Unintentional Voter Mobilization:
Does Participation in Pre-election Surveys Increase Voter Turnout?

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Abstract:
Survey researchers have long been concerned with the question of whether participation in pre-election surveys increases voter turnout. This article presents findings from three large-scale field experiments conducted during the 2002 general election in Maryland, New York, and Pennsylvania. Unlike early studies, which found that participation in pre-election surveys increased voter turnout, we find no significant effect. We argue that the rigorous experimental methodology and large sample size in these three experiments should allay concern that survey participation affects turnout.

Does participation in pre-election surveys increase voter turnout? Survey researchers have been concerned with this question for many years. This article presents findings from three large-scale field experiments concerned with this question. These studies were conducted during the 2002 general election in Maryland, New York, and Pennsylvania. Early studies found that participation in pre-election surveys increased voter turnout (Clausen 1968; Granberg and Holmberg 1992; Greenwald et al. 1987; Kraut and McConahay 1973; Sherman 1980; Yalch 1976). These studies, however, suffer from various methodological defects, including low power. Each of the three experiments reported here has a larger sample than past experiments, thereby providing more reliable results than past research on this question.

Social scientists have long been concerned that people behave differently when aware that they are research subjects. The concern for survey researchers is that the attention paid to respondents' opinions about politics and intentions about voting will stimulate change in the respondents' subsequent voting behavior. Although assessing changes in opinions due to pre-election surveys is quite difficult, we can readily test whether participation in a pre-election survey changes the voter turnout rate of survey respondents.

A series of studies dating back to the 1960s has investigated the claim that pre-election surveys affect voter turnout. Table 1 reports the findings and describes the methodology of these studies.

INSERT TABLE 1 ABOUT HERE

Table 1 shows little methodologically reliable evidence of an increase in voter turnout due to participation in a pre-election survey. Observational studies rely on

assumptions about the process of generating the data, adding unknown levels of theoretical uncertainty to the statistical uncertainty inherent in all survey research. If the assumptions upon which observational studies rely are incorrect, the findings are biased (Gerber, Green, and Kaplan 2004). Furthermore, the two observational studies on this question (Clausen 1968; Granberg and Holmberg 1992) show small effects that are easily within the sampling variability of the pre- and post-election surveys used to estimate the increase in turnout. Thus, the statistical uncertainty about their findings should cause reservations about their conclusions. Finally, the difference in the percentage of respondents who reported voting in these observational comparisons of pre- and post-election surveys could also be a product of an increased likelihood to misreport voting after the election.

An experiment in which the researcher randomly assigns the treatment eliminates the problems of bias and allows more certain statement of causality. However, features of the experimental designs used in early experiments limit their findings. The Yalch (1976) experiments lack proper control groups needed to determine the true effect. The samples in the Kraut and McConahay (1973) and Greenwald, Carnot, Beach and Young (1987) experiments were too small (n=104 and n=60 respectively) to produce reliable results. Furthermore, these samples were drawn from narrow populations (households with Italian surnames and college students living in dorms respectively) which should make us cautious about generalizing to the voting public. More recent experiments – using improved experimental methodology – have challenged the conclusions of the early literature by finding no increase in turnout from participating in a pre-election survey (Smith, Gerber, and Orlich 2003; Spangenberg and Greenwald 1999).

Experimental Design

To test whether participation in a pre-election survey causes an increase in voter turnout, we conducted experiments in conjunction with three statewide pre-election surveys in the 2002 general election.[1] We use surveys administered to random samples drawn from voter registration lists. A voter registration based sample provides an array of information on demographic characteristics and past electoral behavior for all individuals in the sample. Furthermore, the voter registration records available after the election provide a reliable measure of whether voters actually cast a ballot.

The Washington Post conducted the survey in Maryland and the Quinnipiac University Polling Institute conducted the surveys in New York and Pennsylvania. Each survey was conducted by the respective organization as part of its regularly scheduled polling in these states and used its usual facilities and procedures.[2] Each organization designed its own 10-12 minute questionnaire regarding the 2002 election, primarily focused on the gubernatorial races in each state. The questionnaires included items meant to gauge likelihood of turning out to vote, political awareness, candidate recognition, candidate preference, candidate favorability, issues in the campaigns, and self-reported respondent demographics.[3] The surveys were conducted as close as

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[1] These polls were conducted as part of a larger research project on a new voter registration based sampling (RBS) methodology which improves the accuracy and reduce the cost of fielding pre-election public opinion surveys (For details see Deane and Morin 2003; Green and Gerber 2003; Mann 2003; Schwartz and Richards 2003).

[2] TNS Intersearch in Horsham, PA conducted the interviews for the Maryland survey with a maximum of eight attempts, callbacks within the calling period, and refusal conversion attempted after 48 hours. The Quinnipiac University Polling Institute in Mt. Carmel, CT conducted the interviews for the New York and Pennsylvania surveys with a maximum of five attempts, callbacks within the calling period, but no refusal conversion attempts.

[3] Copies of the complete questionnaires are available from the author's website.

possible to the November 5, 2002 election: in Maryland, October 21-25; New York, October 28-November 3; and Pennsylvania, October 21-27.

In each state, a stratified random sample of approximately 40,000 registered voters was drawn from statewide voter registration lists using the Registration Based Sampling (RBS) methodology for pre-election surveys (for details on the RBS methodology, see Green and Gerber 2003; Mann 2003).[4] Since it was only possible to call registered voters with a valid phone number for the survey, all registered voters without phones were excluded from the experiment. The percentage of registered voters with phone numbers in each state was: Maryland 65%; New York 69%; and Pennsylvania 66% (Table 2). In order to determine whether voters actually cast a ballot, we matched our sample to the voter registration lists in Spring 2003 when the 2002 general election turnout data became available. The match rates for voter validation in each state were as follows: Maryland 96%; New York 99%; and Pennsylvania 98% (Table 2). The match rates were equivalent in the treatment and control groups. The results presented use only the successfully matched records.

Insert Table 2 about here

Within the sample of registered voters with phone numbers, registered voters were randomly assigned to either the control group or a treatment group. For a separate research question on the effect of advance notification letters on response rates, a portion of the sample in each state was randomly assigned to be sent an advance letter (for details, see Mann 2003). Although only intended to increase response rates to the survey,

[4] Voter Contact Services (VCS), a commercial vendor of voter registration lists based in Sunnyvale, CA, provided all voter lists for the samples. VCS gathers voter registration information from the county level public agencies who keep this information in each state. These records were updated in the fall of 2002 shortly before we drew our samples.

these letters may also increase voter turnout by reminding recipients of the upcoming election and triggering civic or political interest. Registered voters were randomly assigned to the control group or one of three mutually exclusive treatment groups: 1) attempt to call for the survey; 2) attempt to call for the survey and sent an advance letter; and 3) sent an advance letter. Table 3 reports the number of people assigned to each group and the number who were successfully contacted to complete a survey in each treatment group.

Insert Table 3 about here

Results

Table 3 reports the turnout rates for each of the randomly assigned treatments and the control group. Looking at voter turnout in each state, we find very little variation in turnout between the treatment and control groups. In Maryland, 74.1% of the control group voted, while turnout was 73.0% in the group we attempted to call. Of those whom we attempted to call and who were sent a letter 76.0% voted. 74.3% of those sent only an advance letter voted. In New York, 54.4% of the control group voted, while turnout for both groups we attempted to call was 54.5% (those sent a letter and not sent a letter). Voter turnout was 56.2% in the group that was only sent a letter. In Pennsylvania, 64.3% of the control group voted, while turnout was 64.4% in the group we attempted to call, 66.2% in the group we attempted to call and sent a letter, and 66.0% in the group to which we only sent an advance letter.

In order to calculate the effect of participating in the survey, we need to combine the contact rates with the turnout rates for each treatment group in each state. Directly comparing the turnout rate among the registered voters who completed the survey (i.e.

those who actually received the treatment) with the registered voters who did not complete the survey produces a biased estimate of the effect of the treatment, since factors that make registered voters reachable may also increase the likelihood that they will vote. In order to properly estimate the effect of participating in the pre-election survey, we utilize an estimator that makes use of the fact that the group of registered voters we attempted to reach to participate in the survey (treatments 1 and 2 above) was a random selection of the overall sample. The random assignment to the treatment and control groups means the expected proportion of reachable voters is the same in control group and the group we intended to treat. Thus, assignment to being called for the survey (treatments 1 and 2) is an ideal instrumental variable for participation in the survey because: a) assignment to being called was random and therefore uncorrelated with the unobserved causes of survey completion; b) completing the survey is a function of being assigned to be called, and therefore the endogenous variable (completing the survey) is correlated with the instrument (assignment to be called). Taking advantage of this ideal instrumental variable, we use two stage least squares regression to obtain a proper estimate of the treatment effect (Gerber and Green 2000).[5]

Table 4 presents the results of the two stage least squares regression for each state. Model 1 is estimated without any covariates. The Model 1 estimates are unbiased because they are based on random assignment to the treatments. Model 2 includes covariates available from the voter registration records. Including covariates increases the predictive accuracy of our model, and therefore the precision of the estimates of the

[5] We have no reliable way of measuring who received or read the advance notification letters, so we can only calculate an 'intent to treat' effect for the advance notification letters.

experimental effects. The variables *Age* and Age^2 were calculated from the birth dates in the voter registration records. Using the past voter turnout data about the last two general elections (2000 and 1998) from the voter registration records, each voter was placed into one category of five mutually exclusive categories of past voting behavior. All of the covariates in Model 2 had a statistically significant effect on voter turnout in all three states.

Insert Table 4 about here

Both models indicate that participating in the pre-election survey has no statistically significant effect on voter turnout. In fact, the sign for the treatment (*Survey Participation*) is the opposite of the early findings of an increase in voter turnout for those who respond to a pre-election survey. The weighted average of the effects across the three states using the more precise estimates from Model 2 is a statistically non-significant -0.9 percentage points (standard error = 2.4 percentage points).[6]

To compare these results to past investigations, we calculated a 95% confidence interval for our weighted average results: -5.7 to 3.8 percentage point change in voter turnout. If we take the results of past investigations as the null hypothesis for classical significance testing, our findings cause us to reject the findings of all of the past studies which found a statistically significant increase in turnout except Granberg and Holmberg (1992). As noted earlier, Granberg and Holmberg's (1992) finding of a two-percentage point increase in turnout is within the sampling variability of the pre- and post-election surveys used for their observational study. The findings of Smith, Gerber, and Orlich (2003) and the 1986 experiment reported in Spangenberg and Greenwald (1999) cannot

[6] The state-by-state estimates are available on the author's website.

be rejected, but this is unsurprising since these experiments share our conclusion that there is no statistically significant increase in voter turnout from participating in a pre-election survey.

Our null finding is buttressed by the fact that the statistical power of these three experiments should have been sufficient to detect even a modest increase in voter turnout from participating in a survey if an increase actually existed. The average effect from the past studies in which the authors found a statistically significant result (see Table 1) was 17.0%. The statistical power of a 5% test versus this average increase of 17.0% is greater than .99 for the combined weighted results of the three experiments. Furthermore, the power of a 5% test versus each of the significant findings of an increase in turnout in Table 1 also exceeded .99 when calculated for our three experiments.

Some past research has found that the effects of participating in a survey appear to be strongest among registered voters with a low or moderate history of political participation (Clausen 1968; Granberg and Holmberg 1992). We divided the sample using the 4 past general elections that were available from voter registration records (1994-2000). Registered voters who voted in zero, one or two of the past four elections were coded as low past turnout, those who vote in 3 of the past elections were coded as moderate past turnout, and those who voted in all four past elections were coded as high past turnout.[7] Our finding of no statistically significant increase in voter turnout holds

[7] This coding scheme is slightly different from the procedures from the 1987 experiment conducted by Greenwald, Carnot, Beach, and Young (unpublished but described in Spangenberg and Greenwald 1999 and Smith, Gerber and Orlich 2003) which identified the mediating role of past voter turnout. The difference in our coding of past turnout history is necessitated by the fact that Greenwald, Carnot, Beach, and Young had information about turnout in five past elections, while we have information available

across all three categories of past turnout in each state. Furthermore, the hypothesized pattern conditional on past turnout does not appear in our estimates (results not presented – see also Smith, Gerber, and Orlich 2003).[8]

Similarly, Granberg and Holmberg (1992) suggested that participation in a survey might have different effects conditional on age. Breaking the samples into age categories of 18-24, 35-44, 45-54, 55-64, and 65 and over, we find no statistically significant increase in voter turnout among the age categories. The hypothesized pattern conditional on age does not appear in our estimates (results not presented).

The advance notification letters had the intended effect of increasing the response rate of registered voters completing the survey. Despite containing no mention of voting, the advance notification letter also unexpectedly increased voter turnout. The precision weighted average of the effect across the three states indicates the increase was 1.2 percentage points with a standard error of 0.5 percentage points. These findings are consistent with the findings of Gerber and Green (2000) that each voter mobilization mailing increased voter turnout by about 0.6 percentage points with a 0.3 percentage point standard error, while phone contact had no apparent effect on voter mobilization.

Discussion

In past studies, scholars have proposed two major ideas to explain their findings of an increase in voter turnout from participating in pre-election surveys: 1) stimulation leading to subsequent behavior and 2) fulfilling self-prophecy about behavior. Clausen (1968) suggested that the pre-election survey interview stimulates interest in politics and

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about only four past elections. They assigned all voters who had voted in five past elections to the high turnout category, otherwise their categories were the same.

[8] All results not presented here are available from author upon request.

the respondent's "citizen conscience". There are two significant versions of the self-prophecy theory: 1) predictions about future voting behavior (i.e. response to vote intention questions) evoke a script for subsequent behavior (Sherman 1980) and 2) respondents reduce the cognitive dissonance between their socially desirable prediction of voting in the survey and their actual behavior (Spangenberg and Greenwald 1999; Sprott, Spangenberg, and Fisher 2003). However, none of these mechanisms was sufficient to generate a turnout effect from the 10-12 minute pre-election surveys in our experiments.

Earlier findings that participation in a pre-election survey increased voter turnout raised an ethical concern that surveys might alter the outcome of elections and a methodological concern that survey samples were not actually representative of the electorate. The absence of any effect in our three large-scale experiments should allay these concerns and related criticism of pre-election telephone surveys. Even if we grant that the true effect were at the upper limit of our 95% confidence interval – a 3.8 percentage point increase in voter turnout among survey participants – the substantive impact would be relatively small.

In combination with recent findings that more direct appeals for voter mobilization using telephone contact can also have no statistically significant effect on turnout (Gerber and Green 2000; 2001), our failure to find a significant turnout effect from telephone pre-election surveys suggests that telephone contact does not mobilize voters directly or indirectly.

Our experiments share some characteristics that should induce caution about generalizing: each state was in the Northeastern US, each featured a gubernatorial race,

and the findings are limited to one election. Additional field experiments are needed to be certain that the null result would be replicated in other regions and election types. Fortunately, this is easy to do with any survey using a random sample drawn from voter registration records.

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Table 1
Previous Studies of Whether Participation in a Pre-election Survey Increases Voter Turnout

Authors (Date)	Increase in Turnout[a]	Sample (Treatment / Control)	Place	Election Type and Year	Survey Type	Methodology	Notes on Methodology
Clausen (1968)	6.5% pts* (treatment effect)	Not Reported	US	Presidential 1964	In-person	*Observational:* Comparison of turnout among respondents to national pre-election survey by Survey Research Center and post-election survey by the US Census Bureau.	The author fails to account for uncertainty and bias from the assumptions of observational studies. Sampling variability may account for the reported effect despite drawing 2 samples from equivalent populations.
Kraut and McConahay (1973)[e]	27% pts* (intent to treat effect)	52 / 52	New Haven, CT	US House Primary 1970	In-person	*Experimental*: Individuals with Italian surnames randomly assigned for attempt to contact about their political preferences (treatment) or to a control group.	The sample size is too small to produce reliable results.

Yalch (1976)[e]	21% pts* (treatment effect)	233 / 228	Chicago, IL	City Council Preliminary 1973	Phone	*Experimental:* Randomly selected respondents completed a questionnaire of 60 questions regarding political preferences and attitudes.	These experiments lack a proper control group. The control group for the first experiment was contacted for a survey one month later, so it is not directly comparable to the treatment group.
	22% pts*[b] (treatment effect)	222 / 45,549	Chicago, IL	City Council Run-off 1973	Phone	*Experimental:* Randomly selected respondents completed a questionnaire of 60 questions regarding political preferences and attitudes.	The closest approximation to a control group for the second experiment (used here but not by the original author) is the entire population of the ward, including respondents and those who were not successfully contacted.
Greenwald, Carnot, Beach and Young (1987)	23.2% pts*[c] (treatment effect)	32 / 28	Ohio State Univ.	Presidential 1984	Phone	*Experimental:* Randomly selected respondents among undergraduate students living in dorms at Ohio State University were asked for a prediction about whether they would vote.	The sample size is too small to produce reliable results. Also of concern, turnout could be validated for only 83% of sample (50 of 60), further reducing the reliability of the results.

Granberg and Holmberg (1992)	2% pts* (treatment effect)	4720 / 4999	Sweden	Parliamentary 1973-88 (6 elections)	In-person	*Observational:* Comparison of respondents to the pre-election wave of each Swedish National Election Survey and the post-election wave of each Swedish National Election Survey.	The authors fail to account for uncertainty and bias from the assumptions of observational studies. Sampling variability may account for the reported effect despite drawing 2 samples from the same populations.
Spangenberg and Greenwald (1999)[d]	1.1% pts (treatment effect)	1,139 (treatment and control sizes not reported)	Seattle, WA	US Senate 1986	Phone	*Experimental:* Randomly selected respondents received one of 3 treatments: a) asked for candidate preference and prediction of turnout; b) prediction of turnout or c) their candidate preference only. A control group was contacted 5-6 days after the election.	The authors were concerned with self-prophecy effects and therefore include respondents asked about candidate preference but not a voting prediction as part of the control group in the published findings.
Spangenberg and Greenwald (1999)[d]	8.9% pts	346 (treatment and control sizes not reported)	Seattle, WA	State Legislature Democratic Primary 1987	Phone	*Experimental:* Randomly selected respondents were asked about location of polling place, prediction of voting and reason for voting.	

Smith, Gerber and Orlich (2003)	0.1% pts	271 / 288	Southern New England town	Primary Election 2000	Phone	*Experiment:* Randomly selected respondents were asked for a prediction about whether they would vote and a reason for voting.
	-0.1% pts	300 / 288	Southern New England town	Primary Election 2000	Phone	*Experiment:* Randomly selected respondents were asked for a prediction about whether they would vote.
	-4.2% pts	301 / 288	Southern New England town	Primary Election 2000	Phone	*Experiment:* Randomly selected respondents were asked for a reason for voting.

Notes: [a] Increases in turnout marked with an asterisk (*) were considered by the original author(s) to be statistically significant evidence of an increase in voter turnout based on participation in a pre-election survey. The original author(s) used a variety of hypothesis testing procedures depending on their research design, but adhered to the conventional 95% significance level for whichever test they used. [b] Due to the lack of an appropriate control group in the analysis by the author, the treatment effects reported in Table 1 for Yalch (1976) are calculated as the difference between the turnout in treatment group and the turnout in the city council ward reported in the original article. [c] Greenwald et al's (1987) estimates ranged from 23.2 to 25.2 percentage points depending on how they treated the 10 individuals without a validated vote. [d] Spangenberg and Greenwald (1999) describe the two experiments reported in this table that were conducted in 1986 and 1987 but not previously published. [e] Kraut and McConahay (1973) and Yalch (1976) conducted follow up studies in subsequent elections to see if the increase in voter turnout persisted. Those results are excluded from this analysis because they address a separate question of downstream effects of stimulation rather than directly measuring the effects of participating in a pre-election survey.

Table 2
Sample Sizes

State	Original Sample from Voter Registration Records	# with Phone Number	% with Phone Number	# with Validated Vote	% with Validated Vote	2002 Turnout and Valid Phone
Maryland	39,713	25,680	65%	38,113	96%	27,959
New York	40,055	28,481	69%	39,741	99%	28,221
Pennsylvania	39,981	27,504	66%	39,374	98%	26,976

Table 3
Contact Rates and Turnout Rates by Treatment Group

		Treatment 1: Attempt to Call for Survey	**Treatment 2:** Attempt to Call for Survey and Sent Advance Letter	**Treatment 3:** Sent Advance Letter	**Control Group**
Maryland	Survey Participation	20.7%	25.6%	0.0%	0.0%
	Voted	73.0%	76.0%	74.3%	74.1%
	N	2,040	1,602	1,300	19,906
New York	Survey Participation	15.2%	16.6%	0.0%	0.0%
	Voted	54.5%	54.5%	56.2%	54.4%
	N	2,289	2,295	2,656	20,981
Pennsylvania	Survey Participation	10.7%	21.1%	0.0%	0.0%
	Voted	64.4%	66.2%	66.0%	64.3%
	N	2,939	1,959	982	21,096

Table 4
Precision Weighted Estimates of Effect on Voter Turnout Due to Participation in Survey and Advance Letter*

	Model 1	*Model 2*
	Effect (SE)	Effect (SE)
Survey Participation	-1.6 (2.8)	-0.9 (2.4)
Sent Advance Letter	1.5 (0.5)	1.2 (0.5)

Notes: Estimates based on two-stage least squares regression. Effects reported in percentage points.