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Mobilizing the young vote: Direct mail voter guides in the 2015 Chicago mayoral election*

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Abstract

Previous studies have shown nonpartisan direct mail interventions have a small turnout effect. It is less clear, however, what effect these interventions have with regard to cognitive mobilization of young voters. We report the results of a large (*N*=52,324) experiment conducted during the February 2015 mayoral election in Chicago. A random sample of 22,179 registrants between 18 and 30 were sent a voter guide containing two sets of orienting political information for the five candidates: a list of their endorsements and their policy positions on five issues in the campaign. We find turnout among the treated group was 0.9 points higher than the control group which did not receive a guide (33.6% compared with 32.7%; SE is 0.003). We find, however, that treated registrants in above-median household income census tracts are significantly more likely to vote than treated registrants in lower-income census tracts.

Keywords

Turnout, Chicago, mobilization experiment

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Low rates of participation in elections are often described as a chronic ailment of the American body politic.[1] There are, nonetheless, a variety of proposed interventions to increase participation in American elections. We experimentally assess the effect of an increase in political knowledge on turnout among young voters in a local election. In advance of the first round of the 2015 mayoral election in Chicago we mailed a guide listing endorsements and policy positions of each of the five candidates to 25,000 registrants between the ages of 18 and 30. After the election we located as many of these treated registrants and the prespecified control group as possible in voter registration lists to measure the effect of the treatment of receiving a nonpartisan voter guide. We find, first, that this direct mail treatment significantly increased turnout by about 0.9 percentage points (the standard error around this treatment effect is 0.003). When we interact the mailer treatment with median income at the census tract level, however, we find the treatment had a significant effect on turnout only in census tracts with above-median household income.

We begin with some general observations about turnout in the United States, the potential for voting reforms to increase turnout, the confounding effects of cross-cutting information on political activity, and observe how this study contributes to the largely experimental literature of voter mobilization studies. We then describe the experimental design and our findings before making a few concluding remarks.

Turnout has markedly declined since the heyday of participation in the 1960s (Wattenberg, 2002; Teixeira, 1987). Presidential races generally attract the most attention—and the highest turnout—while local elections tend to feature comparatively low rates of participation (Hajnal and Lewis, 2003, Holbrook and Weinschenk, 2014; Oliver, 2012). Regardless of electoral context, turnout is low among people below the age of 30 (Gimpel et al., 2004; Highton and Wolfinger, 2001; Kaufmann et al., 2008; Leighley and Nagler, 2014; Shea and Green, 2006; Wattenberg, 2012). Focusing our experiment on young people in a local

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election serves to isolate registrants least likely to vote in an election already less likely to exhibit high rates of participation. This approach is not entirely misguided; we know voting is a habit-forming activity (Coppock and Green, 2015; García Bedolla and Michelson, 2012; Gerber et al., 2003; Green and Shachar, 2000; Plutzer, 2002) and thus this experiment may prove fruitful to demonstrate an intervention that pays long-term dividends in terms of turning young voters into frequent voters.

There are a wide array of reforms designed to make the casting of a ballot easier. In a review of the effectiveness of a variety of electoral reforms, Berinsky (2005) finds these institutional avenues of reform are limited. "The true costs of participation lie not just in the expression of opinion but also in the *formation* of political opinions. Existing electoral reforms do nothing to minimize the cognitive costs associated with voting" (Berinsky, 2005: 485, original emphasis). Cognitive mobilization, Berinsky argues, will increase interest in politics, a well-known predictor of participation. One study in this area (Wolfinger et al., 2005) finds states that provide pre-election sample ballots and information about polling places have higher turnout, particularly among young and less-educated registrants.[2] Our experiment is designed to test the proposition that a nonpartisan mailer that includes information related to endorsements and policy positions of mayoral candidates in Chicago will increase turnout among 18–30-year-old registered voters who are not voting for the first time.

This proposition is not universally supported in the literature. Mutz (2002, 2006) demonstrates that encountering political information and discourse contrary to one's preferences can reduce participation in politics. Shi (2016) finds demobilization—on the order of 1 to 2 percentage points—follows from a mailed postcard expressing a view on same-sex marriage in North Carolina that is contrary to the recipient's partisan affiliation. Furthermore, efforts to correct misperceptions based on predispositions are not always successful (Nyhan and Reifler, 2010). It may be the case that this mailed voter guide may reduce turnout among the treated observations. This experiment, in short, provides a context to shed light on the utility of objective information in spurring greater political participation along the lines of Berinsky's call for greater cognitive mobilization.

Direct mail experiments and turnout

Random assignment to a treatment is a pillar of the causal empiricist approach to quantitative research (Samii, 2016). Voter mobilization experiments are one subfield in which this approach has taken root and clarified our understanding of the drivers of turnout. A central finding in this research agenda is that nonpartisan direct mail increases turnout by a significant—but small—amount (Green and Gerber, 2015; Green et al., 2013). The early experiment by Gosnell (1927) found, for example, a mailed reminder about an upcoming Election Day increased turnout in the 1925 municipal election in Chicago by 9 percentage points. Miller and colleagues (1981) find voters aged 21–30 were more likely to vote in a local primary election after receiving a written letter with campaign brochures. In a more recent study, Gerber and Green (2000a) found postcards encouraging voting in the 1996 general election increased turnout by 7 percentage points among unaffiliated voters and have no effect on partisans. An experiment conducted in the 1998 general election found direct mail increased turnout by about 0.6 percentage points (Gerber and Green, 2000b). As García Bedolla and Michelson observe, however, "we know little about the underlying mechanisms that make these short sociocultural exchanges so effective in changing voter behavior" (García Bedolla and Michelson, 2012: 11).

Experimental research designs have been used to estimate mobilization effects among targeted populations, like age and racial (or ethnic) groups. Sociocultural cognition—the claim that an individual's sociocultural context has a role in cognitive outcomes—provides a theoretical link between mobilization experiments and observed outcomes (García Bedolla and Michelson, 2012). Nickerson (2006) finds young voters—once they are registered to vote—are equally responsive to appeals as other voters, but that it is about three times as difficult to contact a young voter. The extent to which minority populations can be mobilized by these appeals is a common topic in the literature, though significant effects are not frequently obtained (Green, 2004; Matland and Murray, 2012; Trivedi, 2005; Wong, 2005). García Bedolla and Michelson (2009), however, find direct mail appeals increase turnout by about 0.7 percentage points among Asian populations in California. The present study is designed to assess if, and to what degree, a mailed voter guide with information that helps orient a voter in relation to candidates in an election increases turnout. In that sense, the present study is most similar in design and intent to García Bedolla and Michelson (2009), though it differs in that the electoral context is a municipal election and the population of interest is young voters.

Experimental design

We assess the mobilizing effect of direct mail solicitations using a random sample of registered voters residing in Chicago. These data are drawn from a list of all registered voters in Chicago provided by a third-party vendor prior to the election (1,603,822 observations in total).[3] For the purposes of our experiment we exclude the following observations from our analyses: those registrants who reside outside of the city of Chicago, or who are younger than 18 or older than 30. We excluded registrants who did not participate in the 2014 general election—but retained registrants who did not participate in the 2011 municipal elections—to identify moderate propensity voters, a population thought to be most responsive to the treatment we deployed in this experiment (Eldersveld, 1956; Highton, 2004; Zaller, 1992).[4] These criteria exclude 1,543,308 observations originally in the dataset. We used a

Table 1. Variable balance in control and treatment groups.

Variable	Control group (*N*=30,145)		Treatment group (*N*=22,179)	
	Mean	Std. Dev.	Mean	Std. Dev.
Individual-level variables				
2015 mayoral election vote	0.327	0.469	0.336	0.473
Female	0.597	0.491	0.593	0.491
Age	25.44	3.41	25.48	3.43
Whites	0.489	0.5	0.492	0.5
Blacks	0.247	0.431	0.246	0.431
Latinos	0.138	0.345	0.136	0.343
Census tract variables				
Log of population	8.2	0.5	8.2	0.49
Median age	34.09	5.8	34.07	5.81
White population %	47.27	34.52	47.25	34.48
Black population %	37.14	40.65	37.16	40.7
Latino population %	21.02	26.3	21.09	26.4
Population % with a high school diploma	21.86	12.12	21.79	12.05
Population % with some college	23.81	10.11	23.84	10.08
Population % with a college degree	22.23	15.33	22.26	15.28
Population % with a professional degree	15.51	13.63	15.49	13.56
Median household income	30.54	15.79	30.38	15.67

Notes: Hotelling's two-group *F* test statistic (1.14) is nonsignificant ($p<0.31$) indicating the control and treatment groups are not significantly different.

computer-based random number generator to assign 25,000 registrants from the remaining 60,514 observations to the treatment group, with the balance kept as a control group. In the post-election registration lists provided by the same vendor 8,190 observations—2,821 within the treated group and 5,369 within the control group—were lost to follow-up.[5] In sum, the data for this experiment include 22,179 treated registrants and a control group of 30,145 observations.[6] The dependent variable in our statistical models is a dichotomous indicator of voting in the 2015 mayoral election, as reported for each observation in the voter registration files.[7]

The control variables in our models are sourced from two datasets. First, the voter registration list itself includes individual-level data on a registrant's gender, age, and race. To control for neighborhood-level effects on turnout (Cho et al., 2006; Gimpel et al., 2004) we collected the total population, median age, median household income, proportion of the population that is white, black, Latino, or other, and the proportion of the population in one of five educational attainment categories from the 2013 American Community Survey for each Chicago census tract in our data. We report variable balance across the control and treatment groups in Table 1. We find no significant differences across the experiment groups, demonstrating randomization in the assignment to the treatment.

The treatment group was mailed an informational booklet in advance of the election.[8] This treatment condition is intended to test the idea, presented by Berinsky (2005), that a barrier to participation in elections is low levels of political information among potential voters. The English-language mailer contained two relevant sets of political information. First, it listed endorsements from prominent groups and individuals for four of the five candidates for mayor.[9] Previous research has shown endorsements are helpful heuristics for voters in cases where party affiliation is unavailable (Bowler and Donovan, 1998; Schaffner et al., 2001). Mayoral elections in Chicago have been contested on a nonpartisan basis since 1999. Second, the mailer reported the position of each of the five candidates for mayor with regard to five salient policy issues in the campaign.[10] This information was intended to aid voters in finding a candidate who shares their policy preferences (Lau and Redlawsk, 1997, 2006). The guide itself did not convey polling place locations—instead it directed the reader to the Chicago Elections Board website. The guide mentions the span of the early voting window, the date of the election, and advised registrants to bring some form of acceptable identification to the polls to avoid any potential problem with ballot access.[11]

Our hypothesis that the mailer will increase turnout in the election will be confirmed, then, if our analyses report a positive and significant result for the treated observations. We use logit and multilevel models (Cho et al., 2006; Gelman and Hill, 2006; Gimpel et al., 2004; Steenbergen and Jones, 2002) to estimate the effects of this treatment condition.[12]

Findings

Our findings demonstrate the treatment increased turnout in the Chicago municipal election, all else being equal. Table 2 contains the results of four statistical models; the coefficients are reported as log odds in each specification. We

Table 2. Turnout effects of voter guide.

Predicting voting in 2015 mayoral election	Model 1	Model 2	Model 3	Model 4
Individual variables				
Voter guide treatment	0.042**	0.043**	0.043**	−0.017
	(0.019)	(0.020)	(0.020)	(0.038)
Female registrant		−0.096***	−0.074***	−0.075***
		(0.019)	(0.019)	(0.019)
Age (18–30)		0.001	−0.001	−0.001
		(0.004)	(0.003)	(0.003)
White registrant		0.422***	0.067*	0.066*
		(0.035)	(0.036)	(0.036)
Latino registrant		0.737***	0.076	0.079*
		(0.040)	(0.048)	(0.048)
Other registrant		0.523***	0.097**	0.096**
		(0.042)	(0.045)	(0.045)
Census tract variables				
Log of population			−0.100***	−0.105***
			(0.031)	(0.030)
Median age			0.017***	0.014***
			(0.003)	(0.003)
White population %			0.000	0.001
			(0.001)	(0.001)
Black population %			−0.007***	−0.005***
			(0.001)	(0.001)
Latino population %			0.007***	0.006***
			(0.001)	(0.001)
% with high school diploma			0.003	0.001
			(0.003)	(0.003)
% with some college			0.012***	0.006**
			(0.003)	(0.003)
% college graduates			−0.000	−0.000
			(0.003)	(0.003)
% graduate degrees			0.006*	0.005
			(0.004)	(0.003)
Median household income			−0.008	
			(0.027)	
2nd household income quartile				0.050
				(0.054)
3rd household income quartile				0.142**
				(0.067)
Top household income quartile				−0.217**
				(0.089)
Guide * 2nd household income quartile				0.006
				(0.054)
Guide * 3rd household income quartile				0.094*
				(0.053)
Guide * top household income quartile				0.125**
				(0.058)
Constant	−0.721***	−0.552***	−0.745**	−0.506
	(0.022)	(0.095)	(0.370)	(0.369)
Observations	52,324	52,324	52,324	52,324
Log pseudolikelihood	−33,221	−32,888	−32,322	−32,294
Wald chi-square	4.686**	393.2***	864.2***	990.9***
Number of census tracts			794	794

Notes: Robust standard errors clustered by census tract are in parentheses. Cells report logit coefficients. The reference racial/ethnic group is black registrants. The income quartiles in Model 4 are in reference to the 1st (bottom) household income quartile.*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

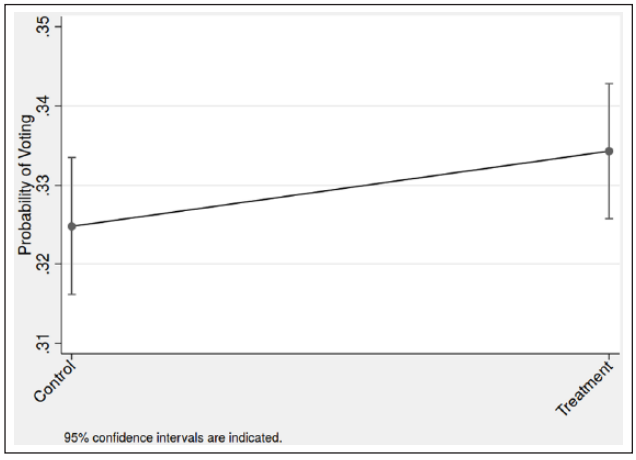

Figure 1. Predicted probability of voting in experiment groups.

first discuss the results in the first three models. The first and least complicated of these models includes only the treatment variable. As we observed in Table 1, there is no significant difference between the control and treatment groups with regard to the battery of explanatory variables. Model 1 shows the voter guide significantly increased turnout in the mayoral election. The treatment effect remains positive and significant as additional individual-level controls are incorporated (see Model 2) and when we estimate a multilevel model of individuals within Chicago census tracts and include demographic data from each census tract (see Model 3). Black registrants are least likely to vote, relative to other racial or ethnic groups, but this difference, with regard to Latino registrants, vanishes in Model 3.[13]

We use the results in Model 2 to estimate the change in the predicted probability of voting in the 2015 mayoral election to get a better sense of the effect of the voter guide, as logit coefficients are difficult to interpret directly. Figure 1 shows the predicted probability of voting, and 95% confidence intervals, for the treatment and control groups. Turnout among the treated observations was about 0.9 points higher than in the control group, 33.6% compared with 32.7% (the standard error is 0.003). A difference of means test shows this absolute difference in turnout is statistically significant (two-tailed $p<0.025$). The relative change in the probability of voting among the treated observations, which can be obtained by exponentiating the treatment coefficient in Table 2, is about 4.4%.

This effect is large relative to the mobilization literature. Green and Gerber (2015: 186–189) report, on the basis of a meta-analysis of 85 studies between 1998 and 2014, the average treatment effect of a nonpartisan mailer without an element of social pressure is 0.523 points. If we limit our consideration to the seven studies in the meta-analysis conducted in the context of a municipal election, the average treatment effect of a mailer is 0.04 points. When compared with other mobilization strategies specifically targeting young voters, these results are more cost-effective than other direct mail experiments, but less effective than phone banking or canvassing and leafleting efforts (see Young Voter Strategies 2006 for a summary of this research).

Is this increase in turnout constant across all observations? We know, for example, that wealthier and better educated citizens are more politically active (Delli Carpini and Keeter, 1997) and more likely to vote (Leighley and Nagler, 2014). Green and Gerber (2015: 63) also find mailers have weak turnout effects among low-propensity voters. It may be the case, then, that the mailer did not increase the likelihood of voting among observations in less wealthy neighborhoods of Chicago. Model 4 presents subgroup analyses of the treatment effect by each quartile of the median household income quartiles in the 794 census tracts in these data. Interestingly, the probability of voting is highest among observations in the 3rd income quartile and lowest among observations in the top quartile, when compared with the reference bottom income quartile. The effect of the voter guide treatment is also only discernible among the top two income quartiles. These results suggest the effect on turnout is limited to observations in more affluent census tracts in Chicago.[14] Additional analyses are reported in the online appendix.

Last, we assess the cost-effectiveness of this mailer. Green and Gerber (2015: 16) state "[i]n order to know whether a campaign tactic is cost-effective, it is necessary to determine how many votes are produced for each dollar spent." We estimate the cost-effectiveness of the mailer, on an intent-to-treat basis, is about $56 (or about 111 mailers were required to produce a single vote). The effectiveness of this mailer is quite high relative to other studies considered by Green and Gerber, who find "one additional vote is generated for every 273 people who receive a conventional nonadvocacy GOTV mailer" and that mailers with a production cost of $0.50 cost $91 per vote (Green and Gerber, 2015: 66).

Conclusions

We sought to empirically test Berinsky's (2005) suggestion that cognitive mobilization is the key to increasing participation in American elections. We narrowed our study to young voters in the 2015 mayoral election in Chicago. Our results indicate the mailed voter guide significantly increased turnout. When we account for income levels in different parts of Chicago, however, we find the treatment only increased turnout among wealthier neighborhoods in the city. We find, like much of the literature on minority mobilization, no significant effect across racial or ethnic groups. The scale of the effect of an increase in political information reported here is similar to recent studies of direct mail (García Bedolla and Michelson, 2009; Gerber and Green, 2000b) though the context of a local election and the focus on young voters is distinctive.

This study suggests additional experimental designs that may shed further light on the role of cognitive mobilization

with regard to voter turnout. The present study is limited to young voters with some prior experience with voting. It is an open question to what degree a similar experiment may mobilize newly registered voters with no prior voting experience. Moreover, a similar experimental framework in conjunction with pre- and post-election surveys could tell us something about the potential for additional political information to change a voter's choice in the course of an election campaign. Replicating this experiment in a variety of electoral contexts, for example open seat races, when an incumbent fails to win reelection, or in localities with a tradition of high turnout in local elections could demonstrate the robustness of these findings and further assess the effects of cognitive mobilization. This experiment only tests the effect of direct mail. Future work should take account of multiple "treatment channels" (Green and Gerber, 2015; Miller et al., 1981) to generate cognitive mobilization and therefore turnout. This study—and other experimental designs—have shown chronic low turnout in American elections by young voters is a treatable condition.

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Authors' Note

This project was previously presented at a Behavioral Ethics Lab seminar at the University of Pennsylvania, at a Global Studies Research Seminar at the University of Tampere, and at a meeting of the Midwest Political Science Association.

Declaration of Conflicting Interest

The authors declare that there is no conflict of interest.

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Supplementary Material

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Notes

1. The debate on whether the question of low turnout is a problem for the conduct of democratic politics is vital, but we are faced with space constraints and unable to summarize that debate here. Instead, we suggest interested readers consult Green et al. (2013) for a review of these topics.
2. See Primo et al. (2007) for a comment on the estimation strategy deployed by Wolfinger et al. (2005). The revised estimates presented by Primo and colleagues, however, demonstrate that turnout is higher among registrants aged 18–24 who do not live with their parents and are mailed a sample ballot (see Table 1).
3. These data were sourced from SmartVAN (http://www.smartvandata.com/smartVAN). These data report slightly fewer ballots cast (a difference of about 4.2%) and more registered voters (about 12.8%) than the official data reported by the Chicago Board of Election Commissioners. As a consequence of the larger set of registered voters, the turnout rate in the total dataset is 28.89% compared with 34.03% reported by the Board.
4. Our intention with these criteria is to exclude first-time and infrequent voters who may need to devote greater time to search costs and transportation to the polling place (Brady and McNulty, 2011).
5. Observations lost to follow-up can be due to a number of factors. For example, a person who moves outside of Cook County and registers to vote in her new location would be lost to follow-up in this experiment. The Census Bureau estimates about 11.5% of Americans moved between 2013 and 2014 (see *Geographical Mobility: 2013 to 2014* reports available at http://www.census.gov/topics/population/migration.html). These 8,190 observations amount to about 13.5% of the total observations in this experiment. This rate is higher than the national average, but it is also well known that younger Americans are more mobile than older cohorts (Squire et al., 1987). Institutionalized persons are also removed from voter registration lists (McDonald and Popkin, 2001). In short, we see no reason to conclude these observations lost to follow-up are a source of bias for the results we report below.
6. We conducted two power analyses with these data. First, we determined we have more than a sufficient number of observations to detect the observed difference in turnout in the control and treatment groups; the minimum detectable sample is 35,441. Second, we determined these data provide more than 95% power (99.22% to be exact).
7. The dependent variable includes registrants who voted either at the polls on Election Day or prior to Election Day. We explored the relationship between receiving a voter guide in the mail and voting early, but found no significant effects among the treated observations.
8. The mailer was designed to convey to the recipient that the information was solely the creation of Chicago Votes and not produced by the Election Board or a candidate's campaign. The original intention for the voter guide was to distribute it by personal interactions, but this distribution strategy was changed to mail as a result of positive feedback from voters and to explicitly test the treatment effect in an experimental framework.
9. The fifth candidate, William Walls, had no endorsements when the voter guide was produced. Walls won 2% of the vote in the general election.
10. The five issues in the mailer were selected after a process of pre-election focus groups with diverse racial, gender, and geographic samples. From the policies these focus groups mentioned, we prioritized issues that were also frequently

mentioned in the news coverage of the election. Lastly, we removed valence issues from the set of policies to be included in the mailer to highlight areas in which the candidates disagreed on salient policies. Candidate positions were determined on the basis of recent statements from the candidate's website or, if no policy position was described on their website, from the most recent public statements made in the media.

11. The voter guide also includes summaries of four non-binding ballot measures and a recommendation for how to vote on those measures. An analysis of the effect of the guides on these ballot measures is beyond the scope of this study, though it is noteworthy that each measure was approved with between 78.99% and 88.9% of votes cast (the results for the measure to suggest an elected school board are only available for 37 of the city's 50 electoral wards).
12. The results presented in Models 3 and 4 in Table 2 are obtained in a random intercept model. The results do not change if we run a random slope model as an alternative specification.
13. It might be the case that the effect of the mailer is actually just a reminder of an upcoming Election Day or an appeal based on civic duty. Neither alternative account, however, appears plausible. With regard to election reminders, Green and Gerber (2015: 63) find these mailers have no effect on turnout. "Five experiments that have tested reminders yield an overall estimate that is a shade less than zero." With regard to civic duty-based appeals, this mailer makes no reference to standard civic duty elements like past turnout, an incentive to vote, or a thankful or scolding tone to increase turnout and thus these themes are unlikely to explain the effect of this mailer.
14. Comparing mean turnout between the control and treatment groups across the four income categories arrives at the same result. Turnout between the treated and control groups is not significantly different in the bottom or second quartile of median household income. Turnout among the treated observations is significantly higher in the third (0.4066 compared with 0.3891, two-tailed $p<0.05$) and top (0.3281 compared with 0.3044, two-tailed $p<0.01$) income quartiles. We ran an identical analysis to assess if the voter guide had an effect on either the racial and ethnic groups in our data or in census tracts with varying proportions of college graduates. These analyses revealed no significant effects. These results are available from the authors upon request.

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