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.

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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: 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 – 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 (Levine et al. 2005: 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 they acquire (Stasser and Titus 1985; 2003; Sunstein 2006). If these criticisms are correct, they suggest that deliberative forums can, at best, have a very limited impact. Sunstein (2006: 14) warns that, "[D]eliberation often fails to aggregate information even as it increases agreement and confidence among group members. A confident, cohesive, error-prone group…is nothing to celebrate."

[1] While some prominent, empirical studies have avoided calling discussion a form of participation (Verba et al. 1995; Burns et al. 2001), the (sizable) literature on interpersonal networks (e.g., Huckfeldt and 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 et al. (2009)).

Do deliberative encounters reverberate through interpersonal discussion networks in the way that Sunstein describes? 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 et al. 2011; Fishkin and 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 (Neblo, 2005; Dryzek 2010; Habermas 1996; Mansbridge 1999; 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 "wilds" of the larger democratic public. 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.

The essential insight from the "hidden profile" paradigm of laboratory experiments is that subjects will tend to focus on their common information, rather than sharing their novel, "private" information with others (Stasser and Titus 2003 2003); this is something that Sunstein, in particular, argues is potentially devastating for deliberation (2006: 83). Extrapolating from this, our focus becomes on discerning whether individuals who attend a deliberative event and acquire "private" information from participating, share that information with others in their network who did not attend the event and are not otherwise privy to it.

On the one hand, mapping "hidden profile" laboratory experiments (e.g., Stasser and Titus 1985) onto formal deliberative events is problematic. The hidden profile relies on an experimental paradigm where subjects are collectively assigned a task, and each given a mixture of shared and private information to discuss. There are clear distinctions between this set-up and typical deliberative events (e.g., Fishkin 2009); the distinctions are perhaps even clearer between formal and lab-based deliberative events on the one hand, and the everyday political discussion that takes place in the mass public (Conover et al. 2002; Eveland et al. 2011).[2] For example, "informal" political discussion is generally embedded in long standing relationships (v. strangers), and takes place through varied and multiple interactions that take unfold over years (v. in minutes). In addition, the conversational dynamics of everyday political talk are also potentially quite different, as topics of discussion are chosen by people (v. assigned to participants), and political discussion may not be explicitly instrumental (at least in the same way as it is directed to be in the structured settings that emphasize group rewards for high performance (Eveland et al. 2011; Lyons and Sokhey forthcoming)).

On the other, although laboratory and formal conversations are clearly different from informal political conversations, extant research leaves us with few expectations about patterns of diffusion surrounding deliberative events. The literature on interpersonal political discussion networks (e.g., Huckfeldt and Sprague 1995; 2004; for a review, see Sokhey and Djupe 2011) has never really addressed this question. That said, if anything, the work on social influence in the mass public 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 et al. 2011; Lyons and Sokhey forthcoming), political expertise (Huckfeldt 2001), persuasion (e.g. Ryan 2013), social pressure (e.g., Sinclair 2012), or some other factor. And, although the hidden

[2] For related discussions, see Ahn et al. (2013), whose experimental work combines "small-group dynamics with network representations of communication" (360)

profile research reports an overemphasis on shared information, it does not state that privately held information is entirely ignored.

In short, whether formal deliberation affects broader political communication in the mass public remains an open question. Accordingly, our objective is to examine the impact of formal deliberative events beyond their immediate participants. To preview, 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 potentially large multiplier effect, and thus even small-scale deliberation may have a relatively broad impact on politics and public opinion.

To test for 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 7pm, on July 28, 2008, and invited 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 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 inter-personal 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 and Lazarsfeld 1955). Informed by this and other works of the Columbia school (e.g., Berelson et al. 1954; Lazarsfeld et al. 1948), contemporary researchers have examined interpersonal political discussion in the context of campaigns, linking it to – among other things – political participation (Rolfe 2012; Fowler 2005); vote choice (e.g., Beck et al. 2002; Huckfeldt and

Sprague 1995; Sokhey and McClurg 2012), attitudinal strength and opinion formation (e.g., Huckfeldt et al. 1995; Levitan and Visser 2008), partisanship (e.g., Kenny 1994; Sinclair 2012), and participation (e.g. Klofstad 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 have a strong tendency to focus on discussing information shared in common *ex ante* (Stasser and 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

To reiterate, 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; Neblo, 2014). To this, we add the observation that political information typically follows a two-step flow; this of course dates to some of the earliest studies in political communication (Katz and Lazarsfeld 1954), where exposure to news spurred interpersonal discussion about that news. We then pair this with the (substantial and growing) evidence of social influence among peers regarding political attitudes (e.g., Huckfeldt and Sprague 2000; Lazer et al 2010; Levitan and Visser 2008; 2009) – a process presumably driven by discussions regarding

politics. When viewed together, the logic that emerges is that the effects of a deliberative event on citizen discourse can be broken down into direct effects on the individuals involved, and subsequently, into 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.

> **Hypothesis 1:** *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 et al. 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 discussions. The logic here is fairly straightforward: one would venture 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 popular 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 electoral politics more generally. In other words, 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.

> **Hypothesis 2:** 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?

We have noted critics' questions about the quality and desirability of deliberation. Some critiques focus on how *individual characteristics* may influence deliberative outcomes – at base, these involve not only SES-related questions of citizen capacity (e.g., Delli Carpini and Keeter 1996; Verba et al. 1995), but also characteristics like conflict avoidance (Mutz 2002; Mutz and Martin 2001) that jell more with studies of personality and politics (e.g., Gerber et al. 2012; Mondak 2010). For example, Sunstein (2006: 206) notes that individuals from low-status groups have less influence in deliberative settings, and are less likely to share the information they hold (Sunstein 2006: 206; c.f. Neblo, 2007); Neblo et al. (2010) examine how conflict avoidance predicts unwillingness to deliberate.

If only the resource-rich (as gauged by SES and interest), or perhaps those with "personality personalities"/certain orientations towards politics, are willing – and able – to 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., McPherson et al. 2001; Marsden 1987).

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 members, we again arrive at the undesirable outcome of an echo chamber – citizens will help spread information, but only when they agree with it. Classic theories of social information seeking (e.g., McPhee 1963; Sprague 1982) suggest something a bit different: here

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 named 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 her propensity for 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 and Martin 2001), this only heightens concerns that subsequent discussions – the "ripple 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 et al. 2004; Mutz 2006) become even more critical, as do considerations of network sophistication (e.g., 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 and Van Alstyne 2011). "Strength" is a somewhat heterogeneous construct that captures frequency of communication, multiplexity, and affect, among other things (Carpenter et al. 2003; Hansen 1999). At the same time, it is often weaker ties that provide novel and potentially disagreeable information (Huckfeldt et

[3] For a discussion of motives as they relate to political discussion, see Eveland et al. (2011) and Lyons and Sokhey, forthcoming.

al. 1995; Granovetter 1973). If we see that only certain types of ties promote further dissemination – i.e., those represented by strong ties (e.g., close friends, family, and relatives) – we have another reason to think that "deliberative ripples" may be less valuable than we 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[4]

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

[4] Data and supporting code will be placed in an online archive upon publication (url to be provided in published manuscript).

roommate studies (e.g., Sacerdote 2001; Klofstad 2007; 2011). 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. 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) (see also Nickerson (2009) for a discussion of experiments and diffusion), 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). 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 (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.[5]

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 et al. 2009; Sokhey and Djupe 2013 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.). In addition to the network battery, the baseline survey included a series of demographic and attitudinal questions that serve as pretreatment control variables (see appendix).[6]

The online town-hall with Sen. Levin took place in July, 2008lasting 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).[7] 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 Senator Levin. A moderator posted the questions sequentially, but only allowed participants to

[5] Variable coding, descriptive statistics, and a cursory comparison of network characteristics between our study and several nationally representative ones (e.g., 2000 ANES) appear in the appendix.
[6] We administered the baseline survey July 18-25, 2008.
[7] These background materials will be provided in the online archive.

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 also 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.[8] Put differently, we supplied a controlled stimulus – exposure to a deliberative event – and then examine 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. Of the 462 people we invited, only 175 chose to participate (37.9% percent compliance rate). If this problem was left unaddressed, we would not be able to tell whether the event produced substantial "ripple effects," or whether people who have lots of conversations chose to participate selectively.[9] In other words, because participant selection is still a concern, we have to account for this selection process to make reliable causal inferences – something well-documented by scholars working on field experiments (e.g., Esterling et al. 2011; Imai 2005).[10]

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

[9] 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.

[10] To reiterate, 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 mainly on comparing the compliers (treated group) to the "pure" controls.

To address this threat, we will 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 and Green 2012: 150). The other is the complier average causal effect (CACE), which is an estimate of the average treatment effect for a subset of the subjects – i.e., 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 and Green 2012: 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 quantity, we utilize two-stage least squared (2SLS) regression, with treatment assignment as the instrument for actual receipt of the treatment (Angrist, Imbens and Ruben 1996; Gerber and Green 2012).[11]

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 (within the differently assigned groups). 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

[11] 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 the response rate appropriate to opt-in survey panels (Callegaro and Disogra, 2008, 1022). Our analyses use conventional list-wise deletion, though in the service of robustness checks – using matching (please see footnote #13), we employed multiple imputation techniques. The results in Table 1 are robust to these different choices/approaches to inference.

condition (with the t-test p-value in parentheses) – i.e., the ITT effect. 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 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 percent more of their network, and discussed Senator Levin with, on average, 5 percent more of their network. Without assignment to the treatment, individuals are likely to discuss detainee policy with 21 percent of their network, and discuss Senator Levin with about 18 percent of their network. With assignment those percentages increase to 30 percent and 23 percent, respectively.

The second row shows the estimated effect of the deliberative sessions on the compliers, or the CACE. Similar to the ITT effects, significant results were found for discussion of the topics of **detainee policy** and **Senator Levin**, but not for general discussion of public affairs. Here the results suggest that compliers who were assigned to the treatment discussed detainee policy with almost 17 percent more of their network, and discussed Senator Levin with about 9 percent more of their network (versus compliers assigned to the control condition).

[Insert Table 1 about here]

For comparison, the third 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 within their networks. In fact, to the extent this affected discussion of topics, the results suggest a negative effect. Since no significant difference is observed between the information condition and the pure control cases, we collapse these conditions for subsequent analyses.

Treatment Effects with Covariates

The above 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 above 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 towards subsequent discussion of these issues (Gerber and Green 2012: 109). To address this issue, Tables 2 and 3 adjust 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 those produced if we had block randomized the sample based on these same factors (Rosenberger and Lachin 2002; Gerber and Green 2012: 114).

The results confirm the conclusions from the previous section, with Table 2 displaying intent-to-treat effects (ITT), and Table 3 complier average causal effects (CACE). There does not appear to be an imbalance in the randomization process that produced the previous estimates.[12] 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").

[12] We also used genetic matching (see Diamond and Sekhon 2010) to further improve balance between group characteristics, and to address model dependence (Ho et al. 2007). This produced the same substantive conclusions. Results are available from the authors upon request.

[Insert Tables 2 and 3 about Here]

Addressing Concerns about Heterogeneous Treatment Effects

However, even if we observe positive impacts of deliberative sessions on subsequent sharing of information, the findings could still be criticized if particular subclasses were not affected – or, worse, actively disadvantaged – by deliberation (e.g. Sunstein 2006; Sanders 1997). Thus in the remainder of this paper, we test whether deliberation's effects are conditioned on monadic (individual attributes) and network/dyadic characteristics (attributes of participant's 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): (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).

Figures 1 and 2 show the results of interacting the treatment with these covariates, producing the conditional intent-to-treat (CITT) effect, and the conditional complier average causal effect (CCACE). The CCACE is calculated using both the treatment assignment as an instrument for receiving the treatment, and interacting the predicted probability of receiving the treatment with the additional covariate/characteristic (Wooldridge 2011: 951-954).[13] The model of the outcome variable is:

[13] There is some debate about whether both the treatment and interaction should be instrumented simultaneously. We found little difference in the substantive results, and also noted inexplicable patterns in the F-tests when running the models using the alternative method.

$$y_i = \beta_0 + \beta_1 TREATED_i + \beta_2 CONDITION_i + \beta_3 (TREATED_i * CONDITION_i) + u_i$$

where

$$TREATED_i = \alpha_0 + \alpha_1 ASSIGNED_i + e_i.$$

Each panel shows a plot of the CITT effect and CCACE, 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 and Green 2012: 298).[14] The F-statistic is calculated as:

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

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.

Since we are doing multiple comparisons, we encounter the multiple comparisons problem (Gerber and Green 2012: 300) – i.e., in testing a large number of group partitions, it is possible that at least one covariate interaction will show statistical 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 Benferroni correction, where the target p-value for significance is divided by the number of hypothesis tests. In the case of Figures 2 and 3, there are 9 hypothesis tests, so the target p-value for significance at the 0.05 level becomes 0.006.

In Figure 1 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

[14] Error bounds on the interaction can be somewhat misleading, especially for the CCACE (since an interaction can be statistically significant while only marginally decreasing (or even increasing) the sum of squared residuals).

evaporates when we use the Benferroni correction standard. We find a similar case for the importance of detention policy in the CCACE results.

While these results are indicative, it is difficult to say with certainty that these effects are significant/insignificant. However, the direction of the detention policy importance effect is surprising, suggesting that those for whom the issue is more important discuss it with a smaller portion of their network. 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 percent 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.

[Figure 1 about Here]

Figure 2 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 Benferroni standard). In both the CITT and CCACE panels, 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 less than 10 percent of the sample) talked with a smaller portion of their network about the Senator after the session.

[Figure 2 about Here]

Next we turn our attention to network/dyadic characteristics, as there are several that might affect subjects' propensity to talk about issues with certain people – 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 et al. 2013), we utilize all three measures.

Figures 3 and 4 display 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 and 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 Figures 3 and 4, spousal dyads reach conventional levels of statistical significance for the issue of Senator Levin (in the case of the CCACE, this passes the Benferroni threshold for significance as well). 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 actually predicts an increase in discussion (although this effect does not approach statistical significance).

[Figures 3 and 4 About Here]

As we noted previously, 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 (below we discuss additional strategies). Here the values for dyadic characteristics are turned into network averages, and the presence of a spouse, relative, friend, co-worker or neighbor are noted with dummy variables. The

dependent variable – as in the first sets of estimates – again becomes the percent of the network with whom the participant discusses the subject matter. This solves the SUTVA violation, but gives us much coarser information.

Figures 5 and 6 show the results of these analyses. In the detainee results, having a relative in the network passes all threshold of significance for the CITT effect, but not for CCACE. As in the last figures, we again see that having a spouse in the network increases the probability of discussing Senator Levin (this passes all significance thresholds in the CCACE). 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 Benferroni cutoff for the 90 percent significance level). *However, it is in the opposite direction of what 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 CCACE, and in the CITT effect for discussion of the topic of **Senator Levin**.

[Figures 5 and 6 about Here]

In the supplementary information file/online appendix, we perform an additional test to check the dyadic estimates (this involves a re-sampling procedure; we do this, specifically, as another caution against potential biases caused by a violation of SUTVA). Across the checks, the conclusions remain the same – the impact of our deliberative session on subsequent discussion was generally positive. And, this "ripple" effect seems to hold, regardless of subgroups, dyadic relationships, and network types. The effect is not limited to some, conditional on certain individual

(e.g., interest) and/or network characteristics (e.g., expertise), and is robust to that most fundamental of social forces, interpersonal disagreement.

Discussion and Conclusion

A frequent critique of formal deliberation is that it can have only a very 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, diffusing throughout the social networks of 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.

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. Further, we view this as a conservative estimate, as 1) it considers only a very thin slice of the alters in the broader networks of participants (given the nature of the name generator/data available), and 2) we are not able to estimate (potential) additional ripple 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 regarding 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 – via 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 towards 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 US Senator. Would, for example, a small group discussion about the same subject have had the same ripple effects? Would other types of exposures (to news, etc) regarding the same subject have had similar effects? The experimental paradigm in this paper 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."

Appendix: Variable Coding and Descriptive Statistics

Variable 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:
▪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.

Network Characteristics/Descriptives

Note: The following descriptive statistics apply to the all respondents interviewed in the initial, pre-treatment wave.

Table A1: 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.2: 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	.33	Disagreement	.44	Disagreement	.56

(Candidate) (0-1)		(Candidate) (0-1)		(Partisanship) (0-1)	
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.3: 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

Table 1: Estimated ITT and 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)
Information Treatment vs. Control (ITT) (N=287)	-0.006 (0.562)	-0.016 (0.674)	0.003 (0.467)

Table 2: Intent-To-Treat (ITT) Effect of Deliberative Sessions Controlling for Confounding Covariates.

	Topic of Conversation								
Covariates		Detention (ITT)			Politics (ITT)			Levin (ITT)	
	Coef.	Std. Err.	P>t	Coef.	Std. Err.	P>t	Coef.	Std. Err.	P>t
Participated in	0.112	0.029	0.000	0.003	0.028	0.928	0.082	0.026	0.002

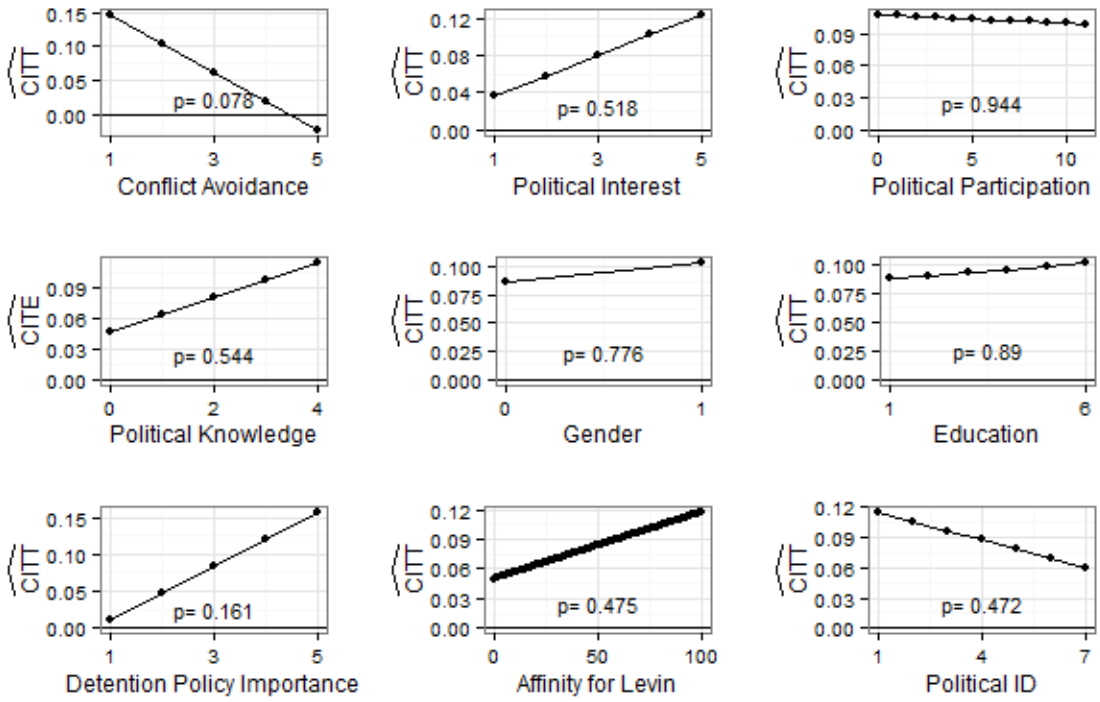
Session									
Political Characteristics									
Pol. Interest	0.048	0.024	0.045	-0.003	0.023	0.881	0.002	0.021	0.926
Affect for Levin	0.000	0.001	0.923	-0.001	0.001	0.172	0.000	0.001	0.970
Pol. Part. Index	0.015	0.008	0.069	0.024	0.008	0.003	0.035	0.007	0.000
Political Knowledge	0.001	0.016	0.939	0.024	0.016	0.130	-0.024	0.015	0.106
Conflict Avoidance	-0.031	0.013	0.021	-0.032	0.013	0.013	-0.020	0.012	0.102
Party ID	0.005	0.008	0.520	-0.013	0.008	0.097	0.011	0.007	0.134
Impt. of Detention Issue	0.027	0.014	0.060	-0.012	0.014	0.365	-0.002	0.013	0.897
Network Characteristics									
Avg. Freq. of Discussion	0.158	0.036	0.000	0.187	0.034	0.000	0.105	0.032	0.001
Avg. Freq. of Disagrement	0.001	0.032	0.963	0.010	0.030	0.734	-0.025	0.029	0.380
Network Sophistication	-0.034	0.035	0.329	0.014	0.034	0.670	0.005	0.032	0.884
Demographics									
Male	0.050	0.032	0.112	0.021	0.030	0.481	-0.010	0.029	0.738
Income	0.001	0.005	0.844	0.001	0.005	0.798	0.000	0.004	0.976
Education	0.002	0.011	0.863	-0.004	0.010	0.692	0.007	0.010	0.492
Age	0.000	0.001	0.723	0.001	0.001	0.624	0.001	0.001	0.252
Married	-0.028	0.034	0.406	0.051	0.033	0.116	-0.008	0.031	0.794
Constant	0.460	0.197	0.020	1.022	0.190	0.000	0.181	0.178	0.311
	n=521								

Table 3: Complier Average Causal Effects (CACE) of Deliberative Sessions Controlling for Confounding Covariates.

	Topic of Conversation								
Covariates		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
Political Characteristics									
Pol. Interest	0.056	0.024	0.017	-0.003	0.023	0.888	0.008	0.021	0.685
Affect for Levin	0.000	0.001	0.915	-0.001	0.001	0.163	0.000	0.001	0.896
Pol. Part. Index	0.011	0.008	0.162	0.024	0.008	0.003	0.032	0.007	0.000
Political Knowledge	-0.001	0.016	0.965	0.024	0.016	0.123	-0.025	0.014	0.071
Conflict Avoidance	-0.024	0.013	0.077	-0.032	0.013	0.014	-0.015	0.012	0.214
Party ID	0.004	0.008	0.640	-0.013	0.008	0.090	0.010	0.007	0.156
Impt. of Detention Issue	0.024	0.014	0.083	-0.012	0.013	0.354	-0.004	0.012	0.774
Network Characteristics									
Avg. Freq. of Discussion	0.157	0.035	0.000	0.187	0.034	0.000	0.105	0.031	0.001
Avg. Freq. of Disagrement	-0.001	0.031	0.975	0.010	0.030	0.732	-0.027	0.027	0.324
Network Sophistication	-0.033	0.034	0.337	0.014	0.033	0.664	0.005	0.030	0.856
Demographics									
Male	0.047	0.031	0.133	0.021	0.030	0.476	-0.012	0.027	0.654
Income	0.000	0.005	0.975	0.001	0.005	0.798	-0.001	0.004	0.871
Education	0.003	0.011	0.760	-0.004	0.010	0.690	0.008	0.009	0.407
Age	0.000	0.001	0.763	0.001	0.001	0.616	0.001	0.001	0.210
Married	-0.023	0.033	0.498	0.052	0.032	0.109	-0.004	0.029	0.892
Constant	0.419	0.196	0.033	1.021	0.189	0.000	0.151	0.172	0.380
	n=521								

Figure 1: Effects for Monadic Characteristics, Post-Treatment Discussion of **Detention Policy**

A. Conditional Intent to Treat Effects (CITT)



B. Conditional Complier Average Causal Effects (CCACE)

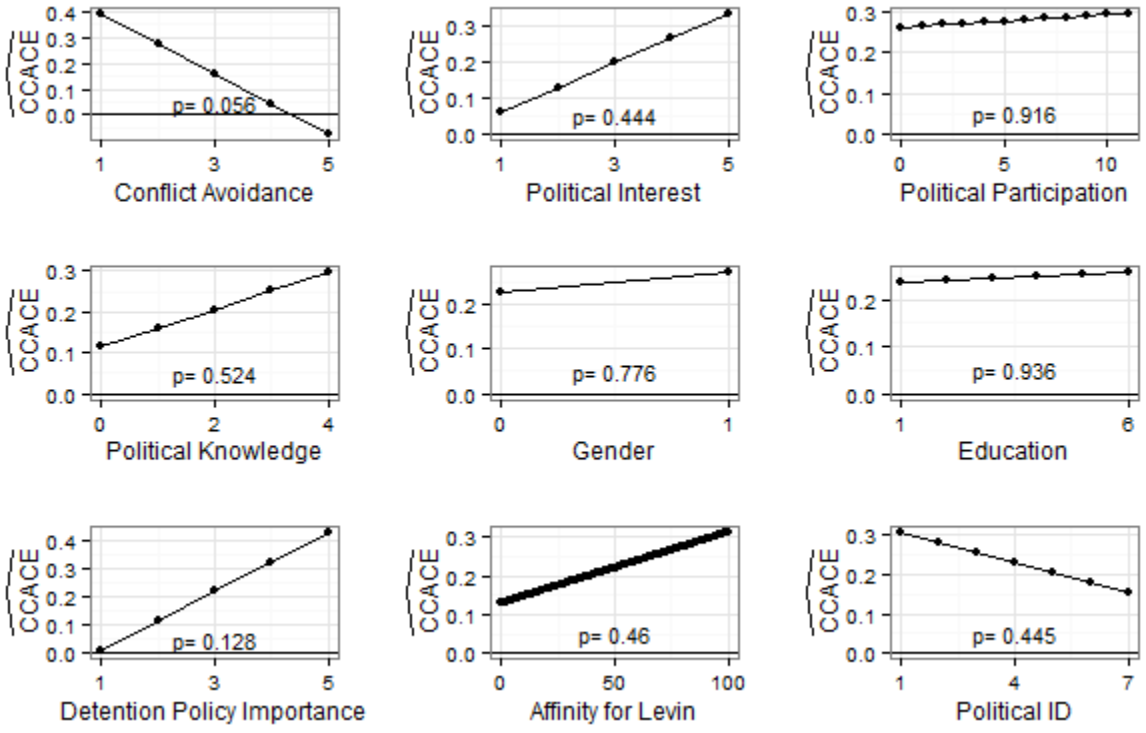
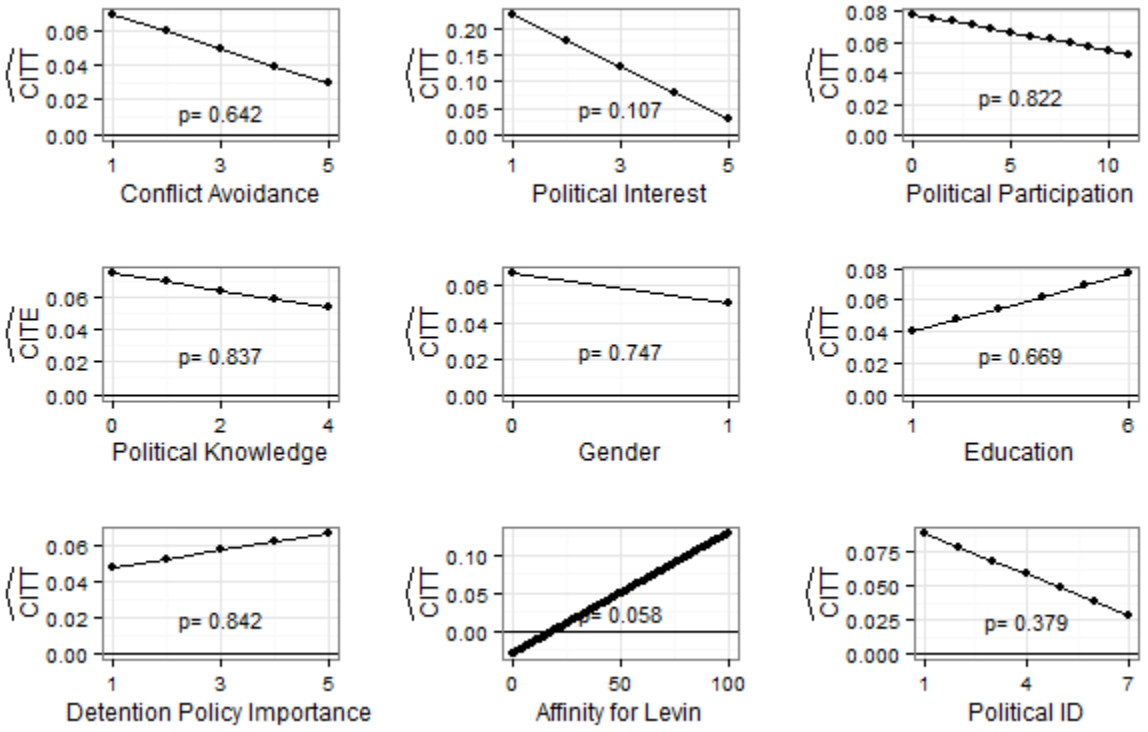


Figure 2: Effects for Monadic Characteristics, Post-Treatment Discussion of **Senator Levin**

A. Conditional Intent to Treat Effects (CITT)



B. Conditional Complier Average Causal Effect (CCACE)

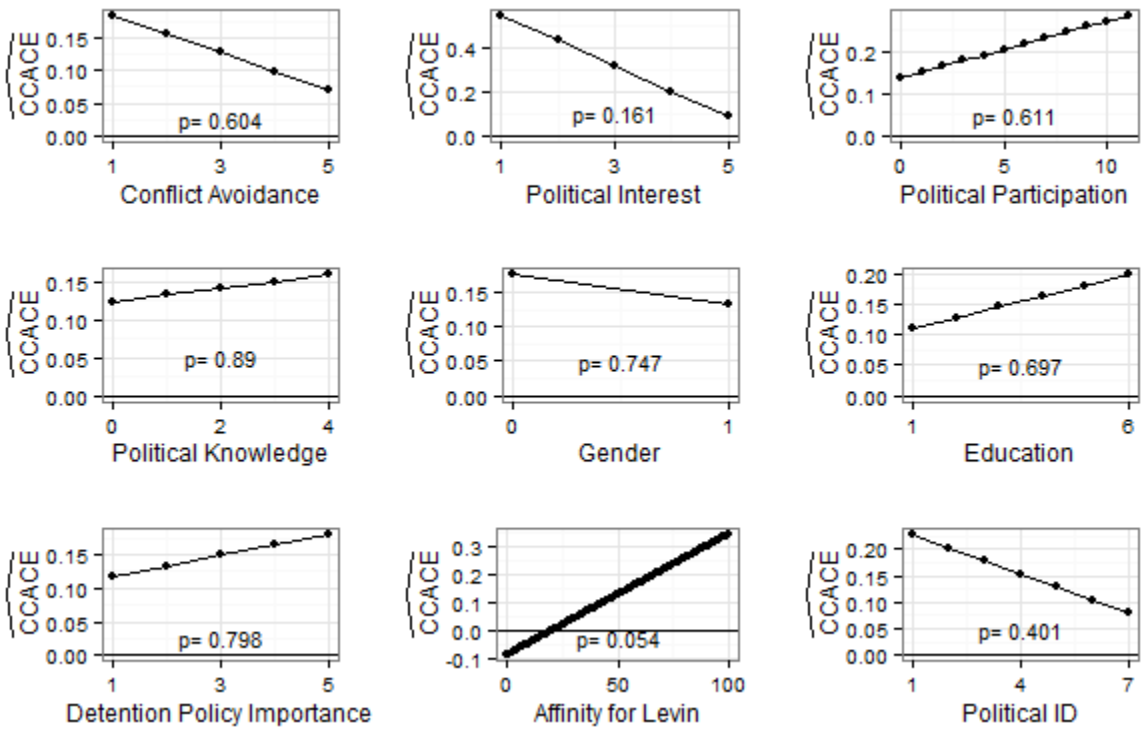
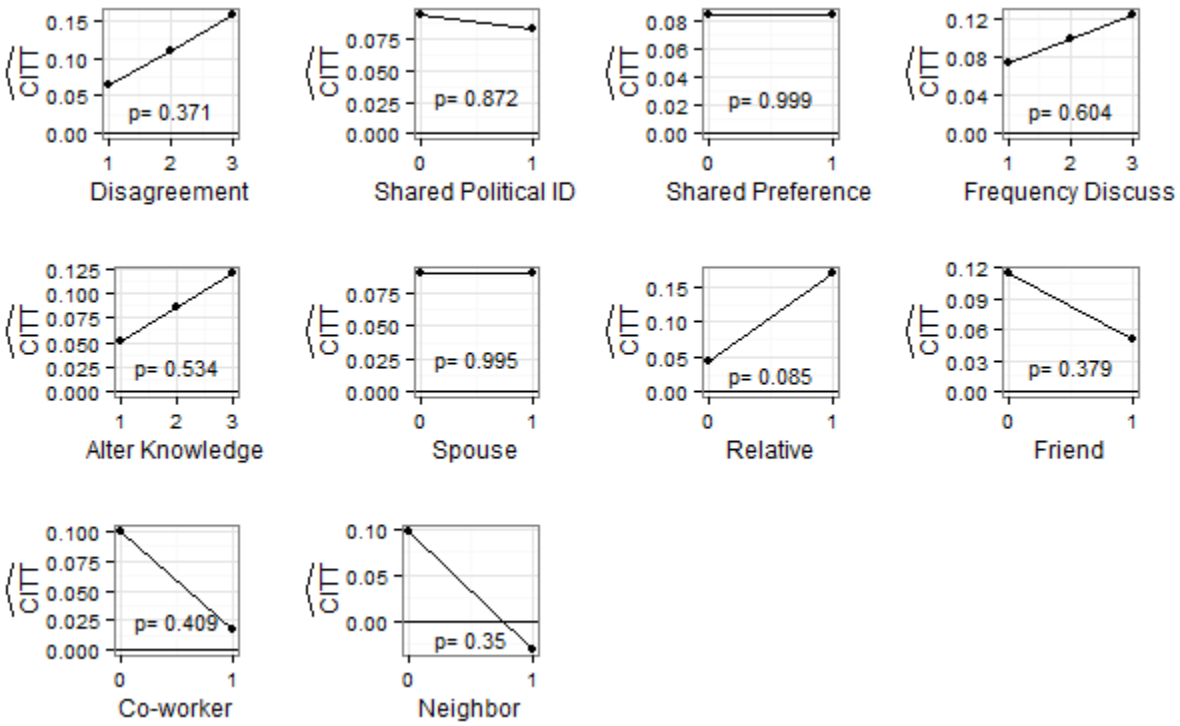


Figure 3: Effects for Dyadic Characteristics on Post-Treatment Discussion of **Detention Policy**

A. Conditional Intent to Treat Effects (CITT)



B. Conditional Complier Average Causal Effect (CCACE)

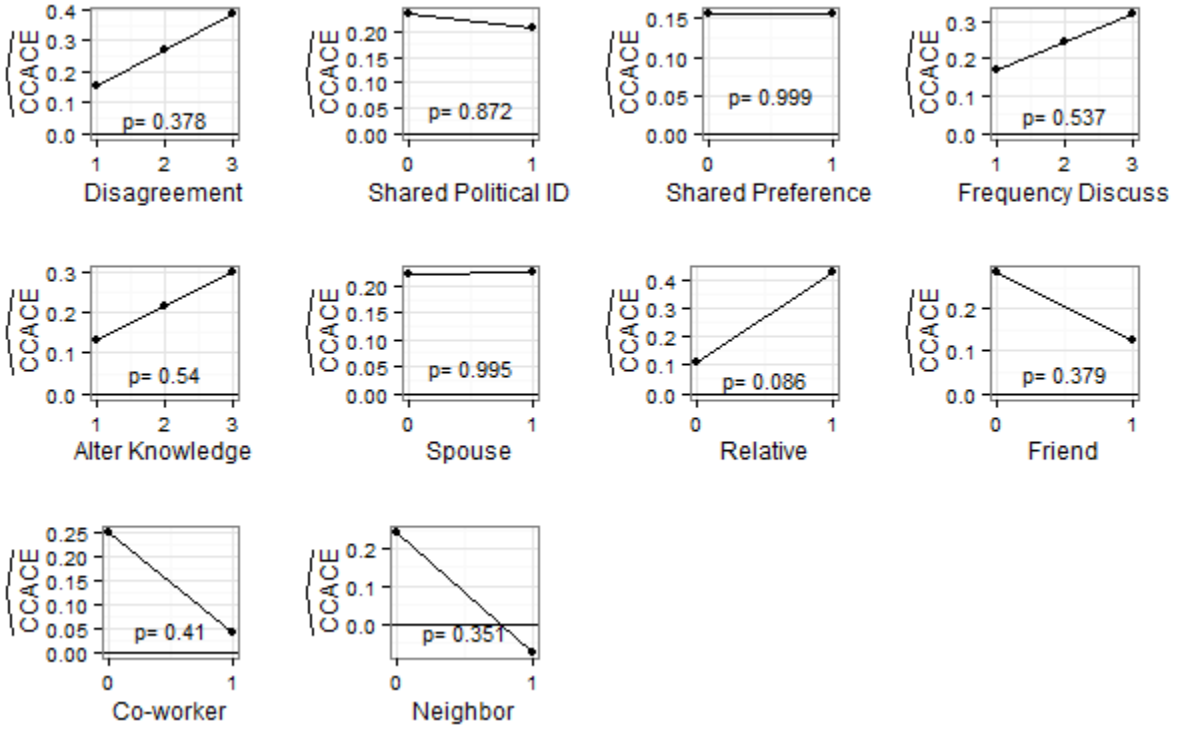
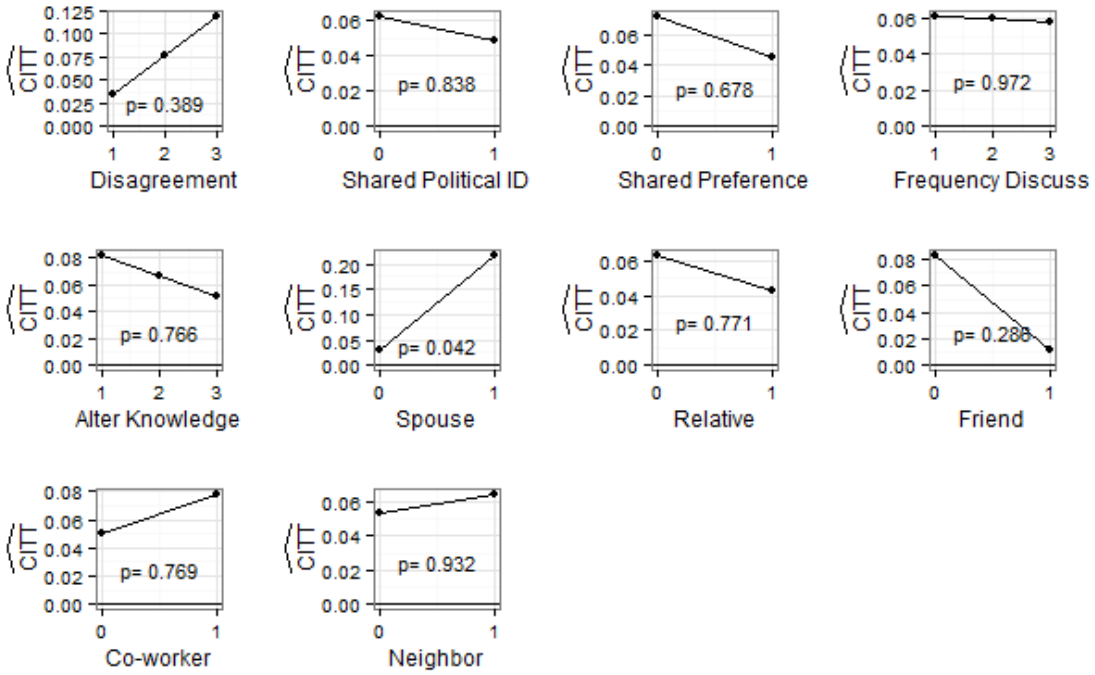


Figure 4: Effects for Dyadic Characteristics on Post-Treatment Discussion of **Senator Levin**

A. Conditional Intent to Treat Effects (CITT)



B. Conditional Complier Average Causal Effect (CCACE)

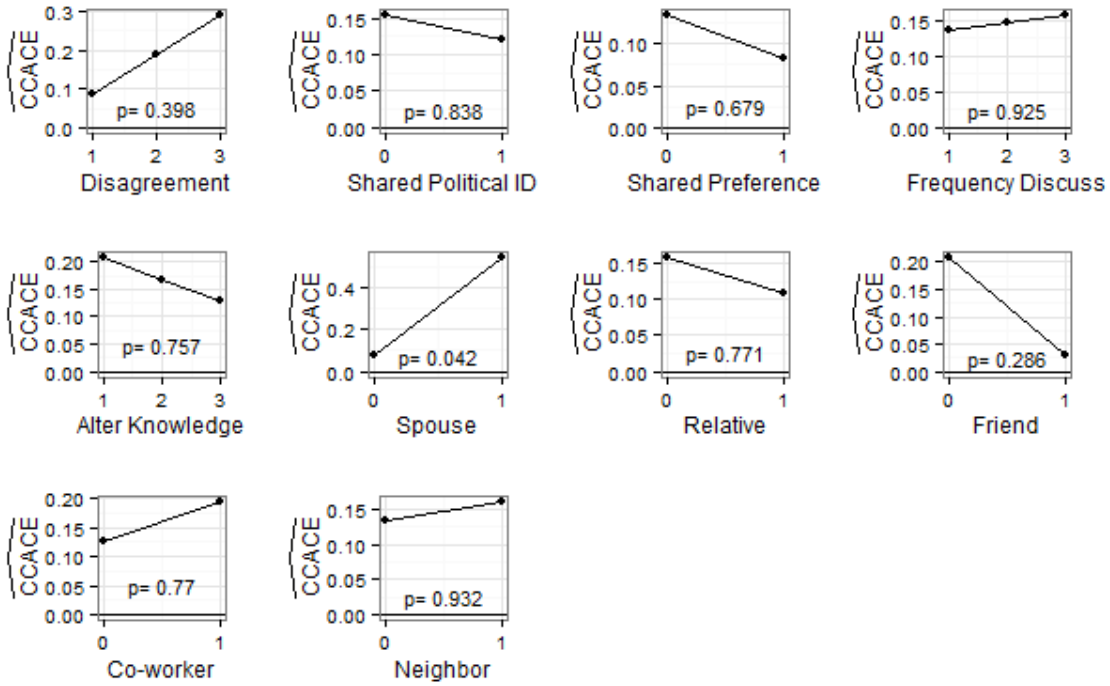
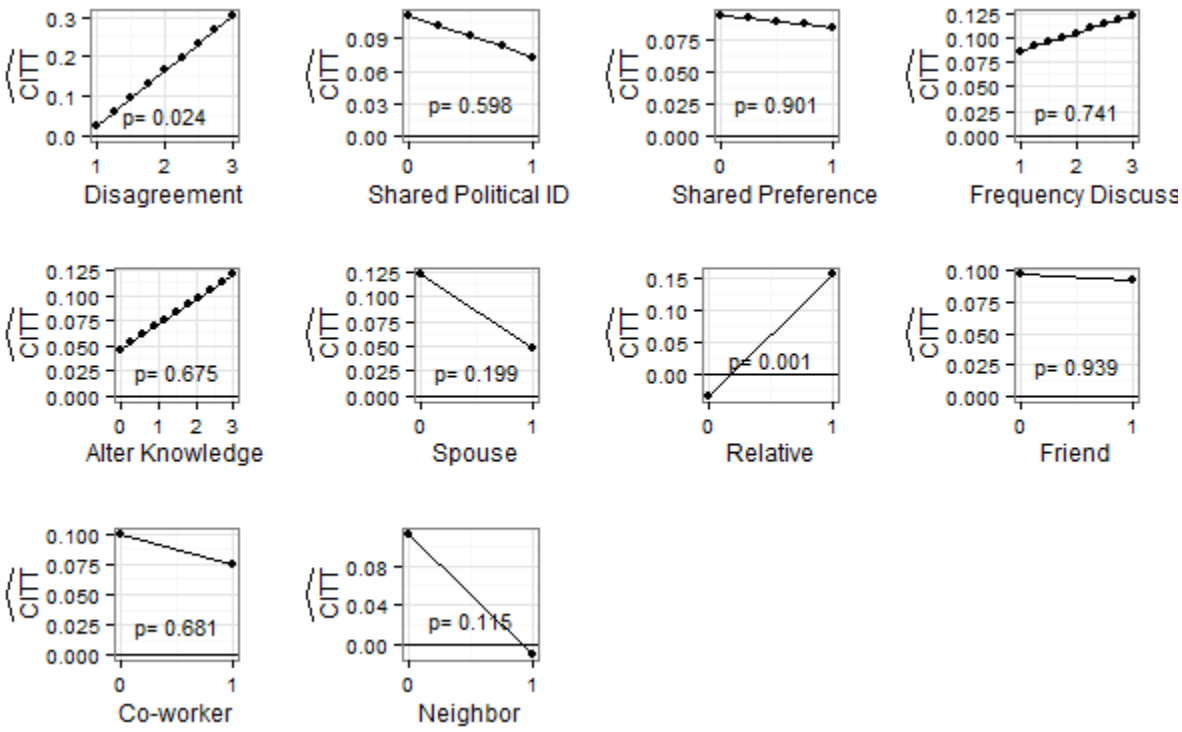


Figure 5: Effects for Dyadic Characteristics in Monadic Data on Post-Treatment Discussion of **Detention Policy**

A. Conditional Intent to Treat Effects (CITT)



B. Conditional Complier Average Causal Effect (CCACE)

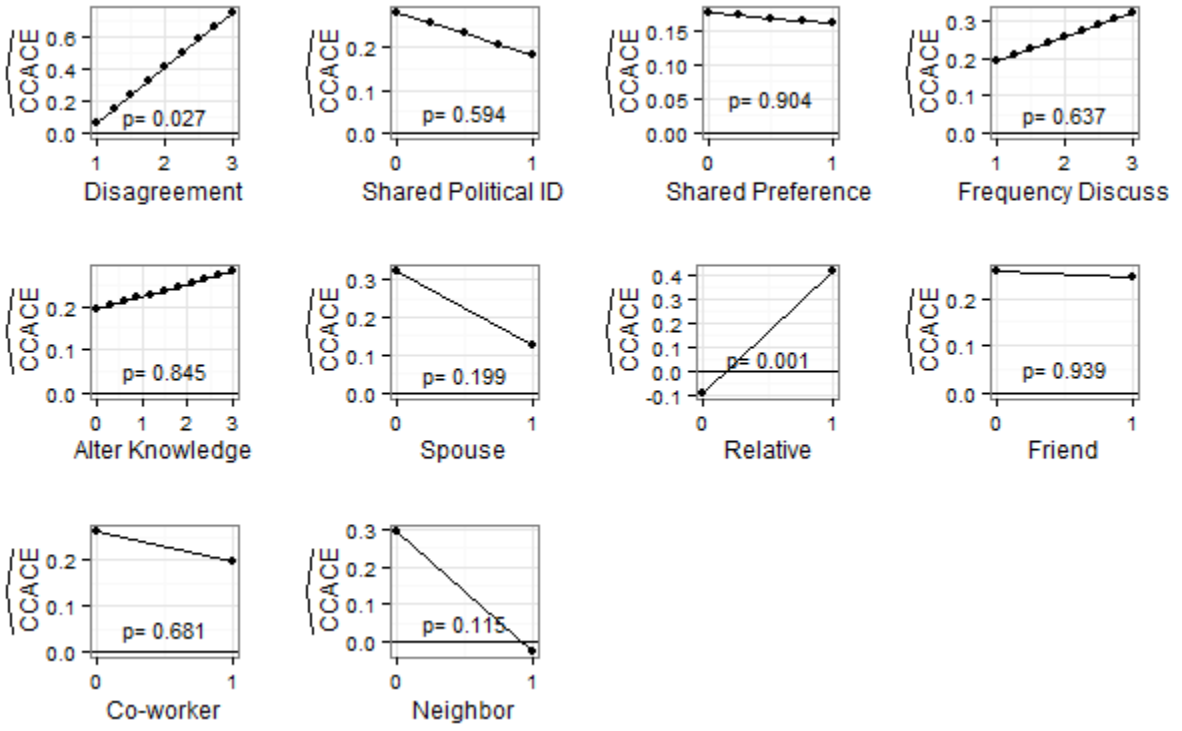
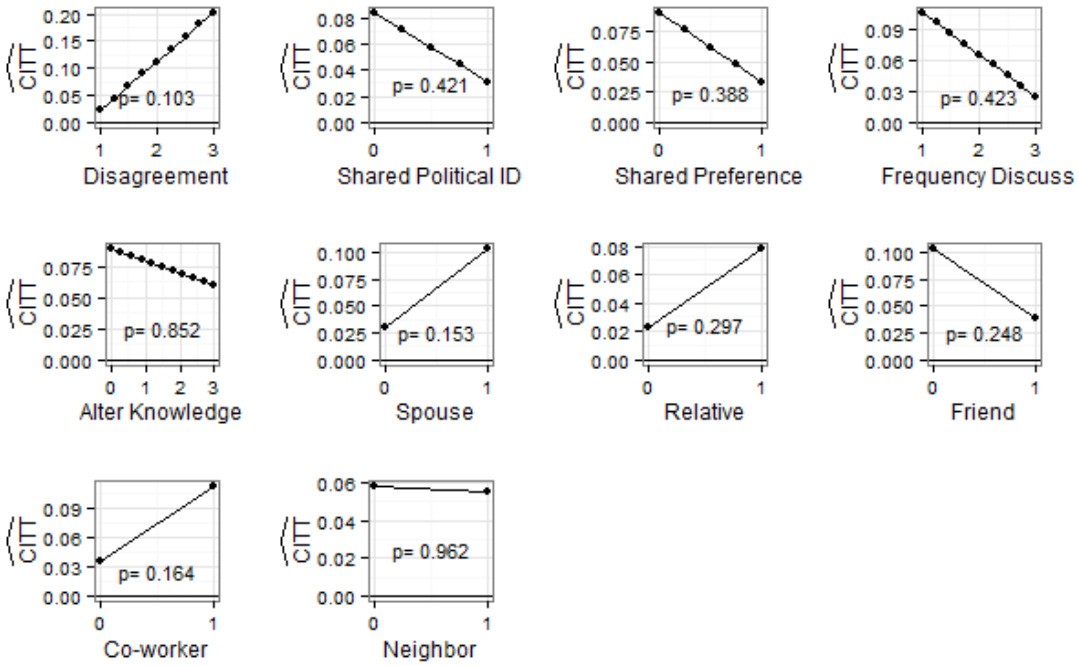
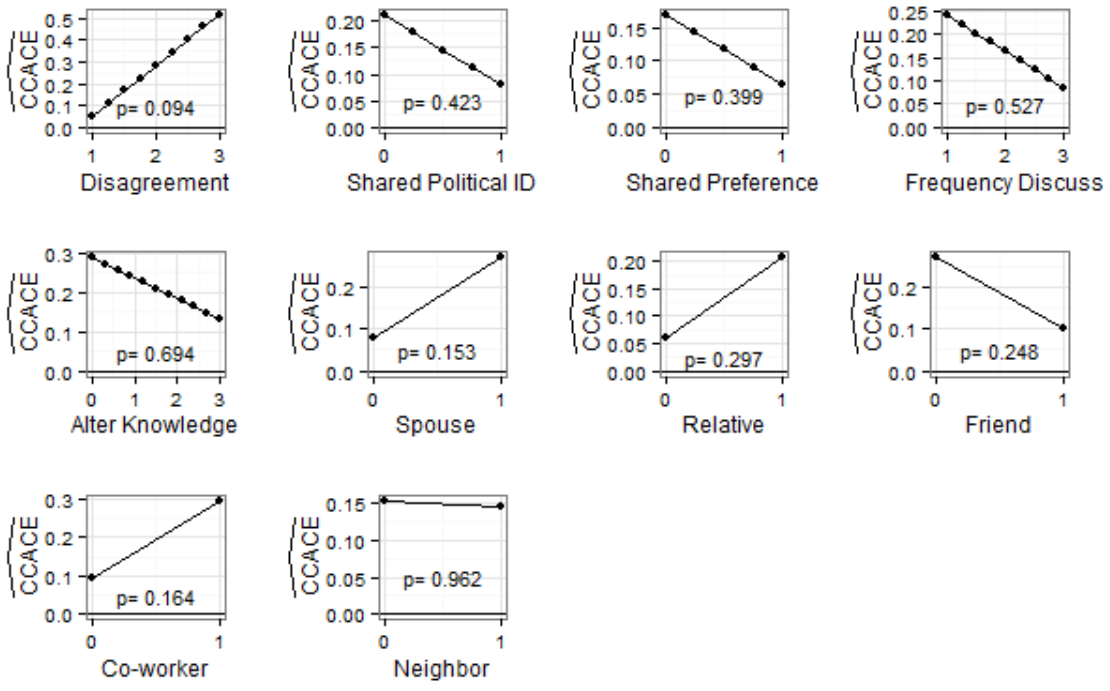


Figure 6: Effects for Dyadic Characteristics in Monadic Data on Post-Treatment Discussion of **Senator Levin**

A. Conditional Intent to Treat Effects (CITT)



B. Conditional Complier Average Causal Effect (CCACE)



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