The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment

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We report the results of a randomized field experiment involving approximately 30,000 registered voters in New Haven, Connecticut. Nonpartisan get-out-the-vote messages were conveyed through personal canvassing, direct mail, and telephone calls shortly before the November 1998 election. A variety of substantive messages were used. Voter turnout was increased substantially by personal canvassing, slightly by direct mail, and not at all by telephone calls. These findings support our hypothesis that the long-term retrenchment in voter turnout is partly attributable to the decline in face-to-face political mobilization.

During the last half-century, a dramatic transformation has occurred in the manner in which voters are mobilized. The election campaigns described by Gosnell (1937), Sayre and Kaufman (1960, chap. 6), and Wolfinger (1974, chap. 4) relied heavily on face-to-face contact between voters and those seeking their support. Notably absent from such accounts are professional campaign consultants, direct mail vendors, and commercial phone banks, all of which have gradually replaced work performed by party activists. The advent of modern campaign tactics (Broder 1971; Ware 1985) has coincided with a decline in the proportion of adults who report working for a political party. Based on an annual aggregation of Roper surveys between 1973 and 1994, Putnam (2000, 41) reports a steady decline in this proportion: Whereas 6% of the public reported working for a political party in the early 1970s, just 3% did so in the mid-1990s.

At the same time, there has been a marked decline in the size and vitality of nonpartisan organizations. In the mid-1960s, 2.4 of every 1,000 women over the age of 20 belonged to the League of Women Voters, compared to .79 in 1998 (Putnam 2000, 438–44). A similar fate has befallen such civic organizations as the Lions, Rotary, and Kiwanis Clubs, which have experienced sharp membership declines since the 1960s. Due to the changing character of both partisan and nonpartisan organizations, voter mobilization has become increasingly impersonal, and messages that once might have been delivered in person are now communicated using mass marketing techniques.

The decline of personal mobilization has arguably contributed to the erosion of voter turnout in the United States since the 1960s. This hypothesis is related to, yet distinct from, Rosenstone and Hansen's (1993) contention that diminishing rates of turnout are a result of a decline in the volume of mobilization activity. As Abramson, Aldrich, and Rohde (1998, 85) point out in their discussion of Rosenstone and Hansen, there has been no clear trend over time in the proportion of American National Election Study (ANES) respondents who report some form of contact with political parties or campaigns, whether personal or impersonal. When read in conjunction with the trend lines compiled by Putnam, the ANES data are consistent with the view that campaigns are reaching as many people as ever but through less personal means.

Our hypothesis about declining turnout rates rests on the claim that personal canvassing mobilizes voters more effectively than other modes of contact that have taken its place, such as direct mail or telephone appeals. The literature on collective action and prosocial behavior supports this conjecture. Studies of blood donations, recycling, and "good deeds" underscore the importance of delivering urgent requests and making vivid the obligation to act (Christensen et al. 1998; Wang and Katzev 1990), and these blandishments seem particularly effective if delivered in person (Spaccarelli, Zolik, and Jason 1989). The special force of face-to-face contact is illustrated by Reams and Ray (1993) and Jason et al. (1984), whose experiments demonstrate that recycling and blood donations are particularly responsive to in-person appeals.

There is good reason to suspect that personal canvassing is an effective means by which to mobilize voter turnout, but its effects have seldom been gauged reliably. Nonexperimental studies, beginning with Kramer (1970), tend to rely on survey data to examine the relationship between turnout and reported contacts with political organizations or candidates (Blydenburgh 1971; Cain and McCue 1985; Caldeira, Clausen, and Patterson 1990; Kramer 1970; Lupfer and Price 1972; Price and Lupfer 1973; Wielhouwer and Lockerbie 1994; see also Huckfeldt and Sprague 1992). Rosenstone and Hansen (1993), for example, regress reported voter turnout on reported contact with candidates or political parties. An important drawback to this approach is that political contact may not be an exogenous predictor of turnout. If parties direct their appeals disproportionately to committed partisans, those most likely to vote will also be most likely to receive contact, and the apparent link between contact and turnout may be spurious. Regression analyses generally include a host of control variables, but it is

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TABLE 1. Results of Previous Voter Mobilization Experiments

Study	Date	Type of Election	Number of Subjects (Including Control Group)	Treatment	Effect on Turnout[a]
Gosnell (1927)	1924	Presidential	3,969	Mail	+1%
Gosnell (1927)	1925	Mayoral	3,676	Mail	+9%
Eldersveld (1956)	1953	Municipal	41	Canvass	+42%
			43	Mail	+26%
Eldersveld (1956)	1954	Municipal	276	Canvass	+20%
			268	Mail	+4%
			220	Telephone	+18%
Adams and Smith (1980)	1980	Special election, City Council	2,650	Telephone	+9%
Miller, Bositis, and Baer (1981)	1981	Primary	79	Canvass	+21%
			80	Mail	+19%
			81	Telephone	+15%

Note: The subjects in all experiments were registered voters. In Eldersveld (1956), subjects were selected in 1953 from those who opposed or had no opinion about charter reform, in 1954 from those who had voted in national but not local elections. This table includes only studies that used random assignment to treatment and control groups (or near-random assignment in the case of Gosnell 1927).
[a] These are the effects given in the tables of the research report. They are not adjusted for contact rates.

unclear whether these covariates eliminate the problem. Another important limitation of survey-based studies is that the researcher has no control over, and often little knowledge of, the frequency or nature of the political contact.[1] The measure of party contact drawn from the ANES, for example, makes no distinction between telephone calls and personal canvassing. If these two modes of contact have different effects, measures that fail to differentiate them will produce distorted results.

More methodologically defensible, although rare, are experimental studies of mobilization, the results of which are summarized in Table 1. The experimental tradition harks back to Gosnell's (1927) studies in Chicago, which assigned certain city blocks to receive nonpartisan mail reminders to register and vote. Gosnell found that turnout increased by 1% in the presidential election of 1924 and 9% in the municipal election of 1925. Adams and Smith (1980) examined the effectiveness of one mode of voter mobilization, telephone calls, and found that partisan appeals had significant effects in a city council election in Washington, D.C. The principal experiment to examine the effects of personal canvassing in conjunction with mailings that used varying types of nonpartisan appeals was conducted by Eldersveld (1956; Eldersveld and Dodge 1954) in two Ann Arbor, Michigan, municipal elections.[2] In both cases the effects of canvassing and mail were statistically significant, but total sample size for all conditions was just 453; the effects can only be inferred with a 95% confidence interval of plus or minus 20 percentage points, so no reliable inferences can be made about the relative effectiveness of different forms of mobilization. The problem of statistical power is even more acute in the Miller, Bositis, and Baer (1981) study of primary election voting in Carbondale, Illinois. It compared the effectiveness of partisan telephone, mail, and personal appeals, but the total sample size was only 215. As did Eldersveld, these authors found personal canvassing to be highly effective, but the range of uncertainty causes the effects to fall short of conventional levels of statistical significance. An additional source of concern is the fact that Eldersveld (1956), Adams and Smith (1980), and Miller, Bositis, and Baer (1981) fail to adjust for their inability to reach some people in the treatment groups. As we explain below, their practice of folding uncontacted subjects originally assigned to treatment groups into the original control group tends to overstate the effectiveness of mobilization campaigns.

Due to the significant limitations of previous research, students of voting behavior do not have a clear sense of either the current magnitude of mobilization effects or the relative effectiveness of different mobilization strategies. Experimental studies have generated the most convincing evidence of causality but are either dated or small in scale. For these reasons we launched a series of turnout experiments in which randomly selected households were exposed to mailings, telephone calls, or personal appeals before the November 3, 1998, general election. Following Eldersveld, we varied the content of appeals, stressing civic duty, neighborhood solidarity, or the prospect of a close election. As did Miller, Bositis, and Baer (1981), we examined the effects of repeated mail contact. Because our sample sizes are roughly 100 times larger than the Eldersveld (1956) or Miller, Bositis, and Baer (1981) studies, we can draw far more precise inferences about the effectiveness of various mobilization tactics. Our

[1] The fact that the independent variable is *reported* political contact raises further concerns. A number of studies indicate that respondent reports are subject to potentially serious measurement error stemming from faulty memory or deliberate misreporting.
[2] The finding that participation can be stimulated by mobilization efforts is consistent with evidence that people subjected to a preelection interview are also more likely to vote, even though the interviews are not designed to encourage voting. For details, see Anderson, Silver, and Abramson 1988; Granberg and Holmberg 1992; Kraut and McConohay 1973; Traugott and Katosh 1979; and Yalch 1976.

TABLE 2. Assignment of Persons to Treatment and Control Conditions

	Number of Direct Mailings Sent				
	None	One	Two	Three	Total
No Telephone Call: Personal canvassing					
No in-person contact attempted	10,800	2,406	2,588	2,375	18,169
In-person contact attempted	2,686	519	625	627	4,457
Telephone Call: Personal canvassing					
No in-person contact attempted	958	1,451	1,486	1,522	5,417
In-person contact attempted	217	385	352	383	1,337
Total	14,661	4,761	5,051	4,907	29,380

findings indicate that personal canvassing is highly effective, much more so than the direct mail and telemarketing campaigns that have come to displace it. The implication is that the decline in voter turnout may be due to the changing character of American campaigns. Although the volume of mobilization activity remains considerable, its increasingly impersonal nature draws fewer people to the polls.

EXPERIMENTAL DESIGN

This field experiment was conducted in New Haven, Connecticut, which has a population of approximately 100,000. In September 1998 we obtained a complete list of registered voters, from which we created a data set of all households with one or two registered voters. To eliminate students from the sample, all names with post office box addresses were excluded, as was one voting ward that encompasses a university and student housing. We were left with 29,380 individuals (22,077 households), whose participation in the 1998 election could be determined from public records.

Our study was designed to measure the effect of personal canvassing, telephone calls, and direct mail appeals on voter turnout. Through a series of random assignments, the sample was divided into control and experimental groups. Table 2 shows the sample sizes of each group for the $2 \times 2 \times 4$ design.[3] The treatment and control groups for the three experiments overlap, such that 10,800 people were assigned no intervention; 7,369 were sent at least one mailing but received no other appeal; 2,686 were slated only for personal contact; and 958 were assigned to receive only telephone reminders. The remainder of the sample, 7,567 people, was assigned to two or more treatments. Assignment to the personal canvassing experiment was designed to be uncorrelated with the telephone and mail experiments, so that it could be analyzed separately. Random assignment to each of the telephone/mail treatments was performed in a manner that made calls more frequent among those who received mail. Thus, these two treatments are correlated, and their effects must be estimated using multivariate methods.

Overall, the treatment group for personal canvassing contained 5,794 people, the control group 23,586. For the direct mail experiment, 14,719 people were in the treatment group, and 14,661 were in the control group. The effectiveness of randomization was checked using voter turnout data from the 1996 presidential election. Based on a chi-square test with 15 degrees of freedom for the 16 groups defined in Table 2, we cannot reject the null of independence between treatments and past voting behavior ($p > .10$).

Personal Canvassing Procedure

During each Saturday and Sunday for four weeks before the election, canvassers were sent to contact randomly selected registered voters. They were paid $20 per hour and were primarily graduate students. New Haven has a substantial minority population and a significant proportion of non-English speakers. More than half the canvassers were African American or fluent in Spanish, and when possible they were matched to the racial and ethnic composition of the neighborhoods they walked.

For safety reasons, all canvassing was done in pairs and ceased at sunset. This procedure constrained both the pool of available canvassers and our ability to contact people not at home during the day. In contrast to conventional canvassing efforts, we targeted certain households rather than entire streets, which meant that more time was devoted to locating specific addresses and walking from one to the next. Consequently, canvassers were able to contact only 1,615 (28%) of the 5,794 people in the personal canvassing treatment group.[4] Examination of the data showed a fairly even

[3] Random assignment was done at the household level. The results we present treat individuals as the unit of analysis; as we point out below, however, the results are very similar when we look separately at households containing one or two registered voters. Also, the standard errors we report are very similar to the ones obtained using statistical methods that allow for unmodeled similarities between household members, such as generalized least squares or resampling.

[4] For the subset of persons not contacted, two supplementary experiments were performed. In certain wards, 719 were randomly chosen to receive a mailer, along with a refrigerator magnet that had the election date printed on it. A separate analysis indicated that this

contact rate across the 29 wards of the city. The average contact rate by ward was 28%, with a standard deviation of 9%.

To bolster the external validity of the experiment, we used a variety of messages when encouraging people to vote. These were developed after a series of conversations with four professional campaign consultants, each of whom suggested effective nonpartisan appeals.[5] The final versions reflect both themes used in actual mobilization efforts and social science explanations of voting: civic duty, close election, and neighborhood solidarity. Upon contacting one or both of the registered voters at each address, canvassers read the following introduction: "Hi. My name is ____. I'm part of Vote New Haven '98, a nonpartisan group working together with the League of Women Voters to encourage people to vote. I just wanted to remind you that the elections are being held this year on November 3d."[6]

In the civic duty version, the script continued: "We want to encourage everyone to do their civic duty and exercise their right to vote. Democracy depends on the participation of our country's citizens." The close election version stated: "Each year some election is decided by only a handful of votes. Who serves in important national, state, and local offices depends on the outcome of the election, and your vote can make a difference on election day." The neighborhood solidarity version stated: "Politicians sometimes ignore a neighborhood's problems if the people in that neighborhood don't vote. When politicians see a lot of people turning out to vote, they know they should pay attention to issues important to people who live around here."

The civic duty script appeals to a sense of obligation. It states a norm that citizens are expected to vote and contends that democracy depends on political participation. This appeal parallels a central explanation of large-scale collective action, the notion that citizens derive intrinsic satisfaction from participation (Riker and Ordeshook 1968). In contrast, the last two messages emphasize two ways in which voting may be instrumental. First, the close election appeal reminds citizens that a single vote can determine the outcome. The odds of casting a pivotal vote are remote, but the stakes are high, and elections are close from time to time. Alternatively, by voting, one calls politicians' attention to the concerns of one's neighborhood. The neighborhood solidarity theme was developed in light of the special political circumstances in the East Shore section of New Haven, where a secession movement has been active for more than a decade. In sum, these appeals encompass leading themes of voter mobilization campaigns.

Direct Mail

The direct mail experiment was intended to measure the turnout effect of both the number of mailings received and the message conveyed. To gauge the first effect, we divided the treatment group into three subgroups and sent one, two, or three mailings, respectively. As shown in Table 2, each subgroup contained approximately 4,900 persons. The mailings were sent out at three intervals: 15 days, 13 days, and 8 days before the election. The subgroup that received two mailings was sent mail on the two dates closest to the election, and the single mailing was sent 8 days before the election. Within each of these groups, we randomly generated three additional subgroups, one for each type of political message. (Subjects who received a certain appeal through personal canvassing received the same type of appeal by mail.) To avoid sending anyone the same mail piece twice, nine different postcards were required, three for each form of appeal. The postcards were prepared by a professional political consulting firm that specializes in direct mail. All nine treatments were three-color postcards. A description of each card is given in the appendix.

Telephone Calls

During the three days before and including the election, a random subset of registered voters received calls urging them to vote. These took place on Sunday evening and all day Monday and Tuesday. A large out-of-state telemarketing firm, which conducts several such get-out-the-vote campaigns during each election cycle, conducted the 30-second calls. Although the telephone numbers taken from the voter registration files were cross-checked using data obtained from Survey Sampling, Inc., many wrong numbers remained. And, as is the norm, only a portion of the potential respondents could be contacted during the three days, despite repeated attempts.[7] Of the 6,754 people in the treatment group, only 2,166 (32.1%) resided in a household in which at least one registered voter completed a conversation with the phone bank. The telephone scripts mirrored those for personal canvassing, but only the civic duty and close election appeals were used. We excluded neighborhood solidarity because that might be an implausible appeal coming from an out-of-state phone bank (it was expected that those called would note nonlocal accents). Subjects other-

subsidiary treatment had a negligible effect that is swamped by a large standard error. For another subset of those not reached (683 persons), canvassers left leaflets if no one was home. This treatment was not strictly random and occurred only in certain wards. We found no differences in canvassing effectiveness between wards with or without leafleting ($p > .10$, one-tailed test). Even if the true effects of leafleting and mailers were equivalent to three mailings, the overall bias to our estimate of the effect of personal canvassing would be less than 1 percentage point.

[5] The League of Women Voters, New Haven Chapter, also helped in the development of the messages and canvassing procedures.

[6] The closing line of the script was varied randomly. With 40% probability, canvassers ended with: "We hope you'll come out and vote." Otherwise, they closed with a question: "Can I count on you to vote on November 3d?" We found the "question effect" nonsignificant ($p > .05$) and collapsed these experimental categories for purposes of analysis.

[7] Since the treatment group and control group are formed by random assignment, the failure to contact the entire treatment group due to wrong or unlisted numbers will not lead to biased results if the estimator in equation 6 is used.

TABLE 3. Effects of Personal Canvassing on Voter Turnout

Unadjusted Relationship between Actual (as Opposed to Attempted) Contact and Voter Turnout

	Not Contacted in Person	Successfully Contacted in Person
Percentage voting	44.5%	59.0%
Number of persons	27,765	1,615

Unadjusted Relationship between Experimental Subgroups and Voter Turnout

	Assigned to the Control Group (no personal contact)	Assigned to the Treatment Group (*attempted* personal contact)
Percentage voting	44.8%	47.2%
Number of persons	23,586	5,794
Number actually contacted		1,615
Contact rate		27.9%

Estimated Effect of Personal Contact on Voter Turnout
Turnout Differential (2.43%)/Contact Rate (27.87%) = 8.7%
Standard Error (2.6)

wise in the neighborhood solidarity condition were given an appeal based on civic duty.

RESULTS

In presenting the experimental results, we first provide tables that show the basic findings on personal canvassing. These convey the main findings in an accessible fashion and also illustrate our statistical method for handling contact rates. We then present a regression analysis that confirms the results but allows us to estimate these and other experimental effects with more statistical precision.

The Personal Canvassing Experiment

To underscore the importance of methodological nuance in interpreting the effects of canvassing, Table 3 shows the effect of in-person contact on turnout in more than one way. The upper half of the table compares the turnout rate of those who were contacted face-to-face with those who were not. The group not contacted, however, is a combination of the control group (whom we made no attempt to reach) and those in the treatment group whom the canvassers were unable to reach. In some previous experimental studies (e.g., Adams and Smith 1980; Eldersveld 1956), the treatment effect is calculated only by this method of measuring the difference in turnout rate between those who were contacted and those who were not. Nonexperimental studies using survey data implicitly take a similar approach as well, since these compare the voting rates of those who report contact and those who do not. In our study the difference between the voting rate of those contacted and those not contacted is very large, 14.6 percentage points.

This number doubtless overestimates the effect of canvassing. If voters who are easier to reach are also more likely to vote, then the canvassing effect is partly spurious. To estimate the effect of the treatment properly, we must distinguish the treatment effect from the higher probability of voting among those easier to contact. One way to do this is to augment the experimental design. We might have added another control group—those with whom canvassers make contact and deliver a nonpolitical message. An alternative is to derive an estimator making use of the fact that the group we intended to treat is a random subset of the entire sample, and therefore the proportion of easy-to-contact voters is the same in the treatment and control groups. We adopted this second approach, which is elaborated by Angrist, Imbens, and Rubin (1996).

To isolate the treatment effect, we reason as follows. Suppose that a subset of the experimental group is contacted. For any given level of canvassing effort, the population can be divided into two groups, those who are reachable and those who are not. Let α be the proportion of the population that is reachable. Let p_{nr} be the probability that a nonreachable person votes, let p_r be the probability that a reachable person votes without the experimental treatment, and let $p_r + t$ be the probability that a reachable person votes after being exposed to the experimental treatment. Our aim is to estimate the value of t. The probability that a randomly selected member of the control group will vote equals:

$$P_C = \alpha p_r + (1 - \alpha)p_{nr}. \quad (1)$$

This probability equals the probability that an individual member of the control group is "reachable" (α) times the probability a reachable person votes (p_r) plus the probability the person is not reachable ($1 - \alpha$) times the probability a person of this type votes (p_{nr}). The probability that a randomly selected member of the treatment group will vote equals:

TABLE 4. Effects of Personal Canvassing on Voter Turnout, by Type of Nonpartisan Appeal

Type of Appeal	Turnout Rate	Number of Registered Voters in Treatment Group	Number of Persons Actually Contacted
Unadjusted Turnout Rates among Experimental Subgroups			
Civic duty	47.2%	1,985	534
Neighborhood solidarity	46.3%	1,881	546
Election is close	48.1%	1,928	535
Control	44.8%	23,586	N/A
Implied Effects of Personal Contact on Voter Turnout			
Civic duty	Turnout Differential (2.43%)/Contact Rate (26.90%) = 9.1% Standard Error (4.3)		
Neighborhood solidarity	Turnout Differential (1.48%)/Contact Rate (29.03%) = 5.1% Standard Error (4.1)		
Election is close	Turnout Differential (3.36%)/Contact Rate (27.75%) = 12.1% Standard Error (4.2)		

$$P_E = \alpha(p_r + t) + (1 - \alpha)p_{nr}, \quad (2)$$

where the difference between equations 1 and 2 is due to the effect of the experimental treatment. Combining equations 1 and 2, we derive an expression for t:

$$t = \frac{P_E - P_C}{\alpha}. \quad (3)$$

Although the population probabilities are not observed, sample data can be used to obtain an estimate of t. First, using the law of large numbers,

$$plim\ V_E = P_E, \qquad plim\ V_C = P_C, \quad (4)$$

where V_E is the percentage of the treatment group that votes, and V_C is the percentage of the control group that votes. Similarly,

$$\frac{N_r}{N_E} = \alpha, \quad (5)$$

where N_r is the number of subjects in the treatment group who were reached for the experimental treatment, and N_E is the number of subjects in the treatment group overall. Using equations 4 and 5, we obtain a consistent estimator of t:

$$plim\ \frac{V_E - V_C}{\frac{N_r}{N_E}} = t. \quad (6)$$

Equation 6 says that, to find the treatment effect, subtract the turnout rate of the control group from the turnout rate of the experimental group and divide this difference by the observed "contact rate," which is 28%. Using this formula, we find that personal contact raises the probability of turnout by 8.7 percentage points, with a standard error of 2.6. The null hypothesis that canvassing does nothing to increase turnout can be decisively rejected, using a one-tailed test ($p < .01$).[8]

Table 4 suggests that the effects of personal contact do not vary significantly across messages. The close election message boosts turnout rates by 12.1%, which is slightly better than the 9.1% associated with the civic duty appeal and substantially better than the 5.1% for neighborhood solidarity. These findings are suggestive, but the standard errors associated with the estimates are far too large to reject the null hypothesis that the messages have equal effects. Looking ahead to the experiments using direct mail and telephone calls, we find a similar pattern of insignificant differences across messages. Since we cannot rule out the view that any plausible mobilization appeal works equally well, the analysis that follows focuses exclusively on the relative effectiveness of delivering the appeal in person, by telephone, or through the mail.

Regression Results

Regression analysis permits us to conduct a more comprehensive analysis, taking into account all the treatments in our experiment. Regression analysis has the further virtue of introducing covariates, such as past voting history, that reduce the unexplained variance in voting rates and allow for more efficient estimation of the experimental effects. For reasons cited above, however, any regression analysis must attend to the possibility that subjects with a higher propensity to vote are easier to reach in person.

Consider the following simple model of how the experimental treatment affects turnout. Suppose again, for purposes of illustration, that the population can be divided into those who are easy to contact and those who

[8] These results remain unchanged when we disaggregate the data according to whether the household contains one or two registered voters. For single-voter households, the effect of personal contact is estimated to be 10.0% (SE = 3.7), compared to 8.2% (SE = 3.6) for two-voter households. These estimates are too similar to be differentiated statistically ($p > .10$).

TABLE 5. Linear and Nonlinear Regression of Voter Turnout on Mode of Contact, with and without Covariates

	Two-Stage Least Squares		Two-Stage Probit
Independent Variables	Coefficient (SE)	Coefficient (SE)	Coefficient (SE)
Personal contact	.087 (.026)	.098 (.022)	.323 (.074)
Direct mailings (0 to 3)	.0058 (.0027)	.0063 (.0023)	.0214 (.0067)
Telephone contact	−.047 (.023)	−.035 (.020)	−.130 (.056)
Registered as Democrat or Republican		.064 (.006)	.217 (.015)
Voted in 1996 general election		.229 (.007)	.589 (.018)
Abstained in 1996 general election		−.231 (.008)	−.824 (.024)
Age		.0188 (.0008)	.0649 (.0022)
Age squared		−.000133 (.000007)	−.000467 (.000020)
Number of registered voters in household (1 or 2)		.056 (.005)	.188 (.014)
Constant	.445		
F	5.86	296.66	
Degrees of freedom	29,376	29,342	29,342

Note: The base category for past voting behavior is the set of people who were not registered in 1996. Not reported in this table are the coefficients associated with each of the 29 wards. The first-stage equations include dummy variables representing the intent-to-treat groups associated with canvassing, phone calls, and direct mail. The first-stage equation also includes covariates for columns 2 and 3. Standard errors for the two-stage probit estimates were obtained using jackknifing.

are not. The probability that a given person in the experiment votes may be expressed as

$$Y = a + b_1X_1 + b_2X_2 + e,$$

where $Y = 1$ if the subject votes, $X_1 = 1$ if the subject is difficult to contact, and $X_2 = 1$ if the subject is actually contacted; 0 otherwise. Given that X_1 is not observed, we might ignore this variable and regress Y on an intercept and X_2. This will yield a consistent regression coefficient estimate only if X_1 and X_2 are uncorrelated, or if b_1 equals 0. These special conditions cannot be expected to hold. Unless everyone in the treatment group is contacted, there will be some correlation between how easy it is to reach a subject and the likelihood they are actually reached. It is also quite reasonable to assume that those who are very hard to reach may also be less likely to vote (i.e., b_1 does not equal 0). Although these points seem straightforward, they have eluded previous research in this area.[9]

The standard solution to the problem of correlation between a right-hand-side variable and the regression error is to find a suitable instrumental variable. In this case, an ideal instrument is at hand. Recall that a valid instrument satisfies two criteria: The variable must be uncorrelated with the regression error, and it must be correlated with the endogenous variable. The probability that subjects are contacted is a function of whether they are randomly selected for the treatment group. This implies that a dummy variable which equals 1 for subjects in the treatment group will be correlated with the endogenous variable. Because the treatment group is generated through random assignment, there is no reason to suppose that those who are easy to contact will be overrepresented. Thus, the expected correlation between the instrumental variable and the regression error is zero.

Table 5 presents two-stage least-squares regression estimates of the effect of each experimental treatment. As indicated earlier, the instrumental variables used in the regressions indicate whether the person was in a given treatment group. For example, the variable *Personal Contact* equals 1 if the subject was contacted, and the instrumental variable equals 1 if the person was in the group that we intended to treat. Note that the instrumental variable will be correlated with the included variable (being in the intent-to-treat group predicts the likelihood that one is contacted), but the instrumental variable is not correlated with the regression error (treatment group status is due to random assignment). A similar procedure applies to the telephone experiment, with intent-to-treat serving as an instrument for actual contact. For the mail experiment, the instrumental variable and the independent variable are the same, since the assumed contact rate is 100%.[10]

Official voting and registration records contain useful information about the sample. For example, we know whether a person voted, abstained, or was absent from the voter rolls in the 1996 general election. We also know an individual's age, party registration, voting ward, and whether s/he is the sole registered adult in the household or is one of two. Each of these covariates contributes significantly to the predictive accuracy

[9] Consider some of the seminal work in this area. Kramer (1970) interprets the higher turnout rate among those reached by a party or candidate as the marginal effect of contact. In the classic study by Eldersveld (1956), those unavailable for personal contact were moved into the control group. This practice results in overestimation of the treatment effect.

[10] Our calculations assume that all of the households we intended to treat by mail received the treatment, an assumption implicitly made in all previous mail experiments. In our case, the voter lists were very current and fewer than 1% of the mailings were returned. To adjust the estimated effects for any failure to receive the mail, divide the coefficients in Table 5 by the supposed contact rate.

of the model, which makes for more precise estimates of the experimental effects. Moreover, this background information affords an opportunity to look for interactions between treatment effects and individual attributes.

The multivariate model enhances slightly the apparent effect of personal and mail appeals. Face-to-face contact raises turnout by 9.8 percentage points, and direct mail raises turnout by .6 percentage points for each mailing. Because our most intensive treatment involved only three mailings, we cannot reliably extrapolate out very far. Direct mail vendors informed us that a regimen of 4 to 9 mailings is common in political campaigns. If this practice is grounded in a correct assessment of how mailings stimulate turnout, we might have drawn even more voters to the polls with additional mailings.

One of the most surprising results to emerge from our experiment is the ineffectiveness of telephone appeals. The commercial firm we retained routinely does this kind of work on behalf of parties, campaigns, and interest groups, often on a very large scale, so it was well qualified. Nevertheless, we find no indication whatsoever that telephone appeals raise turnout. The turnout rate for the treatment group (44.5%) was lower than the rate for the control group (45.5%). (The turnout rate among those actually contacted by phone was 60.7%, but this number is evidently a spurious reflection of the unobserved characteristics of people who are reachable by phone. This result reinforces our concerns about previous work based on nonexperimental data.) Taking contact rates into account and controlling for other experimental features, telephone calls would seem to have diminished turnout slightly, a conclusion that remains unchanged when we look separately at the civic duty and close election messages. Given our initial expectation that telephoning increases turnout, we take this result to mean that the null hypothesis of no effect cannot be rejected using a one-tailed test.

Although negative results such as these are often viewed with some disappointment, they are not uninformative. Both the phoners and personal canvassers communicated the same information, but only the latter influenced subsequent behavior. Reminding people that an election is imminent has no discernible effect per se on turnout. Precisely what distinguishes personal contact is open to speculation. It may make information more salient and memorable, may trigger a feeling of connectedness to the electoral system, or may more credibly convey the urgency of the request.

In analyzing our mode of contact by experiment, we have focused on main effects associated with the randomly manipulated regressors. Augmenting the regression models to allow for interactions, we find them to be jointly insignificant ($p > .05$). For example, mail campaigns do not seem to amplify the effectiveness of personal canvassing, or vice versa, regardless of the content of the appeal. Telephone calls neither increase nor decrease the effectiveness of mail or personal appeals. We find no evidence of a second-order interaction among telephone, mail, and personal contact. The additive nature of these effects runs counter to the hypothesis that get-out-the-vote drives, in and of themselves, signal the importance of the election, thereby raising turnout. If this were so, then those who received a mailer and a telephone call might have been especially impressed by the scope of our Vote '98 campaign. Instead, they reacted much as would be expected based on the behavior of those who received only mail or telephone calls.

Interpretation of the nonexperimental coefficients is clouded by the fact that these regressors gauge voting propensities in an overlapping and incomplete fashion. The effect of party registration, for example, must be understood in light of the fact that we controlled for participation in the 1996 election but not for education. For this reason, we must be cautious about interpreting these coefficients or comparing them to survey-based results. As expected, voter turnout tended to be higher among those who voted in the previous election, were registered with a major party, were older (but not very elderly), and were part of a two-voter household. Taken together, these control variables predict approximately one-quarter of the variance in voter turnout.

We do not find any significant interactions between these background characteristics and the effectiveness of various get-out-the-vote interventions. For example, augmenting the two-stage least-squares regression model to include interactions between personal canvassing and age, past voting history, and major party registration does not significantly enhance the fit of the model ($p > .10$).[11]

Finally, the pattern of main effects we report in Table 5 is amplified somewhat when we use the two-stage probit estimator proposed by Rivers and Vuong (1988) to model the probability of voter turnout. Holding all the covariates constant, the probit regression coefficient of .32 implies that personal canvassing raises turnout from 44.5% (the control group in all experimental conditions) to 57.3%. This change of 12.8 percentage points, is somewhat larger than the 9.8 percentage points obtained using least squares. Probit regression also suggests slightly stronger effects for direct mail. Again, taking 44.5% as a baseline, the probit estimate of .02 implies that three mailings increase turnout by 2.5 percentage points. As before, telephone calls did nothing to increase turnout.

Nonlinear models thus reinforce the central finding that personal forms of mobilization tend to be more effective. Consider the effects of mobilizing a set of average households in the control group, whose baseline probability of voting is 44.5%. At fifty cents per

[11] When we use all of the covariates to generate predicted probabilities of voting and divide the sample into five groups according to their baseline probability of voting (below 20%, 20–40%, 40–60%, 60–80%, or above 80%), the data do not suggest that personal contact has differential effects across these categories. Granted, the contact rate is much higher for those with the highest propensity to vote (38.7%, $n = 2,209$) as opposed to those whose likelihood of voting is less than one-in-five (21.5%, $n = 7,544$), but the effectiveness of personal contact (in logistic units) does not vary significantly. By the same token, we have no evidence that the effectiveness of mail or telephone calls varies depending on the individual's baseline likelihood of participation.

mailer, sending three mailings to each household (containing an average of 1.5 voters) nets roughly one additional voter for each $40 spent. Similar calculations, using $1.50 as the cost per personal contact (10 contacts per hour at $15 per hour), produce an estimate of approximately one more voter for each $8 spent. To be sure, organizing and supervising a canvassing campaign involves significant fixed costs, but even if the effective marginal costs of canvassing were doubled, face-to-face mobilization would still be cost effective.

CONCLUSION

Due to its size and randomized design, our experiment lends precision and nuance to the extensive literature on voter mobilization. In particular, our findings suggest the importance of differentiating between personal and impersonal modes of political contact. Face-to-face interaction dramatically increases the chance that voters will go to the polls. In our study, personal canvassing had a far greater influence on voter participation than three pieces of professionally crafted mail delivered within two weeks of election day. Less effective than direct mail were calls from professional phone banks.[12] Despite our efforts to encourage callers to deliver messages in an engaging, conversational style, the telephone appeals were unmistakably routinized and scripted, and it is possible that recipients detected the fact that the calls were from another state. (The use of out-of-state phone banks in campaigns is by no means atypical. In the New Haven mayoral election of 1999, phone callers had distinctive out-of-state accents.) Whatever personal touch might be conveyed over the telephone undoubtedly diminishes as callers plow through long lists. Our findings concerning voter turnout mirror previous research on blood donation, which shows mail and telephone appeals to be much less effective than face-to-face requests (Jason et al. 1984).

The magnitude of the canvassing effect coupled with the limited influence of direct mail and telephoning lends credence to our hypothesis that falling rates of voter turnout reflect a decline in face-to-face political activity. This hypothesis has been overlooked in previous analyses, which have largely relied on survey data. Surveys rarely draw a distinction between face-to-face and telephone contact; even if they do, they are ill-equipped to gauge the relative effectiveness of personal and impersonal modes. Unlike nonexperimental studies, our research establishes a causal link between canvassing and turnout, and it can survive the charge that canvassing is directed at people with a greater propensity to vote or that the apparent correlation between contact and voting is due to misreports of one or both variables. Contacts were manipulated on a random basis, so that we did not have to rely on respondents to recall whether or how they were contacted. Since the study used public documents to measure voter turnout, none of the experiment relied on self-reports.

Although the size and scope of our study represents a significant advance over previous experimental and nonexperimental research, it leaves certain questions unanswered. First, the messages we employed were strictly nonpartisan. Our findings are consistent with the hypothesis that declines in voter turnout reflect massive retrenchment in the size and vitality of nonpartisan and civic organizations (Putnam 2000), but they speak only indirectly to claims concerning partisan mobilization. Although previous canvassing experiments found both partisan (Miller, Bositis, and Baer 1981) and nonpartisan (Eldersveld 1956) appeals effective, no studies compare the two directly. Whether both types of mobilization have similar effects is thus a question that awaits future experimentation. It may be that the negative tone of certain partisan appeals, particularly when communicated through impersonal means, actually demobilizes certain segments of the electorate, although the evidence is currently mixed (Ansolabehere and Iyengar 1995; Lau et al. 1999).

Second, the small but discernible effect of direct mail implies that the growth in impersonal mobilization may offset some of the decline in personal mobilization. (A similar offsetting pattern may apply to phone banks if, as the results from Adams and Smith [1980] and Miller, Bositis, and Baer [1981] imply, partisan appeals are more effective than the nonpartisan messages we examined.) Our findings are consistent with the notion that declining turnout reflects the disappearance of face-to-face mobilization, but a more thorough understanding of the effects of alternative forms of mobilization is required in order to gauge the historical significance of transformations in both partisan and nonpartisan organizations. In particular, students of electoral politics need to investigate the cumulative consequences of increasing reliance on mail and telephone (as well as the Internet). It may be that over time these modes of contact face the same problem as conversations in a noisy cafeteria: As campaigns must raise their voices louder and louder in order to be heard, voters pay less attention to each message. In 1925, direct mail sent by Harold Gosnell increased turnout by 9 percentage points. Today, the volume of direct mail is vastly greater, and the effect of each piece is much smaller, less than one percentage point per mailer.

Further research also is needed to confirm the generality of our results. It remains to be seen whether they hold in other settings and types of elections, and we are currently engaged in efforts to extend the experiments. Studying the effects of personal canvassing and other forms of contact across different electoral contexts should be viewed as much more than mechanical replication. The distinction we have drawn between personal and impersonal modes of contact suggests the following cross-sectional or cross-temporal hypothesis: Personal contact is more influential when campaigns and organizations emphasize impersonal mobilization. To establish this proposition requires attention to the interaction between the political environment and various mobilization strategies.

Even if our findings have external validity, an impor-

[12] A parallel get-out-the-vote experiment using the same telemarketing campaign in a neighboring city also produced slightly negative effects, based on a sample size of 5,855 in the treatment group and 8,883 in the control group, with similar contact rates.

tant gap remains: We know very little about the mechanism by which personal contact influences voting behavior or why impersonal forms of contact have less effect. Our experiments do not tell us whether personal contact enhances interest in politics, feelings of connectedness to the electoral system, the belief that elections are important, or a sense of obligation to participate. Now that we have a clear indication that canvassing does indeed affect behavior, we plan to augment future experiments with a postelection survey in order to assess the psychological imprint left by canvassers.

Despite limitations, this experiment provides important new clues in the ongoing mystery of why turnout has declined even as the average age and education of the population has risen. A certain segment of the electorate tends not to vote unless encouraged to do so through face-to-face contact. As voter mobilization grows more impersonal, fewer people receive this kind of encouragement. This point is of great practical significance for those who seek to reverse the declining trend in turnout. Many of the recent policy innovations designed to encourage voter participation (e.g., absentee balloting) focus on reducing the costs of voting. Our findings suggest the importance of focusing as well on the personal connection between voters and the electoral process. Face-to-face mobilization efforts have a demonstrable effect on voter turnout. The question is whether the long-term decay of civic and political organizations has reached such a point that our society no longer has the infrastructure to conduct face-to-face canvassing on a large scale.

APPENDIX: DESCRIPTION OF MAILERS

Postcard Texts

The cards were designed and written (in consultation with the authors) by a professional firm that specializes in political direct mail. All nine cards had the same basic layout. Each was 8×12 and had a different picture that covered the entire front side, with some text superimposed on the picture. The reverse side contained only text. As described below, for each of the three messages, a large portion of the text was the same for all cards in the same message category. A description of the nine cards used in the experiment is provided here, along with the text for each. In addition, we include a reproduction of one card to show the layout.

Civic Duty

The card mailed 8 days before the election is shown in figures 1 and 2. The card mailed 13 days before the election used a picture from the civil rights "March on Washington," of the Washington Monument and the Mall filled with people. The text read: "What Were They Fighting For? They Were Fighting for Our Right to Vote. The whole point of democracy is that citizens are active participants in government, that we have a voice in government. Your voice starts with your vote. On November 3d remember your rights and responsibilities as a citizen. Remember to vote."

The card mailed 15 days before the election had a picture of the first page of the U.S. Constitution. The text read: "We The People . . . Have a Duty. We have a Duty to Vote." It also carried the same text as the previous card, beginning with "The whole point . . . ."

FIGURE 1. Text Side

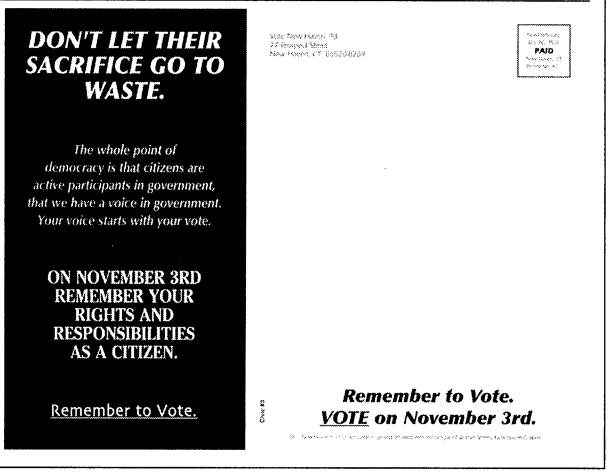

FIGURE 2. Picture Side

Neighborhood Solidarity

The card mailed 8 days before the election used a picture of a crowd of people holding up their hands. The text read: "There is Strength in Numbers. Stand Up and Be Counted. When people from our neighborhood don't vote we give politicians the right to ignore us and concentrate their energies elsewhere. But you can make sure that doesn't happen. By joining your neighbors and voting on election day, you'll send a message to our elected leaders: that you care, and that they should care about your concerns. On November 3d Vote to ensure that we are not ignored. Remember to vote."

The card mailed 13 days before the election had a picture of several houses on a city block. The text stated: "Stronger Neighborhoods Start With You. They Start With Your Vote. Our elected officials listen to one thing only—our votes." It also carried the same text as the previous card, beginning with "When people from . . . ."

The card mailed 15 days before the election showed people crossing a city street. The text read: "Are Your Values Going to be Represented on Election Day? Only if You Vote." It repeated text from the previous card, beginning with "When people from . . . ."

Election Is Close

The card mailed 8 days before the election had a picture of two children at a table in a classroom. The text stated: "Their Future Starts with One Vote. Yours. In an election, anything can happen. This year many elections will be decided by only a handful of votes—will yours be the deciding vote? Don't miss your opportunity to make a difference, don't miss your chance to make an impact in our elections. On November 3d make sure your vote is included, because no election is ever a certainty and every vote counts. On November 3d don't miss your opportunity to make a difference. Remember to vote."

The card mailed 13 days before the election showed Harry Truman holding up the headline "Dewey Defeats Truman." The text read: "History has Shown the Importance of Your Vote. Vote and Be a Part of History." It used the same text as the previous card, beginning with "In an election . . . ."

The card mailed 15 days before the election used a picture of a street sign with an arrow pointing in opposite directions. The text read: "What direction will the country head in? It's up to you and your vote." It carried the same text as the previous card, beginning with "In an election . . . ."

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