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DOES INATTENTION TO POLITICAL DEBATE EXPLAIN THE POLARIZATION GAP BETWEEN THE U.S. CONGRESS AND PUBLIC?

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Abstract Recent studies of the U.S. Congress have demonstrated a substantial difference between the level of partisan polarization displayed by legislators' votes and that shown in citizens' survey responses about those votes. Perhaps public polarization would increase if citizens were more attentive to political debates in Congress. Using natural variation in citizens' levels of political information, I show that citizens who are informed about the partisan alignment of issues have a preference distribution similar to that of Congress, even after the sample is reweighted to resemble the entire public in their political, social, and demographic characteristics via matching. In contrast, using a survey experiment, I show that cue and argument treatments only partially reduce the discrepancy between the views expressed by the public and the voting behavior of Congress on the same issues. Both experimental and observational studies have significant limitations for measuring counterfactuals involving public opinion, and so our understanding of the polarization gap remains unfortunately limited.

Introduction

How do we determine whether legislators are representing the public well? Most frequently, scholars evaluating representation have adopted a form of the delegate ideal of representation (Pitkin [1967] 1972). From this perspective, it is

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axiomatic that legislators ought to act on the preferences of their constituents.[1] In practice, this leads researchers to evaluate representation by comparing measures of legislative behavior to survey measures of public opinion, using several possible metrics (Achen 1978; Golder and Stramski 2010). This approach has been applied to studying ideological congruence between legislators and publics in a wide range of countries with varying electoral institutions (for a review of this literature, see Powell 2004). One reason that the U.S. Congress performs poorly in these evaluations is that members of Congress appear to be more polarized than the public. This relative polarization is along partisan lines: individuals holding mixtures of Democratic and Republican positions across different political issues are far rarer in Congress than in the public.

Citizens' survey responses about political issues may understate their latent level of partisan polarization. The best evidence for the relative polarization of Congress comes from comparing legislators' votes and survey responses about the same issues (Bafumi and Herron 2010; Abramowitz 2010). Bafumi and Herron (2010) use these data to scale individual citizens and legislators in a common left–right political space, enabling a direct comparison of polarization levels by comparing the variance of citizens' scaled positions to that of legislators' scaled positions. But with this measurement strategy, any difference between the variances of citizens' and legislators' scaled positions (a polarization gap) might derive from different levels of familiarity with the issues. Legislators receive information that links their ideology and partisanship to the decision about how they should vote on a particular issue, whereas many citizens do not. Citizens' responses may therefore give the appearance of moderation—a mix of Democratic and Republican positions—when their underlying attitudes are in fact highly partisan. If these citizens were aware of the parties and arguments on each side of an issue, they might express more consistently partisan issue preferences.

Evaluating this hypothesis about information and polarization involves comparing Congressional behavior to the public's counterfactually informed preferences. Counterfactually informed preferences are those that citizens would express if they knew certain information that they actually lack. Previous research has considered how public preferences might change if various kinds of information were provided. These include information contributing to general political awareness as assessed by interviewers (Bartels 1996; Sekhon 2004), general factual knowledge assessed by test items (Delli Carpini and Keeter 1997; Althaus 1998; Althaus 2003; Jessee 2009), and issue-specific

1. The ideal relationship between representatives and the represented is a long-standing problem in normative political theory, with a range of competing conceptions (e.g., Pitkin [1967] 1972; Mansbridge 2003; Rehfeld 2009; Disch 2011). Different conceptions entail different approaches to assessing the quality of representation in existing institutions (Bartels 1990). The prevalence of the delegate approach to evaluating representation may be due to the ease of operationalizing comparisons of legislative action to surveyed public opinion (Rehfeld 2009, p. 219).

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factual knowledge assessed by test items (Gilens 2001). In this paper, I focus on two types of information that, if provided, might help citizens determine the partisan or ideological alignment of a policy proposal. The first are partisan cues: the partisan identities of political supporters and opponents of that proposal. The second are pro–con arguments: the claims made about that proposal by supporters and opponents. Both can help link unfamiliar issues to a citizen's partisan and ideological dispositions (Zaller 1992; Mondak 1993; Kam 2005; Levendusky 2010). Would the polarization gap between the public and Congress disappear if all citizens knew more of this kind of information about how individual issues relate to broader disagreements?

The central methodological obstacle to answering this question is estimating counterfactually informed preferences. How would citizens' opinions change if they became better informed? We can observe the preferences of the subset of citizens who are actually well informed about political debates. I introduce two data sets that show that the citizens who are most aware of the partisan alignment of issues are just as polarized as Congress. However, the observed association between this measure of information and political preferences does not answer the causal question that we must answer in order to estimate citizens' counterfactual preferences (Sekhon 2004). If the entire public were as informed as these relatively informed individuals, would they also be just as polarized in their expressed issue preferences?

One approach to answering this question is to exploit existing variation in information levels. We might learn about the counterfactually informed preferences of an uninformed individual by looking at the preferences of informed individuals who are otherwise similar in observable characteristics (Bartels 1996; Sekhon 2004). This strategy naturally lends itself to matching methods. For each citizen who is not fully informed according to the measure of information, the observed preferences of the most similarly informed citizen(s) are used to infer the citizen's counterfactually informed preferences. When applied to the full population, matching takes the subpopulation of informed individuals and reweights them to more closely resemble the population distribution of matched social, political, and demographic variables. I use kernel matching to show that the association between observed variation in information levels and polarization is not attributable to differences between uninformed and informed citizens in a set of salient social, political, and demographic characteristics.

However, no analysis of cross-sectional variation can establish causality entirely convincingly. The set of matching variables used is necessarily limited, and there is no way to rule out the possibility that some unmeasured factor might generate high levels of political information and high levels of political polarization. An alternative approach to estimating counterfactually informed preferences involves providing information through a survey experiment. This allows clear identification of a causal effect of information and the resulting distribution of public opinion. Because feasible treatments in a survey experiment cannot fully emulate attention to political debate, the

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experimental approach only estimates the opinions of a slightly more informed public. Thus, a survey experiment can assess only whether the polarization gap begins to disappear when we provide information about political debates. I use a survey experiment to show that polarization can be increased by experimentally providing information about the partisan alignment of legislation and about the arguments being made about that legislation.

Observational and experimental methods are both valuable for assessing counterfactually informed public opinion, because they estimate different quantities of interest, make different assumptions, and have different limitations. The matching approach exploits existing variation in political information to assess the consequences of remediating the large disparities in political information that exist among citizens, but is a less reliable tool for making causal inferences. The survey experiment only slightly reduces informational disparities but does so in a more causally robust way. Taken together, the results of these two approaches indicate that differences in standard political covariates do not explain the observed difference in polarization between informed and uninformed citizens, and that providing information about the political debate increases public polarization. The public's lack of information about the debates over political issues at least partly explains why it appears less polarized than Congress.

Data

This paper uses data on Congressional roll-call voting (Poole 2011) and data from two surveys that asked citizens about Congressional votes. The first of these is the 2006 Cooperative Congressional Election Survey (CCES) (Ansolabehere 2006). The pre-election survey used in this paper was fielded over the Internet by Polimetrix from October 6 to November 7, 2006, and some demographic profile data was collected in August 2006. Polimetrix generated a subsample of 38,000 individuals from the datafiles for the 2004 American Community Survey, which is a nationally representative survey with a response rate of 93.1 percent conducted by the U.S. Bureau of the Census (Vavreck and Rivers 2008). Since they could not poll these ACS respondents, Polimetrix used matching to identify similar individuals (in age, race, gender, education, and imputed values of partisanship and ideology) among 150,000 opt-in respondents who were part of an ongoing online panel recruited via advertisement. This methodology yields a large sample selected to mirror characteristics of a national sample, facilitating the analysis in this paper that utilizes a subset of highly informed respondents from that survey. However, this large sample comes at the cost of uncertainty about the representativeness of the sample.

The 2006 CCES asked respondents their preferences on the issues underlying seven roll-call votes taken by the 109th Senate in 2005 and 2006. These

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questions were on bills and amendments banning partial-birth abortion, federally funding stem-cell research, setting a timetable for military withdrawal from Iraq, comprehensive immigration reform, increasing the minimum wage, cutting capital gains taxes, and ratifying the Central American Free Trade Agreement (CAFTA). The questions were asked in this order for all respondents. Respondents received a brief description of the proposal plus arguments for and against each piece of legislation as part of the prompt (the exact text for each of these is provided in table 1). They were then asked about their own preferences: "How about you? If you were faced with this decision, would you vote for or against [legislation]?" with response choices of "For," "Against," and "Don't Know." After respondents answered this question, they were then asked how each of their senators voted: "How about [Senator Name]? Do you think [he/she] voted for or against [legislation]?" with response choices of "For," "Against," and "Don't Know."

The second survey I use is a 2010 experiment fielded through the Time-Sharing Experiment for the Social Sciences (TESS). This survey experiment was fielded over the Internet to a subsample of a nationally representative panel maintained by Knowledge Networks. The broader 60,000-person panel was recruited using a combination of telephone (random-digit dialing) and mail (address-based sampling) recruitment, with Internet access provided by Knowledge Networks to those individuals who did not already have access. The experiment was made available to 3,491 members of the panel, from June 16 to 23, 2010. Individuals were notified that they had a survey assignment by e-mail, with reminders for individuals who did not respond within three days. The final sample size was 2,204, which corresponds to a cooperation rate of 63.1 percent. Including the recruitment process for the Knowledge Networks panel, the cumulative response rate with respect to the target population (CUMRR1) was 6.8 percent.

The survey experiment consisted of questions about six roll-call votes taken in 2009 by the 443 members of the U.S. House of Representatives who served for at least part of that year. These votes were on six bills and amendments: the Lilly Ledbetter Fair Pay Act (roll call 9), the State Children Health Insurance Program (SCHIP) reauthorization (16), the American Recovery and Reinvestment Act (46), the American Clean Energy and Security Act (477), the Unemployment Compensation Extension Act (722), and the Stupak-Pitts Amendment to the Affordable Health Care for American Act (884). For each vote, respondents received a brief description of the legislation (wording provided in table 1) along with an initial information question: "Do you know whether the Democrats or the Republicans in Congress were more likely to support this bill?" with response choices of "Don't Know," "Democrats," and "Republicans."

Then, after the respondents had answered this question, the brief description remained on the screen, and the treatment(s) (wording provided in table 1) appeared along with the second question: "Would you have voted for or against

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Table 1. Prompt and Treatment Wordings for the CCES and TESS Survey Instruments

Survey	Brief description	Partisan cue	Pro–con argument
CCES	We'd like to ask about a proposal in Congress to ban a type of late-term abortion sometimes called "partial-birth abortion."	—	Some argue that late-term abortion is a barbaric procedure and should be banned. Others argue that late-term abortions are extremely uncommon and used only in exceptional circumstances best determined by a doctor, not the Congress. The proposed legislation could also be the opening to a broader ban on abortion.
CCES	Now we'd like to ask you about whether the federal government should fund stem-cell research.	—	Some in Congress argue that this research may lead to cures for diseases and disabilities affecting large numbers of Americans, and should be funded. Others argue that a potential human life has to be destroyed in order to use these cells, and funding it would be unethical.
CCES	Congress also debated a proposal that the president begin phased redeployment of U.S. troops from Iraq starting this year and submit to Congress by the end of 2006 a plan with estimated dates for continued phased withdrawal.	—	Some politicians argue that setting out a plan to withdraw would make Iraqis take responsibility for their country and become more independent of the United States. Others argue that it is too early to start withdrawing, and that doing so would make terrorists grow bolder.
CCES	Another issue is illegal immigration. One plan considered by the Senate would offer illegal immigrants who already live in the United States more opportunities to become legal citizens.	—	Some politicians argue that people who have worked hard in jobs that the economy depends on should be offered the chance to live here legally. Other politicians argue that the plan is an amnesty that rewards people who have broken the law.

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Table 1. *Continued*

Survey	Brief description	Partisan cue	Pro–con argument
CCES	Congress considered a proposal to increase the federal minimum wage from $5.15 to $6.25 within the next year and a half.	—	Some politicians argue that the wage should be increased because it hasn't changed since 1997 and many workers still live in poverty. Other politicians argue that raising the wage might force small businesses to cut jobs and would hurt the economy.
CCES	We'd like to ask about cutting taxes on the money people make from selling investments, also referred to as capital gains. This past year the Senate considered a bill to extend capital gains tax cuts passed in 2001.	—	Some politicians argue that these tax reductions make the economy strong and encourage people to invest more. Others argue that the plan would mostly benefit people who are already rich and that any tax cuts should be shared more fairly among all taxpayers.
CCES	This year Congress also debated a new free-trade agreement that reduces barriers to trade between the United States and countries in Central America.	—	Some politicians argue that the agreement allows America to better compete in the global economy and would create more stable democracies in Central America. Other politicians argue that it helps businesses move jobs abroad where labor is cheaper and does not protect American producers.
TESS	The Lilly Ledbetter Fair Pay Act of 2009 extended the period of time in which workers can sue their employers for discrimination in pay due to race or gender.	98% of House Democrats voted for the bill. 2% of House Republicans voted for the bill.	Supporters argued that women and minorities who are victims of pay discrimination do not find out immediately that their pay is lower than others with similar jobs. Opponents argued that more lawsuits would make it more difficult for businesses to operate.
TESS	The reauthorization of the State Children's Health Insurance Program in 2009 authorized an additional $32.8 billion to expand the health coverage program for children.	99% of House Democrats voted for the bill. 23% of House Republicans voted for the bill.	Supporters argued that the expansion would provide coverage for about 4 million more children. Opponents argued that further government involvement in health care would crowd out private insurance.

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Table 1. *Continued*

Survey	Brief description	Partisan cue	Pro–con argument
TESS	The American Recovery and Reinvestment Act of 2009 provided $787 billion of stimulus to the U.S. economy through a mixture of grants to states, tax cuts, and direct spending on infrastructure.	96% of House Democrats voted for the bill. 0% of House Republicans voted for the bill.	Supporters argued that increased government spending was required to bring the U.S. economy out of recession. Opponents argued that the spending would increase government debt and require higher taxes in the future.
TESS	The American Clean Energy And Security Act of 2009 proposed a cap and trade system to limit carbon dioxide emissions.	83% of House Democrats voted for the bill. 5% of House Republicans voted for the bill.	Supporters argued that limiting carbon dioxide emissions would create clean energy jobs, encourage energy independence, and reduce global warming. Opponents argued that the proposal would increase the price of energy and other goods.
TESS	The Unemployment Compensation Extension Act of 2009 extended the period of unemployment benefits by 14 weeks for Americans who have lost their jobs in the current economic crisis.	93% of House Democrats voted for the bill. 61% of House Republicans voted for the bill.	Supporters argued that without this measure, nearly 2 million Americans would exhaust their unemployment benefits. Opponents argued that extending unemployment compensation would increase the budget deficit and discourage unemployed workers from finding new jobs.
TESS	The Stupak-Pitts Amendment to the Affordable Health Care for America Act of 2009 proposed to make it illegal to use federal funding "to pay for any abortion or to cover any part of the costs of any health plan that includes coverage of abortion" except in cases of rape, incest, or danger to the life of the mother.	25% of House Democrats voted for the amendment. 100% of House Republicans voted for the amendment.	Supporters argued that the amendment is necessary to prevent taxpayers from funding abortion. Opponents argued that the amendment would cause existing private health insurance plans to eliminate abortion coverage.

NOTE.—All respondents to the CCES received the pro–con argument for all questions, whereas the partisan cue and pro–con argument were provided as treatments to subsets of the TESS sample.

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this bill if you had been in the U.S. House?" with response choices of "For," "Against," and "Don't Know." The TESS subjects were randomized into four treatment groups, blocked on responses to the standard seven-category self-reported party identification as recorded for each respondent from previous Knowledge Networks surveys. The four groups received no treatment, a partisan cue treatment, a pro–con argument treatment, and both treatments (a 2x2 design). Each subject received the same treatment(s) on all six bills, with the order of the six bills randomly assigned. The partisan cue treatment consisted of information about the percent of support for the bill/amendment within each party in the U.S. House. The pro–con argument treatment closely resembled that of the CCES: one sentence of arguments in support followed by one sentence in opposition. Thus, the experiment examines both the effect of the pro–con arguments that are given to all respondents in the CCES instrument as well as an additional partisan cue treatment that also might polarize opinion.

Measures

Although some of the following analysis is performed at the level of individual bills, assessing polarization requires aggregate measures of individuals' preferences on a left–right continuum. To construct summaries of legislators' and respondents' profiles of issue preferences, I use mean posterior ideal points from a standard unidimensional Bayesian ideal point estimator (Jackman 2001), implemented in JAGS (Plummer 2008) and R (R Development Core Team 2008). Ideal point estimation is one of many scaling techniques, providing a single left–right "ideal point" or position for each legislator/respondent based on his or her votes/responses.

Item nonresponse ("don't know") on the survey and legislator absence/abstention are treated in the same way. When studying Congressional roll-call voting, legislator absence/abstention is typically treated as missing at random rather resulting from indifference: When members of Congress fail to vote, it is usually for reasons that have little to do with their preferences over the roll-call alternatives. In contrast, "don't know" responses often might imply an intermediate position between support and opposition. As a robustness check, I have compared the mean posterior ideal points from an ordered model treating missing votes and responses as an intermediate category (Quinn 2004) to those from the binary model that treats those responses as missing at random. The results are substantively the same.

Although one might expect that the TESS treatments would substantially change the rate of "don't know" responses, the fraction of "don't know" preference responses changes only slightly across treatment conditions: 0.266 in the control group, 0.279 in the partisan cue group, 0.258 in the pro–con argument group, and 0.234 in the group receiving both treatments. The corresponding fractions of respondents who express no position on any of the six

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issues are 0.061, 0.103, 0.072, and 0.072, respectively. The CCES respondent pool has lower item nonresponse rates: 0.116 of issue preference responses are "don't know," and 0.006 of respondents give "don't know" responses to all seven questions. This may be due to a less representative—more politically engaged—sample (Ansolabehere 2006).

Because all issues in both surveys had clear partisan alignments, the extremity of an individual's estimated ideal point captures the degree to which that citizen or legislator holds a partisan profile of preferences. The basic features of an ideal point distribution are captured by the mean (or median) and standard deviation of the distribution, which respectively measure the center and the degree of polarization. The overall similarity of the entire distributions can be compared using the Wasserstein distance metric d_W, the area between the cumulative density functions of ideal points (Golder and Stramski 2010). Although there are many distance metrics for probability distributions, this measure is easily interpretable as the average distance that members of Congress would have to move along the ideal point scale to match the distribution of public preferences.

To measure levels of political information about the political debate on the queried issues, I use the survey questions about senators' roll-call votes on the CCES survey and about partisan issue alignments on the TESS survey. Both sets of questions provide evidence about whether individual citizens understand the partisan alignments of the issues. Both the CCES and TESS information questions are scaled additively, with a penalty for incorrect answers to correct for guessing: correct answers +1, incorrect answers −1, and "don't know" responses 0.[2]

For the CCES data, the additive information scale approach requires adjustment to account for the fact that question difficulty varies by state. The extreme example is Maine, where guessing the party-implied vote for all fourteen senator votes would have yielded only five correct answers. Correspondingly, Maine respondents have the lowest mean score of any state under an unadjusted scoring rule. To adjust for varying difficulty, I only score respondents' answers on questions where the senator's vote corresponds to the majority position in the senator's party. For further discussion of this and other issues related to these information measures, see online appendix A (please see the supplementary data online). For both the TESS and CCES samples, these scoring rules yield raw scores that range over $\pm K$, where K is the total number of information questions. For presentation, I rescale these to a [0, 1] scale by dividing by K and truncating negative scores to zero. This procedure identifies 9 percent of the CCES sample and 15 percent of the TESS sample as "fully informed" with respect to these information questions.

2. In general, the question of whether incorrect or "don't know" responses imply greater knowledge is difficult (Mondak 1999; Luskin and Bullock 2011); however, for the purposes of the subsequent analysis, only the identification of "full information" individuals is consequential.

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Results for Information and Polarization

The left panels of figure 1 show the distribution of estimated ideal points for all respondents to both surveys.[3] The remaining panels show the ideal point distribution for just respondents with the lowest and highest information scores. Online appendix figures 1 and 2 show the same plots for intermediate information levels (please see the supplementary data online). Congress has a distinctively bimodal distribution of preferences: Democrats on the left, and Republicans on the right. Citizens with low information levels have a moderate, unimodal distribution of preferences because very few of these citizens express consistently partisan profiles of issue preferences. However, the preferences of those citizens who are fully informed are nearly as polarized as members of Congress (the Senate in the case of the CCES survey, and the House in the case of the TESS survey). Previous research has concluded that the public is less polarized on political issues than is Congress (Bafumi and Herron 2010), and that more informed citizens are more polarized than less informed citizens (Palfrey and Poole 1987; Abramowitz 2010). The results here connect these findings. The preferences of the public as a whole are less polarized than those of Congress,

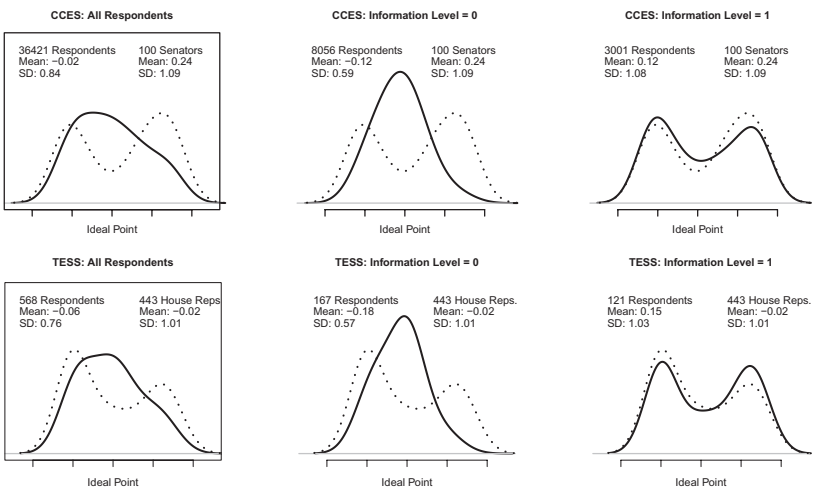

Figure 1. Density (y-axes) of estimated ideal points for CCES and TESS respondents (solid lines) compared to Congress (dotted lines), with panels including all respondents, low-information respondents, and high-information respondents. Only the TESS respondents in the pro–con arguments treatment group are included in the figure.

3. To facilitate comparability between the two surveys, for the TESS survey only the pro–con argument treatment group that most closely approximated the CCES design is shown.

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but the preferences of those in the public who are aware of the partisan alignment of political issues are no less polarized than Congress on those issues.

Figure 2 shows expressed support for a subset of the seven CCES and six TESS bills as a function of information-level and self-reported party identification (PID). The partisan categories are collapsed and independents omitted, (for additional plots with all PID groups and all bills, see online appendix figures 3 and 4). To facilitate comparison to Congressional behavior, the right margin of each plot shows the fraction of Republican and Democratic representatives voting for the bill. On every issue, the difference in preferences between opposing partisans is much larger among more informed citizens.[4] On every issue except immigration reform—on which the Senate is systematically more supportive than the public, regardless of party—the distribution of opinion among the partisans who are aware of the party line on the issues is much closer to the distribution of vote by party in the Senate than is the distribution among partisans who do not know the party line.

Across both surveys, citizens who are highly informed about the set of political issues are just as polarized as Congress on those same issues. In the next two sections, I assess the mechanisms that might lead to this relationship. Underlying the analyses is an attempt to answer two questions: Is it the case that learning the partisan alignment of the issues causes citizens to adopt partisan positions? Or is it the case that only the kinds of people who have partisan positions pay enough attention to politics to learn about these issues?

Results for Estimating Information Effects by Matching

The individuals with maximum scores on the information scale are different from the broader population in respects other than political information levels, differences that could provide alternative explanations for their relative polarization. I assess this hypothesis by reweighting the respondents with maximum scores on the information scale to match the CCES target population's distribution of covariates as closely as possible. This involves combining two sets of weights: matching yields a reweighting of the fully informed subset to replicate the characteristics of the full CCES sample, and the CCES sample weights enables a further reweighting to the CCES target population. Mathematical details are provided in online appendix B. Unlike most applications of matching, the quantity of interest in this study is not an average treatment effect, but instead a population distribution of potential outcomes under a treatment: the distribution of ideal points that would exist if all citizens knew enough to score perfectly on the information questions. This quantity of interest makes kernel

4. This polarization occurs within each PID category: High-information citizens are not just more polarized because they are more likely to be strong partisans (online appendix figure 3). Although the fraction of respondents in certain party-identification categories does increase with information, all categories are well represented at all levels of information.

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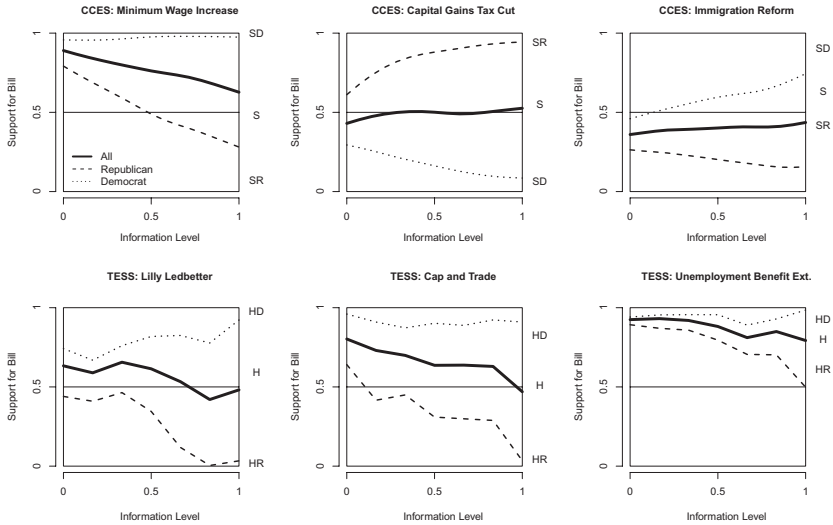

Figure 2. Plots of support for a subset of the CCES and TESS bills, as a function of information score. The thick black line is the average support level among all respondents who expressed a position. The thinner lines are expressed support by party identification. For the CCES questions, the abbreviations in the right margin show levels of support among all senators (S), Senate Democrats (SD), and Senate Republicans (SR); for the TESS questions, the abbreviations in the right margin show levels of support among all members of the House (H), House Democrats (HD), and House Republicans (HR). Only the TESS respondents in the pro–con arguments treatment group are included in the figure.

matching (Heckman, Ichimura, and Todd 1998) relatively appealing because it returns matches for all respondents and is computationally inexpensive.

Generally, one should match only on pre-treatment variables: those that are fixed before citizens learn anything about the political issues. Unfortunately, since all variables on the CCES are self-reported at the time of the survey, none are strictly pre-treatment. To the extent that political information is collected over an entire political lifetime, setting aside the timing of the self-report, the pre-treatment assumption is potentially undermined for a wide range of variables. From the most clearly to the least clearly pre-treatment, I use the following set of matching variables: age, sex, race, income, education, residential location (latitude, longitude, and population density), frequency of church attendance, Catholicism, self-description as a "born-again" Christian, membership in a household with a current union member, and ownership of at least one gun. Party identification is the most difficult case: it is predictively

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powerful, but might change under the hypothetical treatment of becoming more generally informed.[5] The presented analysis does not match on PID; however, all results were replicated with exact matching on seven-category PID,[6] without any changes in substantive conclusions.[7]

Although matching does not entirely resolve discrepancies between the fully informed subset and the broader CCES sample on all dimensions—for example, education disparities are only partly remediated—much of the discrepancy between the uninformed and informed along the matched dimensions is eliminated. If any of these factors were especially important in producing the association between political information and polarization, we would expect to find that matched informed preferences were less polarized than unmatched informed preferences.

Figure 3 shows how these matched informed preferences compare to expressed preferences and to the Senate. Whereas the left panel shows that preferences expressed by the full CCES sample are far less polarized than Senate preferences, the right panel shows that even after reweighting, the preferences of the informed are similar to those of the Senate. It might appear that matching changes the distribution of informed preferences very little; however, there are important differences between the distribution of informed preferences before and after matching. Because being informed is associated with characteristics that are also associated with being on the right (especially being male and having a high income), reweighting via matching shifts the preferences of the informed subset to the left, well to the left of the distribution of Senate preferences. The right panel shows the effect of the election on the distribution of senator preferences. Since the ten new members of the Senate elected for the first time in 2006 did not vote on these seven bills, their votes were imputed based on campaign statements and, where applicable, corresponding House votes (see online appendix table 1). Using these imputed votes to estimate ideal points for the new senators, the discrepancy between matched informed preferences and the distribution of Senate voting was reduced, but not eliminated, by the Republican losses in the 2006 election.

5. Party identification is empirically stable over citizens' lifetimes (Green, Palmquist, and Schickler 2002); however, so too is interest in politics (Prior 2010). Thus, the fact that PID is generally stable tells us little about the stability of PID under the counterfactual of becoming *much* more informed, since so few people are getting this "treatment."

6. Using more conservative subsets of the matching variables also yields similar results.

7. Variable codings are as follows: sex (male, female), age (integer years), race (unordered: white, black, Hispanic, other), family income (fourteen ordered integer categories plus binary variable for refusal), education (ordered: less than high school, high school, some college, two-year degree, four-year degree, postgrad degree), zip code longitude (degrees), zip code latitude (degrees), zip code population density (log population per square mile), church attendance (ordered integer values corresponding to once a week or more, a few times a month, less than once a month, almost never or never, and not sure), Catholic (yes, no), "born-again" (yes, no), gun owner (yes, no, or not sure), currently union household (yes, no, or not sure). See online appendix B and online appendix figure 5 for more information about the quality of the matches.

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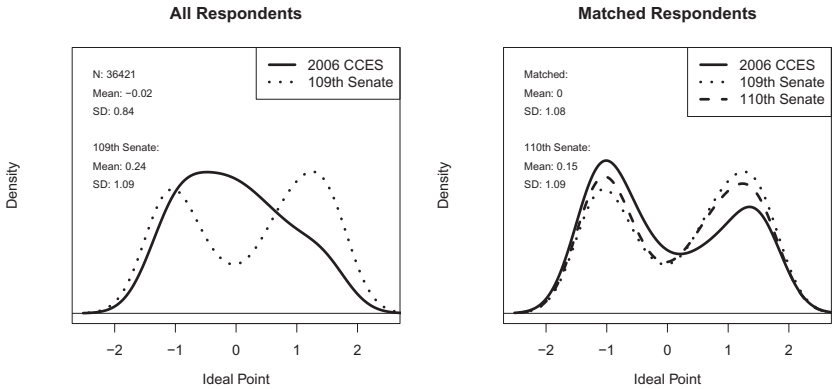

Figure 3. Comparison of public and congressional preferences, before and after matching. The left panel compares the population distribution of expressed preferences to the distribution of preferences in the Senate. The right panel compares the distribution of informed preferences after matching to the distribution of preferences for the 109th and 110th Senates.

Although several of the matching variables predict left–right preferences, very few other than information predict polarization. The relationship between income, information, and ideal point shown in figure 4 helps illustrate why the matched informed preferences are still polarized. Income is among the strongest predictors of both information level and ideal point. Low income predicts being relatively uninformed as well as having left expressed preferences. But although low-income respondents are less polarized than medium- and high-income respondents (top panels of figure 4), this is almost entirely due to their lower average level of information. Low-income, high-information respondents are just as polarized as medium- and high-income, high-information respondents (bottom panels of figure 4). In comparison to low-income, low-information respondents, not only are low-income, high-information respondents more likely to hold consistently left issue preferences, but they are also more likely to hold consistently right issue preferences. When low-income, low-information respondents are matched, they are matched to low-income, high-information respondents who have very partisan profiles of preferences.

The expressed preference distribution for all CCES respondents has a Wasserstein distance of $d_W = 0.37$ from the distribution of Senate preferences. For matched informed preferences, the distance is reduced to $d_W = 0.24$. This reduction in distance is less striking than the associated elimination of the polarization gap because the distance measure penalizes the left–right imbalance between the matched informed preferences and the Senate (figure 3, right panel). However, the result of the 2006 election was to reduce the distance between matched informed preferences and the Senate from 0.24 to 0.16, a reduction in distance of almost exactly one third. Given that only one third of

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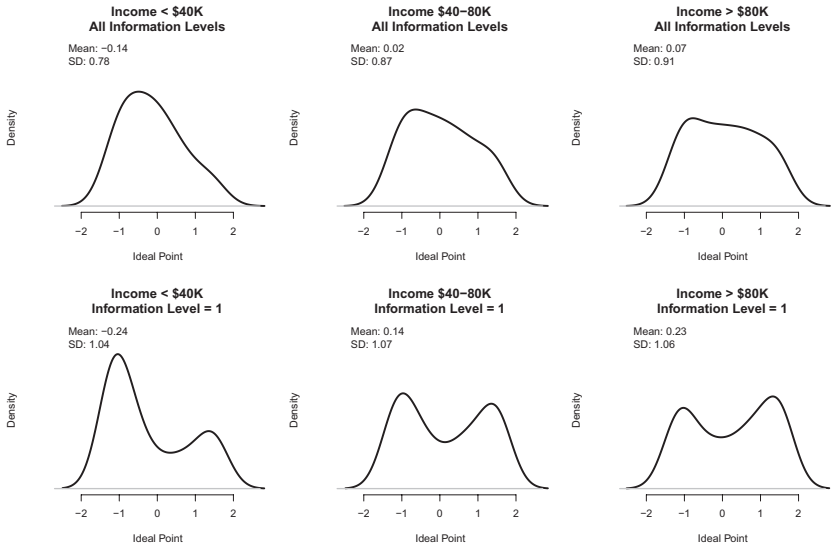

Figure 4. Estimated ideal point densities for CCES respondents with low (left column), medium (center column), and high income (right column). The top row of panels shows the ideal point distributions by income for all respondents; the bottom row shows only individuals with the highest level of information.

the Senate was subject to election, this level of reduction is the most that could be expected from a single election.

Although the results of matching indicate that none of the matched variables explains the observed association between polarization and measured information, that need not imply that a causal effect of information is responsible. Several assumptions would need to hold for the matching estimates to recover the counterfactually informed preferences of the public. First, there must be no omitted variables. In reality, there are always omitted variables, although many typically important political behavior variables are used in the matching. Second, citizens' private information about their preferences or the effect of becoming informed cannot influence their decision to get political information (Heckman 1997). This assumption is likely to be violated because citizens have expectations about whether they will be persuaded by elites' preferences and only those who trust elites enough to take cues bother to learn the elites' preferences. Here, the fact that matching on party identification does not change the results is reassuring because PID provides the most plausible measure of this propensity to adopt the preferences of partisan elites. Third, we must assume that there are no spillover effects between citizens in the hypothetical world where all citizens are highly informed about politics

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(Sekhon 2004). The statistical language for this problem varies: it is sometimes described as a violation of the stable unit treatment value assumption and sometimes as a general equilibrium or social interaction effect (Sekhon 2004; Heckman 2005). This assumption is likely to be violated because citizens' political beliefs are formed in part through communication with other citizens. In a world in which all citizens were well informed about politics, any kind of network effects would lead to a distribution of citizens' preferences that would be different from the distribution of preferences that results from aggregating the individual effects of making each citizen in the current world more informed.

Despite these obstacles to interpreting matched informed preferences as counterfactually informed preferences, the mechanical interpretation of matching is still interesting. Well-informed Americans, reweighted to match the characteristics of the entire population, have a similar distribution of issue preferences to that of the Senate. If one accepts the assumptions necessary to give this information effect a causal interpretation, this indicates that the counterfactually informed preferences of the public are polarized to an extent that is comparable to that of the Senate. But even without the assumptions necessary to give these results a causal interpretation, these results indicate that the discrepancy between the preferences of the informed and the uninformed is not due to differences between those populations in the matched covariates, several of which are strongly associated with political information and preferences.

Estimating Information Effects by Experiment

To argue that information provides part of the explanation for the discrepancy between public and Congressional preferences, it is necessary to show that there is a causal effect of information. The survey experiment embedded in the TESS survey instrument was designed to do this. One way of understanding the polarization gap is that citizens' preferences are less correlated across issues than those of members of Congress (Converse 1964, p. 228). Table 2 shows the correlation between positions on each pair of issues for the House and for respondents in each treatment condition.[8] The informational treatments in the TESS instrument increase the strength of associations between citizens' positions on different issues.

Figure 5 shows the resulting increase in the polarization of ideal points: Where the ideal points of the control group have standard deviation $\sigma = 0.68$, the party vote treatment group has $\sigma = 0.73$, the pro–con argument treatment group has $\sigma = 0.76$, and the both treatment group has $\sigma = 0.79$. Compared to

8. Online appendix figure 6 shows the mean expressed preferences for TESS respondents on each issue as a function of treatment condition.

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Table 2. Correlations (Kendall's τ) between Positions on Issues for the U.S. House (using roll-call votes) and Survey Respondents (using survey responses) Using Pairwise Complete Observations

U.S. House	Fair pay	SCHIP	Stimulus	Cap/trade	Unemploy.	Stupak
Fair pay	1	0.79	0.94	0.78	0.40	–0.74
SCHIP		1	0.77	0.70	0.47	–0.61
Stimulus			1	0.76	0.36	–0.75
Cap and trade				1	0.36	–0.75
Unemployment					1	–0.32
Stupak						1
Control	Fair pay	SCHIP	Stimulus	Cap/trade	Unemploy.	Stupak
Fair pay	1	0.39	0.42	0.38	0.35	–0.16
SCHIP		1	0.44	0.59	0.42	–0.17
Stimulus			1	0.56	0.36	–0.16
Cap and trade				1	0.40	–0.13
Unemployment					1	–0.07
Stupak						1
Partisan cue	Fair pay	SCHIP	Stimulus	Cap/trade	Unemploy.	Stupak
Fair pay	1	0.48	0.49	0.44	0.44	–0.18
SCHIP		1	0.61	0.60	0.42	–0.26
Stimulus			1	0.53	0.45	–0.27
Cap and trade				1	0.35	–0.17
Unemployment					1	–0.17
Stupak						1
Pro–con	Fair pay	SCHIP	Stimulus	Cap/trade	Unemploy.	Stupak
Fair pay	1	0.53	0.68	0.59	0.35	–0.37
SCHIP		1	0.58	0.62	0.48	–0.28
Stimulus			1	0.72	0.40	–0.44
Cap and trade				1	0.39	–0.37
Unemployment					1	–0.15
Stupak						1
Both treatments	Fair pay	SCHIP	Stimulus	Cap/trade	Unemploy.	Stupak
Fair pay	1	0.64	0.63	0.60	0.45	–0.34
SCHIP		1	0.54	0.67	0.50	–0.34
Stimulus			1	0.68	0.43	–0.43
Cap and trade				1	0.35	–0.31
Unemployment					1	–0.21
Stupak						1

the U.S. House, $\sigma = 1.01$, receiving both treatments reduces the polarization gap by 33 percent versus the group receiving neither treatment. A one-sided randomization test of the null hypothesis that this difference is due to sampling variability has $p < 0.001$. The effects of both treatments are also significant when considered separately versus control, though more marginally for the partisan cue treatment ($p = 0.05$) than for the pro–con argument ($p = 0.002$).

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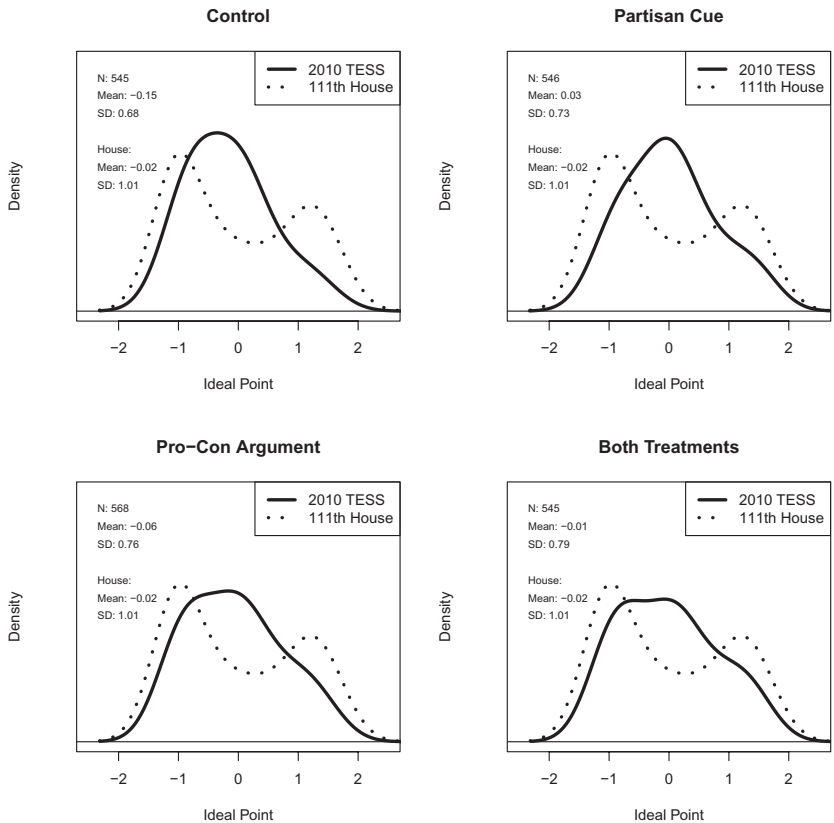

Figure 5. Distribution of ideal points for each of the four TESS treatment groups.

The Wasserstein distance between the control group and the U.S. House is $d_W = 0.36$. Bootstrapping 1,000 survey resamples yields a 95-percent interval from 0.32 to 0.40. The distance between the House and the party vote treatment group is $d_W = 0.35$ (0.30 to 0.40), the pro–con argument treatment group has $d_W = 0.29$ (0.24 to 0.33), and the both treatment group has $d_W = 0.26$ (0.22 to 0.31). These scale similarly to the standard deviations of the distributions because the treatment primarily changes the polarization of public opinion. Comparing the group that received both treatments to the control group, the effect is to reduce the overall distance between public and Congressional preferences by 27 percent (10 to 42 percent).

The pro–con argument treatment appears to have a greater effect than the partisan cue treatment in this experiment; however, this difference is not itself significant ($p = 0.18$), and it is important to recognize that the numerical values of the treatment effects cannot be generalized. They are a result of the

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particular issues being asked about, the particular arguments provided, and *ex ante* information about those issues held by the public. These are high-profile issues, which probably make the treatment effects smaller than if a random subset of Congressional votes were used. On obscure procedural votes, we might expect the effects of these signals to be very large, if only because respondents would have no prior knowledge about the vote. The particular magnitudes of the polarizing effects observed in the experiment are less important than the fact that there are such effects. The survey experiment demonstrates that citizens' expressed preferences are uncertain enough that they are pushed toward more consistently partisan issue preferences by modest informational interventions.

How do we make sense of these results in the context of the matching analysis on the CCES data? First, the treatment effect associated with the pro–con argument indicates that the CCES question format already yields more polarized public opinion measures than a format without that information provision would. Second, the treatment effect associated with providing partisan cues closes only 18 percent of the polarization gap, despite the fact that the matching analysis on the CCES data indicates that a closely related information measure seemingly explains much of that gap.

It is important to keep in mind that the informational treatments provided in the survey experiment are weak by comparison to the naturally occurring variation in political information that is exploited in the matching analysis. Behind different scores on the CCES and TESS information measures lurk lifelong differences in political attention and political sophistication. Without the broader political information that tends to accompany information about the partisan alignment of particular issues, citizens may have difficulty making sense of the cues and arguments they are exposed to in the experiment. As noted earlier during the discussion of the measures, the overall fraction of "don't know" responses is changed little by the treatments. The treatments are too weak to help the least politically engaged respondents make sense of these unfamiliar issues. Still, from this experiment we can see that there is clearly the potential to polarize public opinion by providing information beyond that which citizens have already gathered before they are surveyed.

Conclusion

U.S. citizens who know which party takes which side of high-profile legislative issues are just as polarized as the U.S. Congress on those issues. The fact that most Americans are less polarized seems to be partially due to the fact that they are not paying attention to the lively partisan debate that tends to accompany such issues. However, the analyses in the preceding two sections leave substantial uncertainty as to the exact shape of the U.S. public's preferences about roll-call votes, under the counterfactual in which all Americans were as attentive to partisan debates about issues as their most informed fellow

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citizens. Identifying the preferences that citizens would hold if they were all highly attentive to political debate requires identifying the causal effects of treatments that are too large, and too ill defined, to be the subject of an experiment. Because of this identification problem, we cannot say exactly how close the U.S. Congress comes to the preference distribution that the American public would have, were all citizens more attentive to politics. Nonetheless, we can say that these distributions of preference are at least somewhat closer than those we actually observe in a political world where most citizens are paying little attention to politics.

The two surveys considered in this study demonstrate the same cross-sectional relationship between information and polarization in the public. Given this general similarity, the differences in the results of matching and of the survey experiment almost surely result from the different identification strategies used to try to estimate counterfactually informed preferences. Finding striking differences between observational and experimental identification strategies is nothing new in the social sciences. Experimental studies tend to be viewed as the gold standard, but in cases like the one considered here, it is impossible to run the desired experiment. There is no way to make up for a lifetime of inattention to politics in a few minutes of interaction with a citizen.

Supplementary Data

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

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