MEANS, MOTIVE, AND OPPORTUNITY IN BECOMING INFORMED ABOUT POLITICS: A DELIBERATIVE FIELD EXPERIMENT WITH MEMBERS OF CONGRESS AND THEIR CONSTITUENTS

MEANS, MOTIVE, AND OPPORTUNITY IN BECOMING INFORMED ABOUT POLITICS
A DELIBERATIVE FIELD EXPERIMENT WITH MEMBERS OF CONGRESS AND THEIR CONSTITUENTS

KEVIN M. ESTERLING*
MICHAEL A. NEBLO
DAVID M. J. LAZER

Abstract Deliberative theorists emphasize that citizens' capacity to become informed when given a motive and the opportunity to participate in politics is important for democratic citizenship. We assess this capacity among citizens using a deliberative field experiment. In the summer of 2006, we conducted a field experiment in which we recruited twelve current members of the U.S. Congress to discuss immigration policy with randomly drawn small groups of their constituents. We find that constituents demonstrate a strong capacity to become informed in response to this opportunity. The primary mechanism for knowledge gains is subjects' increased attention to policy outside the context of the experiment. This capacity for motivated learning seems to be spread widely throughout the population, in that it is unrelated to prior political knowledge.

KEVIN M. ESTERLING is Associate Professor of Political Science at the University of California–Riverside, Riverside, CA, USA. MICHAEL A. NEBLO is Assistant Professor of Political Science at Ohio State University, Columbus, OH, USA. DAVID M. J. LAZER is Associate Professor of Political Science and Computer Science, Northeastern University, Boston, MA, USA. This work was supported by the Digital Government Program of the National Science Foundation [IIS-0429452 to K.E., M.N., and D.L.] and a grant from the Ash Institute of Harvard University [to D.L.]. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation (NSF) or the Ash Institute. We thank Alberto Abadie, Janet Box-Steffensmeier, Bear Braumoeller, Luke Keele, Gary King, William Minozzi, and Craig Volden for very helpful comments. Earlier versions of this article were presented at the Annual Meetings of the American Political Science Association, Boston, MA, August 28–31, 2008, and Chicago, IL, August 30–September 2, 2007, and at the Program on Network Governance, Kennedy School of Government, Harvard University, May 2007. *Address correspondence to Kevin M. Esterling, UCR Political Science, 900 University Ave., Riverside, CA 92521, USA; e-mail: kevin.esterling@ucr.edu.

Introduction

Since at least Converse (1964) and Stokes and Miller (1962), survey research
has painted a rather grim picture of the average citizen's knowledge of politics.
From these findings, many scholars draw strong normative conclusions regard-
ing the health of contemporary democracy and its capacity to reach egalitarian
ideals (e.g., Luskin 1990, p. 333). Researchers in this tradition advance a per-
suasive claim: Citizens must possess substantial factual knowledge about gov-
ernment, policies, and politicians in order to judge political actors and to be able
to induce effective accountability (e.g., Delli Carpini and Keeter 1996, p. 56;
Converse 1964, pp. 240–41; Stokes and Miller 1962, p. 532). In survey after
survey, however, the typical American scores poorly across a variety of political
knowledge measures. Moreover, the United States is hardly alone in this regard
(Almond and Verba 1963). It follows that the promises of contemporary de-
mocracy may be realized for the privileged few at the top of society's knowl-
edge pyramid, while for most citizens democracy amounts only to "tragedy or
farce" (Delli Carpini and Keeter 1996, p. 60).

   These cross-sectional findings regarding citizen knowledge, while important
in their own right, fail to assess citizens' dynamic capacity to become informed
when given a reason or motivation to do so (Niemi and Junn 1998; Thompson
and Bell 2006). Social and contextual models of cognition emphasize that the
extent to which people encode new information is situational (Tetlock 1983,
1992). For example, when people believe they will be held accountable (Lerner
and Tetlock 1999) to others regarding their views on a topic, they may feel the
need to possess more accurate information (de Dreu, Koole, and Steinel 2000;
Thompson et al. 1994); they may be more self-critical about their own views;
and they may attempt to contemplate the likely views of others (Tetlock, Skitka,
and Boettger 1989, p. 633). When a person reasons in these more integrative
and complex ways, they are likely to more deeply encode new information,
which in turn improves retrieval and recall (Tetlock 1983, p. 290).

   This dynamic capacity for learning is central to deliberative democratic theory,
since it is citizens' (often latent) capacity to become informed that induces rep-
resentatives to exercise judgment on their constituents' behalf (Habermas 1984;
Hamilton, Madison, and Jay 1961; Lupia and McCubbins 1998; Neblo 2005;
Pitkin 1967). The standard "pop quiz" administration of a political knowledge
survey, however, engages the respondent in a situation where she is not likely
to have recently had such a motivation to contemplate the topics that are tapped
by the knowledge items (Gastil and Dillard 1999; Kuklinski et al. 2001; Prior and
Lupia 2008; Visser, Holbrook, and Krosnick 2007). As a result, the typical citi-
zen's capacity to meet democratic ideals remains an open question.[1]

---

1. This is not to say that political knowledge researchers deny that citizens could learn about pol-
icies. The focus of this literature is on static knowledge rather than on dynamic learning, however,
and hence these studies are designed to capture only the former.

To examine the dynamic process of motivated political cognition, we assess citizens' capacity to become informed via deliberative field experiments (see Barabas 2004; Fishkin and Luskin 2005). In the summer of 2006, we conducted randomized field experiments wherein current members of the U.S. House of Representatives discussed immigration policy with small groups of their constituents using a Web-based platform. In the experiment, we tested whether deliberating with a current member of Congress affects, among other things, constituents' factual knowledge about the topic of immigration policy. In all, twelve current members participated in twenty small-group deliberative sessions.

We find that constituents who participate in a deliberative session demonstrate both an evident willingness and a strong capacity to become informed in response to the opportunity to discuss an issue with their representative. These gains appear to stem from an increased motivation to encode policy information gained outside the context of the experiment. Perhaps most encouragingly, this capacity for motivated learning about policy seems to be spread widely throughout the population, in that it is unrelated to prior political knowledge. This last result is perhaps especially important for the normative hopes of deliberative theory. Contrary to some reasonable fears (Sanders 1997), when citizens are motivated to learn, deliberation does not simply magnify the advantages of those who already have a knowledge advantage.

Deliberative democratic theory aspires to be neither utopian nor merely an apology for the status quo (Habermas 1996). The focus is on how realistic changes to the status quo might better reconstruct and realize the normative goals implicit in democratic practice (Bächtiger et al. 2010; Fishkin 2009). Our field experiments fit quite well with this model. On deliberative grounds, then, our results suggest that contemporary democracy has both more solid underpinnings and more hopeful prospects than previously thought.

Experimental Design

In the summer of 2006 we conducted a field experiment giving a sample of U.S. citizens the opportunity to interact with their current member of the House of Representatives on an important and controversial issue, immigration reform, as part of a small deliberative group. The experimental design was built around online e-townhalls, an application that, to date, is seldom used in Congress but is congruent with Congress's trend toward increasing use of social media.[2]

Twelve House members conducted either one or two online deliberative sessions each with a random sample of their constituents. The number of constituents in each session ranged from eight to thirty.[3] The topic of each session was

2. "Capitol Hill Lawmakers Embrace Social Media," http://www.npr.org/templates/story/story. php?storyId=128000198, accessed January 11, 2011.
3. In a separate study, we replicated the small-group exercises in this article with a large group of 200 citizens interacting with Sen. Carl Levin (D-MI) as a means to demonstrate the scalability of the design, with effects very comparable with those we report below (see the online appendix for more details).

immigration and border security policy, and each discussion lasted for thirty-five minutes. Constituents participated by typing comments or questions into the online discussion platform. The questions and comments were posted to a queue visible only to a screener. The screener, in turn, posted them to the whole group in roughly the order they were received.[4] The member responded to the questions and comments through a telephone linked to a computer. Constituents listened to the member's responses over their computer speakers, and also could choose to read the member's responses via real-time transcription. After thirty-five minutes, the member logged off and constituents were directed to a chat room to have an open-ended discussion about the member's responses and immigration more generally. The chat lasted twenty-five minutes.

In this experiment, the deliberative "treatment" is the discussion with the member combined with the post-session chat. Allowing constituents to discuss the session with one another lends a greater realism to the experiment and so improves external validity, since it is rare for citizens to engage in politics in isolation from one another (see Druckman 2004; Druckman and Nelson 2003).[5]

The Congressional Management Foundation, a non-profit, non-partisan organization,[6] recruited the members of Congress to participate in the study. There was good variation among the members who participated. There were five Republicans and seven Democrats, spread across all four major geographical regions; two women; an African American; and representatives of both parties' leadership. All were running for reelection. And they were diverse ideologically, including one member from each party who voted against their party on recent immigration legislation. Knowledge Networks (KN), an online survey research firm, recruited constituent subjects from the corresponding congressional districts and administered the surveys. KN maintains a probability sample panel of survey respondents that is designed to be demographically representative of the U.S. population.[7]

---

4. The screener played no active role in facilitating the discussion, and had no knowledge of the study hypotheses or the content of the surveys. The screener was instructed to screen questions only if they were patently offensive or vulgar, incoherent, or closely duplicated the content of a previous question. Other than duplication, the need to screen did not arise.

5. Because the session combines the member-to-citizen deliberation with the citizen-to-citizen chat, the experimental treatment consists of both these components. In future research, we hope to disentangle the separate effects of each discussion component.

6. See http://www.cmfweb.org.

7. See http://www.knowledgenetworks.com/ganp/index.html for details. To meet sample-size requirements in each congressional district, KN subcontracted to two other vendors, Survey Sampling International (SSI) and Global Market Insite (GMI). SSI and GMI strive to maintain diverse panels but to a lesser extent than KN. In the models below, we include a fixed effect to account for any differences between the KN panels and the SSI and GMI panels. Because our population is drawn from Internet survey panels, our inferences can only generalize back to this population, which represents Internet-connected citizens in the study's congressional districts. The generality of our findings is limited to the extent that Internet survey panels are more politically engaged than the general public. The treatment effects we identify are limited to this subpopulation (Imai, King, and Stuart 2008).

Each subject (that is, each constituent) was asked to complete a pretest survey,[8] and then was randomly assigned to one of three groups: an information-only (IO) condition, a deliberative-group (DG) condition, or a true-control (TC) condition.[9] Constituents assigned to the information-only condition were asked to read background information (see the online appendix, section B.2) on immigration policy based on Congressional Research Service and Congressional Budget Office reports, edited for brevity and reading level, and to fill out a short background materials (BGM) survey. Those assigned to the deliberative condition also were asked to read this information and take the BGM survey; the BGM survey was administered one week prior to the deliberative session in each congressional district, to those in both the IO and DG groups. In addition, DG subjects were invited to engage in one deliberative session with their member of Congress and the post-session chat. Those in the true-control group were not exposed to either the background information or a deliberative session. That is, our study design included two qualitatively different control groups: The true-control condition helps assess the effect of providing both background information and a deliberative opportunity, while the information-only condition helps assess the effect of the deliberative session itself (including the citizen-to-citizen chat). A comparison between the DG and IO tests the effects of deliberation beyond the mere provision of information, which might be of special interest to those interested in deliberation as an accountability process.

One week following the deliberative session in each congressional district, KN administered a follow-up survey to subjects in all treatment arms. That is, all constituents in a given congressional district, whether they were assigned to the treatment or to a control condition, received the follow-up survey at the same time. Among other measures, the follow-up survey contained a battery of items measuring knowledge of immigration policy (see table 1 below). Since the survey was administered one week after the sessions, we test only for a short-term knowledge gain. We emphasize, however, that the sessions did not "teach to the tests." The member was not aware that the subjects would

---

8. Among the panelists who were invited to participate in the study, the study-specific response rate to the baseline survey was 0.76 by AAPOR response rate 6, which is the response rate appropriate to opt-in survey panels (Callegaro and Disogra 2008, p. 1022). This rate does not account for refusals to join the panels themselves. The experiment had multiple compliance points, and all data missing from subsequent time points are imputed as missing at random under a conditional independence assumption that we describe below (see footnotes 12 and 14).

9. In the baseline survey, prior to assignment, we asked respondents to RSVP their availability for the date and time of the event. Those who expressed an interest in participating in the study but could or would not attend the session were randomized to one of the two control groups. We included this filter question as we had no prior data on the rates at which respondents attend deliberative sessions when invited; the RSVPs helped us determine assignment rates so as not to assign too many or too few respondents to the deliberative condition. See the online appendix, sections B.1.1 and B.3.1, for details.

be tested for their knowledge; the sessions were not designed to recite the facts contained in these items; the background materials did contain the answer to each item but did not in any way highlight the answers; the pretest and post-test surveys were lengthy, and the knowledge items were not featured in any way; and none of the subjects was informed that they would be tested on any material.

In total, we assigned 2,222 constituents to the three experimental conditions: 437 subjects participated in the online deliberative group (DG); 528 received the information only (IO); and 1,257 were true controls (TC).[10] Table 1 gives the set of six immigration policy knowledge items and the response set for each. The DG participants had the highest probability of answering each of the six items correctly on the post-test, showing about an 18-percent improvement over the TC participants and about an 11-percent improvement over the IO participants. The IO participants likewise scored higher than the TC participants on all items, with an average improvement of about 7 percent. One would be incautious, however, to interpret these "as treated" differences as causal, however, since we had both noncompliance and nonresponse in our experiment. It is possible that those who participated in the deliberative sessions and replied to the survey may know more about immigration policy for reasons external to the experiment. Below, we use these knowledge items as indicators of immigration policy knowledge in an item-response model, and test whether participation in the deliberative sessions increases subjects' policy knowledge in a causal model.

CAUSAL ANALYSIS

We estimate a statistical model to examine whether there exists a treatment effect for knowledge gains in the experiment, and whether this treatment effect itself depends on how knowledgeable the subject is prior to the experiment. As with nearly any large field experiment, there are two significant ways that the data collection deviated from an ideal randomized experiment (Gerber and Green 2000). Some subjects failed to comply with their assigned treatment, participating in a session and/or reading the background material, and some failed to respond to the post-treatment follow-up survey.

There is a well-established literature in applied statistics for identifying causal effects in the presence of noncompliance and nonresponse (Barnard et al. 2003; Frangakis and Rubin 1999, 2002; Horiuchi, Imai, and Taniguchi 2007; Mealli et al. 2004). We make use of the method of principal stratification (Frangakis and Rubin 1999, 2002) to identify causal effect estimates, a method that can accommodate noncompliance and nonresponse based on unobservable

10. These cell sizes are for the treatments subjects actually received. The IO and DG subjects who did not comply with their assignments received the TC treatment, so the size of this cell is the largest.

Table 1. Immigration Policy Knowledge Items

	Question	Response Set
1	About how many illegal immigrants currently reside in the U.S.?	a) 100,000; b) 4,000,000; c) **12,000,000**; d) 23,000,000; e) 96,000,000; f) Don't know
2	About how many illegal immigrants come into the U.S. each year?	a) 50,000; b) 200,000; c) **500,000**; d) 2,000,000; e) 10,000,000; f) Don't know
3	About what fraction of illegal immigrants in the U.S. are from Mexico?	a) Less than 1/4; b) Between 1/3 & 1/2; c) **Between 1/2 & 2/3**; d) About 3/4; e) More than 3/4; f) Don't know
4	Under current law, is it a felony to reside illegally in the United States?	a) Yes; b) **No**; c) Don't know
5	Under current law, do companies that want to employ non-citizen immigrants have to prove that doing so will not hurt the employment of U.S. citizens?	a) **Yes**; b) No; c) Don't know
6	Under current law, are illegal immigrants who have lived in the U.S. for five years or more eligible to apply for citizenship?	a) Yes; b) **No**; c) Don't know

NOTE.—Boldface font indicates the correct answer.

characteristics of the subjects (see the online appendix, section C, for an extensive discussion).[11]

The statistical model, diagrammed in figure 1, is a full structural-equation model, in which latent variables are estimated and simultaneously regressed on other latent variables (Bollen 1989). Each arrow is labeled with a parameter to be estimated; an arrow that points to another arrow indicates an interaction term.

11. For this application, principal stratification is preferred to more familiar estimators such as matching, instrumental variables, and conditioning in a regression. These latter estimators return nearly identical point estimates of the causal effects as principal stratification, but for these data they require much stronger assumptions and, as a consequence, have standard errors that are strongly biased downward. Principal stratification makes weaker assumptions and is a more conservative estimator than these standard approaches.
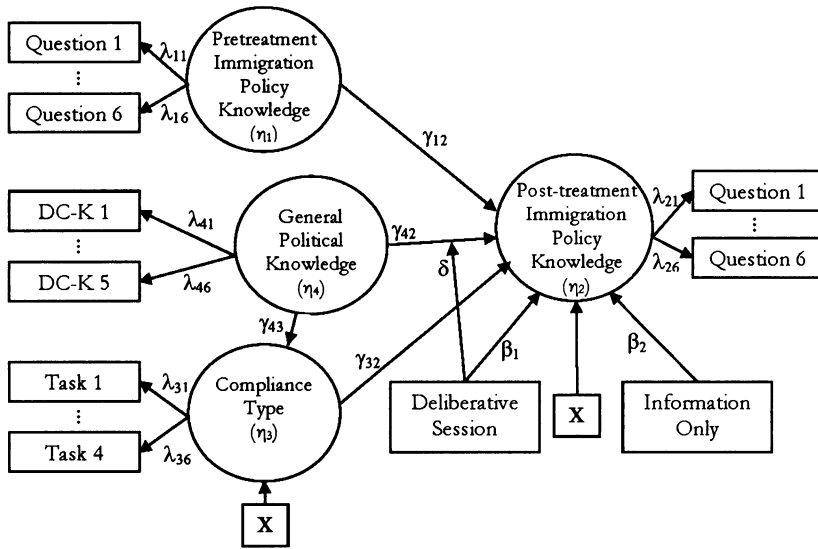
Figure 1. Treatment Effect Model. Variables in rectangles are observed, and variables in circles are measured. An arrow indicates a variable assigned to an equation; an arrow that points to another arrow indicates an interaction term.

Table 2 gives the descriptive statistics for the data used in the statistical models. Table 2 is divided into five sections, and the columns indicate whether the variable is measured prior to the experiment (pretreatment) or as part of the study (post-treatment or endogenous). The first set of variables is the immigration policy knowledge items, which are measured both pretreatment and post-treatment. Since these responses are dichotomous, the columns give the average probability that a typical respondent gives a correct response on each survey. The post-treatment responses are missing among those who did not respond to the follow-up survey.[12] In general, between 17 percent and 35 percent of respondents could correctly answer a given policy question prior to the experiment. In each case, the percent correct nearly corresponds to the item guess rate, or the reciprocal of the number of response categories. The unconditional probability of a correct answer increases slightly across all questions on the follow-up survey. We measure pretreatment and post-treatment immigration policy

_____

12. Dropping these cases from the analysis would require us to assume the data are missing completely at random—a very strong assumption. Instead, all missing endogenous data are imputed in the model below and the uncertainty of these imputations is accounted for in the posterior parameter estimates (Tanner and Wong 1987). For this imputation to produce unbiased results, we need to assume only that the data are missing at random once we have conditioned on compliance type and the latent variables and covariates in the model; see the online appendix, section C.5.

Table 2. Descriptive Statistics

	Pretreatment			Post-treat./Endog.		
	Mean	S.D.	N	Mean	S.D.	N
Immigration Policy Knowledge (η_1, η_2)						
Question 1 Correct	0.303	0.460	2222	0.349	0.477	1235
Question 2 Correct	0.178	0.382	2222	0.249	0.432	1235
Question 3 Correct	0.230	0.421	2222	0.279	0.448	1235
Question 4 Correct	0.275	0.447	2222	0.406	0.491	1233
Question 5 Correct	0.272	0.445	2222	0.326	0.469	1235
Question 6 Correct	0.351	0.477	2222	0.483	0.500	1235
Assignment Indicators						
Deliberative Condition	0.531	0.499	2222			
Information-Only Condition	0.177	0.382	2222			
Compliance Indicators (η_3)						
Participate in Deliberative Group				0.369	0.483	1175
Respond BGM Survey				0.520	0.500	1569
Respond Follow-Up Survey				0.629	0.483	1953
Respond November Survey				0.685	0.465	1342
Political Knowledge (η_4)						
Cheney's Current Job	0.828	0.378	2222			
Branch Determines Const.	0.751	0.432	2222			
Majority to Override Veto	0.532	0.499	2222			
Current Majority Party	0.701	0.458	2222			
Party More Conservative	0.772	0.419	2222			
Control Variables (X)						
College or More	0.420	0.494	2222			
Plus: eight other						
control variables (see online appendix)						

NOTE.—Compliance indicators are observed only among those eligible for each experimental task. For the post-treatment immigration policy knowledge items, we count a skip as an incorrect answer for subjects who completed the majority of the follow-up survey; the outcomes are jointly missing for nonresponders. Among those who otherwise completed the follow-up survey, 13 subjects skipped 1 item, 4 skipped between 2 and 5 items, and 14 skipped all items. See footnotes 12 and 14 for how we impute missing data.

knowledge, which we denote η_1 and η_2, respectively, in item-response models using the battery of six immigration policy knowledge items as indicators.

The second section of table 2 indicates respondents' randomized assignments into the three treatment arms of the study: those assigned to read background material on immigration and then participate in a deliberative session (the "deliberative group," DG) and those assigned to read the background material

but not participate in deliberation (the "information only" condition, IO). Those who were given access to neither the background reading materials nor a deliberative session are true controls (TC).

The compliance variables in table 2 indicate subjects' actual compliance with a series of assigned tasks.[13] These indicate, when subjects are presented with the opportunity, whether they participated in the deliberative session, responded to the BGM survey, responded to the follow-up survey, or responded to a survey we administered after the election. The statistical model below makes use of these behavioral indicators to account for the patterns of noncompliance and nonresponse in the data. We use these indicators to measure compliance type, η_3. Since these variables are all dichotomous, the cells give the compliance and response rates among those assigned to the various experimental tasks.

We expect that the deliberative sessions enhance subjects' motivated cognition (de Dreu, Koole, and Steinel 2000; Lerner and Tetlock 1999; Thompson et al. 1994), which in turn should induce deeper encoding of the policy information (Tetlock 1983). As a result, we predict that respondents who participate in the deliberative sessions, all else equal, will tend to have a higher knowledge of immigration policy, and hence a higher probability of a correct response on each of the six knowledge items, when compared to those who only read the information and the true controls. To test this, we condition η_2 on the indicator for participating in a deliberative session and the indicator for participating in the information-only condition. The estimate for β_1 identifies the difference in post-treatment immigration policy knowledge between deliberative subjects and true-control subjects; β_2 is the difference between information-only subjects and true-control subjects; and $\beta_1 - \beta_2$ is the difference between deliberative subjects and information-only subjects. We test for whether any of these quantities is statistically different from zero.

---

13. Due to budget constraints, about half of the subjects whom we identified as "chronic nonresponders" did not receive the follow-up survey (see the online appendix, section B.1.3). In about half of the congressional districts, subjects assigned to the IO condition who failed to complete the BGM survey, and subjects assigned to the DG condition who failed to respond to the BGM survey and who failed to attend their session, were not administered the follow-up survey. About 269 of 618 chronic nonresponders did not receive the survey. We found that among the 349 chronic nonresponders who did receive the follow-up survey, only about 25 percent responded to the survey. As a result, not administering the follow-up survey to these 269 chronic nonresponders only reduced our return rate by about three percent (for details, see the online appendix, section B.3.2). In all of these cases, we impute the probability distribution of both responding and for the missing responses under the missing-at-random assumption, which we can justify under the conditional independence assumption of latent ignorability, as described in footnote 12. Section B.3.2 reports sensitivity analyses showing that the treatment effects we report below remain nearly identical even under extreme assumptions of how these chronic nonresponders might have performed on the post-test knowledge items if given the chance. Likewise, chronic nonresponders were not mailed the November survey; we use only response to the November survey as a supplemental behavioral indicator for compliance.

A number of studies demonstrate that citizens tend to be knowledge "generalists," in that those with a high general knowledge of politics also tend to know more about specific policy topics (e.g., Delli Carpini and Keeter 1996, p. 270; Gilens 2001; Price and Zaller 1993). In the model, we condition subjects' post-treatment immigration policy knowledge (η_2) on a factor measure of subjects' general political knowledge, which we label η_4. We measure subjects' general political knowledge (η_4) in the baseline survey using the "Delli Carpini and Keeter five" political knowledge indicators (Delli Carpini and Keeter 1993), employing a standard item-response model.

The correlation between general knowledge and issue-specific knowledge suggests the possibility that those with the highest initial knowledge also will have the largest knowledge gains in the experiment (Price and Zaller 1993, p. 138). For example, having extensive prior political knowledge may make it easier for subjects to encode and retrieve new information (Visser, Holbrook, and Krosnick 2007, p. 130). Previous work using cross-sectional data, however, cannot identify whether the correlation is causal, or whether an enhanced motivation to learn drives new learning among all subjects equally. To test if there exists any differential effect of the deliberative sessions for those with higher general political knowledge, we add an additional term interacting general political knowledge with the indicator for having participated in a deliberative session (an effect captured with the coefficient δ).

To ensure that we identify the marginal effect of the treatment, even within the framework of principal stratification, we also control for whether each subject has a college education, as well as her pretreatment knowledge of immigration policy (η_1). We measure pretreatment immigration policy knowledge using an item-response model with indicators for whether or not she correctly answered the immigration knowledge items on the initial baseline survey. We also include eight additional exogenous covariates that previous work has shown to be important control variables.[14] These variables also are shown in figure 1 as the **X** vector. The effects of these variables are not the focus of this article, and we simply treat these as background controls.

ESTIMATION

We estimate the structural parameters in a Bayesian framework using MCMC with data augmentation (Imbens and Rubin 1997; Tanner and Wong 1987). We implement the model in WinBUGS (Spiegelhalter et al. 1996). We assign flat priors for all parameters and standard normal distributions as priors for all latent variables. We estimate three chains using overdispersed starting values and run the chains until the model converges by the Gelman and Rubin (1992) diagnostic.

---

14. These variables are race, gender, employment, need for cognition, and need for judgment (each measured in two ways), and whether or not the subject was from a Knowledge Networks panel. See the online appendix, section C.3, for more detail.

Treatment Effect "Black Box" Results

In this section, we report treatment effects when the treatment is taken to be a "black box." In the next section, we explore specific direct and indirect causal mechanisms of the treatment.

In this experiment, treatment effects are defined in terms of three pairwise comparisons between the treatment group and the two qualitatively different control groups. Figure 2 graphs the posterior distributions of these three average treatment effect estimates for changes in post-treatment immigration policy knowledge, η_2. In this figure, since the knowledge scale η_2 has no natural metric or scale, we report the pairwise differences between the experimental groups in standard deviation units. One can take the mean of each treatment effect distribution as the point estimate of each difference, and the full distribution gives a sense of the uncertainty underlying this point estimate.

As figure 2 shows, each of these average treatment effect estimates is positive and statistically significant. The deliberative-group average immigration policy knowledge is about a third of a standard deviation higher than that of the information-only group, with a 95-percent confidence interval ranging from 0.11 to 0.61 standard deviations. Recall that the η_2 policy knowledge latent variable is assumed to have a standard normal distribution, which implies that four standard deviations capture most of the variation in the policy knowledge variable. Under this assumption, a difference of 0.36 standard deviations represents a change from the 50th percentile to the 63rd percentile. The deliberative group's post-treatment policy knowledge was 0.61 standard deviations higher than the true controls, with a 95-percent confidence interval ranging from 0.38 to 0.84 standard deviations. By this point estimate, the average treatment effect corresponds to a change from the 50th percentile to the 73rd percentile in post-treatment policy knowledge.
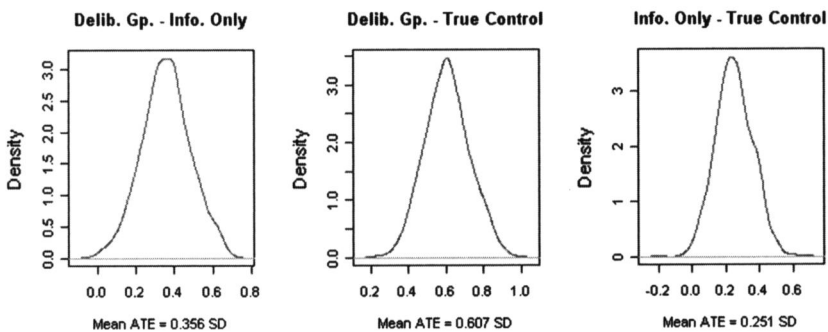

Figure 2. Immigration Knowledge Average Treatment Effect Estimates. Each density shows the full distribution of the estimated treatment effect for each of three experimental comparisons.

The information-only group average knowledge is about 0.25 standard deviations above the true controls, a difference that is statistically significant but substantively smaller than the deliberative treatment effect. One can think of this result as the effect of a separate treatment, providing policy information to the subject and administering a subsequent test. That this treatment effect is positive, while perhaps no surprise, is also an important result from the perspective of deliberative democracy. The result suggests that part of the reason citizens do so poorly in "pop quiz" knowledge surveys is that they are administered in something of an informational vacuum. Participating in a deliberative session appears to enhance the encoding of policy information beyond the mere provision of information.

In the treatment effect causal model, we use the six immigration policy knowledge items to measure each subject's underlying knowledge on the topic. We can retrieve the effect of the treatment on the individual immigration policy knowledge items via the treatment's effect on the subject's measured knowledge, η_2, as the treatment effect "reverberates" through the pathways of the model. Figure 3 graphs these treatment effects on the knowledge items, where the treatment effect is the expected difference between the experimental groups in the probability of getting the item correct. In this figure, the treatment effect distributions are summarized as box plots, where each box indicates the first- to third-quartile range of the induced difference in the probability of getting each item correct. The largest effect in each graph is in the comparison of the deliberative group to the true controls (DG - TC), followed by the comparison of the deliberative group to the information-only group (DG - IO), and the smallest effects are from the information-only to true-control comparisons (IO - TC). For example, the largest treatment effects in the deliberative-group to true-control comparison are a 20-percent increase in the probability of getting question one correct and a 15-percent increase on question six. Notice that each of the eighteen comparisons is statistically significant, or nearly so, as the boxplot whiskers (indicating 95-percent confidence intervals) typically do not capture zero. The interquartile ranges for each of these comparisons are mostly distinct; the boxes have little or no overlap across each comparison.

In this statistical model, we are able to test whether those who have a high general knowledge of politics also tend to know more about this specific policy topic (e.g., Delli Carpini and Keeter 1996, p. 270; Gilens 2001); in terms of figure 1, this relationship is captured in the estimate for γ_{42}. Not surprisingly, and consistent with Price and Zaller (1993), we find that general political knowledge (η_4) and pretreatment immigration policy knowledge (η_1) are both highly correlated with post-treatment immigration policy knowledge, as is the indicator (included in **X**) for whether the subject completed college. Price and Zaller (1993) examine only cross-sectional data, however, and so cannot consider whether knowledge gains are different between high and low knowledge citizens, in response to an event that motivates subjects' cognition. We are able to test whether those with a high general knowledge of politics are
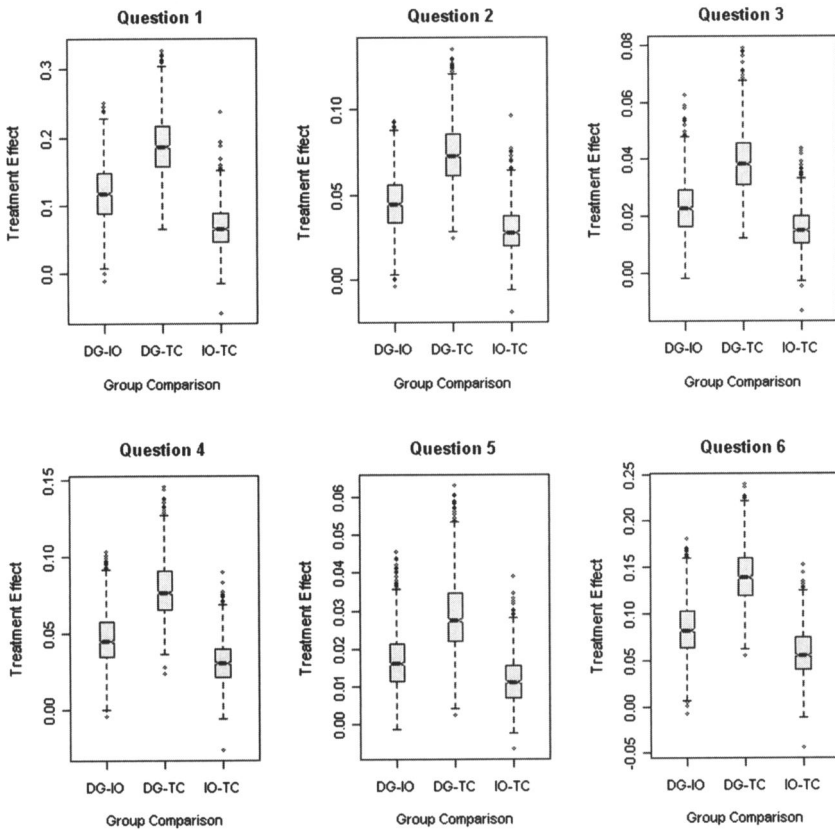
Figure 3. Average Treatment Effect Estimates by Question. Each plot shows three differences in the probability of a correct response: "Deliberative Group" - "Information-Only Group" (DG - IO); "Deliberative Group" - "True Control" (DG - TC); "Information-Only Group" - "True Control" (IO - TC). The boxes indicate the interquartile range in the distribution of each probability difference, and the whiskers indicate 95-percent confidence intervals for each difference.

disproportionately responsive to the deliberative treatment; that is, whether subjects with a prior knowledge advantage have a greater capacity to learn about policy relative to those without such an advantage. The model includes a term that interacts general political knowledge with exposure to the deliberative session, an effect captured in the estimated parameter δ, and we find no evidence that those with a knowledge advantage disproportionately gain from the deliberative sessions. The point estimate for δ is very near zero, and the 95-percent confidence interval for this effect ranges from -0.3 standard deviations to 0.2 standard deviations.

This result showing that prior political knowledge does not affect the capacity to gain additional knowledge has important implications for deliberative theorists interested in democratic practice. Some have argued (e.g., Sanders 1997) that deliberation has the potential to magnify the advantages of those who come to deliberative sessions already equipped with high levels of information. While we do find that those who begin the session with more information also end the session with more information, we also find that the capacity for motivated learning does not depend on pre-deliberative informational advantages.

Disaggregating the Causal Mechanisms

This first model takes the experiment as a "black box," in that it can demonstrate the existence of treatment effects but does not disaggregate the mechanisms by which any treatment effects are realized. We estimate a second model in an attempt to disaggregate the mechanisms or the causal pathways for knowledge gains (see the online appendix, section C.7, for details). This model investigates two pathways for knowledge gains: a direct pathway for whether the sessions themselves are informative to participants, and an indirect pathway for whether participating in the deliberative sessions induces subjects to encode more information regarding immigration policy outside the experiment.[15] The model identifies direct and indirect causal effects by modeling both paths within the framework of principal stratification (see Imai et al. 2010; Mealli and Rubin 2003; Rubin 2004).

In the first causal pathway, compared to those in the information-only and true-control conditions, those in the deliberative condition might receive information in the course of the online deliberation itself. To test whether the sessions themselves are directly informative, we included a variable in the model that summed the number of immigration policy knowledge items for which someone in the session happened to give a correct answer (mean 1.91 items, sd 0.98, $N = 2,222$). This variable can range from zero to six. The coefficient for this variable, however, was small and statistically insignificant, indicating that the bulk of knowledge gains did not come from information in the sessions themselves.

The second causal pathway involves a two-step mechanism: We test first whether there is a treatment effect wherein those participating in the deliberative sessions have an enhanced motivation to attend to relevant policy information outside the confines of our experiment, and we simultaneously test whether

---

15. A third pathway exists in that subjects who anticipate they will attend a deliberative session may pay closer attention to the background reading materials. We have only poor proxies for such effort (e.g., time spent on the BGM survey) that did not provide solid evidence for a causal effect on knowledge gains.

attending to external information increases immigration policy knowledge for those in the deliberative group. We have two good candidates for post-treatment measures of the motivation to attend to information outside the experiment, and so we run the model separately for each of these two variables. The first variable measures subjects' response to the statement "It is a citizen's duty to keep informed about politics even if it is time consuming." We code those who "Strongly Agree" or "Agree" with this statement as one, and we code those who "Neither Agree nor Disagree," "Disagree," and "Strongly Disagree" as zero (mean 0.848, $N = 1,214$).[16] The second variable measures subjects' response to the question "Other than for this survey project, have you ever talked about illegal immigration with anyone?" We code this variable one if they did and zero if they did not (mean 0.860, $N = 1,085$). The results of this model are summarized in table 3.

Consider the first step in the pathway, whether there is a treatment effect on attention to information outside the experiment. We find that those in the deliberative group are 11 percent more likely than those in the information-only group to believe it is their duty to keep informed about politics (with a 95-percent confidence interval ranging from 5 percent to 18 percent), and 6 percent more likely than true controls (with a confidence interval from zero to 10 percent). We also find that those in the treatment group were 9 percent more likely to discuss immigration policy knowledge outside the experiment when compared to the information-only group, and 15 percent more likely than the true controls. These effects are statistically significant; the corresponding 95-percent confidence intervals are 3 to 15 percent, and 9 to 21 percent.

Now consider the second step in the pathway, whether attending to external information sources enhances immigration policy knowledge. Importantly, we test for this latter effect separately for each of the experimental groups. We find that the total effect of having immigration policy discussant partners outside the experiment is statistically significant only for those who participated in the deliberative sessions. Among deliberators, having discussant partners on immigration policy increased immigration policy knowledge by 0.60 standard deviations, or about 15 percent of the total variation in policy knowledge. Similarly, the point estimates for the direct effect and interaction terms on an increased duty to keep informed closely paralleled the results regarding immigration discussants, though in this case they were not statistically significant.

We take the response for these two items as indicating that the deliberative session enhanced subjects' motivation to attend to information broadly outside the experiment, with discussion partners one of many possible sources of extrinsic immigration policy information (Niemi and Junn 1998). These results

---

16. The binary logit model for this dichotomized dependent variable is equivalent to estimating a generalized ordered logit that relaxes the parallel regression assumption used in ordinary ordered logit (Williams 2006), where we model crossing the threshold between not agreeing to agreeing separately from other thresholds. The other basic threshold (moving from disagree to neutral) is not especially meaningful in this context.

Table 3. Two-Step Mechanism Model Results

	Δ Prob.	Confidence Interval	
		2.5%	97.5%
First Step:			
Deliberation Affects Duty to Keep Informed			
DG - IO	0.109	0.046	0.177
DG - TC	0.058	0.003	0.102
Deliberation Affects Propensity to Discuss with Others			
DG - IO	0.090	0.034	0.149
DG - TC	0.148	0.091	0.206
Second Step:			
Duty Affects Knowledge			
DG - TC	0.419	−0.140	0.981
IO - TC	0.212	−0.507	0.924
Discussing Affects Knowledge			
DG - TC	0.595	0.025	1.131
IO - TC	0.407	−0.449	1.224

NOTE.—The mechanism model has two steps (see the online appendix, section C.7, for full details). The first step tests whether there is a treatment effect on an indicator for motivation to encode policy information. The second step tests whether the indicator for motivation is related to policy knowledge, separately for the DG and IO groups.

highlight the way cognition and one's social context intertwine—that translating information into knowledge is, in part, conditioned on how one interacts with others (Lerner and Tetlock 1999).

Discussion

In our experiment, we find that citizens display both a willingness and a capacity to become informed when given the opportunity to interact with their member of Congress. The treatment effects we observe appear to stem from deliberative-group subjects' increased motivation to seek out and encode relevant policy information outside the confines of our experiment. This suggests a fundamental shortcoming of a cross-sectional "pop quiz" approach to political knowledge: that it artificially eliminates the social context that is often necessary for respondents to seek out, encode, and consolidate political information into long-term memory (Tetlock 1983). We argue that the pessimistic normative

conclusions that dominate the survey research literature on political knowledge may follow largely from the assessment methods themselves that fail to capture the dynamics of motivated learning in society.

We observe that those subjects with a higher general knowledge of politics do have a better overall grasp of immigration policy (as found in Delli Carpini and Keeter 1993, p. 1184, 1996, p. 270; Price and Zaller 1993). Delli Carpini and Keeter (1996, p. 271) interpret this shift in the intercept of the outcome equation as a threat to the fundamental principle of equality among citizens, in that those who sit at the top of society's knowledge pyramid—and who are privileged in so many other ways—also know more about specialized policy topics. In our experiments, however, the capacity to become informed in response to a political event is largely orthogonal to a general knowledge of politics (Gilens 2001; Jerit, Barabas, and Bolsen 2006). This finding suggests a widespread equality with respect to citizens' capacity for motivated learning, and counters some concerns expressed among scholars interested in the practice of deliberative democracy (e.g., Sanders 1997).

Conclusion

The capacity to become informed about policy is central to Madisonian and deliberative representation (Habermas 1996; Hamilton, Madison, and Jay 1961, pp. 320–25; Pitkin 1967, pp. 222–23). Our experiment demonstrates that citizens have a capacity to become informed, and that they are willing to exercise this capacity when given a motive and opportunity. Indeed, it appears that deliberation itself can induce motivated learning (Tetlock, Skitka, and Boettger 1989), and that a well-designed deliberation can help all citizens reach normative expectations for the informed discourse at the heart of deliberative theory.

We have reason to believe that this capacity to become informed is not limited to the confines of our experiment; members of Congress themselves believe that citizens exercise this capacity often enough to maintain real accountability in practice (Arnold 1990, p. 68; Fenno 1978, p. 231; Kingdon 1989, p. 248). In this sense, contemporary democracy is on a much stronger footing than is suggested in many existing studies of political knowledge, which measure static political knowledge but not dynamic changes in knowledge that coincide with opportunities to participate in politics.

But even if one were to assume that this capacity to become informed is limited to the kind of elite/nonelite deliberative institution that we have created, one can still hold out hope that our results portend future improvements in the distribution of citizen knowledge as Internet technologies further penetrate society. As members increase their use of social media,[17] along with the increasing penetration of Internet access in society, the emergence of digital government practices such as online townhalls may pave the way for citizens to

---

17. We note that two of the House members who participated in this study subsequently conducted online sessions on their own.

more regularly interact with their member of Congress and other publicly accountable actors. This would afford citizens more regular opportunities to exercise their apparent capacity to learn about politics and policies, and so better approximate widely shared normative goals for a knowledgeable democratic society.

Supplementary Data

Supplementary data are freely available online at http://poq.oxfordjournals.org/.

References

Almond, Gabriel A., and Sidney Verba. 1963. *The Civic Culture: Political Attitudes and Democracy in Five Nations.* Princeton, NJ: Princeton University Press.

Arnold, R. Douglas. 1990. *The Logic of Congressional Action.* New Haven, CT: Yale University Press.

Bächtiger, Andrè, Simon Niemeyer, Michael Neblo, Marco R. Steenbergen, and Jürg Steiner. 2010. "Disentangling Diversity in Deliberative Democracy: Competing Theories, Their Blind Spots, and Complementarities." *Journal of Political Philosophy* 18(1):32–63.

Barabas, Jason. 2004. "How Deliberation Affects Public Opinion. *"American Political Science Review* 98(November):687–701.

Barnard, John, Constantine E. Frangakis, Jennifer L. Hill, and Donald B. Rubin. 2003. "Principal Stratification Approach to Broken Randomized Experiments: A Case Study of School Choice Vouchers in New York City." *Journal of the American Statistical Association* 98(462):299–323.

Bollen, Kenneth A. 1989. *Structural Equations with Latent Variables.* New York: John Wiley and Sons.

Callegaro, Mario, and Charles Disogra. 2008. "Computing Response Metrics for Online Panels." *Public Opinion Quarterly* 72(5):1008–1032.

Converse, Philip E. 1964. "The Nature of Belief Systems in Mass Publics." In: *Ideology and Discontent,* ed. David E. Apter, London: Free Press of Glencoe.

de Dreu, Carsten K. W., Sander L. Koole, and Wolfgang Steinel. 2000. "Unfixing the Fixed Pie: A Motivated Information Processing Approach to Integrative Complexity." *Journal of Personality and Social Psychology* 79(6):975–87.

Delli Carpini, Michael X., and Scott Keeter. 1993. "Measuring Political Knowledge: Putting First Things First." *American Journal of Political Science* 37(November):1179–206.

———. 1996. *What Americans Know about Politics and Why It Matters.* New Haven, CT: Yale University Press.

Druckman, James N. 2004. "Political Preference Formation: Competition, Deliberation, and the (Ir)relevance of Framing Effects." *American Political Science Review* 98(November):671–86.

Druckman, James N., and Kjersten R. Nelson. 2003. "Framing and Deliberation: How Citizens' Conversations Limit Elite Influence." *American Journal of Political Science* 47(October):729–45.

Fenno, Richard F. 1978. *Homestyle: House Members in Their Districts.* Boston: Little, Brown.

Fishkin, James S. 2009. *When the People Speak.* New York: Oxford University Press.

Fishkin, James S., and Robert C. Luskin. 2005. "Experimenting with a Democratic Ideal: Deliberative Polling and Public Opinion." *Acta Politica* 40(September):284–98.

Frangakis, Constantine E., and Donald B. Rubin. 1999. "Addressing Complications of Intention-to-treat Analysis in the Combined Presence of All-or-none Treatment-noncompliance and Subsequent Missing Outcomes." *Biometrika* 86(2):365–79.

———. 2002. "Principal Stratification in Causal Inference." *Biometrics* 58(March):21–29.

Gastil, John, and James P. Dillard. 1999. "Increasing Political Sophistication through Public Deliberation." *Political Communication* 16(3):3–23.

Gelman, Andrew, and Donald B. Rubin. 1992. "Inference from Iterative Simulation Using Multiple Sequences." *Statistical Science* 7(November):434–55.

Gerber, Alan S., and Donald P. Green. 2000. "The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment." *American Political Science Review* 94(September):653–63.

Gilens, Martin. 2001. "Political Ignorance and Collective Policy Preferences." *American Political Science Review* 95(June):379–96.

Habermas, Jürgen. 1984. In: *The Theory of Communicative Action, Volume One: Reason and the Rationalization of Society, transl,* ed. Thomas McCarty, Boston: Beacon Press.

———. 1996. *Between Facts and Norms: Contributions to a Discourse Theory of Law and Democracy, transl.* and ed. William Rehg. Cambridge, MA: MIT Press.

Hamilton, Alexander, James Madison, and John Jay. 1961. *The Federalist Papers,* ed. Clinton Rossiter. New York: New American Library.

Horiuchi, Yusaku, Kosuke Imai, and Naoko Taniguchi. 2007. "Designing and Analyzing Randomized Experiments: Application to a Japanese Election Survey Experiment." *American Journal of Political Science* 51(July):669–87.

Imai, Kosuke, Luke Keele, Dustin Tingley, and Teppei Yamamoto. 2010. "Identification of Causal Mechanisms from Experimental and Observational Data." Paper presented at the Third Annual West Coast Experiments Conference, University of California–Los Angeles, Los Angeles, CA, USA.

Imai, Kosuke, Gary King, and Elizabeth A. Stuart. 2008. "Misunderstandings among Experimentalists and Observationalists about Causal Inference." *Journal of the Royal Statistical Society, Series A (Statistics in Society)* 171(April):481–502.

Imbens, Guido W., and Donald B. Rubin. 1997. "Bayesian Inference for Causal Effects in Randomized Experiments with Noncompliance." *Annals of Statistics* 25(1):305–27.

Jerit, Jennifer, Jason Barabas, and Toby Bolsen. 2006. "Citizens, Knowledge, and the Information Environment." *American Journal of Political Science* 50(April):266–82.

Kingdon, John W. 1989. *Congressmen's Voting Decisions.* 3rd ed. Ann Arbor: University of Michigan Press.

Kuklinski, James H., Paul J. Quirk, Jennifer Jerit, and Robert F. Rich. 2001. "The Political Environment and Citizen Competence." *American Journal of Political Science* 45(April):410–24.

Lerner, Jennifer S., and Philip E. Tetlock. 1999. "Accounting for the Effects of Accountability." *Psychological Bulletin* 125(2):255–75.

Lupia, Arthur, and Mathew D. McCubbins. 1998. *The Democratic Dilemma: Can Citizens Learn What They Need to Know?* New York: Cambridge University Press.

Luskin, Robert C. 1990. "Explaining Political Sophistication." *Political Behavior* 12(December):331–61.

Mealli, Fabrizia, Guido W. Imbens, Salvatore Ferro, and Annibale Biggeri. 2004. "Analyzing a Randomized Trial on Breast Self-examination with Noncompliance and Missing Outcomes." *Biostatistics* 5(2):207–22.

Mealli, Fabrizia, and Donald B. Rubin. 2003. "Commentary: Assumptions Allowing the Estimation of Direct Causal Effects." *Journal of Econometrics* 112:79–87.

Neblo, Michael A. 2005. "Thinking through Democracy: Between the Theory and Practice of Deliberative Politics." *Acta Politica* 40(2):169–81.

Niemi, Richard G., and Jane Junn. 1998. *Civic Education: What Makes Students Learn?* New Haven, CT: Yale University Press.

Pitkin Hanna Fenichel. 1967. *The Concept of Representation*. Berkeley: University of California Press.

Price, Vincent, and John Zaller. 1993. "Who Gets the News? Alternative Measures of News Reception and Their Implications for Research." *Public Opinion Quarterly* 57(Summer):133–64.

Prior, Markus, and Arthur Lupia. 2008. "Money, Time, and the Ability to Answer Political Knowledge Questions." *American Journal of Political Science* 52(January):169–83.

Rubin, Donald B. 2004. "Direct and Indirect Causal Effects via Potential Outcomes." *Scandinavian Journal of Statistics* 31:161–70.

Sanders, Lynn M. 1997. "Against Deliberation." *Political Theory* 25(3):347–76.

Spiegelhalter, David, Andrew Thomas, Nicky Best, and Wally Gilks. 1996. *BUGS 0.5: Bayesian Inference Using Gibbs Sampling Manual (version 2). Technical Report, MRC Biostatistics Unit*.

Stokes, Donald E., and Warren E. Miller. 1962. "Party Government and the Saliency of Congress." *Public Opinion Quarterly* 26(Winter):531–46.

Tanner, Martin A., and Wing Hung Wong. 1987. "The Calculation of Posterior Distributions by Data Augmentation." *Journal of the American Statistical Association* 82(398):528–40.

Tetlock, Philip E. 1983. "Accountability and the Perseverance of First Impressions." *Social Psychology Quarterly* 46(December):285–92.

———. 1992. "The Impact of Accountability on Judgment and Choice: Toward a Social Contingency Model." In *Advances in Experimental Social Psychology*, ed. Mark P. Zanna pp. 331–76. New York: Harcourt Brace Jovanovich.

Tetlock, Philip E., Linda Skitka, and Richard Boettger. 1989. "Social and Cognitive Strategies for Coping with Accountability: Conformity, Complexity, and Bolstering." *Journal of Personality and Social Psychology* 57(4):632–40.

Thompson, Erik P., Robert J. Roman, Gordon B. Moskowitz, Shelly Chaiken, and John A. Bargh. 1994. "Accuracy Motivation Attenuates Covert Priming: The Systematic Reprocessing of Social Information." *Journal of Personality and Social Psychology* 66(3):474–89.

Thompson, Nicola, and Derek R. Bell. 2006. "Articulating Political Knowledge in Deliberation." *Contemporary Politics* 12(September–December):287–300.

Visser, Penny S., Allyson Holbrook, and Jon A. Krosnick. 2007. "Knowledge and Attitudes." In: *The SAGE Handbook of Public Opinion Research*, eds. Wolfgang Donsbach, Michael W. Traugott, pp.127–40. Thousand Oaks, CA: Sage Publications.

Williams, Richard. 2006. "Generalized Ordered Logit/Partial Proportional Odds Model for Ordinal Dependent Variables." *Stata Journal* 6(1):58–82.