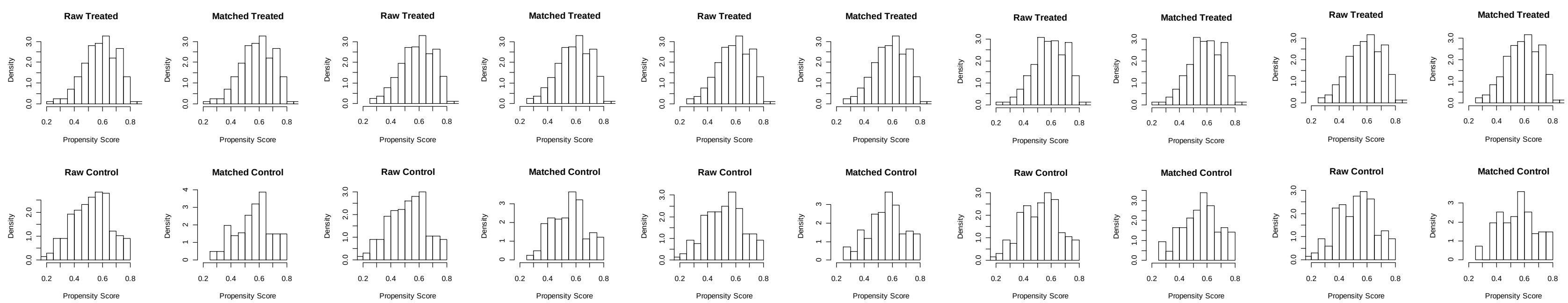

Figure 2: Balance Improvement on 5 imputed, matched versions of the data set. For each data set, the left column is treated vs. controls pre-matching; the right column is treated vs. controls post-matching. Genetic Matching (Diamond and Sekhon 2005; Sekhon forthcoming) was implemented via *MatchIt* (2007a; 2007b), and histograms were produced using *MatchIt*.

Table 1: Predicting Discussion in Dyads, across Specific Issues
(Logit Estimates on Matched Data)

	"Detainee Policy"			"Senator Levin"			"Politics & Public Affairs"		
Variables	B	Robust SE	P	B	Robust SE	p	B	Robust SE	P
Attended Session (Treatment)	.91	.27	***	.84	.29	***	-.06	.26	
Political Interest	.57	.19	***	.01	.17		.11	.16	
Gender	.29	.25		-.07	.24		.38	.25	
Income	.06	.04		.00	.04		.01	.04	
Education	-.14	.10		.03	.10		.06	.08	
Age	-.01	.01		.01	.01		.01	.01	
Participation	-.00	.06		.06	.06		.11	.07	
Pol. Know	.04	.14		-.18	.13		.16	.13	
Conflict Avoidance	-.10	.11		-.08	.11		-.11	.10	
Married	-.57	.30	*	-.24	.29		.22	.28	
Freq. of Disc.	1.08	.17	***	1.15	.17	***	.99	.16	***
Freq. of Disagreement	-.02	.14		-.29	.15	**	-.30	.15	**
Discussant Expertise	.19	.18		.02	.17		-.18	.17	
Party ID	.02	.05		.05	.05		-.03	.06	
Impt. Of Detainee Policy	-.20	.14		-.01	.12		-.08	.12	
Intercept	-4.15	1.03	***	-2.74	1.00	**	-1.32	.92	
N=896									

Source: Levin Panel Study; ***p≤.01, **p≤.05, *p≤.1, *two-tailed tests*
Note: Robust Clustered Standard Errors are Employed to Account for Stacked Dyads.
Estimates are averaged across 5 versions of the matched data.

Table 2: Predicting Discussion in Dyads: Moderating Effects of Spouses (Logit Estimates on Matched Data)						
	Detainee Policy			Senator Levin		
Variables	B	Robust SE	p	B	Robust SE	p
Attended Session (Treatment)	.78	.29	***	.50	.32	.11
Political Interest	.65	.20	***	.13	.18	
Gender	.34	.26		-.08	.27	
Income	.07	.04		.01	.04	
Education	-.15	.11		.01	.11	
Age	-.00	.01		.02	.01	.11
Participation	-.01	.07		.07	.07	
Pol. Know	.08	.15		-.11	.14	
Conflict Avoidance	-.11	.11		-.12	.13	
Married	-.83	.32	***	-.72	.34	***
Freq. of Disc.	.88	.17	***	.88	.17	***
Freq. of Disagreement	.01	.14		-.23	.16	
Discussant Expertise	.26	.18		.13	.18	
Party ID	.02	.06		.06	.06	
Impt. Of Detainee Policy	-.20	.14		.00	.13	
Spouse Dyad	.61	.38	.11	.56	.41	
Treatment*Spouse Dyad	.90	.49	*	1.88	.53	***
Intercept	-4.74	1.08	***	-3.44	1.13	**
N=896						

Source: Levin Study; ***p≤.01, **p≤.05, *p≤.1, *two-tailed tests*
Robust clustered standard errors are employed to account for the stacked dyads.
Estimates are averaged across 5 versions of the matched data.

Table 3: Other Types of Ties do not Moderate Treatment Effects (selected estimates from 12 separate specifications) (Logit Estimates on Matched Data)						
	Detainee Policy			Senator Levin		
	B	Robust SE	p	B	Robust SE	p
Treatment	.84	.30	***	.98	.31	***
Relative Dyad	-.15	.38		-.21	.46	
Relative* Treatment	.21	.44		-.39	.51	
Treatment	.98	.30	***	.93	.40	**
Friend Dyad	-.11	.32		-.27	.46	
Friend*Treatment	-.18	.41		-.26	.52	
Treatment	1.02	.29	***	.97	.34	***
Co-worker Dyad	.44	.46		.79	.51	
Coworker*Treatment	-.71	.54		-.76	.57	
Treatment	.91	.27	***	.89	.32	***
Neighbor Dyad	-.73	1.22		.55	.74	
Neighbor*Treatment	.16	1.28		-.72	.84	
Treatment	.72	.35	**	.68	.40	*
Dyad shares Partisanship	-.35	.38		-.08	.43	
Shared PID*Treatment	.37	.41		.33	.45	
Treatment	.81	.54		.78	.53	
Freq. of Disc. in Dyad	1.02	.32	***	1.11	.28	***
Freq. Disc.*Treatment	.08	.37		.04	.32	
N=896						

Source: Levin Study; ***p≤.01, **p≤.05, *p≤.1, *two-tailed tests*
Note: Each set of selected estimates comes from a separate, full model that includes all the control covariates included in Table 1. Robust clustered standard errors are employed to account for the stacked dyads. Estimates, for each model, are averaged across 5 versions of the matched data.

Table 4: The Moderating Effects of Spouses – The Probability of Discussing Topics in a Dyad by Profiles

Detainee Policy	Prob.	(2.5%, 97.5%)	*Senator Levin*	Prob.	(2.5%, 97.5%)
Control; Not Married; Discussant not Spouse	.25	(.13 .38)	Control; Not Married; Discussant not Spouse	.22	(.12 .34)
Treated; Not Married; Discussant not Spouse	.41	(.31 .51)	Treated; Not Married; Discussant not Spouse	.32	(.22 .43)
Control; Married; Discussant not Spouse	.12	(.07 .18)	Control; Married; Discussant not Spouse	.12	(.07 .19)
Treated; Married; Discussant not Spouse	.23	(.17 .30)	Treated; Married; Discussant not Spouse	.18	(.13 .25)
Control; Discussant is Spouse (Married)	.20	(.10 .35)	Control; Discussant is Spouse (Married)	.19	(.09 .33)
Treated; Discussant is Spouse (Married)	.57	(.42 .70)	Treated; Discussant is Spouse (Married)	.71	(.57 .83)

Estimates taken from Table 2; probabilities are the result of 1000 simulations conducted in *Zelig* (Imai et al. 2009), with all variables set to mean values (save those set in a profile/used in the interaction).

Table 5: Nodal, "Opinion Leader" Characteristics do not Moderate Treatment Effects (selected estimates from 4 separate specifications) (Logit Estimates on Matched Data)						
	Detainee Policy			Senator Levin		
	B	Robust SE	p	B	Robust SE	p
Treatment	0.92	0.28	***	0.86	0.31	***
Ego's Level of Political Knowledge	.11	.32		-.01	.37	
Ego Pol. Know.* Treatment	-.08	.34		-.21	.39	
Treatment	.93	.28	***	0.85	.30	***
Ego's Level of Education	-.09	.30		.09	.29	
Ego Educ. *Treatment	-.15	.34		-.06	.31	
N=896						

Source: Levin Study; ***p≤.01, **p≤.05, *p≤.1, *two-tailed tests*

Note: Each set of selected estimates comes from a separate, full model that includes all the control covariates included in Table 1. Robust clustered standard errors are employed to account for the stacked dyads. Estimates, for each model, are averaged across 5 versions of the matched data.

APPENDIX

1) VARIABLES AND CODING

Name Generator:

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.

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

Dependent Variables:

▪topics of discussion: 1=discussed the topic in dyad; 0=did not discuss it

Independent Variables:

▪Treatment (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.

2) Balance Statistics and Network Characteristics

Table A1: Balance Statistics, Standardized Difference in Means between Treatment (Complier) and Control Groups

	Data Set 1	Data Set 2	Data Set 3	Data Set 4	Data Set 5
	Non-matched Data				
Distance	0.575	.576	.583	.580	.583
Variables					
Political Interest	-.089	-.089	-.089	-.089	-.089
Gender	-.095	-.095	-.095	-.095	-.095
Income	-.144	-.155	-.160	-.160	-.154
Education	-.087	-.087	-.087	-.087	-.087
Age	-.051	-.051	-.051	-.051	-.051
Participation	.050	.050	.050	.050	.050
Pol. Knowledge	-.039	-.039	-.039	-.039	-.039
Conflict Avoidance	-.323	-.323	-.323	-.323	-.323
Married	-.224	-.224	-.224	-.224	-.224
Party ID	-.199	-.198	-.203	-.200	-.211
Impt. Of Detainee Policy	-.188	-.188	-.188	-.188	-.188
Church Attend.	-.220	-.219	-.219	-.216	-.224
	Matched Data				
Distance	.095	.047	.031	.057	.040
Variables					
Political Interest	-.083	.002	.060	.009	.023
Gender	-.012	-.024	-.044	-.024	.012
Income	.012	-.099	-.006	-.050	-.010
Education	-.071	.015	-.064	-.073	-.017
Age	.173	-.045	.054	-.007	.083
Participation	.049	.017	.059	.074	.075
Pol. Knowledge	-.094	-.083	-.070	-.087	-.072
Conflict Avoidance	-.020	-.046	-.024	-.022	-.041
Married	-.012	-.095	-.025	-.062	-.062
Party ID	-.002	-.023	.015	.013	.046
Impt. Of Detainee Policy	-.028	.024	-.030	.000	.062
Church Attend.	.072	.044	.057	.042	.003

Genetic Matching (Diamond and Sekhon 2005; Sekhon n.d.) implemented in R library *MatchIt*. Balance statistics computed using *MatchIt* (Ho et al., 2007a; 2007b)

Note: The following descriptive statistics apply to the all respondents interviewed in the initial, pre-treatment wave; the results presented in the paper utilize a subset of the data (treated compliers vs. pure controls).

Table A2: Networks in the Levin Study

Dyad Characteristics		Overall Network Characteristics	
% that are…		***Averages***	
Spouse	14.0	Size (0-3)	2.66
Female	34.6	Disagreement (Partisanship) (0-1)	.56
A relative	35.7	Freq. of Discussion (0-2)	.98
A friend	41.9	Freq. of Disagreement (0-2)	.48
A co-worker	14.3	Level of Knowledge (0-2)	1.29
A fellow church member	9.2		
A member of some other group	10.0		
A neighbor	7.0		
Totals:	2,391 dyads	900 respondents (wave 1; 91.6% of these reported one or more discussants)	

Table A.3: The Levin Study in Comparison:
Network Characteristics in Other Ego-Centric Studies (Averages)

2000 ANES		1992 CNEP		Levin Study	
Size (0-4)	1.86	Size (0-5)	3.78	Size (0-3):	2.66
Disagreement (Candidate) (0-1)	.33	Disagreement (Candidate) (0-1)	.44	Disagreement (Partisanship) (0-1)	.56
Freq. of Discussion (0-3)	1.46	Freq. of Discussion (0-3)	1.60	Freq. of Discussion (0-2)	.98
---	---	Freq. of Disagreement (0-3)	1.34	Freq. of Disagreement (0-2)	.48
Level of Knowledge (0-2)	.93	Level of Knowledge (0-2)	1.05	Level of Knowledge (0-2)	1.29

Table A.4: Disagreement in the Levin Study Dyad-Level Disagreement and Expertise				
% of dyads…				
	Discuss Politics	**Disagree about Politics**		**Level of Political Expertise**
Very Often	21.9	9.3	*A Great Deal*	39.4
Often	54.8	29.3	*Some*	51.0
Rarely	23.3	61.4	*Not Much*	9.4
Supporting Same Political Party as Ego		44.8		

Dyadic Disagreement by Partisanship

	Partisanship of Ego		
% of dyads that disagree…	Democrats	Republicans	Independents
Often/Very Often	40.3	37.0	40.9
Rarely	59.7	63.0	59.1

ENDNOTES

[1] All data, derivatives thereof used in this paper, and supporting code will be placed in an online archive upon publication of this paper (url to be provided in published manuscript).

[2] Polimetrix recruited subjects 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. On the other hand, this population may be reasonably representative of the people who attend political events, which is a central focus of our effort.

[3] We administered the baseline survey July 18-25, 2008.

[4] These background materials will be provided in an online archive, along with all data and supporting code used in this paper.

[5] We administered the post-treatment survey August 5-8, 2008.

[6] These response rates are calculated using AAPOR RR6, which is the response rate appropriate to opt-in survey panels (Callegaro and Disogra, 2008, 1022). The full study design included three conditions: the treatment (the webinar), a partial control group (that would receive information only), and a pure control group. In this paper, we focus on comparing the compliers (treated group) to the "pure" controls.

[7] The question of "who participates" is in itself an important one, which we have directly examined in another paper using two distinct, yet related studies (cites omitted). For present purposes we treat this question as a methodological annoyance.

[8] *MatchIt* and other matching packages require that there be no missing data (Ho et al. 2007a; 2007b). On this point, Ho et al. recommend the following: "If there are missing values in the data set, imputation techniques should be used first to fill in ("impute") the missing values (both covariates and outcomes), or the analysis should be done using only complete cases (which we do not in general recommend)" (2009: 38).

[9] The number of dyads in the data sets is 896 – this comes from the 327 "egos," being stacked to account for up to three discussants per person. 88.4% of the 327 respondents listed three discussion partners; 3.4% listed two; 2.1% listed one; 6.1% listed no discussants.

[10] These probabilities are the result of 1,000 simulations, estimated using *Zelig* (Imai et al. 2009). All other covariates were held at mean values. The baseline probability of discussing detainee policy was .17 (95% CI: .11, .25; 95% CI for first difference: .07; .25); it was .16 for Levin (95% CI: .10, .24; 95% CI for first difference: .05, .22).

[11] The full array of parameter estimates is available upon request.