

Research Note

Does Email Boost Turnout?*

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ABSTRACT

Political campaigns are just now learning how to put the Internet to best use. Low transaction costs and huge economies of scale tempt campaigns to move traditional activities online, but the effectiveness of virtual campaigns is unknown. This paper conducts 13 field experiments on 232,716 subjects to test whether email campaigns are effective for voter registration and mobilization. Both registration and turnout were unaffected, suggesting that email, while inexpensive, is not cost-effective.

Learning to use television as a campaign medium took politicians years, and candidates are now beginning to figure out how to use the Internet. An intuitive place to begin is by using the Internet to accomplish work previously done with older technology such as mail, phones, or face-to-face canvassing. The Internet's low transaction costs and massive economies of scale could alter the strategies parties employ in every facet of campaigning. The same economics that push businesses to move online are also present in the political realm. Unfortunately, studies of Internet usage during campaigns generally report on the content of websites (Farmer and Fender 2003, Norris 2003, Ward and Gibson 2003, Farmer and Fender 2005, Xenos and Foot 2005) and do not measure how voters respond to the online campaign activities.¹

This paper evaluates the effectiveness of email as a voter mobilization tool by conducting 13 field experiments. Direct mail has been shown to be an effective, albeit expensive, means of increasing voter turnout (Gerber *et al.* 2003). Examining all known randomized

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¹ For an exception, see Lupia and Philpot (2005), who study how online news sites affect the political interest of young adults.

experiments evaluating direct mail, Green and Gerber (2004, p. 60) estimate that takes 133 pieces of mail to create one vote and at 50 cents per mailing the cost for each vote is \$67. By saving on printing, supplies, and postage, email could be more cost effective than direct mail at mobilizing voters.

This logic guided the spending of millions of dollars during the 2004 Presidential election. Entirely web-based groups such as MoveOn.org grabbed headlines for their pioneering use of the Internet for political mobilization, but traditional campaigns also engaged in significant online mobilization. During the final week of the election roughly one-third of email contact from both parties focused on encouraging turnout (Reich 2005).² After the election, one of the pressing questions in the Democratic Party was the future of the 2.7 million supporters on Kerry's email lists (Faler 2004). The utility of the web for collecting donations is evident, but whether email lists engage voters and increase participation is more difficult to measure.

There are five primary reasons to think that email may be an effective tool at mobilizing voters. The first reason is that campaigns view email as an effective tool. According to the 2004 Post-Election Pew Internet & American Life Project tracking survey, 25% of Internet users received email encouraging them to vote for a candidate. If campaigns devote resources toward voter mobilization via email amid the financial and labor constraints near Election Day, at minimum the technique is viewed as plausibly effective. This belief is bolstered by the fact that people reporting the receipt of campaign email in the 2004 Pew Internet survey were 10 percentage points more likely to vote than those reporting no email contact from campaigns.³

Second, voters themselves view email as increasing voter turnout. Eighteen percent of respondents thought "email and other material you saw or read online this year" encouraged them to vote (Pew 2005). Citizens were not merely passive consumers and acted upon the belief that email boosts turnout; 13% of citizens reported sending "emails urging people to get out and vote without reference to a particular candidate" and 12% "sent emails urging people to vote for a particular candidate" (Pew 2005). A sizable portion of the electorate clearly believes they are affected by email and act on the belief that others are as well.

The third reason is that direct mail has been demonstrated to be effective at increasing turnout. The non-partisan direct mail studied by Gerber and Green was essentially junk mail: highly impersonal, from an anonymous source,⁴ and easy to ignore. At its worst, email differs only in that it is slightly harder to walk to the waste basket than to hit the "Delete" button. Thus, email should be expected to affect turnout decisions in roughly the same manner as direct mail.

The fourth reason is that email elicits trust and cooperation in laboratory settings. Frohlich and Oppenheimer (1998) conducted a series of prisoner dilemma games where participants were randomly assigned to communicate either face-to-face, via email, or

² Kim and Margolis (2005) collected 99 emails sent in total from the Kerry campaign from July 1st through Election Day. The Bush campaign used email slightly less, sending only 73 messages during this period.

³ Controlling for interest in the campaign, age, and state reduces this figure to 9 percentage points.

⁴ A few of the mail experiments created fictitious organizations.

not communicate at all. Email exhibited two-thirds the effectiveness of face-to-face communication at increasing levels of cooperation. The extent to which this laboratory finding translates to voter turnout in the field is an empirical question this paper tests.

Finally, the political psychology literature has shown that trusted sources affect political attitudes (e.g., Lupia and McCubbins 1998, Druckman 2001). If trusted media sources can raise the perceived importance of topics in candidate evaluation (Miller and Krosnick 2000), it is possible that communication from a trusted source can similarly raise the perceived importance of voting and thereby boost turnout. A trusted source is certainly employed in the Working Assets study (described below) where members of an online community signed up to receive registration and turnout reminders from the organization. However, if demographic similarity is sufficient to constitute a trusted source (e.g., Kuklinski and Hurley 1994), then both the Youth Vote and Votes for Students studies (described below) satisfy this condition as the recipients of the email were young registered voters and students, respectively. Furthermore, hyperlinks embedded in emails make researching the emailing organization far easier than researching organizations that send direct mail. Thus, there is a good reason to expect email to have a small but demonstrable ability to mobilize voters.

To test this hypothesis, three non-partisan organizations conducted 13 experiments in three different elections to determine the usefulness of email for voter registration and increasing voter turnout.⁵ The organizations randomly divided their target audience into a treatment group, which received a series of emails encouraging registration and turnout, and a control group that received no attention from the organization. Given that the 13 combined experiments involve 232,716 subjects, any differences in registration and turnout between the treatment and control groups will be directly attributable to receipt of the email. However, no mobilization from email was detected with regards to either registration or turnout.

DESIGN

Unsolicited: Votes for Students 2002

Student names and email addresses were purchased from or donated by five universities during the 2002 congressional election.⁶ Students with valid email addresses were randomly divided into treatment and control groups (see the top panel of Table 1 for the

⁵ All of the experiments were non-partisan in nature, but prior research indicates that there is little difference between partisan and non-partisan messages with regards to voter mobilization (Nickerson *et al.* 2006, Panagopoulos n.d.).

⁶ The dual residency that often accompanies college education makes determining registration and turnout rates more difficult than in the other studies. In order to obtain unbiased estimates of email efficacy with regards to registration, both home and school address must be available for the treatment and control group. Because obtaining voter registration and history data for every region in the United States is impractical, only those students residing in specified regions are included in the analysis. This paring of the sample poses no problems for internal validity, but does raise some concerns over external validity (i.e., students from other regions of the country may behave differently). Given that the 58,311 remaining students from the five schools provide a large sample

Table 1. Effect of email on voter registration

Organization	Experiment	Treatment % registered	Control % registered	Treatment effect	<i>p</i> -value	
Votes for students	California Polytechnic Institute	25.0% [14730]	25.2% [1636]	-0.1% (1.1)	0.55	
	Eastern Michigan University	48.4% [3312]	49.7% [6652]	-1.3% (1.1)	0.89	
	University of Minnesota Twin Cities	50.5% [7305]	51.7% [389]	-1.2% (2.6)	0.68	
	University of California at Irvine	33.2% [7838]	33.7% [5160]	-0.4% (0.8)	0.70	
	University of Georgia-Atlanta	52.2% [10496]	50.1% [793]	2.1% (1.8)	0.12	
	Working assets	California	15.1% [74538]	15.2% [34062]	-0.2% (0.2)	0.76
		Colorado	71.0% [8124]	72.4% [3802]	-1.4% (0.9)	0.94
Michigan		52.5% [8196]	53.4% [3722]	-0.9 (1.0)	0.81	
Minnesota		58.7% [6000]	58.8% [2750]	-0.1% (1.1)	0.54	
Missouri		58.1% [2979]	60.7% [1352]	-2.5% (1.6)	0.94	
North Carolina		30.4% [9207]	31.0% [4351]	-0.6% (0.8)	0.76	
Clark County, NV		62.3% [1757]	63.9% [793]	-1.7% (2.1)	0.79	
Pooled			-0.4 (0.2)	0.96		

Numbers in brackets report *N*.

Numbers in parentheses report standard errors.

Reported *p*-values test one-tailed hypothesis that email mobilizes.

Pooled numbers were calculated using random effects meta-regression.

number of students assigned to each condition for each school in the experiment). The treatment groups received a battery of email messages encouraging voter registration and voter turnout, while students in the control group received no communication from Votes for Students prior to Election Day. Because the email addresses were provided by

of students from four disparate states, it is reasonable to conjecture that the results hold for a broad cross-section of college students.

the school administration, acting as the Internet Service Provider to the students, the email addresses were extremely accurate and only a handful of addresses were found to be undeliverable. Similarly, very few students made use of the “opt out” link in the Votes for Students email messages. Thus, for all practical purposes, every student in the treatment group received the assigned battery of email messages over the mobilization campaign.

Because an email message reaches a person’s inbox does not guarantee that the email message will be read. Given that between one-third and one-half of all email is unsolicited bulk mail, many subjects in the treatment group probably deleted the message from Votes for Students before reading it. The program used by Votes for Students allows the rate at which subjects opened email to be tracked.⁷ The average open rate across the five schools in the experiment was 20% with a high of 26% at the California Polytechnic Institute and a low of 11% at the University of Georgia in Atlanta. Thus, not every student assigned to the treatment group read the email, but it is also clear that the email campaign was not completely ignored.⁸

Just as campaigns design direct mail envelopes to convey information without being opened, subjects simply deleting the email from Votes for Students were minimally exposed to Get Out The Vote messages. The “From” line would have contained the name “Votes for Students” and the “Subject” line contained a summary blandishment to vote. So, it is possible that merely being sent an email by the campaign might boost turnout without reading the full text. Because of the uncertainty surrounding how many subjects opened the email and questions regarding what precisely constitutes a successful application of the treatment, the boost in voter turnout from *reading* an email encouraging voting is not estimated. Rather, the effectiveness of the overall campaign is evaluated, measuring the registration and turnout increase for each email *sent* by the campaign.

The email campaign consisted of two phases. The first phase sent an introductory email from Votes for Students that also encouraged recipients to register to vote. A link was provided to information on how to download the proper forms. The same email also encouraged people already registered to vote to apply for an absentee ballot. Given the number of students who lived away from their voting jurisdiction (i.e., parents) the hope was that early encouragement would boost absentee voting rates. This email was sent before the registration deadline in each state. The second phase of the campaign began a week prior to Election Day and encouraged turnout. The numerous emails used humor and startling facts to engage readers. The low opt-out rate and high open rate suggests that Votes for Students succeeded in this goal.

⁷ A message was counted as read if it is opened in an html compatible email program (i.e., web mail). Subjects using Microsoft Outlook will overstate the open rate because previews of the email when the user highlights the subject line will be counted as opening the email. Subjects using non-html compatible email programs (such as Unix based systems like Pine) or programs where open reporting is forbidden will understate the open rate.

⁸ Direct mail experiments suffer from the same problem, but determining which subjects actually read the correspondence is impossible.

Opt Out: Youth Vote 2003, Houston

The target sample consisted of roughly 145,000 registered voters under the age of 26 in Houston. The list of registered voters was given to a political consulting/lobbying firm specializing in Internet outreach, who matched registered voters against the subscriber list of large Internet Service Providers in the area (e.g., A.O.L., Earthlink, and Juno) that were not educational institutions (i.e., no .edu suffixes). The firm indicated that the email addresses were only for individuals who at some point indicated that they were willing to receive messages from third parties. The 13,185 (a yield of roughly 9%) subjects with a matching email addresses were then emailed an invitation to participate. The 12,772 individuals who did not opt out over the next few days (a yield of 98%) were then randomly placed into treatment and control groups.⁹

These subjects were then sent a brief welcome email and divided into evenly balanced treatment and control groups.¹⁰ The treatment group in each city was sent a series of three emails leading up the election. Emails typically began with a short quiz and an invitation to explore the YouthVote website. The conclusion of the email included a brief blandishment to vote. Unfortunately, open rates are not available for each email because of the lack of technology within the campaign. However, the click through rates to the Youth Vote website (5%–8% for each email) are available and demonstrate that a substantial subset of the treatment group not only read the emails, but were sufficiently interested to seek further information.

Opt In: Working Assets 2004

Unlike the first two campaigns, Working Assets sent email only to those individuals who came to the Working Assets website and specifically requested to be on a mailing list reminding individuals to register and vote. People signed up from all over the nation, but 161,633 people signed up from the seven experimental areas. After individuals entered name and address information into the website, the subject possessed a 68.5% chance of being assigned to the treatment group and receiving a series of emails from Working Assets encouraging registration and turnout.¹¹ Open rates were not recorded, but given the trusted nature of the email source and the vibrant nature of Working Assets' online community, it is likely that the emails were read by a large percentage of the subjects assigned to the treatment group.

The chief complication of the Working Assets experiments is that subjects self-entered data in contrast to the Votes for Students experiments, which relied upon standardized school databases, and the Youth Vote experiment that used official voter lists.¹² Data entry fields helped to standardize the data (e.g., street number, street direction, etc.),

⁹ With an opt-out rate of only 2%, the email experiment is scarcely different from receiving unsolicited email.

¹⁰ A randomization check shows no correlation of note between past voter history and assignment to the treatment group. The chi-squared yields a *p*-value of 0.41.

¹¹ Randomization checks comparing treatment and control groups for age, missing data, and past voter history when available found no systematic differences between the two groups.

but irregularities still occurred in naming conventions (e.g., “Bob” instead of “Robert”). Efforts were made to crosscheck standard name and address variants. Ultimately, 44,681 experimental subjects were positively identified on the official list of registered voters, or 28% of the subjects in the Working Assets experiments.¹³ Since these individuals were randomly assigned to treatment and control groups, the sample truncation poses no threat to internal validity. External validity is another matter; the experiment is silent on the ability of email to mobilize individuals who did not accurately input name or address information.

The next section presents the results of, first, the registration portion of the experiment, and then turnout among those individuals registered to vote.

RESULTS

Table 1 summarizes the registration results from the 12 Votes for Students and Working Assets experiments. In 11 of the 12 experiments, no mobilization effect was detected. The University of Georgia at Atlanta experiment conducted by Votes for Students was the only experiment where the treatment group was registered to vote at a higher rate than the control group. This result is not statistically significant and likely a result of sampling variance since UGA–Atlanta was the school with the lowest open rate. Thus, there appears to be no difference in behavior between subjects who received unsolicited email and subjects who opted to receive email. Each individual experiment is noisy and many of the standard errors are larger than the coefficients, which is the pattern one would expect to find in a null result. Taken together, the resulting estimate of the effect of email on voter registration is a fairly precise -0.4 percentage points with a standard error of 0.2.

The picture is similar for voter turnout among those registered (see Table 2). In only five of the thirteen turnout experiments did the treatment group vote at higher rates than the control group. For all three types of subject recruitment strategies (i.e., unsolicited, opt-out, and opt-in), the bulk of the experiments point toward no mobilization

¹² One advantage of the self-entered data is that Working Assets possessed the most accurate information on the subject’s residence. Voter registration rolls often include deadwood, and consumer databases possess a lag in updating addresses for individuals. If a person were to use Working Assets’ registration tool to register at a new address, consumer databases may not know where to find the individual and voter files would only be updated if the person moved within the same state or county. In contrast, Working Assets would know the address to match against turnout lists because the person provided Working Assets with the most up to date information at the beginning of the campaign.

¹³ Skyline Public Works matched lists of individuals registered to vote by civic organizations focusing on youth. On average, 73% of these individuals were matched, but missing and nonstandard data caused match rates as low as 32% for some of the groups. Skyline matched against data directly taken from the voter registration cards, whereas the subject inputted data used by Working Assets may have differed from official voter files. Thus, the 28% match rate for Working Assets may be typical of such campaigns.

Table 2. Effect of email on voter turnout among registered voters

Organization	Experiment	Treatment % turnout	Control % turnout	Treatment effect	<i>p</i> -value	
Votes for students	California Polytechnic Institute	34.7% [3688]	32.0% [412]	2.7% (2.5)	0.14	
	Eastern Michigan University	48.6% [1604]	49.4% [3309]	-0.8% (1.5)	0.71	
	University of Minnesota Twin Cities	70.7% [3687]	66.2% [201]	4.5% (3.3)	0.08	
	University of California at Irvine	31.3% [2606]	32.7% [1738]	-1.4% (1.4)	0.84	
	University of Georgia-Atlanta	25.9% [5478]	27.5% [397]	-1.6% (2.3)	0.75	
	Youth vote coalition	Houston	9.0% [6386]	9.2% [6386]	-0.3% (0.5)	0.69
	Working assets	California	86.5% [11237]	87.1% [5192]	-0.6% (0.6)	0.85
Colorado		89.9% [5771]	90.1% [2753]	-0.2% (0.7)	0.62	
Michigan		90.2% [4303]	89.5% [1986]	0.7 (0.8)	0.18	
Minnesota		94.4% [3522]	94.2% [1617]	0.1% (0.7)	0.43	
Missouri		56.1% [1731]	56.8% [820]	-0.7% (2.1)	0.64	
North Carolina		85.4% [2799]	86.6% [1349]	-1.2% (1.2)	0.85	
Clark County, NV		88.1% [1094]	87.8% [507]	0.3% (1.7)	0.42	
Pooled			-0.2 (0.3)	0.79		

All subjects in the above analysis are registered to vote.

Numbers in brackets report *N*.

Numbers in parentheses report standard errors.

Reported *p*-values test one-tailed hypothesis that email mobilizes.

Pooled results were calculated using random effects meta-regression.

effect. Again, each individual experiment is inconclusive, but once the results are pooled together a relatively precise estimate of -0.2 percentage points with a standard error of 0.3 emerges. Thus, email does not appear to be an effective means of moving registered voters to the polls.

CONCLUSION

Given that direct mail shares many characteristics with email and is effective at engaging voters, it is surprising that email neither increases registration rates nor boosts voter turnout. The null finding is especially surprising in the Working Assets experiment where the sender is a trusted source that existing literature suggests should be maximally persuasive. These experiments strongly suggest that political campaigns employing email as a get out the vote tool are wasting their time. Email is a low cost form of campaign communication, but it is also of little benefit for registration and turnout. Email may be useful for other campaign activities (e.g., organize rallies, solicit donations, disseminate information), but those topics are beyond the scope of this inquiry.

Readers should resist the temptation to conclude from Table 1 that email actively *decreases* voter registration rates because the 95% confidence interval surrounding the -0.4 percentage point intent-to-treat estimate barely encompasses zero. Psychological mechanisms by which email deterred registration could be hypothesized, but most would strain credulity. If emails were truly demobilizing, then campaigns should send email to the supporters of opponents in order to decrease registration rates. That such a strategy would be ridiculed affirms the implausibility of the demobilization interpretation of the data. The evidence does not permit confidently rejecting the null hypothesis of no effect.

Several alternative interpretations of the findings are noteworthy. Skeptics may argue that email is effective at registering people to vote and subjects registered at addresses the experimenter cannot track. Thus, the conclusion that email is ineffective is erroneous because the technology registered members of the treatment group into unobservable areas. This interpretation of the findings does not appear to hold for either set of registration experiments contained in this analysis. In the Votes for Students experiments, both the campus and home addresses are included for each subject. Given the amount of communication that colleges send to home and campus addresses, both addresses are likely to be correct. In order for the unobserved registration hypothesis to be true, one would have to believe that either email was effective only for those individuals for whom the university did not have accurate addresses or these individuals moving out of the experiment are more numerous than the other subjects. Neither explanation seems plausible. Similarly, subjects presumably entered current contact information into the Working Assets database. In order to obscure a powerful positive treatment effect, thousands of people would have to enter data, move in the next few weeks, and then be reminded by the treatment email to register at the new address. The scenario is convoluted and unlikely to be true. The simplest interpretation is that email is ineffective as a voter mobilization tool.

Variation across the experiments conducted by the three organizations assuages some legitimate concerns about the external validity of these findings. Votes for Students focuses on college students and the Youth Vote Coalition targeted non-college youth under the age of 26, but the Working Assets experiments spanned all ages with a mean age in the late 30s. The studies were conducted in mayoral, congressional, and presidential elections. Votes for Students targeted email addresses with educational affiliations, but Youth Vote and Working Assets sent email to every type of account. Subjects in the

Working Assets experiment self-selected into the program, but self-selection is not an issue for either the Youth Vote Coalition or Votes for Students experiments. None of the organizations detected any increase in registration or turnout. Furthermore, political campaigns face the exact same hurdles in collecting and matching email addresses as these organizations experienced in conducting the experiments (Bimber and Davis 2003). It is possible that some audiences are more receptive to email outreach than others, but the results of these 13 experiments describe the behavior of a broad cross-section of voters.

The experiments demonstrate that email can have at most a modest effect on voter registration or turnout. Suppose that email boosted voter turnout by 0.1 percentage points, which would be near the upper limit of the 95% confidence interval and 13 times less effective than leaflets. To create one vote, a campaign would be forced to send 1000 emails to registered voters. At 10 cents to obtain each address (considerably less than most consultants charge), a single voter would cost \$100 making email more expensive than face-to-face canvassing (Green *et al.* 2003), volunteer phone calls (Nickerson 2006), leaflets (Nickerson *et al.* 2006), and even direct mail (Gerber *et al.* 2003). Thus, the notion that email is attractive because of the low expense is incorrect.

Moreover, email can only alter the outcome of the very largest campaigns. Campaigns target only likely supporters for getting out the vote efforts. If a campaign emailed one-third of registered voters in an average size House District (i.e., 500,000) and email boosts turnout by 0.1 percentage points, only 175 votes would be created. A similar strategy in a large state like Ohio (i.e., 6,000,000 registered voters) would yield roughly 2000 votes – a sum that could swing an election in only the most extreme circumstances. Thus, even under this best-case scenario, email would prove ineffective as a strategy for mobilizing voters in most elections.

These results fit neatly into the pattern of voter mobilization results where the effectiveness of a technology is directly proportional to its personalized nature (Nickerson 2007). High cost and relatively intimate face-to-face contact successfully moves people to the polls, whereas direct mail does little to change behavior. Given the ubiquity of unsolicited email and the low transaction costs associated with the medium, email should exhibit little success in mobilizing voters and this expectation is borne out. Thus, the study constitutes confirmation of a prediction made by the personalization hypothesis of voter mobilization. That said, it is somewhat surprising that no mobilization effect was detected. This finding suggests that there is a lower limit to how impersonal a mobilization message can be and still move people to the polls.

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