RESEARCH NOTES

The Effect of a Nonpartisan Get-Out-the-Vote Drive: An Experimental Study of Leafletting

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A field experiment assessed the effects of a nonpartisan voter mobilization drive. On the weekend before the 1998 general election, voters in the treatment group received an 8″ × 11″ card on which was printed a nonpartisan encouragement to vote. This treatment had no effect on the turnout rates of registered Republicans and Democrats, but it increased the turnout of those voters unaffiliated with a major party by approximately 7%. We find that the treatment was particularly effective at increasing voting among those unaffiliated voters who voted in 1996.

Over the past several decades, turnout in U.S. presidential elections has fallen sharply. From a postwar peak of nearly 65% in the 1960s, presidential election turnout has declined by nearly one-fourth, to a 1990s average of just over 50%. The falloff in midterm election turnout has been nearly as great. This large decline in participation has been the source of deep concern. Some worry that low turnout biases policies against less affluent citizens, while others regard participation as an intrinsic democratic good and find the low levels of participation disturbing (Arendt 1958, Barber 1984, Lijphart 1997, Pateman 1970). Still others suggest that low voting rates signal the deterioration of our civic life (Putnam 1995).

Recent scholarship has attempted to isolate the key factors behind lower turnout. One influential account is that of Rosenstone and Hansen (1993), who argue that a large part of the decline can be traced to a decrease in the mobilization efforts of candidates and political parties. Their statistical analysis suggests that over half of the decline in voting since the 1960s can be attributed to reduced voter mobilization.[1]

[1]These authors employ a somewhat broad definition of "mobilization," a term that encompasses traditional voter contact activity by both parties and candidates as well as voter participation that is indirectly motivated by activities associated with social movements.

THE JOURNAL OF POLITICS, Vol. 62, No. 3, August 2000, Pp. 846–857
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Rosenstone and Hansen's important work focuses renewed attention on some basic questions concerning both voter mobilization and research design. How hard is it to increase election turnout through voter contacts? Is it very difficult to change this political behavior, or does it take just a simple phone call or note to cause citizens to alter their behavior and show up on election day? Rosenstone and Hansen base their conclusions on regression analysis employing survey data, with the respondent's reported turnout serving as the dependent variable and reported political contact as the independent variable. This methodology, or close variants of it, is common in the literature (e.g. Caldeira, Clausen, and Patterson 1990, Kramer 1970). While Rosenstone and Hansen present their findings with care, this methodology has some pitfalls. First, it is not clear that the voter contact variable is truly an exogenous variable. For example, a strategic party often engages in voter contacts targeted at fellow partisans, presenting partisan voters with persuasive messages to reinforce their underlying political convictions. This effort should target those most likely to vote since the returns on such activity are greatest if focused on those who are very likely to be casting ballots. In general, unless the regression model includes all the variables that influence the party's targeting decision, there will be bias in the regression results.[2] A second potential problem relates to misreporting, which can occur in a variety of forms. Since those who vote are more engaged in politics, it is possible that they are more likely to remember and therefore report receiving a political contact. This would bias upward the estimated effect of political contact on the likelihood of voting. It is possible that respondents who say they voted, when they actually did not, also misrepresent their level of involvement in other aspects of political life, such as whether they had interactions with a political party. This also would tend to bias coefficient estimates upward.

The two issues we have raised are versions of methodological troubles that commonly plague nonexperimental, regression-based studies: endogenous variables and measurement error. This is not to say that the previous regression results are wrong. It does suggest that if possible, multiple methodologies should be pursued to increase our confidence in the results. This paper describes the findings of an experiment designed to help answer some questions about how voter contacts might alter turnout behavior.

Our study was a field experiment. We provided a random sample of voters with a political stimulus and then observed their behavior in an actual election. This work is part of a long and largely neglected tradition of political research. In a pathbreaking study, Harold Gosnell (1927) launched an experimental Get-Out-The-Vote (GOTV) drive in Chicago. This ambitious field experiment, though

[2] A related, but distinct, problem occurs if the decision to contact certain voters and bypass others is related to knowledge of which voters are most likely to respond to the party's call to vote. In this case, the effect of contact is correctly estimated for that subpopulation most responsive to the contact, but the coefficient should not be interpreted as a correct estimate of the marginal effect of the contact on the typical voter.

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brilliantly executed and analyzed, has had only a modest effect on modern political science. There have been some additional field experiments, and some have even focused on political mobilization.[3] However, these studies are far outnumbered by both quasi-experimental research and laboratory experiments.

This research note has several objectives. The main objective is to measure whether a modest political stimulus, in this case delivery of a single printed card (described below), can increase turnout. We find that under some conditions, even this small encouragement can have an important and statistically significant effect on turnout levels. We then analyze the implications of the findings for strategies aimed at reversing the decline in election participation witnessed in recent decades. We estimate that roughly one-fifth of the decline in midterm election turnout could be reversed by a carefully planned, $10-million GOTV campaign. A secondary objective of this work is to spark some interest in a neglected research methodology. For every field experiment in political science, there are at least 100 analyses of nonexperimental data. It is our belief that much can be learned from field experiments, and we hope to play some part in helping to bring field experimentation into the standard methodological tool kit.

Experiment Design

The experiment was designed to measure the effect of a minimal stimulus on voter turnout. The study was conducted in Hamden, Connecticut, a mostly middle-class suburb of New Haven. We initially selected 50 streets in a moderately affluent portion of Hamden, then randomly chose a subset of 25 streets to receive the experimental treatment. The 50 streets included in the study were very similar; all were streets of single-family homes on approximately quarter-acre lots. The experimental treatment was placement of a single $8'' \times 11''$ printed card either between the screen door and the front door or in the mailbox. There was no attempt to speak with anyone, and there were fewer than five cases of contact between residents and those distributing the cards. The cards were distributed on the Saturday and Sunday before the November 3, 1998, election.

The printed cards contained a nonpartisan get-out-the-vote message. The message appealed to citizens' sense of civic duty and reminded them that Tuesday was election day and that they should remember to vote. The card was produced by experienced political mail consultants. The piece was printed in three colors and had a professional look. The cards cost approximately $.30. (A reproduction of the card is available upon request.)

[3] Earlier field experiments include Adams and Smith 1980, Eldersveld 1956, Eldersveld and Dodge 1954. None of these studies examined how experimental effects might differ across voters of different partisan stripes. Nonexperimental work includes Blydenburgh 1971, Cain and McCue 1985, Caldeira et al. 1990, Kramer 1970, Lupfer and Price 1972, and Price and Lupfer 1973. (See also Huckfeldt and Sprague 1992.)

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TABLE 1

Sample Statistics

	Entire Sample	Experimental Group	Control Group
% Democrats	40.2%	41.1%	39.4%
% Republicans	21.5%	18.5%	24.3%
% Voted in 1998	65.5%	65.2%	65.8%
% Voted in 1996*	85.6%	84.7%	86.4%
% New Voters	11.2%	11.7%	10.7%
Sample Size	2021	984	1037

**Note:* The percentage voting in 1996 is among those voters in the sample who were also registered to vote in Hamden in 1996. Approximately 89% of those registered in 1998 were also registered in 1996.

Findings

Table 1 shows the sample statistics. There was a total of 2,021 registered voters in our sample; 984 were assigned to the experiment group and 1,037 to the control group. Table 1 confirms the similarity of the experimental and control groups. There were slightly more Democrats and slightly fewer Republicans in the experiment group, though this disparity did not exceed levels expected due to chance. The percentage of each group that was affiliated with a major party was 59.6% for the experiment group and 63.7% for the control group. Table 1 also shows the percentage of each group that was registered to vote at the same address in both 1996 and 1998. Nearly 90% of the 1998 registered voters were also registered in 1996. The exact percentage of new registrants was 10.7% in the control group and 11.7% in the experiment group. Among those in the entire sample who were registered in 1996, 85.6% had voted. The 1996 turnout rate among the control group was slightly higher than among the experiment group.

Table 2 shows the least-squares regression results. The coefficients indicate the percentage change in turnout associated with a one unit change in the independent variable.[4] The variables included in the regressions are: *Experiment Group* (a dummy variable equal to 1 for voters in the experiment group), *Voted in 1996* (a dummy variable equal to 1 if the voter was registered in Hamden in 1996 and cast a ballot in the 1996 general election), and *new voter* (a dummy variable equal to 1 if the voter was not registered in Hamden in 1996). Some regressions also include dummy variables for the voters' Democratic and Republican party registration.

Table 2, columns 1a and 1b, show the regression results when the sample is all 2,021 of the registered voters. The data analysis shows that when Demo-

[4]Least squares is used since the results are very easy to interpret. In the appendix, a table shows the results when the regressions in Table 2 are performed using probit analysis. There are no important differences in the results.

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TABLE 2

The Effect of Experiment on Turnout Level, OLS

	(1a) All Obs	(1b) All Obs	(2) Unaffiliated	(3) Dem and Rep	(4) Dem only	(5) Rep only
Experiment Group	.005	.008	.072*	−.03	−.012	−.073
	(.019)	(.020)	(.032)	(.024)	(.030)	(.040)
Voted in 1996	.578**	.551**	.562**	.555**	.481**	.636**
	(.029)	(.029)	(.042)	(.041)	(.056)	(.060)
new voter	.379**	.358**	.287**	.451**	.335**	.50**
	(.040)	(.039)	(.060)	(.053)	(.068)	(.088)
constant	.171**	.118**	.086**	.247**	.318**	.174**
	(.029)	(.030)	(.041)	(.040)	(.056)	(.058)
Democrat	—	.131**	—	—	—	—
		(.022)				
Republican	—	.100**	—	—	—	—
		(.026)				
R squared	.17	.18	.20	.13	.08	.20
Sample Size	2021	2021	774	1247	813	434

Note: $*p < .05$, $**p < .01$. Standard errors in parentheses.

cratic, Republican, and unaffiliated voters are all considered together, there was no important increase in turnout associated with the experimental stimulus. The small coefficient on the experimental group dummy variable, which suggests that the experimental treatment was associated with a .5% or .8% increase in turnout, falls far short of conventional levels of statistical significance. These regressions also suggest that the probability of voting in the 1998 elections is very strongly associated with previous voting behavior. Among those who voted in 1996, there was a 58% higher probability of voting in 1998 than among those who did not vote in 1996.

Columns 2–5 in Table 2 show the effect of the experimental treatment for partisan sub-samples: Democrats, Republicans, and unaffiliated voters.[5] The experimental treatment had no statistically significant effect on the turnout of Democrats or Republicans but had a large and statistically significant effect on the turnout of unaffiliated voters. The regression in Table 2, column 2 shows that the experimental treatment was associated with a 7.2% increase in voter turnout among the unaffiliated voters. This coefficient is significant at the .05 level using a two-sided test. Since it is only reasonable to expect either no effect or a positive effect on turnout from the experiment, a one-sided test is clearly justified. In that case, the hypothesis that the effect of the treatment is zero can be

[5]Connecticut is a closed primary state and at the time of registration voters are asked if they wish to affiliate with a party or be listed as unaffiliated. Of those voters in the sample who are not affiliated with a major party, only a handful are not unaffiliated.

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TABLE 2a

The Effect of Experiment on Turnout Level, OLS Excluding New Voters

	(1a) All Obs	(1b) All Obs	(2) Unaffiliated	(3) Dem and Rep	(4) Dem only	(5) Rep only
Experiment Group	.001	.005	.086**	−.043	−.033	−.072
	(.020)	(.020)	(.034)	(.025)	(.031)	(.041)
Voted in 1996	.562**	.555**	.562**	.555**	.480**	.636**
	(.029)	(.029)	(.041)	(.040)	(.055)	(.059)
constant	.153**	.126**	.078	.253**	.330**	.173**
	(.029)	(.030)	(.041)	(.040)	(.055)	(.057)
Democrat	.085**	.116*	—	—	—	—
	(.021)	(.023)				
Republican	—	.084**	—	—	—	—
		(.027)				
R squared	.19	.20	.22	.15	.10	.23
Sample Size	1795	1795	684	1111	714	397

Note: $*p < .05$, $**p < .01$. Standard errors in parentheses.

rejected at very close to the .01 level.[6] The coefficients on the experimental treatment variable for the Democrats and the Republicans are not statistically significant, and the coefficients are less than zero. Rather than interpret these findings to suggest that receiving the treatment might lower turnout, we believe that this is due to random variability.[7]

We have no record of new registrants' voting behavior before they moved to Hamden; therefore, we do not know whether new registrants are habitual voters. The implicit assumption in Table 2 is that new voters did not vote in 1996, though in these regressions we allowed the new voters to have a separate intercept that at least partially corrects for the inaccuracy of the implicit assumption. Table 2a shows regression results for a model that incorporates a slightly different treatment of the new registrants. In Table 2a, rather than include a dummy variable for those voters who are new registrants, we exclude all new registrants from the sample. This alteration reduces the sample size by around 10% but does not change any of the findings. The main difference between the findings in Table 2 and Table 2a is that the estimated effect of the experimental treatment on unaffiliated voters is slightly larger, increasing from 7.2% to 8.6%.

The regression results show a strong treatment effect for the unaffiliated voter. Table 3a presents data consistent with this finding. In 1996, turnout among un-

[6] The p value is .012.

[7] These results also suggest that those in the control group were, even controlling for past voting behavior, less likely to vote than those in the experimental group. This would *strengthen* the case for our finding of a strong experimental effect for the unaffiliated voters in the treatment group.

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TABLE 3a

1996 and 1998 Turnout Rates among Unaffiliated Voters

	Voted in 1996	Voted in 1998	Change, 1996 to 1998
Experiment	77.4%	60.0%	−17.4%
Control	80.8%	53.3%	−27.5%

Note: There are 350 observation in the experiment group and 334 observations in the control group.

TABLE 3b

1998 Turnout Rates among Unaffiliated Voters, by Turnout in 1996

	Voted in 1996, Voted in 1998	Did Not Vote in 1996, Voted in 1998
Experiment	73.4%	13.9%
	(271)	(79)
Control	63.3%	10.9%
	(270)	(64)
Difference (Experiment − Control)	10.1%	3.0%

Note: The numbers in parentheses are the number of cases in each cell.

affiliated voters was approximately 80% for both the treatment and control groups. As is expected, turnout in the midterm elections was lower than 1996 general election turnout. In 1998, turnout dropped among both the treatment and control groups. However, the turnout rate for unaffiliated voters in the control group dropped much more than for those in the experimental group. Among those in the experimental group, the decline in turnout was fully 10% less than among the control group. Given the randomized design of the experiment, the probability that a difference as large as that shown in Table 3a is due to chance is less than 1%.[8]

Table 3b shows turnout rates for unaffiliated voters according to whether the unaffiliated voter cast a ballot in 1996. The data suggest something about how the experimental stimulus worked to raise turnout. The experimental treatment had a large effect on those who voted in 1996 and only a small effect on those who did not vote in the earlier election.[9] Thus, it appears the stimulus provided

[8]The value of the F statistic is 6.94, and the .01 level for a null hypothesis of no effect is $F(1,683) = 6.63$.

[9]When this contrast is reestimated using probit, the experimental effect proves to be twice as large among past voters as compared to past nonvoters.

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by the experimental treatment was enough to get those who had a record of participating in previous elections to vote in 1998; the stimulus was not nearly as effective in changing the behavior of those who lacked a propensity to vote. This pattern implies that a stimulus like that used in this experiment will be more successful at raising midterm election turnout, where higher levels of turnout can be generated by retaining the participation of those who voted in the presidential year, than at boosting presidential turnout, where any additional turnout would require the motivation of those who do not participate even in high turnout elections.

Discussion

The basic finding of this paper is that even a modest stimulus can lead to sizable increase in the turnout rate of unaffiliated voters. In this section, we first consider what might account for the findings and then turn to the implications of our research.

The main result of our experiment is that while there was no effect on the turnout levels of registered Democrats and Republicans, a small experimental stimulus raised turnout among unaffiliated voters by more than 7%. Several factors might account for why there were different effects for partisan and nonpartisan voters. The most likely explanation is that partisans received adequate encouragement to vote from either their political parties or fellow partisans, while the unaffiliated do not receive nearly as much attention. If there is decreasing effect to each contact, then providing a stimulus to those who have been given little attention will have greater effects. Our interviews with local political activists confirm the plausibility of this explanation for the observed differences across partisan and unaffiliated voters. The district town chairman for an area covering approximately one-half of the sample provided an extensive description of the party activities in recent elections. He reported that the neighborhoods in our study were very well organized politically. He described the level of organized political activity that took place in the neighborhoods right before each recent election. To avoid awkwardness in exposition, let us suppose this activist works with the Democratic party. According to his account, all the Democrats, and many unaffiliated voters, receive persuasion/mobilization phone calls about five days before the election. The activist described a typical phone call. After introducing himself as a neighbor, the caller identifies himself as the district Democratic town chairman. The caller then says he hopes the recipient will go out and support the Democratic candidates this year. The call concludes with a description of some of the things that Democratic candidates have done for the neighborhood.

There is also party activity on election day. In Connecticut, the polls close at 8:00 p.m. At about 5:30 p.m., the names of those who have yet to vote are collected and at 6:00 p.m. two or three local activists make some phone calls.

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They first call people they know, then all other Democrats. Subsequently, they also call many of the unaffiliated voters, though these calls are not given top priority. Sources reported that in addition to this party activity, both parties had candidates who actively contacted partisan voters before the election. Based on this description of party activity, it appears that the partisan voters received significant attention; the unaffiliated voters received some attention, but less.

A second possibility why the experiment raised the turnout of unaffiliated voters but not those registered with a party is that nonpartisan voters were particularly moved by the nonpartisan appeal to civic responsibility presented on the printed card. Compared to the hard-edged partisan appeals common in campaign literature, our leaflet strikes a less confrontational, less divisive note. One side of the card was a large picture of American soldiers raising the flag over Iwo Jima, with the caption "They fought . . . so that we could have something to vote for." The reverse continues with the admonition, "Don't let their sacrifice go to waste" and "Remember your rights and responsibilities as a citizen . . . *Remember to Vote*." Our study suggests that this sort of patriotic appeal may be effective at influencing nonpartisan voters.

A third possibility is that the findings are due to chance. A related concern is that the estimated effects of the experimental treatment are larger than the true effects. One check on the plausibility of the estimated effects is to compare the turnout of the unaffiliated voters in the experiment group and the turnout rates of the partisan voters since it might be fair to assume that many of the partisan voters received at least as much encouragement to vote as the unaffiliated voters in the experimental group.[10] In 1998, the turnout rate for registered Democrats was 73.9%, for registered Republicans, it was 68.7%, and for the unaffiliated in the treatment group, turnout was 60%. The experimental treatment thus appears to reduce the gap in turnout between partisan voters and unaffiliated voters by about one-third (the 7 percentage-point boost in turnout resulting from the experimental treatment, which moves these voters from approximately 53% to 60% turnout, still leaves unaffiliated voters about 11 percentage points short of the turnout levels among partisan voters).

The experimental findings have some important implications for how turnout levels might be increased. Turnout in midterm elections has declined substantially in recent decades, falling from an average of 45% of eligible voters in the 1960s (a post WWII peak) to about 34% in recent years, an 11 percentage-point decline in turnout.[11] Some of this decrease is due to a change in the way we define the voting-age population. Approximately 2.5 percentage points of the turnout drop can be attributed to giving the vote to 18–21-year olds, who

[10] This interesting comparison was suggested by Steve Ansolabehere.

[11] The exact numbers depend on how both the numerator and denominators are calculated. Rosenstone and Hansen (1993), for instance, report slightly higher numbers for both the 1960s and recent elections. The change in turnout between the two periods, however, is nearly identical.

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have very low levels of participation (Rosenstone and Hansen 1993).[12] The remaining 8.5% represents a real decline in participation.

The results suggest a relatively easy way to reverse some of this turnout decline. In our sample, roughly 40% of the registered voters were not affiliated with a political party.[13] Suppose that nationwide, approximately 25% of registered voters could be classified as unaffiliated with a major party.[14] According to the results of this paper, if these voters are similar in their response to those in our treatment group, providing a simple voting stimulus to these unaffiliated voters would increase their turnout rate in midterm elections by about 8 percentage points. Using the 25% figure for the proportion of unaffiliated voters in the U.S. electorate, nationwide application of our experimental treatment translates into an estimated 2 percentage-point increase in turnout among registered voters. Given the 75% registration rate for voting-age adults, this effect translates into a 1.5 percentage-point increase in turnout among the voting-age population. If we assume a cost of $.20 per postcard for materials purchased in very large orders and $.10 for card delivery, then based on an estimate of 37 million unaffiliated voters, for a little over $10 million, approximately one-fifth of the recent turnout decline could be reversed (18% = (1.5%/8.5%)).[15] This $10-million expenditure represents less than half of 1% of the total amount spent by candidates in the 1996 election cycle. Future research will focus on whether this turnout increase can be extrapolated to higher spending levels and other locations. Given the tradition of relatively strong party organizations in Connecticut's urban areas and near-suburbs, it is possible that the effect of incremental mobilization efforts is even larger for an average population. If so, perhaps turnout can be raised substantially for a relatively modest amount of money.

[12] This estimate is based on Table 7-1, Rosenstone and Hansen (1993). Rosenstone and Hansen provide an estimate of the effect of the younger electorate on presidential election turnout (−2.7%), as well as separate regression results for models predicting presidential and midterm election turnout. They do not say exactly how much the younger electorate lowered midterm turnout. The coefficient estimates and the marginal effects of the younger electorate are very similar for the two models, so the number used in the text is a fair estimate. The argument we are making in the paper does not hinge on any exact value.

[13] This is not very different from the percentage of independent voters indicated in recent surveys. The preelection National Election Study, for example, tallied 39% independents in 1992 and 34% independents in 1996 (Abramson, Aldrich, and Rohde 1998, 167).

[14] The overwhelming majority of voters live in states where there is party registration. According to the Congressional Research Service, in California, 16% of voters are not affiliated with the Democrats or Republicans; other examples of large states are New York, 24%; Florida, 13%; New Jersey, 54%; and Pennsylvania, 8%. Of the four largest states, only Texas does not have party registration.

[15] This calculation assumes that each unaffiliated voter, rather than each household that contains an unaffiliated voter, would receive their own notice about the elections. In the experiment, the stimulus was one notice per household, so the cost to increase turnout levels may be overestimated slightly.

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Appendix

A1. Probit results.

Table A1 shows the results of probit estimation of the equations in table 2a.

TABLE A1

The Effect of Experiment on Turnout Level, Probit

	(1) All Obs	(2) Unaffiliated	(3) Dem and Rep	(4) Dem only	(5) Rep only
Experiment Group	.0133	.219*	−.102	−.045	−.26
	(.061)	(.097)	(.08)	(.098)	(.14)
Voted in 1996	1.62**	1.65**	1.53**	1.31**	1.86**
	(.098)	(.15)	(.14)	(.18)	(.23)
New Voter	1.07**	.93**	1.11**	.87**	1.45**
	(.12)	(.19)	(.17)	(.21)	(.30)
Constant	−.95**	−1.27**	−.68**	−.47**	−.98**
	(.097)	(.15)	(.13)	(.18)	(.21)
Log Likelihood	−1136.5	−450.1	−664.5	−434.8	−226.2
Sample Size	2021	774	1247	813	434

Note: $*p < .05$, $**p < .01$. Standard errors in parentheses.

Manuscript submitted 22 April 1999
Final manuscript received 17 August 1999

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