The political consequences of uninformed voters

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ABSTRACT

Americans fail to meet the democratic ideal of an informed electorate, and the consequences of this political ignorance are a topic of significant scholarly debate. In two independent settings, we experimentally test the effect of political information on citizens' attitudes toward the major parties in the U.S. When uninformed citizens receive political information, they systematically shift their political preferences away from the Republican Party and toward the Democrats. A lack of knowledge on the policy positions of the parties significantly hinders the ability of low-socioeconomic-status citizens to translate their preferences into partisan opinions and vote choices. As a result, American public opinion—and potentially election results and public policy as a result—is significantly different from the counterfactual world in which all voters are informed.

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1. Introduction

Americans fall short of the democratic ideal of a well-informed electorate. As a result, citizens may have difficulty translating their policy preferences into partisan opinion and vote choices. This, in turn, can cause aggregate opinion and election results to diverge from the counterfactual world in which all voters are informed. This paper aims to assess the extent of this dilemma in the American political context. What are the consequences of the public's lack of political knowledge? What would happen if the American public were more informed?

Previous studies have tackled this question with observational data both in the United States (Althaus, 1998; Bartels, 1996; Gilens, 2001; Levendusky, 2011; Sekhon, 2004) and elsewhere (Bhatti, 2010; Hansen, 2009; Oscarsson, 2007; Tóka, 2007), concluding that more political knowledge in the electorate would have minimal effects or benefit right wing parties. However, confounding variables, reverse causation, and measurement error could plague the interpretation of these results and bias their estimates of the effects of information.

In order to overcome these methodological challenges, we present two, independent, randomized, controlled experiments which test for the effects of information about the parties' policy stances on aggregate partisan opinion. Despite many significant differences between the two experiments including the experimental designs, subject pools, the issues discussed, and the method by which information is delivered, both experiments yield the same result. Exogenous increases in policy-specific political knowledge produce a relative increase in support for the Democratic Party. In short, American public opinion—and potentially election results and public policy as a result—appears to be significantly different from the counterfactual world in which all voters are informed about the positions of the parties.

2. How much do people know and how much does it matter?

In the context of this paper, the terms *political knowledge* and *political information* are used interchangeably to refer to citizens' knowledge about the major party's

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positions on key issues of public policy. While the effects of general political knowledge may be interesting and distinct from the effects of domain-specific knowledge, they are outside the scope of this study.[2] Early survey researchers measured the extent of political knowledge in the American electorate, and the results are disconcerting: citizens did not know what the parties stood for (Berelson et al., 1954) or the main points about major policies (Campbell et al., 1960). The subsequent decades saw little improvement, despite the fact that average educational attainment increased significantly (Delli Carpini and Keeter, 1991, 1996). These findings have real implications, as information about the parties' stances and policies allows voters to update their partisan preferences and political beliefs (Carsey and Layman, 2006; Dancey and Goren, 2010; Levendusky, 2009). Taken together, the literature calls into question the quality of collective public opinion and the effectiveness of the entire democratic process (Campbell et al., 1960; Converse, 1964).

Optimistic scholars have tried to alleviate concerns about the public's lack of political knowledge by arguing that heuristics and aggregation produce outcomes that look *as if* a fully informed electorate made the decision. While voters can use cognitive shortcuts (Lupia, 1994; Mondak, 1993; Popkin, 1991; Robertson et al., 1976; Schaffner and Streb, 2002), researchers have not demonstrated that they use these shortcuts regularly and effectively. In fact, reliance on cues can lead voters astray (Kuklinski and Hurley, 1994) and less informed citizens are less able to employ shortcuts successfully (Lau and Redlawsk, 1997).

A second defense against public ignorance is that random errors in collective opinion cancel out in a large electorate (Condorcet 1785; Page and Shapiro, 1992). This claim, however, requires the assumption that random errors occur equally. For example, if the same numbers of people incorrectly vote for the Democrats and the Republicans, then the election outcome will be no different than in the case in which all voters are fully informed. However, this assumption of symmetric errors is unsupported by empirical evidence. There are systematic differences in political knowledge throughout the population, and this should affect the relative number of uninformed voters within each party's camp.[3] In the Supporting Information we present a more formal treatment of this question—a model of an election with uninformed voters—which informs our hypothesis about the effects of political knowledge in the American context.

Cognitive shortcuts and statistical aggregation do not solve the problem of political knowledge, but does this matter? Althaus (2006) echoes the views of many democratic theorists in questioning whether the entire enterprise of research on political information is misguided: "But what core tenet of democratic theory is being offended by the mass public's apparent lack of civic-mindedness?" (p. 83). The purpose of this paper is not to take a stand on how the existence of uninformed citizens challenges democratic theory, but rather to answer a specific empirical question that stems from the political ignorance found within American society. Would public opinion—and potentially elections and public policy as a result—be different if the population were more informed? Because the ability to form political attitudes that are aligned with our interests is "mediated by the quality and quantity of political information we can bring to bear on an issue" (Althaus, 1998, 547), uninformed citizens may be less able to translate their policy preferences into partisan opinions. By experimentally testing for the effects of political information, we assess the extent of this problem and suggest potential remedies which will improve the extent to which public policy reflects the preferences of citizens.

3. Previous empirical evidence on the effects of information

Previous researchers have taken several methodological approaches to assess the effects of political knowledge, the most prominent of which is correlational. Informed citizens are more likely to be ideologically extreme (Palfrey and Poole, 1987), vote for incumbent presidents, and support Republican presidential candidates (Bartels, 1996). Researchers find a similar trend in Europe: knowledge is correlated with support for right leaning parties in European Parliament, Danish, and Swedish elections (Bhatti, 2010; Hansen, 2009; Oscarsson, 2007). However, confounding variables and reverse causation could plague the interpretation of these results.[4] Even panel methods which aim to overcome these problems (Levendusky, 2011; Sekhon, 2004) potentially suffer from the similar problems of time-varying confounding variables, reverse causation, or attenuation bias.

Deliberative polls (Fishkin, 1991, 1997) provide another opportunity to assess the effects of political information, and researchers find that opinions do change over the course of deliberation (Sturgis, 2003). However, these attempts at creating a knowledgeable public sphere generate problems similar to those of the correlational analyses. First, participants typically self-select into attendance (Denver et al., 1995; Tringali, 1996) and there is no comparable control group, which is needed to estimate the causal effect of deliberation. Second, a weekend of debate and deliberation is not the same as raw information. Biases in the material and presentations could sway the results

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[2] See Zaller (1985, 1986, 1992) and Iyengar (1986) for discussions and analyses of general political knowledge and its relationship to domain-specific knowledge.

[3] College graduates and high-income individuals demonstrate greater levels of political knowledge (Delli Carpini and Keeter, 1996). Additional research shows further informational inequalities: white, male, and older individuals are on average more informed than minority, female, and younger individuals (Bennett, 1988; Delli Carpini and Keeter, 1996; Neuman, 1986; Sigelman and Yanarella, 1986).

[4] In this context, an example of omitted variable bias is that watching cable news or listening to talk radio might cause a citizen to both become more informed and to change her attitudes in systematic ways. An example of reverse causation would be that voters who already support the Republican Party may be more likely to become informed. Either problem could lead to significant biases when estimating the causal effects of information.

(Daves, 1996; Merkle, 1996), as can the opinions of other participants.

The problems of omitted variables, reverse causation, and self-selection can never be fully overcome with statistical modeling of observational data. Such challenges can only be overcome through a randomized experiment or the discovery of a natural experiment. To explicitly test the effects of political information, we have designed and fielded two independent survey experiments. The experiments differ from one another in important ways but yield similar results, bolstering our confidence in both the internal and external validity of our findings. Both experiments—involving different experimental designs, samples of respondents, issues discussed, and methods for the delivery of information—find that an exogenous increase in political information causes a relative shift in partisan opinion toward the Democratic Party.

4. Hypothesis about the effect of information in the U.S

As previously discussed, informational deficits will not necessarily cancel out in the aggregate. If uninformed voters sometimes make mistakes and vote for the "wrong" party, election results will be biased away from the party that better represents the greater share of uninformed voters. In the Supporting Information, we present a model that formalizes this claim. In short, if citizens whose policy preferences align with Party A are less likely to be informed compared to the citizens whose preferences align with Party B, then Party A will receive a smaller vote share than it would in the counterfactual world in which all voters are fully informed. As a result, an increase in policy-relevant information would provide individuals with a more clear picture of how their political interests and partisan preferences align, which would enable them to "sort" into the party that is best suited to them (Levendusky, 2009).

With this intuition we can hypothesize about the direction of bias in U.S. elections. Young people, low-income citizens, racial minorities, and females are less likely to be politically informed (Bennett, 1988; Delli Carpini and Keeter, 1996; Neuman, 1986; Sigelman and Yanarella, 1986) and more likely to hold policy positions in line with Democratic Party (Edlund and Pande, 2002; McCarty et al., 2006). From these empirical regularities, we conclude that Democratic supporters—those who would support the Democratic Party if they were informed—are less likely to be informed about the party's positions.[5] Our hypothesis, therefore, is that if the American electorate had

more information, the Democrats would receive more support than they currently do.

In order to test the effects of information, we conducted two randomized, controlled experiments in the fall of 2010. The timing of the experiments should make it difficult to find results supporting our hypothesis. First, because our experiments took place during the 2010 midterm election season, respondents were likely exposed to other political stimuli in their daily life. This additional noise could drown out our experimental effects. Second, the election took place during a recession with a Democratic president that resulted in the Republican Party making significant gains in Congress. Moving respondents toward the Democrats during a time when public opinion was swinging away from them is not easy. The results we find, therefore, appear to be in spite of and not because of the timing of the experiments.

5. Experiment 1

Our first experiment employs a three-wave design, in which a convenience sample of subjects was recruited online. Although online convenience samples are a relatively new resource, Berinsky et al. (2012) show that the demographics of such respondents are comparable to those of ANES and CPS respondents. Moreover, the authors successfully replicate the results of several significant social science experiments with subjects recruited in this way.[6]

In the first wave, we collected baseline information on respondents' demographic characteristics, political knowledge, and political attitudes – untainted pre-treatment attitudes. First, we asked respondents to evaluate the Democratic and Republican Parties on a single screen. The exact question read, "On a scale of 0–10, how well do the Democrats and the Republicans represent your interests where 0 means 'does not represent my interest at all' and 10 means 'represents my interests almost perfectly'?" (emphasis in original) This question was asked in all three waves of the survey and serves as our dependent variable. Feelings toward the political parties are not the only outcome of interest in U.S. elections. Ideally we would run the same experiment with the entire electorate during campaigns and see how vote choices change. Given that such an experiment is unfeasible, we must rely on a proxy. Asking respondents to rate how well the political parties represent their interests is an intuitive way to measure political preferences. From a theoretical standpoint, voters should cast a ballot for the candidate that will provide maximum utility (Downs, 1957). In this setting, the party that best represents one's interests would be a good proxy for vote choice in Downs' model. One additional benefit to this question is that it forces respondents to think about the parties in relative terms and allows respondents to shift their party evaluations without sacrificing party identification, which is known to be stable (Green et al., 2002).

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[5] We are not claiming that all informed low-income, women, or African Americans would vote Democratic if they were informed. Nor are we claiming that these groups ought to vote Democratic. Because these groups tend to be less informed and are traditionally part of the Democratic base, we expect that increased knowledge will, on average, benefit the Democratic Party. For example, a young, low-income, minority citizen may have progressive views on taxes, minimum wage, and other economic policies, but she may not know that the Democratic Party better represents her interests. As a result, political information should make her more likely to support Democratic candidates.

[6] Subjects for Experiment 1 were recruited through Amazon Mechanical Turk.

Table 1
Summary of experimental design.

	Wave 1	Wave 2	Wave 3
Treatment		X	
Party evaluations	X	X	X
Knowledge questions	X	X	X
Field dates	11/3/10–11/10/10	11/18/10–11/28/10	12/2/10–12/8/10
Valid responses	851	493	335

X indicates the corresponding component was included in the wave.

Second, we asked respondents factual questions about politics to gauge their level of political information. We asked four general questions about current affairs and the American government as well as six specific policy questions about where the political parties stand on abortion, gay marriage, homosexuals in the military, minimum wage, Earned Income Tax Credit (EITC), and unemployment insurance.[7,8]

In the second wave, we randomly provided political information to half of the participants. We told subjects that we were interested in their reactions to newspaper articles. Rather than providing the information outright, the information was embedded in letters to the editor in order to minimize experimental demand effects (Zizzo, 2010). All respondents read two letters to the editor about a blood drive and two letters about building a dog park. In addition, respondents in the treatment group read six additional letters, one letter on each of the following issues: abortion, gay marriage, homosexuals in the military, minimum wage, EITC, and unemployment insurance. While realistic, these political letters to the editor contained factual information, such as the national Republican Party platform on abortion or the partisan breakdown of the most recent roll call vote on unemployment benefits.[9] We treated respondents by informing them about the positions of the parties on these keys issues. We focus on issue-specific information because this type of information, as opposed to general political information, potentially holds more important political consequences (Gilens, 2001). After a few filler questions about newspapers and letters to the editor to bolster our cover story, we asked respondents to evaluate the two parties using the same question from Wave 1. Finally, we repeated the same knowledge questions at the end of Wave 2 in order to test the extent to which subjects learned from the treatment.

While using letters to the editor improves our research design by minimizing demand effects, it also opens the door for other concerns because the information is mixed with emotional prose. The three economic letters were written from a Democratic perspective, while the three social letters were written from a Republican perspective. We chose this design because low information citizens tend to be closer to the Democratic Party on economic issues and closer to the Republican Party on social issues (Bartels, 2006). We are confident that the emotional slant of the letters does not pose a problem for our inferences, and we discuss these issues in the section on alternative explanations.

In the third wave, which took place one month after the initial survey, we re-asked the dependent variable and political knowledge questions. This final wave allows us to test whether the treatment had any lasting effect on political knowledge or party evaluations. If respondents do shift their party evaluations, is the effect evident several weeks later? Table 1 provides a summary of the experimental design. In total 851 respondents took the Wave 1 survey, 493 took both Wave 1 and Wave 2, and 335 completed all three waves.[10]

In order to analyze our data and cleanly interpret the results, we recoded a number of variables to range from 0 to 1. First, we summed the six issue-specific knowledge questions to create a measure of political knowledge. Respondents who answered all six issue-specific questions correctly are coded as a 1, those who answered five correctly received a value of 5/6, etc. We refer to the issue-specific knowledge scale simply as *Knowledge* throughout the rest of the paper. We similarly rescaled the four general

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[7] Full question wordings are available in the Supporting Information. We chose these specific issues, three social and three economic, because they are currently important to voters and the parties have distinctly different stances on each issue.

[8] While cheating on knowledge questions is always a possibility with Internet surveys, Berinsky et al. (2012) find no evidence of Mechanical Turk workers cheating in order to correctly answer factual questions. Additionally, randomization ensures that we should only be concerned about cheating if we suspect that those in the treated and control groups cheat at different rates. Given previous research and the randomization process, we believe the factual questions can be taken as an accurate measure of respondents' political knowledge.

[9] Following the letters to the editor, we asked respondents two multiple-choice recall questions to determine whether they actually read and paid attention to the letters. Both questions concerned the letters about the blood drive and the dog park. 75 percent of respondents correctly answered both recall questions, while only 4 percent were expected to answer them correctly by chance. This suggests that respondents actually read the letters to the editor and retained much of the information provided to them. The three economic letters to the editor and the three social letters to the editor were blocked together, such that respondents would read all three letters of one theme before moving on to the three letters with the other theme. Respondents in the treatment group were randomly assigned to read the either the economic or social letters to the editor first. Within each block of letters to the editor, the order of the letters was randomized. We do not find heterogeneous treatment effects based on whether respondents read the economic or social letters to the editor first.

[10] Panel attrition between Waves 1 and 2 does not pose a problem for internal validity, because we randomly assigned subjects into treatment conditions at the beginning of Wave 2. Attrition between Waves 2 and 3 could be problematic if the experimental treatment influenced subjects' chances of returning for Wave 3. However, subjects in the treatment and control group joined Wave 3 at the same rates ($p = .674$, two tailed *t*-test). In the Supporting Information we include a balance table that shows the demographic similarities of the treatment and control groups across the second and third waves of the survey.

knowledge questions to range between 0 and 1 with higher numbers indicating more correct answers. Issue-specific knowledge measures a respondent's ability to correctly identify the parties' positions on abortion, gay marriage, homosexuals in the military, minimum wage, EITC, and unemployment benefits. General knowledge measures a respondent's ability to correctly answer factual, non-policy questions about politics. While we briefly analyze the general political knowledge measure in Fig. 2, issue-specific knowledge is of more interest for this study.

We coded our dependent variable based on respondents' placements of the parties. We subtracted each respondent's placement of the Republican Party from her placement of the Democratic Party and recoded the difference to range from 0 to 1, with higher values representing greater support for the Democratic Party. We refer to this variable as *Party Evaluation*. By way of example, if someone gave the Democrats a score of 10 and the Republicans a score of 0, her party evaluation score would be 1. For the respondent who gave the Republicans a score of 10 and the Democrats a score of 0, her party evaluation score would be 0.

A table of summary statistics is provided in the Supporting Information. Subjects were, on average, young, well-informed, well-educated, and ideologically left-of-center. While our Internet sample is not perfectly representative of U.S. voters, the sample poses no challenge to the internal validity of our experiment due to the randomization of the experiment. If anything, a highly informed and left leaning sample will impede our ability to detect an effect of information on partisan attitudes.

First, we test whether the experimental treatment had the expected effect on knowledge: Did people learn from the treatment? The experimental treatment should increase the issue-specific knowledge of uninformed respondents, but it should have no effect on general political knowledge. For example, subjects in the treatment group should be more likely to know where the parties stand on abortion, but they should not be more likely to know that Timothy Geithner was the U.S. Secretary of the Treasury.

To assess the effect of the treatment on political knowledge, we present two non-parametric analyses.[11] Fig. 1 presents the average issue-specific knowledge in Wave 2 across different levels of initial issue-specific knowledge for the treated and untreated respondents. We expect our experiment to affect subjects differently according to their prior levels of political knowledge (Zaller, 1992), and this non-parametric analysis allows us to visualize this variation. We see that the experimental treatment dramatically increased the issue-specific knowledge of the treated subjects relative to the untreated subjects. Also, as expected, the effect was greater for subjects who began with lower levels of knowledge in Wave 1.[12] If a subject answered none of the
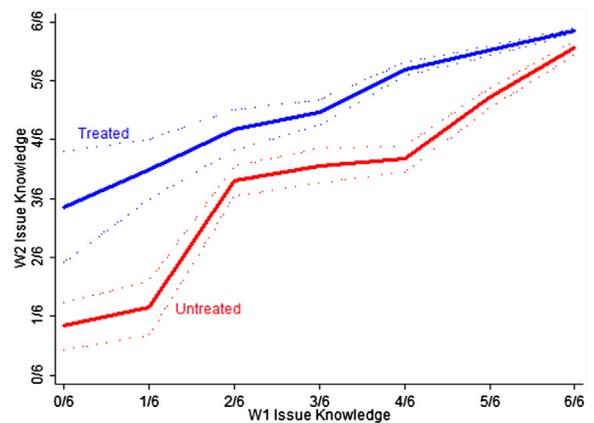

Fig. 1. Effect of treatment on issue-specific knowledge. The figure plots the average level of issue-specific knowledge in Wave 2 across the 7 possible different levels of issue-specific knowledge in Wave 1. Comparing these separate plots for the treatment and control groups, we see that the treatment dramatically increased the issue-specific knowledge of those who were initially uninformed but had little effect on the informed respondents. Dotted lines indicate standard errors.

issue-specific knowledge questions correctly in Wave 1, Fig. 1 shows that they would on average answer approximately 3 of 6 correctly in Wave 2 if they were in the treatment group compared to just 1 of 6 if they were in the control group. Those respondents in the treatment group did, in fact, learn from the experimental letters to the editor. Fig. 2 presents the same analysis for general political knowledge and, as expected, the treatment had no effect on subjects' ability to answer general political questions.

Next, we assess the effect of the treatment on party evaluations. Again, we take a similar non-parametric approach. Here the *y*-axis represents residual party evaluation, which we calculate in two steps. First, we regress Wave 2 party evaluation on Wave 1 party evaluation and party identification. Then we subtract the OLS predicted values from each respondent's party evaluation score. The resultant residual measures the extent to which respondents shifted toward or away from the Democratic Party between Waves 1 and 2. Based on our initial coding of the *Party Evaluation* variable, residuals greater than zero indicate that respondents moved closer to the Democrats between Wave 1 and Wave 2. In contrast, residuals less than zero mean that respondents moved toward the Republican Party. We plot the averages of these residuals across the different possible values of issue knowledge in Wave 1 separately for the treated and control subjects.[13] Consistent with our hypothesis, Fig. 3 shows that the treatment shifted uninformed respondents toward the Democratic Party but had little to no effect on informed respondents. Upon receiving political information, uninformed respondents shifted more than .15 points, 15% of the

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[11] Some readers may be unfamiliar with the graphical, non-parametric presentation of results in Figs. 1–3. The Supporting Information presents the same findings through more traditional tables.

[12] The relationship between Wave 1 and Wave 2 knowledge for the untreated group is consistent with regression to the mean. As respondents revert back to their own personal mean, extreme values of knowledge (both low and high) in Wave 1 are pulled back toward the general mean in Wave 2.

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[13] This process of residualizing the dependent variable improves statistical precision but is not necessary for generating unbiased estimates. The Supporting Information presents the same analysis shown in Fig. 3 with change scores. The results are substantively identical, but as expected, the estimates are slightly less precise.
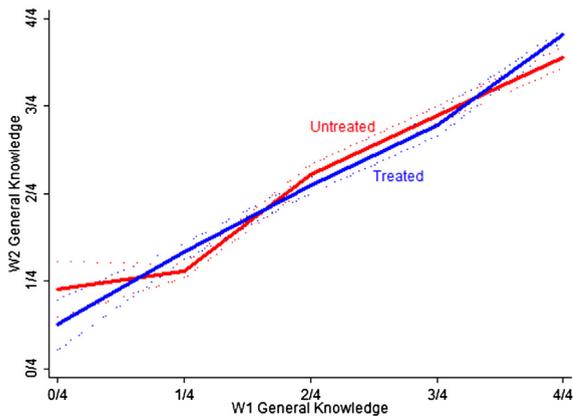
Fig. 2. No effect of treatment on general political knowledge. The figure plots the average level of general knowledge in Wave 2 across the 5 possible different levels of general knowledge in Wave 1. As expected, the treatment, which only provided issue-specific information, had no effect on general political knowledge. Dotted lines indicate standard errors.
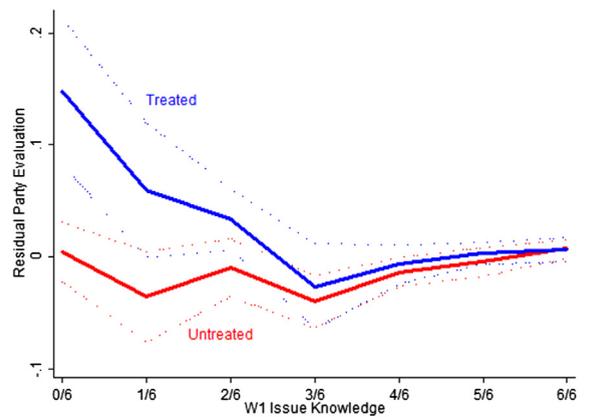

Fig. 3. Effect of treatment on democratic preference across levels of prior knowledge. The figure plots respondent's average residual party evaluations in Wave 2 across different levels of issue-specific knowledge in Wave 1. These residual party evaluations were calculated from a regression of Wave 2 party evaluations on Wave 1 party evaluations and Wave 1 party identifications. We see that the treatment dramatically shifted uninformed respondents toward the Democratic Party but, as expected, had no effect on informed respondents. Dotted lines indicate standard errors.

entire scale, toward the Democratic Party. Those in the control group, however, saw virtually no movement in their party evaluation between waves.

To explicitly test these experimental effects, we specify a parametric model and estimate it using OLS. For several different dependent variables, we estimate the effect of the treatment on the uninformed respondents through the following equation:

$$DV = a + \beta_1{}^*Treatment + \beta_2{}^*Knowledge1$$
$$+ \beta_3{}^*Treatment^*Knowledge1 + \beta_4{}^*Party1 + \varepsilon, \quad (1)$$

where *Treatment* is a dummy variable indicating whether the respondent was in the treatment (1) or control (0) group, *Knowledge*1 is respondents' level of issue-specific knowledge from Wave 1, and *Party1* represents each respondents' party evaluation from Wave 1. We expect that the treatment will have an effect on those who are initially uninformed, so we interact our treatment indicator with pre-treatment knowledge. As a result, β_1 represents the average treatment effect for respondents who are initially uninformed. This will be our coefficient of interest. We estimate this regression with four different dependent variables, issue-specific knowledge in both Wave 2 and Wave 3 and party evaluations in Wave 2 and Wave 3.

Having estimated the effect of the treatment on issue-specific knowledge, we can estimate the causal effect of issue-specific knowledge on party evaluations by two-stage least squares (2SLS). This procedure uses the treatment and the treatment interacted with prior knowledge as excluded instruments for issue-specific knowledge.[14] With a strong first-stage effect (F-statistic of 34.7), we generate predicted values of issue-specific knowledge in the first stage and then substitute these fitted values into the second stage. Using

party evaluations in both Wave 2 and Wave 3 as dependent variables, we estimate the causal effect of knowledge on party attitudes through the following second stage regression:

$$Party\ (2\ or\ 3) = a + \beta_1{}^*Predicted\ Knowledge\ (2\ or\ 3)$$
$$+ \beta_2{}^*Knowledge1 + \beta_3{}^*\ Party1 + \varepsilon. \quad (2)$$

Here, β_1 represents the causal effect of issue-specific knowledge on party evaluations for the types of respondents who are influenced by the treatment. For those respondents who are already fully informed or who fail to learn from the treatment, we cannot estimate the effect of knowledge. However, the subpopulation of respondents who are influenced by the treatment is precisely the subset of voters in whom we are most interested.[15]

This empirical strategy is motivated by the fundamental problem that political knowledge is an endogenous variable that is influenced by both partisan opinions and confounding variables that also influence partisan opinions. While a naïve regression of partisan opinions on political knowledge would produce biased estimates of the effect of knowledge, our experimental treatment provides exogenous variation in political knowledge. As such, we can use assignment to the treatment condition as an instrument for knowledge in order to back out the average effect of information for the individuals whose information levels are influenced by the treatment.

Table 2 presents the results of all 6 estimations. The first two columns verify the graphical results from Figs. 1 and 3,

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[14] See Hansford and Gomez (2010) and Gabel and Scheve (2007) for recent examples of studies using interaction terms in an IV model in this same way.

[15] These parametric regressions allow the effect of the treatment to vary linearly across different levels of prior knowledge. The Supporting Information presents more flexible models that allow for independent effects at each level of prior knowledge. The results of these more flexible analyses, including the 2SLS estimates of the effect of knowledge on partisan evaluations, are nearly identical to those presented here.

Table 2
The causal effects of knowledge on party evaluations.

	First	Reduced	2SLS	First	Reduced	2SLS
	W2 issue	W2 party	W2 party	W3 issue	W3 party	W3 party
W2 knowledge			**.206** (.095)*			
W3 knowledge						**.278** (.193)
Treatment	**.337** (.065)**	**.108** (.046)*		**.148** (.081)	**.048** (.043)	
Treatment*W1 knowl.	−.283 (.076)**	−.123 (.055)*		−.115 (.093)	−.042 (.050)	
W1 knowledge	.755 (.045)**	.042 (.027)	−.147 (.075)*	.743 (.067)**	.026 (.040)	−.186 (.132)
W1 party	−.023 (.028)	.919 (.028)**	.925 (.029)**	−.006 (.037)	.949 (.020)**	.950 (.021)**
Constant	.195 (.043)*	.021 (.025)	.001 (.035)	.213 (.062)**	.000 (.034)	−.055 (.061)
Mean of DV	.813	.578	.578	.807	.564	.564
St. dev of DV	.238	.257	.257	.242	.255	.255
Observations	493	493	493	335	335	335
R-squared	.480	.773		.439	.854	
SER	.174	.123		.180	.098	
F-statistic	**34.7**			**4.9**		

Robust standard errors in parentheses; * significant at 5%; ** significant at 1%.
All variables are scaled to range from 0 to 1. The "Treatment" coefficient can be interpreted as the effect of the treatment for the least informed respondents in our sample. Columns 1 and 4 show that the treatment increased the issue-specific knowledge of respondents in Waves 2 and 3, respectively. Columns 2 and 5 show that the treatment shifted the partisan attitudes of these respondents toward the Democratic Party in Waves 2 and 3, respectively. Columns 3 and 6 show the local average treatment effect of issue-specific knowledge on partisan attitudes in Waves 2 and 3, respectively, for those types of subjects whose knowledge was influenced by the treatment. Increases in issue-specific knowledge cause a significant shift in partisan attitudes toward the Democratic Party. The bold coefficients indicate the specific quantity of interest estimated from each regression. The non-bold coefficients represent the coefficients on control variables which are important for the estimation of the quantities of interest but need not be interpreted directly.

respectively. The experimental treatment strongly increased the issue-specific knowledge and the Democratic support of uninformed respondents. For those who were initially uninformed, their issue-specific knowledge increased by .34 points on a scale from 0 to 1 (about 2 questions out of 6) relative to the uninformed respondents in the control group, and their party evaluations shifted .11 points toward the Democratic Party. These effects represent substantively large movements in both issue-specific knowledge and party evaluations. Columns 4 and 5 show that much of these effects persists into Wave 3. Even several weeks later, the treatment caused issue-specific knowledge to increase by .15 points and Democratic support to increase by .05 points relative to control respondents. Due to lower response rates, the persistent effects are measured with less precision, but the substantive size of the effects is still meaningful.

Columns 3 and 6 of Table 2 represent our 2SLS estimates of the causal effect of knowledge on party evaluations in Waves 2 and 3. The coefficients on issue-specific knowledge indicate that an uninformed respondent would

increase her support for the Democratic Party by .21 and .28 points in the second and third waves, respectively, if she became fully informed on the six issues in this study.[16] When uninformed respondents are given information, they dramatically shift toward the Democratic Party and away from the Republican Party.

A closer analysis of our data reveals the likely explanation of our findings. Low information respondents tend to be economically disadvantaged relative to high information respondents. From our demographic data gathered in the first wave, we know that these low-information respondents have lower incomes, are more likely to have an unemployed person in their family, and are more likely to have a minimum wage earner in the family. Moreover, their beliefs are economically more liberal than high information respondents. They are more likely to believe that poor people pay too much in taxes and rich people pay too little in taxes, and they have lower thresholds for what counts as middle and upper class. The effect of the treatment is strongest for these economically disadvantaged respondents suggesting that our results are largely driven by the fact that uninformed respondents systematically overestimate their degree of agreement with the Republican Party. Once they learn where the parties stand on economic issues such as minimum wage, unemployment, and EITC, they increase their support for the Democratic Party relative to the Republican Party.

As described, our *Party* variable is constructed from respondents' separate evaluations of the Democratic and Republican Parties. We can further dissect our findings by analyzing the effect of our treatment on Democratic and Republican evaluations separately for self-identified members of each political party. Table 3 provides estimates of the treatment effect for uninformed respondents (β_1 from Equation (1)) on their separate evaluations of the Democratic (left column) and Republican (right column) parties. Respondents who identified themselves in Wave 1

[16] There are two potential concerns with our 2SLS estimates, but neither poses a problem for our inferences. First, we might worry that respondents in the treatment group learn the positions of the parties, update their partisan attitudes, and then forget the factual information before we re-interview them in Wave 3 (Lodge et al., 1995). This violation of our exclusion restriction would cause an upward bias in our estimate in column 6. However, the fact that the Wave 2 and Wave 3 estimates are similar suggests that this is not a problem. Second, 2SLS can yield biased results with small samples and multiple weak instruments. However, this is not a problem for three reasons: (1) Our instruments are strong. An F-statistic of 35.4 is well above conventional thresholds for a powerful instrument. (2) The direction of bias would work against us because OLS significantly underestimates the true causal effect of interest. (3) LIML estimates, which are approximately median unbiased (Angrist and Pischke, 2008), are nearly identical to our 2SLS estimates. Additional challenges to our analysis are discussed in the subsequent section, "Alternative Explanations."

Table 3
Variation in the treatment effect by party ID.

Wave 1 party ID	Party evaluated	
	Democratic party	Republican party
Democrat	.505 (.899)	.727 (1.018)
Independent	−.321 (.862)	−2.651 (.765)**
Republican	2.561 (1.353)	−1.003 (1.253)

Robust standard errors in parentheses; * significant at 5%; ** significant at 1%.

Both dependent variables are coded to range from 0 to 10. The coefficients estimate the effect of the experimental treatment on attitudes toward the Democratic and Republican Parties, respectively, for uninformed respondents who identified in Wave 1 as Democrats, Independents, and Republicans, respectively. The six coefficients were estimated from six separate OLS regressions.

as Democrats (top row, $n = 190$), Independents (middle row, $n = 198$), and Republicans (bottom row, $n = 105$) are analyzed separately. This analysis allows us to understand what drives our results. Democratic respondents are unaffected by the treatment. Independents receiving the treatment do not change their evaluations of the Democratic Party but they dramatically decreased their support for the Republican Party, decreasing their Republican scores by 2.7 on the scale from 0 to 10. Even Republicans receiving the treatment increase their support for the Democratic Party by 2.6 points and decrease their support for the Republican Party by 1 point. Uninformed Democrats already support the Democratic Party, so while they learn from the treatment and come to recognize that the Democrats do, in fact, represent their interests, there is little room for them to shift their support toward the Democrats. However, uninformed Independents and Republicans tend to over-estimate their level of agreement with the Republican Party. Therefore, when they become more informed on these six important issues, they decrease their support for the Republican Party. This is consistent with psychological findings that familiarity breeds contempt; in the absence of information, individuals overestimate their affinity and similarity with others (Norton et al., 2007). Lastly, uninformed Republicans underestimate their level of agreement with the Democratic Party and increase their Democratic support upon receiving the treatment.

6. Experiment 2

To replicate our results from Experiment 1, we designed a similar experiment on a module of the 2010 Cooperative Congressional Election Study (CCES). This experiment differed from the previous study in several important ways. First, we could not employ a three-wave design. Rather, the entire experiment took place as part of a single module on the post-election survey. Second, the experimental treatments came to subjects without a cover story. As opposed to embedding information in letters to the editor, we simply provided subjects with factual statements about the parties' positions.[17] Third, the CCES experiment informed

subjects in the treatment group about a different but overlapping set of issues: capital gains tax, minimum wage, abortion, and gay marriage. Lastly, after weighting, the CCES provides a nationally representative sample of U.S. citizens, so results of this experiment are externally applicable to the greater population.

As a result of these differences, we expect this experiment to produce smaller effects than Experiment 1. Because subjects underwent our experiment in the middle of a large political survey, they had already received political information and therefore may have paid less attention to our specific treatment. Also, since the treatments carried no cover story, there was a greater chance that subjects would reject the information or fail to incorporate it into their later survey responses (Ariely and Norton, 2007). Lastly, this experiment involved fewer issues, and capital gains tax is a cognitively difficult issue relative to unemployment and EITC in the previous experiment. For a low-income, uninformed voter, it may be more difficult to immediately understand if and how capital gains tax influences her wellbeing. All of these differences have the potential to decrease any effects of the experiment and hinder our ability to detect the effect of information.

As before, we randomly assigned some respondents to receive political information. After answering a series of political information questions, 441 subjects were informed about the positions of the parties on four key issues: capital gains tax, minimum wage, abortion, and gay marriage, while 1235 received no information. Finally, all subjects were asked to evaluate both parties as in the previous experiment, and each individual's relative party evaluations were again coded from 0 to 1 with higher values representing more support for the Democratic Party relative to the Republican Party.

Fig. 4 presents the primary result of the experiment with kernel regressions, similar to the nonparametric approach taken in Fig. 3. For each respondent we calculated her residual party evaluation by removing variation associated with differing party identifications and ideologies provided before the experiment.[18] Then, we plot the relationship between residual party evaluations and prior political knowledge separately for respondents in the treatment and control groups.[19] As in the previous experiment, informed subjects are uninfluenced by the experimental treatment, but uninformed subjects systematically

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[17] Exact treatment wordings are available in the Supporting Information.

[18] Since we cannot track changes in the *Party Evaluation* variable over time, the residual party evaluation allows us to detect effects of the experiment that are independent of pre-treatment party identification. Residual party evaluations were calculated by regressing the party evaluation variable on dummies for pre-treatment party identification and ideology and subtracting OLS predicted values from each respondent's party evaluation. Our results are not sensitive to this specification. As before, the residualization is not necessary for unbiased estimates but improves statistical precision.

[19] Prior political knowledge is calculated by combining a large battery of information measures in a factor analysis. The variable is rescaled to range from 0 to 1. The results are not sensitive to this particular measure of knowledge. The Supporting Information presents a simpler version of Fig. 4—similar to Fig. 3—that utilizes the number of issue-specific questions answered correctly as the measure of information. The substantive results are unchanged.
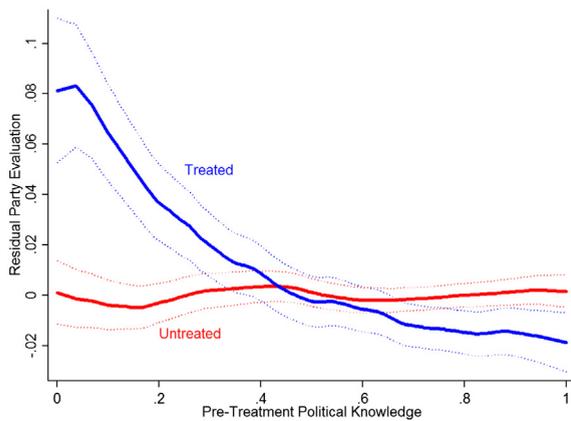
Fig. 4. Effect of experimental treatment in the CCES. The figure presents two kernel regressions of respondents' residual party evaluations in Wave 2 on their political knowledge in Wave 1. These residual party evaluations were calculated from a regression of Wave 2 party evaluations on Wave 1 party identification and ideology. As with the previous experiment, the treatment dramatically shifted uninformed respondents toward the Democratic Party but had no effect on informed respondents. Dotted lines indicate standard errors.

shift toward the Democratic Party and away from the Republican Party as they become informed about the parties' positions. The size of the effect is slightly smaller than in the previous study, but the effect of information for uninformed voters is still substantively and statistically ($p < .01$) meaningful. Parametric estimates of this effect are provided in the Supporting Information.

7. Alternative explanations

We have established and replicated a clear experimental effect in which subjects randomly treated with political information shift their preferences toward the Democratic Party. We draw the conclusion that, in contrast to previous observational work, the current deficit of political knowledge in the electorate actually benefits the Republican Party. Challenges to our conclusions may arise in two general forms. First, there may be something other than information that caused our experimental subjects to shift their attitudes toward the parties. Second, there may be something unique about our experiments which limits the external validity of our results in the real world.

The first challenge to our findings is that our results might arise not from information but from priming. Perhaps our treatment caused respondents to reweight the importance of certain issues in addition to informing them. We address this concern in several ways. First, we see no effect of our treatment for those respondents who were initially fully informed, consistent with our claim that our treatment only influences attitudes by increasing information. Second, all respondents, including those in the control group were asked whether they knew the parties' positions on the treatment issues so, in that sense, all respondents were primed. This priming would actually make it more difficult to find differences between the treatment and control groups. One additional concern may be that

low-information respondents are more susceptible to priming. While a plausible concern, previous research has not found evidence of differential effects of priming based on political awareness. While low-information respondents may be more persuadable in their opinions (Huber and Lapinsky, 2006; Zaller, 1992), they also have a more difficult time linking the primed issue to the evaluation at hand. The net result is that priming seems to occur at the same rate for all levels of political awareness (Iyengar and Kinder, 1987). Additionally, as discussed earlier, subjects were primed to feel positively toward both the Democrats and the Republicans.

Similarly, we might worry that the emotional content rather than the informational content of the treatments contributed to our results. After all, Experiment 1 embedded information in letters to the editor which contained emotional, personal arguments in favor of one policy over another. While a valid concern, we find a similar effect in the CCES study when the treatments were direct and bore no emotional content. The emotional content of the letters alone cannot explain our results.

The second potential challenge to our findings is that there may be something unique about our experiments that limits the generalizability of our findings to the real world. Surely, our experiments do not mimic the typical acquisition of political knowledge. Perhaps information that is naturally acquired will have different effects than information artificially acquired in an experiment. On one hand, we concede this possibility. On the other, there is reason to believe that the artificial nature of the experiment may actually depress the effects of information. Voters may be more resistant to information in a political survey compared to information they acquire naturally. Also, as discussed previously, the fact that these experiments took place during an election cycle during which Republicans made large gains on account of the weak economy stacks the deck against our hypothesis. If anything, our results may underestimate the effects of political information.

We also might worry that our results are driven by the unique nature of the few issues that we were able to test. Our experiments involved minimum wage, EITC, unemployment, capital gains tax, abortion, gay marriage, and gays in the military; but our results may have been different if we chose different issues. We attempted to choose both moral and economic issues of broad interest and importance on which the parties have clearly divergent positions. We cannot say whether different issues would generate different results, but these issues represent a significant portion of debate in American politics and are likely representative of the issues that weigh most heavily on voters' minds during an election. We must also acknowledge that our online convenience sample in Experiment 1 is not nationally representative and our estimates there may not be generally applicable to the greater American electorate. However, our CCES sample is nationally representative, and the similarity of our results across both settings provides assurance that our results cannot be explained entirely by the uniqueness of our samples.

Similarly, we cannot speak to the effects of political knowledge during other time periods. As a result, to the

extent that our results differ from previous correlational studies, we cannot confidently say whether the difference is driven primarily by statistical biases, differing samples, differing time periods, or some combination of these factors. However, we can partially address this question by replicating the correlational analyses with our data. In the 2010 CCES sample, we find a strong correlation between political knowledge and our party evaluation variable in the opposite direction of our experimental estimates. In our sample, just as in previous correlational studies, political knowledge corresponds with more relative support for the Republican Party. However, exogenous increases in issue-specific knowledge create shifts toward the Democratic Party. These findings indicate that correlational analyses can severely misestimate the effects of information, and the differences between our results and previous findings are not simply a result of different time periods. Despite several limitations to external validity which we openly acknowledge, our findings provide the best available evidence on the effects of issue-specific knowledge in the modern American electorate.

8. Discussion and conclusion

The effects of political information on political behavior hold crucial consequences for the quality of democratic representation. Many Americans are uninformed about the positions of the major political parties on key issues, and this lack of information distorts their ability to translate their preferences into partisan preferences and vote choices. Our experimental designs allow us to assess the direction and extent of these distortions in aggregate public opinion and electoral behavior.

Our results stand in contrast to a growing body of literature that uses observational data to estimate electoral outcomes with a fully informed body of voters. While these works are instrumental in opening the door to an important question, weighty assumptions are necessary to estimate the causal effects of information. Information is a variable that is inherently endogenous and is not distributed randomly throughout the population. An experiment whereby the information is provided at random allows us to isolate the effect of information on partisan attitudes.

While our results contradict much previous work, the findings comport with our theoretical predictions. Increases in political knowledge will benefit the party that is better aligned with the population of uninformed voters. The natural constituents of the Democratic Party have consistently been shown to have lower rates of political knowledge relative to others in the population. As such, when the electorate receives more policy-relevant information it systematically shifts toward the Democratic Party.

These results are not just statistically significant; they are substantively significant as well. The causal effect of issue-specific knowledge on partisan attitudes is comparable to the mean difference between Republicans and Independents or between Independents and Democrats. Additionally, we see the largest effect among self-identified Independents who, as we are told year after

year, can affect an election outcome. These changes in feeling towards the parties are also persistent. Weeks after being presented with the information, we still detect a treatment effect. The effects would likely be even greater if respondents were constantly exposed and re-exposed to information, as they would be if they were tuned into an election cycle.

While outside the scope of the present study, the dilemma of unequal political information is inherently intertwined with that of unequal political participation. Both phenomena can significantly distort the way in which public preferences are translated into public policies. Under-represented groups like ethnic minorities and low-income citizens are less likely to turn out to vote and therefore less likely to have their interests represented in government. However, this study shows that participation is only part of the challenge. Even if these citizens were brought to the polls, public policy might still skew away from their preferences because those same groups are also less likely to be informed about the positions of candidates and parties. Further experimental research might explore the extent to which political information and participation causally relate to one another. Would under-represented citizens be more likely to participate in the democratic process if they were more informed? Would under-represented citizens be more informed if exogenous factors brought them to the polls? We hope that future researchers will tackle these important questions.

We live in a society with significant informational and representational inequalities. Not everyone is politically informed, and certain types of people are systematically more informed than others. The result of these knowledge disparities within the electorate is that informed voters have an easier time translating their political preferences into a vote that best represents their interests. Uninformed voters, by contrast, may make voting mistakes by not fully understanding the electoral choices on offer or how these choices relate to their interests and preferences. The aim of this paper is to demonstrate the aggregate and electoral consequences of these information imbalances. Increased political information would lead to greater representation for these unheard voices and election results that more accurately reflect the preferences of the public.

Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.electstud.2013.09.009.

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