SHORT ARTICLE

Cross-Pressure and Voting Behavior: Evidence from Randomized Experiments

Kyle Endres, Duke University
Costas Panagopoulos, Northeastern University

Cross-pressured partisans are commonly viewed as persuadable, and campaigns routinely target these voters in elections. Yet evidence of the causal impact of policy cross-pressures on voting behavior is limited. We deployed randomized experiments to examine whether (and how) nonpartisan information that highlighted policy cross-pressures affected voting in the 2015 Kentucky gubernatorial election. Our results suggest partisans conflicted with their party's gubernatorial nominee on the issue of Kynect, Kentucky's health-care exchange, who were exposed to information about the candidates' positions were more likely to report defection intentions in a preelection survey, but these did not necessarily materialize on Election Day. Information exposure seems to have produced few discernible effects on voting overall, based on self-reports in postelection surveys we conducted, but an examination of validated voting records suggests cross-pressured partisans were generally more likely to abstain when provided with the policy positions of both gubernatorial candidates.

Even in the current, polarized political climate, the fact that many partisans disagree with their affiliated parties on one or more policy issues persists. Campaigns opportunistically exploit these tensions, routinely targeting these cross-pressured voters using so-called "wedge" issues that highlight political cleavages (Hillygus and Shields 2008). Prior studies have observed cross-pressured partisans defect at higher rates (Hillygus and Shields 2008; RePass 1971), but much less is understood about the role of campaign information in the decision-making process. For example, does information documenting cross-pressures and issue agreements with the out-party's candidate cause partisans to defect or to abstain? Previous research has speculated about answers to these questions but has not evaluated them directly in the context of actual elections.

In this study, we assess the impact of exposing cross-pressured partisans to the positions of both major-party candidates on a conflicted issue in the 2015 Kentucky gubernatorial election. We conducted two original experiments in which we randomly assigned subjects to receive information about each candidate's position on the future of the state's health insurance exchange, known as Kynect, either in a survey setting or by mail. The experiments allow us to determine whether partisans who are cross-pressured on a key policy issue abstain from voting or vote for the other party's candidate at higher rates when primed with information that allows them to confirm they are cross-pressured but aligned with the out-party candidate on the conflicted issue, compared to cross-pressured partisans who are not provided this information.

BACKGROUND AND EXPECTATIONS

Partisan allegiances exert outsized influence on voters in elections (Campbell et al. 1960). All else being equal, partisans generally vote for their preferred party's candidates. In fact,

Kyle Endres (kyle.endres@duke.edu) is a postdoctoral research associate at the Duke Initiative on Survey Methodology at Duke University, Durham, NC 27708. Costas Panagopoulos (c.panagopoulos@northeastern.edu) is professor of political science and director of big data and quantitative methods initiatives at Northeastern University, Boston, MA 02115.

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American voters are reliably supporting the same party election after election more than any period since the 1950s (Smidt 2017). Some partisans, however, struggle to decide between their partisan and other predispositions, such as policy positions (Hillygus and Shields 2008; Lavine et al. 2012), when they pull in different directions. These partisans, who can represent at least one in four voters (Hillygus and Shields 2008), are often referred to as ambivalent, cross-pressured, or persuadable due to their common ground with the other party.[1]

Most research on cross-pressured partisans is observational. Studies along these lines often compare how campaigns affect voting decisions for congruent and issue-cross-pressured partisans, but it is unclear which (if any) voters in these studies encountered policy messages on their conflicted issue(s). One way to overcome this challenge is to rely on experimental approaches. To date, such experiments are exceedingly rare, and the single one of which we are aware involved hypothetical candidates (Hillygus and Shields 2008). We extend this line of research by conducting a large-scale survey to identify partisans who were cross-pressured on a specific issue (Kynect) and provide a random subset of them with each candidate's position on the conflicted issue prior to an actual election.

Existing studies have focused exclusively on vote choice (e.g., Hillygus and Shields 2008) or explored reported turnout differences separately with null findings (e.g., Lavine et al. 2012). But defection is not the only option for voters. When individuals' preferences overlap at least to some extent with both candidates, abstention may be sensible. Formal models have shown that abstention is an optimal decision when voters consider each candidate to be the best choice in some situations (Ghirardato and Katz 2006). Similarly, when voters share issue positions with each candidate, they may perceive the costs of voting to exceed the expected benefits accrued from their party's candidate winning instead of the opposing candidate (Downs 1957). It is conceivable these partisans may prefer to abstain rather than to defect.

We designed and implemented a randomized experiment to test empirically several hypotheses implied by the theoretical discussion above. We hypothesize that exposing voters to information highlighting a clash between their issue positions and those held by their party's candidate will increase the likelihood of both abstention (failure to vote) and defection (voting for the other party's candidate), relative to voting for their party's candidate. It is less clear whether cross-pressured voters would be more likely to abstain rather than defect, but arguments advanced in Ghirardato and Katz (2006) imply that dissonance may be minimized by abstention.

Experimental setting and design

Our study is designed to assess the effects of providing partisans with information about each candidate's position on a cross-pressured issue in the 2015 Kentucky gubernatorial election. The election was an open-seat contest between Jack Conway (D) and Matt Bevin (R). A leading issue was the future of Kynect, the state's health insurance exchange. Bevin vowed to eliminate it while Conway would continue it (WKYT 2015). This clear difference between the candidates on Kynect provided an opportunity for us to evaluate the effects of information connecting the positions of each candidate for partisans who previously indicated their personal position aligned with the out-party's candidate.

Sample

Our population of interest was registered Kentucky voters who identified as either Democrats or Republicans and reported a position on Kynect that aligned with the position of the other party's candidate. To identify a sample of individuals who met these criteria, we surveyed registered voters approximately 10 days prior to the election using interactive voice response (IVR; automated surveys in which participants answered prerecorded questions using their phone's keypad. The first three survey items enabled us to identify a total of 3,174 voters who were cross-pressured on Kynect (from the 23,848 who participated in the survey).[2] These subjects were assigned, using simple randomization, to one of two, experimental strata: a survey experiment and a parallel mail experiment. Subjects were then randomly assigned to treatment or control conditions within each stratum. The treatment groups were informed about both candidates' positions on Kynect, via either a survey prime or a postcard mailing (see below and app. B; apps. A–C are available online), while the control groups were not exposed to this information.

Treatments

The delivery mode was randomly varied, but the salient content of the treatments was designed to be nearly identical. All

1. Hillygus and Shields (2008) find 67% of partisans disagreed with their party on at least one issue they considered personally important in 2004, and we found that 22% of Kentucky partisans overall in our pre-election survey disagreed with their party's gubernatorial nominee on Kynect.

2. Note that 3,372 individuals were identified as cross-pressured, but 198 of them exited the survey while we were collecting additional pretreatment covariates; an additional 142 individuals (in the survey experiment) quit the survey before reporting their vote intention. All subjects for whom we have outcome measures are included in the corresponding analyses that follow.

subjects were encouraged to vote and informed that Kynect was an important issue. The treatment groups were also provided with nonpartisan information on each candidate's position. Specifically, they heard (or were mailed):

> November 3, 2015 is Election Day in Kentucky! Please vote! We will elect a new governor in this election. There are many important issues in this election, including the issue of Kynect, the Kentucky health care exchange. *(As you may know, the Democratic and Republican nominees for governor in the November 2015 election have different positions on the Kynect health insurance issue. Jack Conway, the Democratic candidate, supports Kynect, while Matt Bevin, the Republican candidate, wants to eliminate it.)*

The control groups received the same message minus the italicized sentences that referenced the divergent positions of the two candidates. Subjects in the survey experiment were probed about their vote intentions immediately after this item. In addition to vote intentions, we obtained official records of voter turnout and conducted a follow-up survey to collect postelection outcome measures. All subjects who completed the preelection survey were called to participate in the postelection survey;[3] 889 subjects in the survey experiment (55%) and 904 subjects in the mail experiment (59%) participated in the second-wave survey.

Results

We examine the effects of our interventions using several, key outcome measures: self-reported preelection vote intentions, postelection voting behavior reported in surveys we conducted, and validated voting obtained from official records. We test separately for treatment effects among four samples: the full survey sample, the full mail sample, and subsamples from each experimental stratum that provided demographic information (age and gender) that matched their records in the voter file.[4] We expect the treatment effects for validated turnout to be concentrated in the matched sample since inconsistent demographic information suggests someone other than the targeted voter participated in the survey.

Average treatment effects for each of the four samples are summarized and displayed visually in figure 1 (see app. A for corresponding regression tables).[5] We estimate parallel models both with and without the inclusion of available, pretreatment covariates and focus on the covariate-adjusted models (plotted in black) that account for imbalances due to chance.[6] We use multinomial logistic regression models to estimate the effects of information on vote intention and vote choice and logistic regression for validated turnout from the official voter records. Vote choice and vote intention are coded as "0" for subjects who voted (or intended to vote) for their party's candidate, "1" for the out-party's candidate, and "2" for abstaining. Validated turnout is coded as "0" for subjects who abstained and "1" for voters.

We begin by examining preelection vote intentions. The results, displayed in figure 1, suggest assignment to the treatment significantly increased intent to defect but did not raise abstention intentions. Assignment to the treatment group increased defection intentions by 7.78 percentage points ($p = .002$, two-tailed) for the full survey sample and by 9.15 points for the matched survey sample ($p = .006$, two-tailed), on average, while the treatment exerted virtually no effect on intent to abstain. Notwithstanding these statistically significant and substantively sizable differences in defection intentions, we are mindful that persuasive effects can decay over time (Gerber et al. 2011) and that social desirability tends to inflate turnout reports in surveys (McDonald 2003), so we proceed to estimate the effects of the issue information on vote choice reported in the postelection survey we conducted and on turnout from official voter records.

The postelection survey probed subjects about their voting behavior in the election, which allows us to assess whether the impact of the treatment persisted to Election Day for the survey experiment stratum and to evaluate the mail experiment.[7] For the survey experiment, the treatment effect is generally positive for defection and negative for self-reported abstention, though neither is statistically significant at traditional levels. Figure 1 shows that assignment to the treat-

3. Regressing wave 2 participation on an indicator variable for assignment to the treatment verifies that there was not a significant difference in participation between the treatment and control groups (survey: Coef. = .01, SE = .10, $p = .91$; mail: Coef. = −.08, SE = .10, $p = .43$).

4. Restricting the analyses to subsets of subjects whose self-reported age and gender match the voter file was not included in the preanalysis plan.

5. We also explore the possibility of partisan heterogeneity in app. C.

6. An F-test of the significance of all of the available covariates on treatment assignment is insignificant in the survey experiment stratum ($F(13, 1615) = 1.41$; $p = .15$) as well as in the mail experiment stratum ($F(13, 1531) = 0.80$; $p = .66$), implying random assignment produced balanced experimental groups overall in both strata. Appendix A includes detailed balance tables.

7. The postelection survey asked participants to identify each candidate's position on Kynect. The treatment had a significant effect on correctly identifying the positions of both candidates (survey: Coef. = .29, SE = .16, $p = .08$, two-tailed; mail: Coef. = .32, SE = .16, $p = .05$, two-tailed). The treatment also had a significant effect on identifying the position of their party's candidate for the survey (Coef. = .30, SE = .16, $p = .06$, two-tailed) but had a statistically insignificant effect for the mail experiment (Coef. = .18, SE = .15, $p = .24$, two-tailed). See table A15 (available online) for more details.

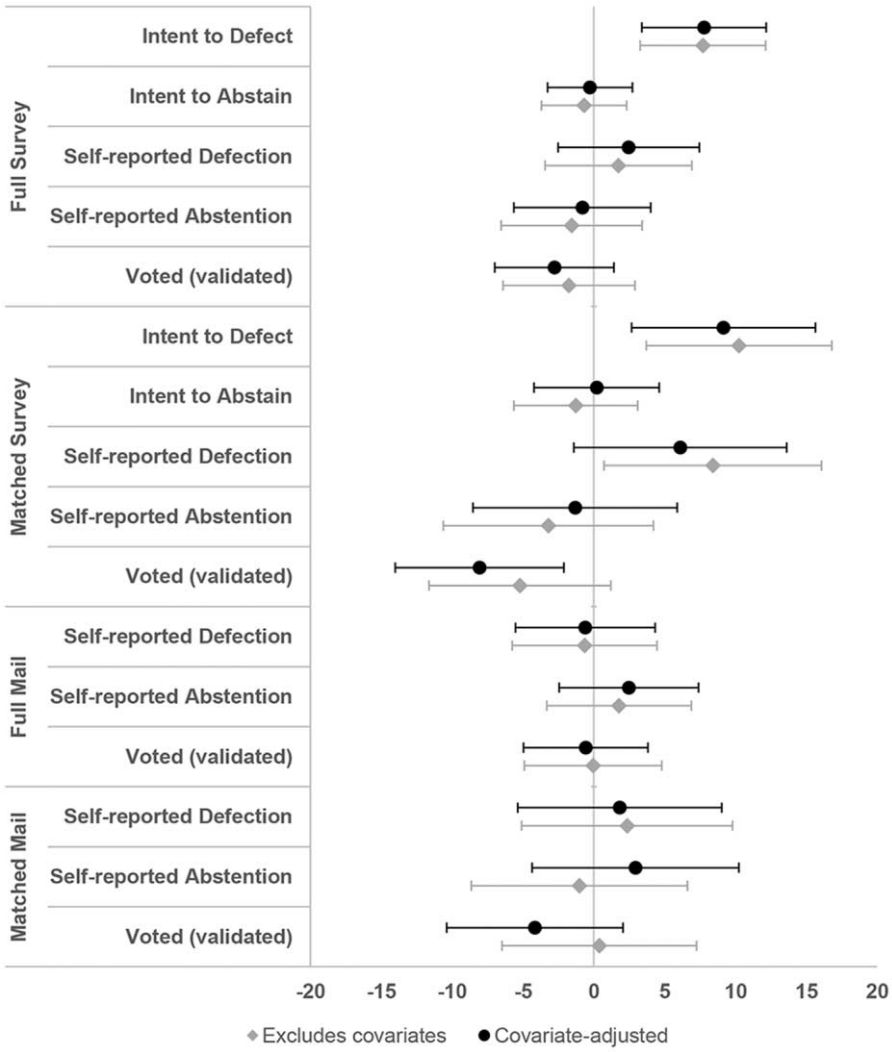

Figure 1. Average treatment effects of information on cross-pressured partisans' voting behavior. Percentage point difference between the treatment and control groups for each sample and outcome variable, with 95% confidence intervals. The average treatment effects for intent to defect and intent to abstain were estimated using a multinomial logistic regression model based on reported vote intention in the preelection survey. The treatment effects for self-reported defection and self-reported abstention were estimated using a multinomial logistic regression model based on reported vote in the postelection survey. The treatment effect for voted (validated) was estimated using a logistic regression model with official turnout status from the Kentucky voter file.

ment group increased reported defection by 2.46 percentage points ($p = .33$, two-tailed) for the full survey sample and by 6.09 percentage points ($p = .11$, two-tailed) for the matched survey sample, on average, increases that are notably smaller than the treatment effects observed immediately following the delivery of the information documenting the candidates' policy positions. While generally consistent with the direction of the defection effects in the preelection survey, we interpret this finding to suggest the potency of the information likely diminished between the survey and the election.[8] The mailed issue information did not seem to influence either defection or self-reported turnout. The estimated average effect of the treatment was a 0.61 percentage point decrease in reported defection ($p = .81$, two-tailed) and a 2.47 percentage point increase in self-reported abstention ($p = .33$, two-tailed) for the full mail sample, while the corresponding estimated average effects in the matched mail sample were increases of 1.83 percentage points for defection ($p = .62$, two-tailed) and 2.94 percentage points for abstention ($p = .43$, two-tailed), as shown in figure 1.

As an alternative outcome measure, we also analyze validated turnout obtained from official Kentucky voter records.

8. We note the vote intention results hold when we restrict the analysis to participants who completed both surveys, with a 13-point increase in intent to defect ($p < .001$, two-tailed), on average, for the treatment group based on estimates from the covariate-adjusted model.

Based on the full survey sample and both of the matched samples, the treatment groups appear to have abstained from voting at higher rates than the control groups, as shown in figure 1. Using the covariate-adjusted models, we estimate that assignment to the treatment decreased turnout by 2.78 percentage points ($p = .098$, one-tailed) for the full survey sample, 8.06 percentage points ($p = .004$, one-tailed) for the matched survey sample, and 4.16 percentage points ($p = .096$, one-tailed) for the matched mail sample, on average. We suspect that social desirability bias, which, as we note above, is commonly associated with turnout overreporting in surveys (McDonald 2003), may account for the discrepancy between the treatment effects we observe on actual, validated turnout (negative), compared to self-reported abstention intentions (null), so we are inclined to view the findings based on validated turnout as more reliable, noting that the evidence on this score is mixed.

DISCUSSION

This study represents one of the first, of which we are aware, that accounts for the specific issue cross-pressures of individual voters and the information they encounter. It is also one of the few studies to directly compare information treatments delivered in the context of a survey to those delivered in the "field." By conducting our experiments in an actual election and validating turnout using official Kentucky voting records, we offer more conclusive evidence that policy information can influence both candidate preferences and turnout for cross-pressured partisans. We concede the results we describe above are somewhat mixed, but we conclude that the preponderance of the evidence suggests, although not unequivocally, that highlighting policy incongruence likely affects voting behavior among cross-pressured partisans in elections. These voters appear to abstain at higher rates and to waver in their support for their parties' candidates when they are exposed to information that highlights issue disagreements. At the very least, this policy information seems to cause these voters to plan to defect to the out-party at higher rates in the immediate aftermath of exposure, even if they do not ultimately follow through on these intentions on Election Day. Defection intentions among such voters are heightened in the short run, but some evidence suggests these effects can decay rapidly, vanishing almost entirely within a few days. This finding implies persuasion may be possible when campaigns correctly identify and contact issue-cross-pressured partisans, but timing may be critical, as the effects seem to evaporate quickly. By contrast, emphasizing policy incongruence rarely seems to cause cross-pressured partisans to indicate they plan to abstain from voting, but at least some of the evidence we present above suggests they actually do so.

Notwithstanding these intriguing possibilities, our results should be situated within the proper context and, if possible, investigated in other elections with other policy issues. Health care and the health insurance exchanges were contentious issues often linked to racial, partisan, and economic considerations throughout the Obama presidency. Providing cross-pressured partisans with policy information on a less salient issue may produce effects of a different magnitude. Our findings also highlight the muted influence of policy messages delivered in real-world settings (e.g., via mail) in which campaigns must compete for recipients' attention. Even though they often pointed in the same direction, the results we report in the survey experiment were generally stronger than in the mail experiment. Attenuated findings are not uncommon in direct comparisons of experiments conducted in the field versus the laboratory (Jerit, Barabas, and Clifford 2013), but they do raise questions about the potency and external validity of experimental manipulations conducted in laboratories (or, in our case, surveys).

Only additional research can address these possibilities, but subsequent information interventions along these lines will need to grapple with potent and complex ethical considerations, especially in light of the evidence we uncovered. Researchers increasingly exploit opportunities to investigate information effects in real-world settings (Zimmerman 2016), but our results underscore that information is not necessarily innocuous. It can, as we find, demobilize voters or affect vote tallies (Johnson 2018), raising concerns about effects on study participants as well as on broader communities (Zimmerman 2016). These concerns are serious and legitimate. Even in studies like ours that do not involve deception, the benefits of raising voter awareness and facilitating informed decision making by providing factual, nonpartisan information about candidates' issue positions must be weighed against the possibility of adverse effects, including voter withdrawal or disengagement. We are also mindful that unscrupulous actors could be motivated to deploy these tactics for nefarious ends. Such calculations are not always clear-cut. Some may view rational abstention to be preferable to voting, perhaps unintentionally for the "wrong" candidate, on the basis of incomplete (or inaccurate) information, but these perspectives remain unsettled. We value shedding light on weighty theoretical questions about voter decision making and behavior, but researchers going forward must contend with the potentially demobilizing effects of such treatments.

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