Do Surveys Other than ANES Mobilize the Electorate?[*]

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Abstract

Surveys are important instruments for understanding political behavior, but they may also affect the behavior of the respondents who we are trying to study. Warren Miller once said that the American National Election Studies (ANES) is "the most expensive voter mobilization project in American history." We extend the current research on mobilization effects of pre-election surveys by examining the effects of two phone surveys and one mixed-mode (phone and internet) survey on the likelihood of voting in the 2018 midterm election in Florida. Using a series of differences-in-differences, multivariate matching with differences-in-differences, and lagged dependent variable models we find no mobilization effects on simply contacting respondents by either phone or web, the turnout rate of those respondents who completed a web survey a year earlier increased by 8.6 percentage points, while phone respondents who completed a survey one month before the election day had a turnout increase of 14.6 percentage points. These results show that engaging with the survey has a higher and long lasting effect compared to contacting alone.

Key words: pre-election survey, turnout, mobilization, self-selection

Movements of the heavenly bodies are not affected in any discernible way by the fact that there are people on earth recording the apparent movement. Similarly, it is almost inconceivable that the planets would alter their orbits because of Kepler's discovery and publication of the laws of planetary motion. The social and behavioral sciences are different in that the objects under investigation may behave differently as a result of the research. – Granberg and Holmberg (1991, 240)

Much of what we know about electoral behavior comes from decades of analysis of survey data. In contrast to laboratory experiments, where randomized treatments and controlled conditions help to establish strong internal validity, the natural strength of surveys lies in the ability to glean information from a representative sample of people about their actual opinions and behaviors. Yet, the external validity of surveys is seemingly undermined by the recurrent finding that self-reported rates of voter turnout usually exceed the actual proportion of votes casts relative to the voter eligible population, sometimes by a lot. While much of the difference between survey reports and official turnout rates is attributable to over-reporting by non-voters (Clausen 1968; Traugott and Katosh 1979; Silver, Anderson and Abramson 1986; Granberg and Holmberg 1991; Bernstein, Chadha and Montjoy 2001; Karp and Brockington 2005; Ansolabehere and Hersh 2012; Holbrook and Krosnick 2009; Brockington and Karp 2002; Burden 2000; Martinez 2003; McDonald 2003), other research has shown that the American National Election Study's pre-election wave can have a modest mobilizing effect on survey respondents (Clausen 1968; Bartels 1999; Jackman and Spahn 2019). As Warren Miller once remarked, "ANES is the most expensive voter mobilization effort in the American history."

It might not be surprising that surveys with significant political content would mobilize voters. After all, a survey is an "ongoing conversation in which respondents conduct their own share of thinking and question answering " (Sudman et al. 1996, 55) about political issues and candidates, and thus may prime respondents to think and, more importantly, act like voters through a process of activation (Clausen 1968), identification (Bryan et al. 2011), or self-prophecy (Greenwald et al. 1988; Spangenberg and Greenwald 1999, 2001). Panel surveys with both pre-election and post-election waves, such as the ANES, may also be vulnerable to the Hawthorne effect, in which people adapt their behavior knowing that they are research participants and are likely to be asked again about their participation (Parsons 1974). Despite these expectations, there is mixed evidence on whether surveys other than the ANES

generally stimulate voter participation, and no research that we could find on whether those stimulus effects extend to internet surveys, which are becoming more common. Differing from previous research we put emphasis on the threat that respondent self-selection might have on the validity of our analysis. Therefore, we estimate a series of differences-in differences, multivariate matching with differences-in-differences, and lagged dependent variable models. We find no mobilization effects of simply contacting respondents by either phone or web, however, respondents who completed a web survey a year earlier and phone respondents who completed a survey one to two months earlier were more likely to have voted in the 2018 general election than those who were contacted but did not complete the survey.

1   Literature Review

Given the importance that the American National Election Studies (ANES) have had on our foundational knowledge about electoral behavior, much of the previous research on the mobilization effects of surveys have understandably been focused on these effects in the ANES. Clausen (1968, 596) was among the first to highlight the discrepancy between the "official" turnout rate and the proportion of ANES respondents who reported voting in the 1964 presidential election, which he attributed to several sources, including the sampling frame, response error (overreporting), and the potential that pre-election "interview grilling may have sharpened (a respondent's) sense of political self-interest and awakened his citizen conscience sufficiently to have motivated him to go to the polls." Clausen (1968) observed that the reported turnout in the 1964 ANES was 6 points higher than in the Census, and 6.5 points higher than in a contemporaneous SRC economic study, both of which lacked a pre-election wave that might have stimulated voting. Many years later, Bartels (1999) would observe a similar 6.5 point difference in reported turnout rates between 1996 ANES panel respondents and a fresh cross-section sample drawn for the same study, though he argued that the difference might be the result of a combination of panel attrition (differential panel mortality) and panel conditioning (stimulus) effects. In their analysis of the 2012 face-to-face component of the ANES, Jackman and Spahn (2019) estimate the mobilization effect to be 4.5 points, based on the difference between the validated voter turnout rates of ANES interviewees and of non-interviewed eligible adults in the same household as ANES interviewees. To the extent that a long ANES in-home interview may have directly or indirectly stimulated participation by others in the household, that estimate would be biased downward. Similar effects are evident

in the Swedish National Election Studies, in which Granberg and Holmberg (1991) and Persson (2014) detected an average "treatment" effect of the pre-election survey of 2 points (which was considerable in the context of the usual high turnout in Sweden), though Persson (2014) notes that the effect is stronger among those with the lowest propensity to vote in the first place. On the whole, this evidence suggests that while overreporting accounts for most of the difference between survey reports and "official" turnout rates, mobilization by pre-election surveys also accounts for some of that difference.

Mobilization effects have also been observed in surveys other than the omnibus national election studies in the United States and elsewhere, including Chicago municipal elections in 1973 (Yalch 1976) and in Congressional primary elections in New Haven in 1970, though the authors of the latter study (Kraut and McConahay 1973) suggest that reduction in alienation or changes in the respondents' self-concepts as alternatives to Clausen's simple stimulus hypothesis as the mechanism by which surveys could mobilize respondents. Experiments have also suggested that the content of the surveys might matter, as surveys which ask respondents whether they intend to vote may generate fulfillment of a self-prophecy (Greenwald et al. 1987; Spangenberg and Greenwald 1999, 2001; Smith, Gerber and Orlich 2003), paralleling the findings in the Get Out The Vote (GOTV) literature that affirming that one would vote, even to a stranger, creates a level of a social contract that some people feel obligated to fulfill. Other interviews that refer to the likelihood of the respondent "to be a voter" (noun) as opposed to referring to the likelihood of the respondent "to vote" (verb) also have greater mobilization effects, seemingly through changing the self-concept. Nickerson and Rogers (2010) also find that respondents who are asked and who have viable plans to vote are more likely to follow through by actually casting a vote.

However, null findings are not unknown in this literature. Smith, Gerber and Orlich (2003) were unable to replicate the findings by Greenwald and his co-authors in support of the self-prophecy hypothesis, and Mann (2005) did not detect any mobilization intent-to-treat effect in sizeable but short telephone surveys conducted in New York, Pennsylvania, and Virginia by the Washington Post and Quinnipiac in the two weeks prior to the 2002 general election.

The three main questions that we address in this study are

1. whether participating in a pre-election survey increases someone's likelihood of voting in the upcoming election;

2. whether participating in a web survey has a different mobilization effect than telephone

3

surveys.; and

3. whether completing and engaging with the survey has a higher mobilization effect than just being contacted and not completing the survey.

No studies that we have found thus far (nor that Mann (2005) noted in his Table 1) have neither investigated the effects of internet surveys on voter participation nor controlled for respondent's self-selection effects. Based on the generally null to weak effects of email contact on voter turnout in the GOTV literature (Nickerson et al. 2007; Stollwerk 2006), we might not expect that internet surveys would have a significant mobilization effect, although the interactivity in completing the survey may distinguish it in that regard from simply receiving any of several email messages exhorting the recipient to vote. Moreover, while there is some evidence that mobilization by face-to-face surveys might be durable (Persson 2014), the durability of mobilization effects of phone surveys is mixed. In the following section we explain the data and the identification strategy used in this study.

2   Data and Methods

We use data from three original surveys of Florida registered voters. The first was conducted in October 2017, and was a dual mode survey of 5,794 internet and 823 phone respondents, focused primarily on the anticipated 2018 US Senate race between then-Governor Rick Scott and incumbent US Senator Bill Nelson. The second and third surveys conducted in September and October 2018, were both telephone surveys (with respectively 646 and 1,051 respondents), based on a sampling frame of registered voters with phone numbers in the September 2018 voter file.

We obtained the January 2019 voter file, and matching on the voter ID number, we determined whether sampled registrants in each of the three surveys voted in the November 2018 general election, in which Floridians decided on races for US Senator, US Representatives, Governor and Lt. Governor, four other statewide Cabinet positions, state legislative seats, and referenda on twelve amendments to the state constitution. In addition, we were able to determine voter history in six previous elections (2014, 2016, and 2018 both primary and general elections), as well as age, sex, race, and party registration.

The 2018 US Senate election in Florida was anticipated to be one of the key races that would determine the balance of power in the US Senate. Bill Nelson, the three term Democratic incumbent, ran against the state's two-term Republican governor, Rick Scott. To assess

the horse-race and public opinion in the state on salient issues such as immigration, health insurance, tax reform, and Confederate monuments, we conducted three original surveys of Florida registered voters. From Monday, October 11, 2017, through Tuesday, October 17, 2017, we conducted a dual mode survey (phone and web) randomly assigning registrants into a mode. Our respondents were sampled from the voter file to avoid any post-survey matching (Berent, Krosnick and Lupia 2016). To do so, we obtained the latest copy of the Florida voter file (September 2017) from the Florida Department of State's Division of Election. The dataset contained 12,857,207 active Florida voters (inactive voters were removed from the sample), of those 367,492 cases had a phone number and an email address. An identical questionnaire was administered in both phone and web samples. The questionnaire contained questions about intention to vote in the 2018 elections, vote choice for the upcoming Florida horse-race 2018 US Senate election, and a series of questions on policy, including health insurance, legal immigration, Confederate monuments, and taxes. Surveys were conducted in both English and Spanish, and the average phone survey length was about 12 minutes long.

In September (17th-19th) and October (23rd-26th) of 2018, we conducted two phone surveys of Florida registered voters. Interviews were conducted in English and Spanish. The phone numbers used for this survey were sourced from the voter file database provided by the Florida Division of Elections' August 8, 2018 update. The sample frame was comprised of potentially likely voters who reside in Florida. Potentially likely voters were determined by vote history and having voted in the any of the following elections: 2014 primary election, 2014 general election, 2016 primary election, or any two of these elections – the 2016 presidential preference primary, the 2016 general election or the 2012 general election. We administered identical questionnaires within years to control for any questionnaire design effects. The mean interview time for both 2018 phone surveys was about 3.18 minutes. Respondents were asked a series of questions on vote intentions, vote choice, and a few policy questions.

For each survey, we created four dummy variables identifying how a registrant was involved in the pre-election survey. Specifically,

- **Web complete** = 1 if the participant completed the survey on the web, 0 otherwise;

- **Phone complete** = 1 if the participant completed the survey on the phone, 0 otherwise;

- **Web attempted/contacted, not complete** = 1 if we attempted to contact the respondent by email, but were unable to complete the survey, 0 otherwise; and

- **Phone attempted/contacted, not complete** = 1 if we attempted to contact the

respondent by phone, but were unable to complete the survey, 0 otherwise.

- **Not contacted** (with scores of 0 on each of these dummy variables) consists of registrants who were sampled, but never contacted to participate in the survey.

First, we estimate a series of differences-in-differences (DD) models, controlling for the way registrants were involved in the survey (contacted, completes), demographics, party registration, vote history (varying from 0 [registrant never voted] to 6 [registrant voted in all the six previous elections]), and county fixed-effects. Our DD framework consists of two periods; before being contacted or completing the survey ($t = 2016$) and after being contacted or completing the survey ($t = 2018$). Before the treatment, neither the treated nor the control group were subject to the treatment; after the treatment, the treated group received the treatment and the control group was not subject to the treatment. The respondents in the control group are not introduced to the treatment neither in year 2018 nor in year 2016 and the respondents in the treated group are only introduced to the treatment in 2018. We create a unbalanced panel dataset using registrant's information for the 2016 and 2018 elections. The dependent variable is coded as 1 if the registrant voted in the 2018 elections and 0 otherwise. We use a year dummy coded as 1 for 2018 and 0 for 2016. We also introduce a set of different treatment and control groups depending on the estimated model. The main identification assumption of the differences-in-differences approach is the common trend assumption. The common trend assumption implies that voting trends between the respondents in the treated group and those in the control group will be the same in the absence of the treatment, i.e., $E(Y^0_{i2018} - Y^0_{i2016}|D_{i2018} = 1) = E(Y^0_{i2018} - Y^0_{i2016}|D_{i2018} = 0)$, where $D_{i2018} = 1$ denotes the set of respondents in the treatment group and $D_{i2018} = 0$ denotes the set of respondents in the control group. The pair of potential outcomes $(Y^0_{i2018}, Y^0_{i2016})$ are the respondents potential outcomes in the absence of treatment in both years.

The DD approach is a powerful technique that addresses the self-selection problem. It removes any unobserved factors both at the treatment and time level. We specify parametrically the two DD models as follows

$$Y_{it} = \alpha_0 + \alpha_1 \text{Contact}_i + \alpha_2 \text{After}_t + \boldsymbol{\tau}(\textbf{Contact}_i \times \textbf{After}_t) + x'_{it}\delta + \zeta_i + \epsilon_{it} \qquad (1)$$

$$Y_{it} = \alpha_0 + \alpha_1 \text{Complete}_i + \alpha_2 \text{After}_t + \boldsymbol{\tau}(\textbf{Complete}_i \times \textbf{After}_t) + x'_{it}\delta + \zeta_i + \epsilon_{it} \qquad (2)$$

where Contact_i is a dummy, taking value of 1 if the respondent was contacted; After_t is a dummy, taking value of 1 if year is 2018; τ is our estimate of interest and the DD estimator

(ATT); x_{it} is a vector of individual-level characteristics; ζ_i are county fixed effects. In equation (2) Complete$_i$ is a dummy, taking value of 1 if the respondent completed the survey.

The common trend assumption is a strong assumption, which is usually violated and leads to biased point estimate of the average treatment effect on the treated (ATT). There are many possible ways where respondents who were contacted (completed the survey) are different from the ones who were not contacted (not completed the survey), and the existence of these differences can drive the changes in turnout and bias the estimates (ATT) in equations (1) and (2). To test for the common trend assumption, we use data from the 2014 and 2016 elections and find evidence against the assumption. Therefore, to address the violation of the common trend assumption, we use two different approaches; (1) matching on previous voting outcomes with differences-in-differences, and (2) lagged dependent variable models (for more see ONeill et al. (2016)). By matching respondents on pre-treatment turnout history, we force the pre-treatment trends to be similar, which makes the common trend assumption more plausible. Also, matching improves the balance of the unobserved confounders with time varying effects and applying DD to the matched data can address potential problems that arise due to time-varying observed and time-invariant unobserved confounders. Hence matching and DD combine the strengths of the two approaches (Heckman, LaLonde and Smith 1999).

The lagged dependent variable (LDV) model is another design we use to estimate the average treatment effect on the treated when the common trend assumption is violated. We specify the LDV model parametrically as follows

$$Y_{it} = \gamma_0 + \gamma_1 \text{Contacted}_i + \gamma_2 \text{Didn't vote}_{it-h} + \boldsymbol{\tau}\big(\textbf{Contacted}_i \times \textbf{Didn't vote}_{t-h}\big) + x'_{it}\delta + \zeta_i + \epsilon_{it}$$
(3)

$$Y_{it} = \gamma_0 + \gamma_1 \text{Completed}_i + \gamma_2 \text{Didn't vote}_{it-h} + \boldsymbol{\tau}\big(\textbf{Completed}_i \times \textbf{Didn't vote}_{t-h}\big) + x'_{it}\delta + \zeta_i + \epsilon_{it}$$
(4)

where $h = 2, 4$. Controlling for past voter history, *Didn't vote*$_{t-h}$, approximates for the unobserved confounders. Following Angrist and Pischke (2009), we assume that the non-treated potential outcome, Y^0_{it}, is independent of the treatment assignment, $D_{it} = \text{Completed}_i \times$ Didn't vote$_{t-h}$, once we control for individual-level characteristics, x_{it}, and past voter outcome, Y_{it-h}, i.e., $E(Y^0_{it}|x_{it}, Y_{it-h}, D_{it}) = E(Y^0_{it}|x_{it}, Y_{it-h})$. Voter's past outcomes are affected by both observed and unobserved confounders. Voters with similar past outcomes are more likely to be alike in terms of their unobservables (Abadie, Diamond and Hainmueller 2010)

7

and including past voter history in the estimation controls for the unobserved confounders.

3 Analysis and Results

In Table 1, we show turnout rate by treatment group for the three surveys. In the analysis we treat the mixed-mode survey as two separate samples to isolate any survey mode effects. The preliminary descriptive statistics shown in Table 1 suggest that our surveys did have positive effect on turnout as those registrants who participated in a survey were more likely to turnout to vote. In the 2017 mixed-mode survey, those who completed the survey in the web had a 10 percent higher turnout rate than those who completed the survey on the phone, who in turn, had about a 5 percent higher turnout rate than the control group who were never contacted. Attempted contact without survey completion had no discernible effect on turnout.

Similarly, the turnout rates of those who completed the 2018 phone surveys were also higher than the control group (7 points for the September survey, and 9 points for the October survey), and attempted contact without completion also had no discernible mobilization effect.

Table 1: Turnout Rate by Survey and Contact Group

	Turnout 2018	Sample size
	%	n
September 2017 Web Survey		
Control (sampled, not attempted)	0.72	170,673
Attempted by web, no complete	0.70	189,961
Attempted by web, complete	0.87	5,794
September 2017 Phone Survey		
Control (sampled, not attempted)	0.72	170,673
Attempted by phone, no complete	0.69	6,794
Attempted by phone, complete	0.77	823
September 2018 Phone survey		
Control (sampled, not attempted)	0.80	30,425
Attempted by phone, no complete	0.80	8,993
Attempted by phone, complete	0.87	646
October 2018 Phone survey		
Control (sampled, not attempted)	0.79	27,136
Attempted by phone, no complete	0.71	21,940
Attempted by phone, complete	0.88	1,051

Note: Column percentage for turnout rate by treatment group.

However, the behavioral difference in turnout observed across groups in Table 1 might be due to the composition of the groups. Recall that registrants are randomly assigned into groups in each survey (control, web, and phone in 2017, and control and phone in 2018), but respondents who completed the surveys may have different characteristics than those who we attempted to contact but failed to complete the survey. To assess the group sample composition, Table 2 shows the means and standard deviations for several demographic characteristics that were available in the voter file. As Table 2 shows, overall the balance of covariates is similar across the control group and the attempted (non-completed) groups with respect to sex, age, race, party registration, and vote history. However, those who completed our 2017 survey were more male, white, and had a more active voter history than the control group. In addition, Phone completes were more Democratic and web completes slightly more Republican than controls in the mixed-mode survey. In 2018, phone respondents were more male (in October), whiter (in September), less Republican (in October), and had a longer vote history (in September). Thus, it's possible that the higher turnout rates that we observe among completes simply reflect some of these differences in covariates, rather than a causal mobilization effect. To account for that possibility, we estimate a series of differences-in-differences models of turnout on survey participation, controlling for registrants' demographics, party of registration, vote history, and county fixed effects.

Table 2: Balance of Covariates

	Sex (known) μ(sd)	Female μ(sd)	Age μ(sd)	White μ(sd)	Democrat μ(sd)	Republican μ(sd)	Vote history μ(sd)
September 2017 Web Survey							
Control (sampled, not attempted)	0.993(0.081)	0.551(0.497)	48.274(18.130)	0.673(0.468)	0.379(0.485)	0.372(0.483)	2.741(1.737)
Attempted by web, no complete	0.992(0.085)	0.556(0.496)	46.926(17.825)	0.658(0.474)	0.383(0.486)	0.362(0.480)	2.640(1.712)
Attempted by web, complete	0.995(0.064)	0.449(0.497)	54.403(17.135)	0.811(0.390)	0.374(0.484)	0.402(0.490)	3.601(1.675)
September 2017 Phone Survey							
Control (sampled, not attempted)	0.993(0.081)	0.551(0.497)	48.274(18.130)	0.673(0.468)	0.379(0.485)	0.372(0.483)	2.741(1.737)
Attempted by phone, no complete	0.992(0.086)	0.571(0.494)	47.021(17.871)	0.653(0.475)	0.384(0.486)	0.366(0.481)	2.618(1.726)
Attempted by phone, complete	0.996(0.060)	0.501(0.50)	47.953(18.537)	0.690(0.462)	0.429(0.495)	0.359(0.480)	2.873(1.665)
September 2018 Phone Survey							
Control (sampled, not attempted)	0.993(0.083)	0.575(0.494)	55.265(18.249)	0.587(0.492)	0.428(0.494)	0.383(0.486)	3.391(1.590)
Attempted by phone, no complete	0.992(0.085)	0.582(0.493)	55.879(18.220)	0.665(0.471)	0.404(0.490)	0.419(0.493)	3.441(1.588)
Attempted by phone, complete	0.995(0.068)	0.555(0.497)	56.147(19.751)	0.664(0.472)	0.434(0.496)	0.398(0.490)	3.777(1.617)
October 2018 Phone Survey							
Control (sampled, not attempted)	0.994(0.073)	0.577(0.493)	54.898(18.224)	0.642(0.479)	0.410(0.491)	0.419(0.493)	3.428(1.610)
Attempted by phone, no complete	0.991(0.094)	0.571(0.494)	53.733(18.760)	0.579(0.493)	0.420(0.493)	0.357(0.479)	2.896(1.796)
Attempted by phone, complete	0.988(0.107)	0.518(0.499)	54.389(20.446)	0.659(0.474)	0.434(0.495)	0.360(0.480)	3.353(1.781)

Note: Control group consist of sampled cases but not attempted. Attempted phone/web includes cases that were contacted using one of the modes but did not complete the survey.

The extant literature uses across-subject analysis, however, that analysis is vulnerable to the possibility that an unknown, unmeasured third variable might be affecting someone's willingness to participate in the pre-election survey and their likelihood of participating in the 2018 election. We estimate differences-in-differences models to evaluate the within-registrant behavioral change after being exposed to one of the treatments. In the DD estimates, each respondent is represented by two cases in the dataset[1]. The first case denotes her turnout in the 2018 election, following the treatment of survey participation, and the second denotes her turnout in the 2016 election, prior to the treatment of survey participation[2]. If the treatment's effect on the 2018 outcome is real, we would expect to see a significant difference-in-difference effect (ATT).

In Figure 1, we plot the average treatment effect estimated using the DD model for registrants who were contacted (treatment group) compared to those who were part of the sample but never invited to participate in the survey (control group)[3]. Each quadrant plots the DD estimate for each survey sample. Referring to Figure 1, contacting registrants a year earlier or two months before the election day had no effect on their probability of voting in the 2018 general elections. However, consistent with the previous literature arguing that contacting people has a short lived effect, we find that those registrants who were contacted a month earlier had a 3.1 percentage points positive effect on their probability of voting in the 2018 general elections.

Nevertheless, who is mobilized by get-out-the-vote efforts is likely dependent on the electoral context (Arceneaux and Nickerson 2009). We also know that those who voted more frequently in the previous elections are more likley to vote in future elections (see Gerber, Green and Shachar (2003); Denny and Doyle (2009); Green and Shachar (2000); Shino and Smith (2018)). In addition, mobilization appears to have the greatest effects among high propensity voters in low salience elections, and among low propensity voters might be more

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[1]The datasets we use are unbalanced panel data because we drop those registrants who were not of a voting age during the 2016 general elections. Those registrants appear in the dataset only in 2018.

[2]For example, Table 1A in Appendix A includes all registrants in the 2017 sampling frame, so the number of cases is twice the number of registrants, with separate cases for each registrant's 2016 and 2018 outcome (vote participation).

[3]In Appendix A we show the tables for the respective models. In Table A1 we show the estimates from a linear model of voter turnout regressed on contact in 2017, a year dummy, and the interaction between those two variables (ATT), with county fixed effects and controls for vote history, race, sex, party registration, and age.

affected in high salience elections. Previous research has suggested that the mobilization effects of pre-election surveys are greater among the lowest propensity voters in a high-turnout context (Persson 2014), so we expect that to be the case among our registered voter sample in the November 2018 midterm election in Florida. The common trend assumption does not hold for these models, therefore, following Angrist and Pischke (2009) and ONeill et al. (2016) we use two different estimation techniques to make the assumption plausible.

First, we use matching on previous outcomes with differences-in-differences. We use exact matching and match registrants on whether they voted in the 2016 elections, party of registration, sex, age, and race. As shown in Figure 1, contacting registrants two months before the election day increased their turnout by 8.7 percentage points. Contacting registrants a month prior to the election day increased their turnout by 19.1 percentage points. No such effect is observed on registrants contacted a year before the election day. In other words, contacting people close to the election day increases their probability of turning out to vote.

Second, we estimate regression models with lagged dependent variable controlling for registrants turnout in the 2014 and 2016 general elections respectively. The estimate we plot in Figure 1, is the interaction between election year and registrant's turnout record[4]. As we can see, contacting those who did not vote in the 2016 elections a month before the 2018 Election Day does not have any positive effects on their probability of voting in the upcoming election, as they are still less likely to vote.

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[4]Registrant's turnout record is coded 1 if they did not vote and 0 if they voted.

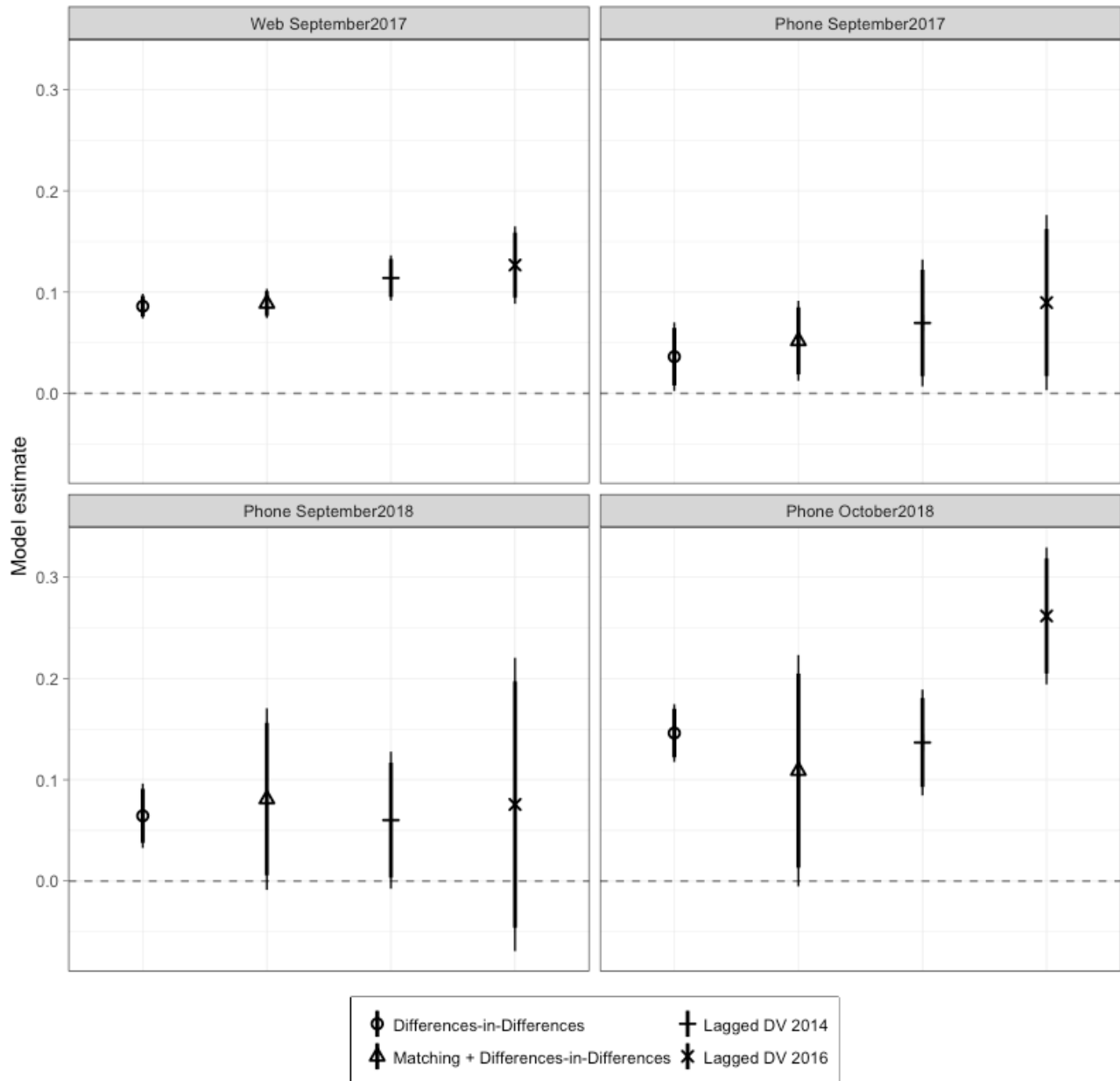Figure 1: 2018 General Turnout for Registrants Who Were Contacted
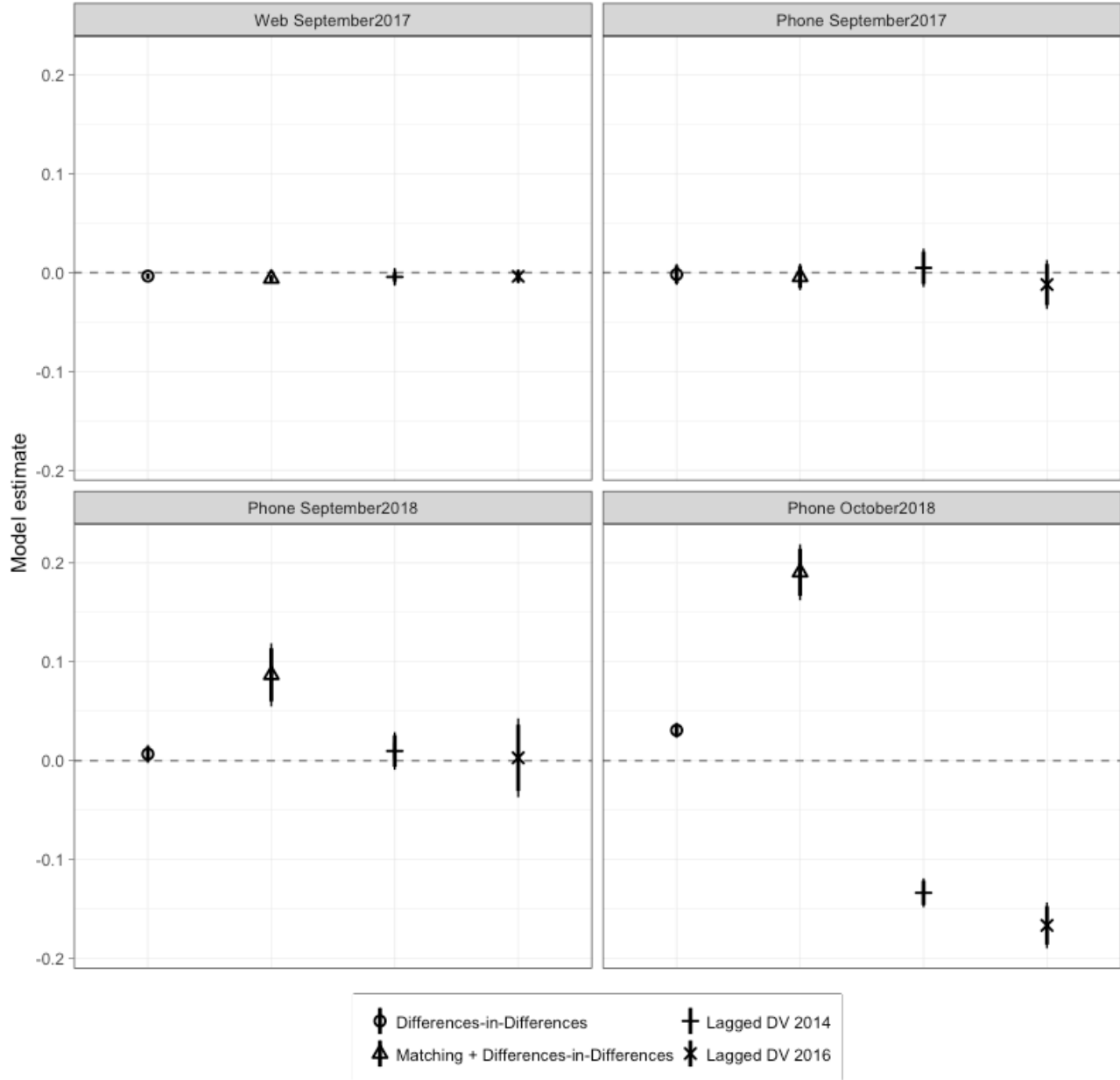
Note: Estimated linear models for 2018 general election turnout. The dependent variable across the models is whether the respondent voted in the 2018 elections coded as 1 or 0 otherwise. The treatment group is composed of those contacted to participate in the survey and the control group is composed of those who were never contacted. Each model controls for registrant's demographics and county fixed-effects. We plot both the 90% and 95% confidence intervals. See Appendix A for tables.

In this study, we hypothesize that whether someone has been been contacted to participate or completed the survey might have heterogeneous effects on their probability of turning out to vote. The results in Figure 1, consistently show that contacting people increases their probability of turning out to vote only if *contacting* is done close to the Election Day. In Figure 2, we estimate similar models but the treatment group in this case consists of those registrants who completed the survey. While, the control group is composed of those registrants who were contacted to participate but did not complete the survey. Indeed, Figure 2 does tell a different story about the voting behavior of those who completed and engaged with the survey compared to those who did not.

As shown in Figure 2, completing a web survey a year prior to the election does increase someone's probability of turning out to vote. Completing the web survey a year before the election increased the turnout by 8.6 percentage points. We observe this effect to persist also for those registrants who completed the survey on the phone in 2017, even though, it is reduced to 3.6 percentage points. However, the effect of completing a survey before the Election Day increased turnout by 14.6 percentage points. Given that the common trend assumption is violated for this group of registrants, we estimate the ATT using exact matching on past outcome with differences-in-differences and lagged dependent variable models. The estimated ATT for the matched dataset shows similar results to the DD estimates. For example, the ATT estimated using the matched dataset shows that completing a web survey a year before the election increased the turnout by 8.9 percentage points.

An important finding shown in Figure 2, is that those registrants that did not vote in 2014 or 2016 elections and completed the survey in 2017 or 2018 were more likely to vote in the 2018 elections. Referring to the lagged dependent variable model for the 2014 elections, we find that those registrants who did not vote in 2014 and completed the web survey had an increase of 11.4 percentage points. Similar positive effects are observed across all samples, however, completing the survey a month prior to the election day increased the turnout among those who did not vote in the 2014 by 13.7 percentage points. Consistent effects are observed also for those who did not vote in the 2016 elections and completed one of our surveys, with the exception of the September 2018 sample.

Figure 2: 2018 General Turnout for Registrants Who Completed the Survey

Note: Estimated linear models for teh 2018 general election turnout. The dependent variable is whether the respondent voted in the 2018 elections coded as 1 or 0 otherwise. The treatment group is composed who completed the survey and the control group is composed of those who did not. Each model controls for registrant's demographics and county fixed-effects. We plot both the 95% and 90% confidence intervals. See Appendix B for tables.

As Clausen (1968) suggested many years ago, engaging citizens in surveys appears to prompt their engagement at the polls. Our analysis shows that contacting people close to the election day increases their probability of turning out to vote. Supporting the hypothesis that the contact effect is short lived. Furthermore, we distinguish between survey contact and completion effects. One important finding of this study is that completing a survey even a year before the election increases someone's probability of turning out to vote. In other words, the survey completion effect has a more long lasting effect on turnout compared to survey contact. Also, completing a web survey a year before the election has a higher effect than completing a phone survey.

4   Discussion

Using differences-in-differences, matching with differences-in-differences, and lagged dependent variables we address the issues that can arise from respondents self-selection. Self-selection is a serious issue that can bias the point estimates of interest and leads to flawed causal effects. In this study we find consistent evidence that our pre-election surveys increased voter participation of survey respondents in the 2018 Florida midterm elections. We also find that the mode of completing the survey matters, as those who completed the 2017 survey on the web appear to have a higher probability to turnout to vote in 2018 than those who were interviewed over the phone. Similar to Persson (2014), we also find that participating in a pre-election survey has a high mobilization effect on the low propensity voters. Most importantly, contacting people by phone close to the Election Day has a positive effect on the likelihood of voting, but that effect appears to dissipate over time. While, completing a survey appears to have more long lasting mobilization effects.

While these effects are relatively small, these results raise questions about the cumulative mobilization effects of hundreds of surveys by academics, media, interest groups, and campaigns. Our findings suggest that the act of responding, rather than being contacted, increases the likelihood of voting, so the actual effects of our instruments on who participates may be restricted by the plummeting response rates in phone surveys. More generally, we might wonder whether these mobilization effects vary by the source of the survey (university, media, or campaigns), whether that source is identifiable to the respondent, and by the content and length of the survey. Of course, this is also a single-state analysis. While Florida is a large and very diverse state, and in recent elections, has been an important battleground in presi-

dential elections and in deciding majority control of the US Senate, the academic community would benefit from replications in states with other political environments and with surveys of varying sources, lengths, and content.

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Appendix A: Tables for Figure 1

Table 1A: Differences-in-Differences Model for Survey Contact Effect on General Turnout

	Web 2017 $\beta(se)$	Phone 2017 $\beta(se)$	Phone Sept'18 $\beta(se)$	Phone Oct'18 $\beta(se)$
Treatment		Survey Contact		
Baseline and comparison years	2016-2018	2016-2018	2016-2018	2016-2018
(Intercept)	0.479***	0.484***	0.697***	0.581***
	(0.003)	(0.004)	(0.011)	(0.011)
Year2018	−0.128***	−0.128***	−0.155***	−0.149***
	(0.001)	(0.001)	(0.002)	(0.003)
Contacted	0.003**	0.002	−0.003	−0.056***
	(0.001)	(0.004)	(0.003)	(0.003)
Year2018×Contacted	−0.003	−0.002	0.007	0.031***
	(0.002)	(0.005)	(0.005)	(0.004)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	723,153	351,944	78,442	97,869

Note: The baseline the contacted variable are those registrants who were sampled but never invited to participate in the survey or non-contacts. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 2A: Matching with Differences-in-Differences for Survey Contact Effect on General Turnout

	Web 2017 $\beta(se)$	Phone 2017 $\beta(se)$	Phone Sept'18 $\beta(se)$	Phone Oct'18 $\beta(se)$
Treatment		Survey Contact		
Baseline and comparison years	2016-2018	2016-2018	2016-2018	2016-2018
(Intercept)	0.767***	0.768***	0.848***	0.723***
	(0.003)	(0.005)	(0.012)	(0.015)
Year2018	−0.127***	−0.128***	−0.161***	−0.146***
	(0.001)	(0.001)	(0.002)	(0.003)
Contacted	−0.001	−0.010*	−0.018	−0.100***
	(0.001)	(0.005)	(0.012)	(0.010)
Year2018×Contacted	−0.006**	−0.004	0.087***	0.191***
	(0.002)	(0.007)	(0.016)	(0.014)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	713,132	348,922	57,576	53,988

Note: The baseline the contacted variable are those registrants who were sampled but never invited to participate in the survey or non-contacts. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects.
***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 3A: Linear Model with Lagged Dependent Variable (2014) for Survey Contact Effect on General Turnout

	Web 2017	Phone 2017	Phone Sept'18	Phone Oct'18
	$\beta(se)$	$\beta(se)$	$\beta(se)$	$\beta(se)$
(Intercept)	0.685***	0.689***	0.734***	0.686***
	(0.005)	(0.007)	(0.020)	(0.019)
Contacted	0.002	−0.005	−0.003	−0.005
	(0.002)	(0.008)	(0.006)	(0.005)
Didn't vote 2014	−0.210***	−0.211***	−0.170***	−0.142***
	(0.002)	(0.002)	(0.005)	(0.005)
Contacted×Didn't vote 2014	−0.004	0.005	0.010	−0.134***
	(0.003)	(0.010)	(0.010)	(0.008)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	362,369	176,356	39,431	49,292

Note: The baseline the contacted variable are those registrants who were sampled but never invited to participate in the survey or non-contacts. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 4A: Linear Model with Lagged Dependent Variable (2016) for Survey Contact Effect on General Turnout

| | Web 2017 | Phone 2017 | Phone Sept'18 | Phone Oct'18 |
	$\beta(se)$	$\beta(se)$	$\beta(se)$	$\beta(se)$
(Intercept)	0.605***	0.607***	0.673***	0.647***
	(0.005)	(0.007)	(0.020)	(0.019)
Contacted	−0.000	−0.000	0.001	−0.022***
	(0.001)	(0.005)	(0.005)	(0.004)
Didn't vote 2016	−0.389***	−0.390***	−0.397***	−0.291***
	(0.003)	(0.003)	(0.010)	(0.010)
Contacted×Didn't vote 2016	−0.004	−0.012	0.003	−0.167***
	(0.004)	(0.013)	(0.020)	(0.012)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	362,369	176,356	39,431	49,292

Note: The baseline the contacted variable are those registrants who were sampled but never invited to participate in the survey or non-contacts. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Appendix B: Tables for Figure 2

Table 1B: Differences-in-Differences Model for Survey Completion Effect on General Turnout

	Web 2017 $\beta(se)$	Phone 2017 $\beta(se)$	Phone Sept'18 $\beta(se)$	Phone Oct'18 $\beta(se)$
Treatment		Completion of Survey		
Baseline and comparison years	2016-2018	2016-2018	2016-2018	2016-2018
(Intercept)	0.478***	0.469***	0.689***	0.466***
	(0.004)	(0.019)	(0.023)	(0.016)
Year2018	−0.133***	−0.133***	−0.153***	−0.124***
	(0.001)	(0.006)	(0.004)	(0.003)
Completed	−0.056***	0.001	−0.018	−0.042***
	(0.004)	(0.012)	(0.012)	(0.010)
Year2018×Completed	0.086***	0.036*	0.064***	0.146***
	(0.006)	(0.017)	(0.016)	(0.015)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	386,219	15,010	18,905	44,753

Note: The baseline the contacted variable are those who were contacted but did not complete the survey. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 2B: Matching with Differences-in-Differences for Survey Completion Effect on General Turnout

	Web 2017 $\beta(se)$	Phone 2017 $\beta(se)$	Phone Sept'18 $\beta(se)$	Phone Oct'18 $\beta(se)$
Treatment		Completion of Survey		
Baseline and comparison years	2016-2018	2016-2018	2016-2018	2016-2018
(Intercept)	0.774***	0.718***	0.918***	0.844***
	(0.004)	(0.021)	(0.043)	(0.058)
Year2018	−0.137***	−0.145***	−0.134***	−0.132***
	(0.001)	(0.007)	(0.011)	(0.013)
Complete	0.024***	0.001	0.002	−0.063
	(0.005)	(0.014)	(0.033)	(0.043)
Year2018×Complete	0.089***	0.052*	0.081	0.109
	(0.007)	(0.020)	(0.046)	(0.058)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	381,458	13,936	1,986	1,690

Note: The baseline the contacted variable are those who were contacted but did not complete the survey. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 3B: Linear Model with Lagged Dependent Variable (2014) for Survey Completion Effect on General Turnout

	Web 2017 $\beta(se)$	Phone 2017 $\beta(se)$	Phone Sept'18 $\beta(se)$	Phone Oct'18 $\beta(se)$
(Intercept)	0.684***	0.675***	0.717***	0.668***
	(0.007)	(0.035)	(0.044)	(0.029)
Completed	0.034***	0.015	0.053**	0.101***
	(0.007)	(0.024)	(0.019)	(0.018)
Didn't vote 2014	−0.216***	−0.219***	−0.162***	−0.284***
	(0.002)	(0.012)	(0.009)	(0.006)
Completed×Didn't vote 2014	0.114***	0.069*	0.060	0.137***
	(0.011)	(0.032)	(0.035)	(0.027)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	193,538	7,525	9,506	22,537

Note: The baseline the contacted variable are those who were contacted but did not complete the survey. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table 4B: Linear Model with Lagged Dependent Variable (2016) for Survey Completion Effect on General Turnout

	Web 2017 $\beta(se)$	Phone 2017 $\beta(se)$	Phone Sept'18 $\beta(se)$	Phone Oct'18 $\beta(se)$
(Intercept)	0.601***	0.597***	0.680***	0.611***
	(0.007)	(0.033)	(0.043)	(0.028)
Completed	0.074***	0.032	0.069***	0.115***
	(0.006)	(0.016)	(0.016)	(0.014)
Didn't vote 2016	−0.394***	−0.414***	−0.398***	−0.469***
	(0.003)	(0.014)	(0.018)	(0.007)
Completed×Didn't vote 2016	0.127***	0.090*	0.076	0.262***
	(0.020)	(0.044)	(0.074)	(0.035)
Controls	✓	✓	✓	✓
County fixed-effects	✓	✓	✓	✓
N	193,538	7,525	9,506	22,537

Note: The baseline the contacted variable are those who were contacted but did not complete the survey. Control variables include: vote history, race, sex, party registration, and age. All models are estimated with county fixed-effects. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$