

Hunting the Elusive Young Voter

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ABSTRACT. It is generally assumed that mobilizing young voters is infeasible. This belief can be broken into two separate questions: (1) Are young people responsive to Get Out The Vote appeals? (2) Is it possible to deliver the message to the intended audience? By pooling together the results of six GOTV field experiments, this paper demonstrates that young voters are equally responsive to blandishments to vote as mature voters, but that they are roughly three times more difficult to contact. [Article copies available for a fee from The Haworth Document Delivery Service: 1-800-HAWORTH. E-mail address: <docdelivery@haworthpress.com> Website: <<http://www.HaworthPress.com>> © 2006 by The Haworth Press, Inc. All rights reserved.]

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Research dating to the 1940s has consistently shown that young citizens of the United States vote at lower rates than older citizens and are

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less likely to feel connected to the electoral process. Low voter turnout among young citizens has grown more acute since 1972, when the nationwide voting age was lowered to 18. The proportion of eligible citizens in the age group 18-24 who went to the polls declined from 49.6% in 1972 to 40.8% in 1984 to 32.4% in 1996 (US Census Bureau 2002), three presidential elections in which incumbents won by large margins. In fact the decline in overall turnout since 1972 is largely attributable to entering cohorts voting at lower rates than their predecessors (Chen 1992, 142-147).¹

Low turnout among any group is interesting (and worrisome), but youth are the focus of this analysis for two primary reasons. The first reason is logistical: it is easy to isolate young registered voters from the rest of the population. Since one must be at least eighteen years of age to vote in the United States, most jurisdictions record birth date on voter registration databases and make the files publicly available. The second reason is theoretical: voting has been shown to be habit forming (Plutzer 2002; Gerber and Green 2001; Green and Shachar 2000). It is unknown whether it is possible to increase the rate of turnout among those who are not in the habit of voting because they are young or whether various mantles of adulthood need to be adopted before individuals become responsive to blandishments to vote. If young people can be convinced to vote now, voter turnout rates are likely to benefit for years to come.

Political parties undertake the vast majority of voter mobilization and this outreach largely ignores young persons in the United States. Because young people vote at low rates, parties assume efforts to boost turnout will not be as cost effective and concentrate on higher turnout age groups (Bennett 1991). As campaigns attend to other constituencies, young voters are neither central to the framing of campaign issues nor the object of voter mobilization efforts. The net result is a vicious cycle where young people feel alienated from the campaign process and vote at lower rates, which then causes campaigns to ignore young citizens further. This cycle disconnects the political world from young citizens. The assumption of political parties that young persons cannot be mobilized will be tested in this paper and the feasibility of such mobilization efforts will be evaluated.

The feasibility of boosting youth voter turnout will be broken into two component parts: (1) Once contacted will youth be receptive to blandishments to vote and turn out at higher rates? (2) Will attempts to contact young people frequently hit the intended target, that is, are they cost effective? There is good reason to believe that Get-Out-The-Vote (GOTV) will not be well received by youth. Recently eligible voter

cohorts are more cynical and less trusting of government and political parties than prior cohorts were when they came of political age (Bennett 1997). Cynical, inattentive, and apathetic are not adjectives normally associated with populations easy to mobilize.² Similarly, attempts to contact young voters may be frustrated. Young people in the United States are not residentially stable (Wolfinger and Rosenstone 1980; Squire, Wolfinger, and Glass 1987; Highton and Wolfinger 2001), which means that contact lists are likely to be inaccurate. Also, it is plausible that persons under the age of thirty are less likely to be home because they exhibit a more transient social life and are less likely to have responsibilities (such as children) at home.

It will be shown that the obstacle to mobilizing young American voters is not lack of responsiveness to the message, but rather reaching the audience to give the message in the first place. The demonstration of this thesis will rely upon door-to-door canvassing experiments from six cities where age is available as a covariate.³ By partitioning the sample into subjects under the age of thirty and those thirty and over, it is possible to determine whether face-to-face GOTV appeals have the same power for young voters as older voters.⁴ After establishing any difference in response to GOTV appeals, the difficulties in targeting and contacting youth will be discussed.

RESPONSE TO THE MESSAGE

To gauge the responsiveness of young persons to GOTV appeals a meta-analysis of six randomized controlled experiments will be performed. Unlike studies of voter mobilization that examine the correlation between voting and contact with campaigns, experimental studies randomly manipulate whether voters are approached by campaigns. Experimental control eliminates two problems associated with observational data that is not experimentally manipulated. First, if campaigns target voters who are especially likely to go to the polls, the observed correlation between contact and voter turnout may be spurious. Second, if respondents' recollection of whether they were contacted is vague or distorted, the correlation between self-reported contact and turnout will misrepresent the true causal influence of contact. It should also be noted that because subjects are randomly assigned to treatment and control groups, both observable and unobservable characteristics associated with voting are proportionally represented in the treatment and control

groups, thus obviating the need for control variables to generate unbiased estimates of the treatment effect. (For an explanation of the estimators used in the analysis, see Appendix A.)

The canvassing experiments were conducted in six separate elections: a school board election in Bridgeport, CT; a mayoral race in Detroit, MI; a large slate of city posts in Minneapolis, MN; a Congressional election in New Haven, CT; a city council and mayoral race in Raleigh, NC; and a mayoral election in St. Paul, MN. The experimental protocol used was virtually identical in each case. Households from the official list of registered voters were placed into treatment or control groups. One individual was randomly selected from each household in the treatment group to be listed on the sheet that canvassers used to record with whom they spoke. The particular pitches given on each doorstep varied slightly between sites, but all contacts were informal and brief. Canvassers were equally inexperienced and received roughly equivalent training.⁵ The New Haven experiment was conducted in 1998; all the others took place in 2001.⁶ With the exception of Raleigh, the protocols were executed faithfully with minimal problems.⁷ After recording which subjects in the treatment group were contacted, the official voter history of each person in the treatment and control group was gathered from the county clerk. A major strength of the design is that neither the contact nor the voting relies upon the recollection and self-reporting of the subjects in the experiment.

Each of the studies is statistically powerful enough to demonstrate the efficacy of face-to-face contact upon the general populace, but no single one of them contains sufficient numbers of young people to determine if they react differently to the treatment. For example, the 1998 New Haven experiment contains 4,500 people under the age of 30 who respond to the treatment at one-third the rate of the general population (see Table 1), but the associated standard error is so large that the difference is not distinguishable from chance. However, when pooled with results from field experiments conducted in other cities the number of young people available increases to 7300.

Pooling assumes equal treatment effects across time and place. It might strain credulity to believe that inner-city Detroit and suburban North Carolina have the same response to a knock on the door. Each setting may have a unique political and social culture that alters the way home visits and encouragement to vote are interpreted. Similarly, Congressional, mayoral, and city council elections may be expected to exhibit different magnitudes of treatment effect, given the wide variance

TABLE 1. Results for Six Field Experiments Partitioned by Age

Election Type	New Haven House		Bridgeport School Board		Detroit Mayor	Minneapolis Mayor / City Council		St. Paul Mayor	Raleigh Mayor / City Council	Pooled W/O Raleigh	Pooled W/ Raleigh
	No	Yes	No	Yes	Yes	No	Yes	Yes	Yes		
Competitive											
Number of Subjects	4501	782	372	782	782	892	498	855			
Assigned Treatment	19%	50%	47%	50%	50%	50%	49%	40%			
Control Turnout	19.4%	19.9%	3.0%	19.9%	19.9%	12.2%	12.8%	9.1%			
Treatment Turnout	20.1%	24.2%	6.3%	24.2%	24.2%	14.7%	16.2%	8.6%			
Contact Rate	18.4%	31.9%	24.1%	31.9%	31.9%	18.3%	27.8%	37.8%			
Treatment Effect	3.6% (8.1)	13.3% (8.9)	13.6% (7.4)	13.3% (8.9)	13.3% (8.9)	13.7% (12.0)	12.0% (10.8)	-1.4% (5.3)	10.9% (4.0)	6.4% (3.2)	
Youth											
Number of Subjects	24879	4412	1528	4412	4412	1973	1648	3896			
Assigned Treatment	20%	50%	50%	50%	50%	50%	50%	35%			
Control Turnout	49.4%	46.8%	11.4%	46.8%	46.8%	30.4%	44.9%	33.7%			
Treatment Turnout	52.0%	48.4%	15.2%	48.4%	48.4%	32.2%	49.2%	33.1%			
Contact Rate	29.6%	30.8%	28.7%	30.8%	30.8%	18.6%	34.6%	47.1%			
Treatment Effect	9.0% (2.7)	5.3% (4.9)	13.3% (5.7)	5.3% (4.9)	5.3% (4.9)	9.6% (11.1)	12.4% (7.1)	-1.2% (3.4)	9.2% (2.0)	6.4% (1.7)	
Difference Between Young and Old	5.3% (8.5)	-8.0% (10.2)	-0.3% (9.3)	-8.0% (10.2)	-8.0% (10.2)	-4.2% (16.4)	0.4% (12.9)	0.2% (6.3)	-1.7% (4.5)	0.0% (3.6)	
P-value	0.73	0.22	0.49	0.22	0.22	0.40	0.51	0.51	0.35	0.5	

Numbers in parentheses are standard errors.
P-values test the one tailed hypothesis that young people are more difficult to mobilize.

in party activity and election salience. These are valid concerns and to that end results for each site are reported separately.⁸

Table 1 summarizes the experiment in each city and compares the treatment effect for both the mature and young samples (see Appendix A for an explanation of how the treatment effect is calculated). The estimated treatment effect for both youth and mature is nearly 10%. That is, if 100 citizens were met face-to-face, 10 additional votes would be created. The standard errors associated with these estimates are small enough to conclude that the mobilization detected is not due solely to chance. The standard errors associated with the treatment for younger voters are larger than the standard errors for mature voters. This is most likely due to smaller sample sizes, but it is also possible that young citizens are more varied in their response to face-to-face canvassing. The differences between the two sub-populations vary considerably across experiments.⁹ Any one particular experiment would hardly be definitive, but the seven taken together allow the bounds of our uncertainty to shrink. Since we are only concerned with demonstrating that young people are at least as mobilized by GOTV appeals, the p-values reported are one-tailed.

Comparing across sites, the consistency of the treatment effect for both the young and mature samples is remarkable.¹⁰ The only site that appears to be an outlier is Raleigh, which was the site where implementation of the protocol was not smooth. In every analysis conducted in this paper, the odds of the Raleigh effect being drawn from the same sample as the other cities are extraordinarily small (see Appendix B for an example). It is the opinion of the author that the problems associated with conducting the experiment make Raleigh non-comparable to the other five sites. However, the Raleigh results are reported so as to allow the reader to decide for herself.

The rightmost columns of Table 1 report pooled results for the difference between mature and young voters. The conclusion drawn is that young people are equally mobilized by contact as mature individuals. We cannot definitively reject the hypothesis that young people are less effectively mobilized than mature people conditional on contact though the standard error is small enough to guarantee the more modest conclusion that it is possible to mobilize young voters. The best estimate is that 109 new voters are created for every 1,000 contacted. If Raleigh is included in the pooled analysis, this number drops to 64 votes created.

The estimates in Table 1 rely upon a simple comparison of the rates of turnout between the treatment and control groups. The randomized nature of the design ensures that the results obtained are not biased. However, it

is possible to increase the precision of the estimates by including control variables, such as past voter participation and neighborhood, and moving to a two-stage least squares estimator (see Appendix A). Pooling voter history is tricky since experiments differ in time and place and not every voter is eligible for every election (e.g., registered independents often cannot vote in primaries). Additionally, the extent of the voter history varies from county to county. To solve these problems, the only history controlled for is whether or not the person voted in the prior Presidential election.

Table 2 reports the 2SLS results. Here the value of pooling is evident. While the older samples have sufficient size to obtain reliable results, none of the youth samples possesses similarly small confidence intervals. Only when the experiments are pooled together can precise results for young voters be obtained. The estimated effect of GOTV contact on youth (with the exception of Raleigh) is remarkably consistent across sites. The estimated mobilization effect also is only slightly smaller than that found in Table 1, concluding that 95 additional voters turn out for every 1,000 contacts.

The results of the 2SLS model suggest that older voters may be slightly more responsive to GOTV appeals than younger voters. However, the estimated difference is a mere 0.5%, which is within a standard error of both the youth and mature estimated treatment effect. Thus, young residents likely are only slightly more difficult to mobilize than their older counterparts.

Neither of the two models discussed thus far has accounted for the dichotomous nature of voting. The major problem is OLS may produce inadmissible predictions that voting will occur with less than zero probability. Rivers and Vuong (1988) propose a two-stage conditional probit estimator that parallels the instrumental variables estimator described above. The probit transformation ensures that predicted vote probabilities are confined to the range between 0 and 1, which is especially important since our focus is on a low participation group and a few of the experiments exhibited very small rates of turnout (e.g., Bridgeport).

Table 3 presents the two-stage conditional probit coefficient estimates for both young and mature voters in each site. The coefficient estimates of contact for both young and mature voters are highly significant and substantively large. If the baseline rate of voting were 50%, contacting a young registered voter would increase the likelihood of voting to 64.8%, a 14.8 percentage point increase. Given that young people do not vote at high rates, it is useful to consider the magnitude of

TABLE 2. Two-Stage Least Squares Analysis of Six Field Experiments Partitioned by Age

	New Haven	Bridgeport	Detroit	Minneapolis	St. Paul	Raleigh	Pooled W/O Raleigh	Pooled W/ Raleigh
Youth								
Contact	0.061 (0.082)	0.109 (0.086)	0.070 (0.082)	0.153 (0.116)	0.129 (0.111)	-0.004 (0.053)	0.095* (0.041)	0.057* (0.033)
N	4501	353	723	880	509	826		
Mature								
Contact	0.096** (0.026)	0.152** (0.059)	0.085** (0.039)	0.095 (0.099)	0.109* (0.068)	-0.003 (0.033)	0.100** (0.019)	0.074** (0.016)
N	24879	1453	4231	1947	1699	3834		
Difference	0.035 (0.086)	0.043 (0.104)	0.015 (0.091)	-0.058 (0.153)	-0.02 (0.130)	0.001 (0.062)	0.005 (0.045)	0.017 (0.037)
P-value	0.66	0.66	0.57	0.35	0.44	0.51	0.46	0.32

Each 2SLS model controls for voting in the prior Presidential election and neighborhood (the unit of randomization).

Numbers in parentheses represent standard errors.

P-values test the one tailed hypothesis that young people are more difficult to mobilize.

* means coefficient significant at < 0.05; ** means coefficient significant at < 0.01.

TABLE 3. Two-Stage Conditional Probit Analysis of Six Field Experiments Partitioned by Age

	New Haven	Bridgeport	Detroit	Minneapolis	St. Paul	Raleigh	Pooled W/O Raleigh	Pooled W/ Raleigh
Youth								
Contact	0.207 (0.301)	1.023 (1.401)	0.448 (0.404)	0.695 (0.684)	0.533 (0.553)	0.030 (0.384)	0.380* (0.208)	0.301* (0.183)
N	4501	353	723	880	509	826		
Mature								
Contact	0.254** (0.069)	0.678* (0.337)	0.422** (0.157)	0.261 (0.364)	0.325* (0.194)	-0.033 (0.106)	0.297** (0.058)	0.220** (0.051)
N	24879	1453	4231	1947	1699	3834		
Difference	0.047 (0.309)	-0.345 (1.441)	-0.026 (0.433)	-0.434 (0.775)	-0.208 (0.586)	-0.063 (0.398)	-0.083 (0.216)	-0.081 (0.286)
P-value	0.56	0.41	0.48	0.29	0.36	0.44	0.35	0.39

Each analysis controls for voting in the prior Presidential election and neighborhood. Numbers in parentheses represent standard errors. P-values test the one tailed hypothesis that young people are more difficult to mobilize. * means coefficient significant at < 0.05; ** means coefficient significant at < 0.01.

the treatment effect at lower levels of baseline turnout (see Table 1). Detroit's youth in the control group voted at a rate of 20%, face-to-face contact would be estimated to raise the rate of turnout to 32.2%. Contact would raise rates of turnout for a 10% baseline (near Minneapolis and St. Paul) by 84% to 18.4%. Effects can be seen even at extremely low rates of turnout as in Bridgeport where only 3% of young registrants went to the polls, but a knock on the door provides a 123% increase to 6.7%.

The precise estimates at very low levels of turnout should be taken with a grain of salt. The probit model weighs small increases in the tails of the distribution more heavily than would a linear probability model, which assumes a constant effect across the range of the independent variable. However, looking across Table 1 it is not clear that low turnout groups are less mobilized in an absolute sense. The relative increase in turnout because of contact in Bridgeport is enormous (i.e., 3% to 16% is a 533% increase), but the absolute increase (a 13% percentage point treatment effect) is at least as large as most other sites. For this reason, it is not clear that the two-stage conditional probit model is superior to 2SLS in this instance. Regardless, we can conclude that turnout rates will remain low, but it is definitely possible to mobilize young voters. In fact, the probit results suggest that youth may be easier to mobilize than other low voting groups in the United States.

Comparing the effectiveness of contacting young registrants to older registrants, the two-stage probit model supports the earlier finding that youth are easier to mobilize. In four of the seven experiments youth were found to be easier to mobilize than older voters. However, even pooled together this finding does not approach statistical significance. At a baseline rate of turnout of 50% the difference is only 3.3%, but it does make it unlikely that young registered voters are more difficult to mobilize than older cohorts.¹¹

Thus, the accepted wisdom that young people are not responsive to appeals to vote is false. It should be noted that the above analysis only considers those youth registered to vote. It is possible that those individuals not registered are more alienated and less likely to respond to blandishments to vote. Unfortunately, this is a hypothesis that is beyond the scope of the data collected. It can be concluded that registered American youth are at least as responsive to such exhortations as the older voters. However, the next section will demonstrate that mobilizing younger registrants is costly because they are difficult to contact.

TRACKING YOUNG VOTERS

It is simply difficult to track down young people to deliver the mobilization message. Young people literally represent moving targets. Between March 1999 and March 2000, over 43 million people, roughly 16% of the population, changed residence according to the Census Bureau. Among 20-29 year olds, over one-third moved—over twice the rate of all other age groups (U.S. Census Bureau 2000). This means that databases need to be updated frequently to account for changing addresses and phone numbers. Since common sources for voting information are databases whose updates depend upon the US Postal Service Change of Address form and since young people seldom file this form, the contact information listed for young people is disproportionately incorrect. Political parties would have to spend considerable energy simply finding young party members, whereas reliable phone and address lists for mature voters are readily available.

Even the largest consumer research firms cannot reliably maintain lists of young people. For example, over a non-random sample of 18 counties across the United States,¹² the Census Bureau counted 1,652,057 people between the ages of 18 and 24 residing in the counties. However, InfoUSA, a large and well-regarded vendor of consumer lists, could provide an address or phone number for only 500,492 individuals in that age group or 30% in the age category. InfoUSA has records for roughly 250 million people in this country, that is, 87% of the population.¹³ Since people in the age group 18 to 24 constitute 9.6% of the population and 70% of them are not listed, this age group accounts for almost exactly half of the individuals missing from the database. While this age group is not attractive to political parties, it is very attractive to marketers. Consumers in their twenties are among the most lucrative demographic groups to target; if a large consumer research firm with substantial profits to gain cannot do so, it is folly to expect political campaigns to succeed.

Political parties usually rely upon middlemen to supply the names and addresses of potential supporters/voters. These firms generally buy information from source firms like InfoUSA in order to update lists and fill in useful information, so the middleman lists cannot be more inclusive than the source list. Additionally, these middlemen typically fail to weed out completely incorrect numbers and addresses for individuals. Invalid and incorrect phone numbers waste the time of parties trying to mobilize their constituency prior to the election.

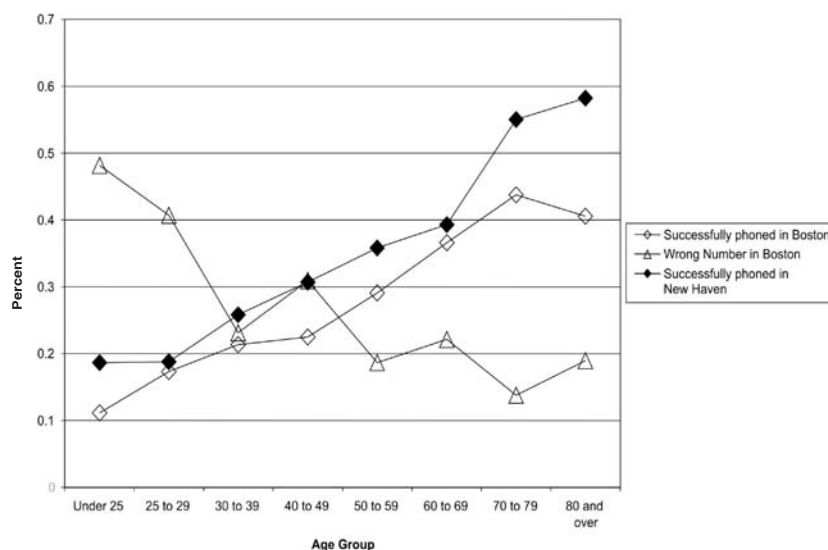
The 1998 New Haven experiment (Gerber and Green 2000) contained a phone portion in addition to the door-to-door portion and used the services of Survey Sampling, Inc to provide phone numbers for registered voters in the city.¹⁴ The residents were called by a professional phone bank frequently employed by political campaigns. The firm was also used for a similar GOTV phone campaign in Boston in 2001 involving 10,000 registered voters. In this instance, the calls were made by a coalition of local environmental groups that were experienced in conducting mobilization campaigns. In each case, Survey Sampling provided its most up-to-date listing of phone numbers available.

Figure 1 depicts the rate of contact with respondents by age group. For both New Haven and Boston, the odds of being contacted by the phone campaign are low for 18-24-year-olds (19% in New Haven and 11% in Boston) and rapidly increase among older age groups. In fact, the odds of a seventy-year-old being spoken to (55% in New Haven and 44% in Boston) are roughly three times greater than the odds of a twenty-year-old being contacted. The hypothesis that the correlation between age and contact is due entirely to chance can be rejected at the 0.01 level.¹⁵

It is possible that younger people are less likely to be at home during the hours that calls were made or that young people screen their calls more assiduously than old people. But much of the problem has to do with incorrect numbers. The callers in Boston also kept record of which households and phone numbers were those of incorrect people (see Figure 1).¹⁶ Here we see the inverse relationship; young registered voters are more than 3 times as likely as older voters to have the incorrectly listed phone number (48% v. 14%). The hypothesis that the correlation between age and wrong numbers is due to chance is also rejected at the 0.01 level.¹⁷ It should be further noted that these wrong numbers are understated since they represent only those households that the callers could confirm were incorrect. There were certainly other incorrect numbers where callers received answering machines, busy signals, or no answer. The large quantity of wrong numbers frustrates callers and represents wasted resources for a calling campaign. Given the poor quality of the numbers being provided for young people (at least half were invalid in this sample) to political parties, it would make economic sense to concentrate on older voters.

Parties also rely upon the county clerk for lists of registered voters. Here again, the information for young people is not nearly as reliable as that available for older people. County election boards must follow strict rules in the maintenance of the voter rolls and generally update entries only when the voter takes the time to fill out the correct forms (often at the polling place on election day-too late for the parties to contact

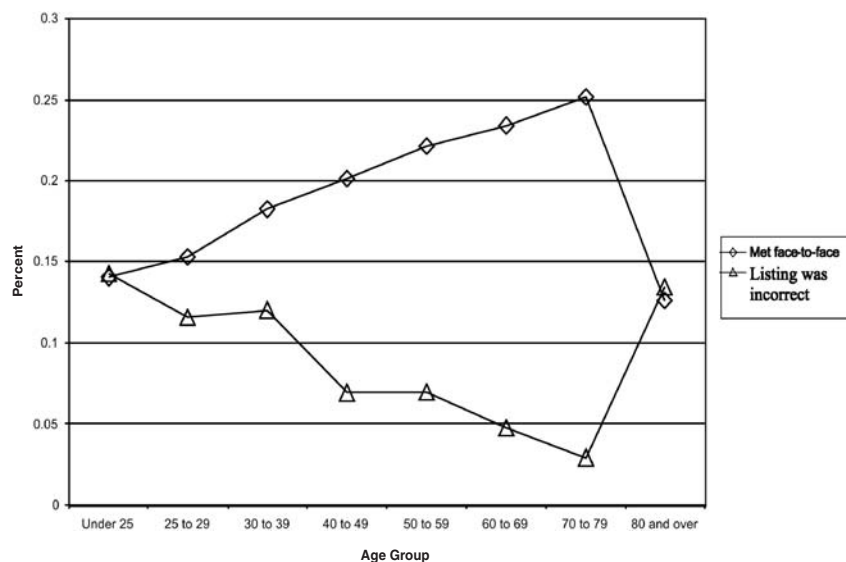
FIGURE 1. Phone Contacts by Age in New Haven and Boston



the individual). The five studies door-to-door experiments conducted in 2001 relied upon county clerk records. The difficulty in reaching young voters instead of mature voters can be gauged by comparing the rates of successful contact for different age groups (see Figure 2).

As in the earlier phone example, the likelihood of being contacted is very low for the youngest voters (14%) and much higher for the older cohorts (25% for people in the age group 70-79). This difference is not entirely attributable to incorrect information, but poor quality is definitely a major component. Figure 2 also presents the percent of cases where the canvasser could determine that the person no longer lived at the address (i.e., the new resident confirmed that the intended subject no longer resided there). Again, the odds of the information being incorrect are almost five times greater for those persons in the age group 18 to 24 than for those in the age group 70 to 79 (14% vs. 3%). In fact, the likelihood of contacting the intended young voter was equal to the likelihood of discovering she had moved. In contrast, a canvasser was 12 times more likely to contact the intended 70-year-old than to discover she had moved.¹⁸ The fact that a disproportionate amount of the dead wood on the official voter files consist of young people makes targeting this

FIGURE 2. Pooled Contact Rates for Five Sites by Age



demographic group a dubious allocation of resources for a campaign intent on mobilizing voters and winning an election.

It should be noted that deadwood in voter rolls is not necessarily a bad thing. Efforts to clean up the files usually are based upon recent voting activity and periodic postcards. This purging of the files inevitably eliminates poor residents much more often than wealthy ones primarily because the poor may abstain from many elections in a row (a common purging criterion) and move more (hence, attempts by the board of elections to locate the resident are likely to be thwarted). So county clerks are faced with two undesirable options: either have accurate lists that disenfranchise powerless segments of the population or accept that the voter files are not up to date and cannot be trusted. In either case, it is difficult to target younger residents.

The preceding analysis considered only registered voters. As Timpone (1998) points out, the registration process is often the major barrier to voting. The people who are registered to vote are likely to be easier to track down and contact than those individuals who are not even registered. Since young people are registered at much lower rates than are more mature adults, the problem of mobilizing unregistered individuals

again makes the youth an unappetizing group to target for a political campaign. Thus, all of the above analysis understates the obstacles to tracking down young voters.

CONCLUSION

This paper has demonstrated that it is possible to mobilize young people to vote, but that contacting them is problematic. Given the difficulty in locating young people, the most effective means of mobilizing them may be through groups that work with youth as a part of their daily mission. Churches, employers, community organizations, nightclubs, clinics, retailers, social services, and schools all interact with people under the age of thirty on a daily basis. The youth encountered during the course of everyday business would by definition be easy to contact. In other words, the correct strategy may be to let the young people come to the mobilization message rather than vice versa.

Voter registration and mobilization is obviously peripheral to the mission of most organizations who cater to young people (neighborhood organizations and schools may be an exception). However, the effort need not be a major undertaking. The typical non-partisan door-to-door canvassing analyzed in this paper lasts no more than half a minute. Filling out voter registration forms to register for the first time or to update contact information also takes very little time. Even something as simple as including a voter registration card with a grocery store receipt, in addition to coupons for oil changes, would be a step in the right direction. Non-partisan GOTV efforts have been shown to be effective, so the organizations need not adopt an official political position.¹⁹

This same logic can be applied to other populations that do not vote on a regular basis. Families with an income under \$25,000 move at the same rate as people under the age of 30, one-third every year (U.S. Census Bureau 2000), and presumably are just as difficult to contact as a result. Attempts to mobilize low-income neighborhoods are further frustrated by the relative density of non-citizens in such neighborhoods.²⁰ Registering voters in the course of everyday transactions may miss the most disenfranchised individuals, but it begins to address the problem.

The strategy of enlisting the support of groups that work with young people has another distinct advantage over trying to track down individual voters. Namely, organizations tend to be more stable, fewer in

number, and easier to contact. Farming out the work of registration and GOTV to the proper organizations makes the job manageable. Not every group will agree to participate; however, given the inefficiencies of conducting a direct mobilization campaign, even partial success will be an improvement.

Political campaigns, in particular, are poorly suited for mobilizing young voters. The operational time frame is short, so low contact rates are a major frustration. Also, young people do not have significant income and therefore do not make campaign contributions. And while they often adopt the same political leanings as their parents, young voters are not as steadfast in their preferences as older voters.²¹ The high rate of abstention and lack of alternative benefits affords political parties and campaigns little reason to court the young voter.

However, there is little reason why political parties could not attempt to do so. Parties do not have finite time horizons and ideally constitute a broad social network. Replacing the more modern centralized party structure with an older neighborhood model where captains know most individuals in their neighborhood would solve the problem of tracking down young voters. Recent elections in the United States have seen a resurgence in voter mobilization by political parties. Grassroots mobilization, such as door-to-door canvassing, is time consuming and labor intensive. To conserve resources parties have targeted easy-to-reach likely voters and purged young persons from the mobilization lists. The findings in this paper strongly suggest that targeting hard to reach groups may not be efficient, but that young people should not be purged from party lists because they can be mobilized.

It should be noted that the mobilization data in this paper relies entirely upon face-to-face canvassing. This decision was made because in order to detect a difference between the mobilization of young and mature voters, a sizable mobilization treatment effect must be present. Face-to-face canvassing typically has a treatment effect of 8-10% while phone and mail have demonstrated only marginal effectiveness in moving voters to the polls. However, this means that the results drawn are not necessarily applicable to other forms of voter mobilization. It is possible that Internet, phone, and mail may have different effects upon young and mature registered voters. Given the high cost of orchestrating grassroots mobilization campaigns, these other methods may be more cost effective in mobilizing youth than door-to-door canvassing.

The findings here suggest several promising avenues for future research. First, it is unknown to what degree these findings translate to other low turnout groups. It is entirely possible that the young are

capable of being mobilized because their habits are malleable, whereas other low turnout groups are more inert. Second, the reason for the low rate of contact among youth has not been addressed here. Young people may be home less often (or at different hours) than older persons. It is also possible that the low contact rate is entirely the result of high residential mobility. Third, it is not clear what portion of the face-to-face meeting causes the increase in turnout. Prior analysis of GOTV drives has offered a few suggestions, but carefully dissecting the events on the doorstep may disentangle the separate influence of reminding, social pressure, and feelings of duty. Finally, much more research needs to be done to understand the conditions under which mobilization occurs. Different electoral atmospheres, populations, political cultures will respond to the same GOTV appeal in different ways. However, at this early point in the research, it is impossible to predict reliably the ways in which these factors will matter.

NOTES

1. McDonald and Popkin (2001) argue that voting rates as a whole are not declining, but even their estimates show a steep decline (but non-monotonic) among voters in the age group 18 to 20.

2. On the other hand, if young people are not alienated and merely are waiting for political parties to reach out to them, then voter mobilization campaigns might be more effective with young populations than mature populations.

3. Door-to-door canvassing will be used rather than phone or mail, because it is the mobilization technique with the largest treatment effect. Other forms of mobilization may be as cost effective, but it is much easier to detect differences in response to treatment when the baseline response is 8.5% for door-to-door rather than 0.6% for direct mail (both estimates are from Gerber and Green 2000).

4. Interactive models confirm these findings and are available in Appendix C.

5. Detroit differed in that the canvassers tended to be African-American, female and teenagers. New Haven differed in that most of the canvassers were graduate students.

6. Raleigh was the only election that did not take place in November. Raleigh holds the city general election in October with a run off in November as necessary.

7. Before the canvassing began, there was a miscommunication on printing and canvassers were forced to stand outside in a driving rain awaiting canvassing materials. This may have served to demoralize volunteers. During the canvassing, some white residents refused to open their doors to black canvassers. Two black canvassers were accosted by white residents and expelled from a neighborhood. A coincidental canvassing effort by white supremacists seeking to deport Arabs raised residents' general level of hostility to canvassers; indeed, the local police stopped and questioned some of the white canvassers in the GOTV campaign, thinking that they were white supremacists.

8. One could account for the hypothesized heterogeneity in treatment effect by including dummy variables and interaction terms for each site. This approach, however, does not take seriously the unobserved nature of the heterogeneity and artificially constrains the error term for the model to be the same across all sites. The site dummy will account for differences in the baseline voting, but not in heterogeneity of treatment effects. Interaction terms will help estimate the differences in response across sites, but this can only be performed on observed variables. Surely, if political culture makes a difference, some of the factors of interest will be unmeasurable and be components of the error term. Conducting separate analyses for each site allows the error term to vary across sites. However, such a model is presented in Appendix B.

9. It is interesting to note that the standard error for youth treatment effect in New Haven is LARGER than the standard error for the youth treatment effect in Bridgeport despite the New Haven experiment involving twelve times more subjects than Bridgeport. This is because the number of subjects is only one component of the variance of the estimator which is calculated as follows $p(1-p)/C^2 * N * T * (1-T)$ where p = the baseline rate of voting, C = the contact rate, N = the number of subjects in the experiment, and T = the percentage of subjects assigned to the treatment group. The standard error in Bridgeport is helped by low baseline turnout, a higher contact rate, and a fifty-fifty split between treatment and control groups.

10. The variance in contact rate across sites is not a problem for the analysis. Sites with lower contact rates have less certainty with respect to their estimates and, thus, the associated standard errors are larger (see note 9). When pooling the results, the experiments with larger standard errors are then weighted less heavily than more precise experiments.

11. A model utilizing an interaction term between contact and youth confirms these results (see Appendix C).

12. Suffolk and Middlesex County, MA; Miami-Dade County, FL; Orange, Wake, and Durham County, NC; Washtenaw County, MI; St. Louis and Jackson County, MO; Polk County, IA; Hennepin and Ramsey County, MN; Harris County, TX; Pulaski County, AR; Denver and Boulder County, CO; Multnomah County, OR; and Alameda County, CA.

13. Ironically, the Census Bureau is one of the largest customers of InfoUSA.

14. The phone analysis is not reported here.

15. The p-value reported uses Pearson's test. A chi-square analysis provides identical results.

16. The corresponding statistics on wrong numbers in New Haven were not available.

17. Here the chi-square test rejects the null hypothesis only at 0.04.

18. The convergence between contact and incorrect listing in the 80+ age category is due to the confluence of moving into retirements communities, death, and small sample size.

19. There is a normative danger in having organizations that hold power over individuals, such as welfare offices and churches, pressuring people to vote. Even the most innocent of intentions could be misconstrued by an individual leading her to believe future benefits are conditional upon voting and voting in a particular way. This perceived coercion should be taken seriously and may limit the types of organizations recruited to mobilize lower turnout populations.

20. Non-citizens comprise only 5% of the total population, but they are generally concentrated in low-income neighborhoods in urban centers.

21. On the other hand, the relatively less stable preferences of young people make them a relatively larger portion of the persuadable electorate to which candidates could appeal.

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APPENDIX A

Experiments where subjects are randomly placed into the treatment and control group have two chief advantages over observational studies. The first is the statistical and modeling assumptions are modest. Endogeneity and omitted variables may plague regression analysis, but the exogenous decision to treat or not in experimental analysis sidesteps these issues. The second advantage, which stems directly from the first, is the ease of computation. To calculate the intent-to-treat effect, simply subtract the observed voter turnout rate of the control group from the observed rate of turnout in the treatment group.

The principal complication that arises in controlled randomized experimental studies of voter mobilization is that some citizens assigned to the treatment group cannot be reached. The intent-to-treat effect must be distinguished from the effects of actual contact. If everyone in the treatment group is actually contacted, the intent-to-treat effect is identical to the actual treatment effect. In practice, however, contact rates are lower than 100% because target voters are often unavailable when canvassers visit their residences.

Consider the linear probability model¹

$$Y = a + bX + u, \quad (1)$$

where Y is a dichotomous $\{0,1\}$ variable indicating whether a citizen cast a vote, and $X \in \{0,1\}$ represents whether he or she was actually contacted by a canvassing campaign. The coefficient b is the treatment effect, the boost in turnout caused by contact with the mobilization campaign. Contact is itself a function of whether a person was assigned to the treatment or control condi-

tion of the experiment. Let the variable Z , also a dichotomous $\{0,1\}$ variable, represent the random assignment to one of these experimental groups, such that

$$X = cZ + e. \quad (2)$$

To estimate the actual treatment effect (b) given a contact rate (c), the intent-to-treat effect (t) must be adjusted as follows:

$$t/c \Rightarrow b. \quad (3)$$

In other words, to estimate the actual treatment effect, the intent-to-treat estimate is divided by the observed contact rate. This estimator is equivalent to performing a two-stage least squares regression of vote (Y) on actual contact (X) using randomization (Z) as an instrumental variable (Angrist, Imbens, and Rubin 1990; Gerber and Green 2000). So long as information about the rate at which subjects assigned to the treatment group are actually contacted by the canvassers is available, the effects of contact can be accurately estimated. The two-stage least squares estimator also allows for the inclusion of control variables which can correct for imbalances between treatment and control groups due to chance.

A similar approach may be used for nonlinear probability models. One complication in studying local elections is that voter turnout tends to be very low, particularly among certain subgroups, such as registered voters under the age of thirty. OLS may produce inadmissible predictions that voting will occur with less than zero probability. Rivers and Vuong (1988) propose a two-stage conditional probit estimator that parallels the instrumental variables estimator described above. The probit transformation ensures that predicted vote probabilities are confined to the range between 0 and 1.

NOTE

1. This section draws heavily upon the analysis in Gerber, Green, and Nickerson 2003.

APPENDIX B

Voting in 2001 General Election Using City Interaction Terms in 2SLS

Variable	Coefficient	Standard Error
Contact	0.086107**	0.023849
Contact X Bridgeport	0.058126	0.080441
Contact X Detroit	-0.00408	0.048204
Contact X Minneapolis	0.029378	0.095053
Contact X Raleigh	-0.09029*	0.038732
Contact X St. Paul	0.028761	0.064731
History	0.114545**	0.006698
History X Bridgeport	0.096103**	0.02345
History X Detroit	0.52337**	0.015186
History X Minneapolis	0.279792**	0.018702
History X Raleigh	0.222235**	0.016588
History X St. Paul	0.274052**	0.028003
Youth	-0.27819**	0.007484
Youth X Bridgeport	0.234315**	0.02828
Youth X Detroit	0.148107**	0.019936
Youth X Minneapolis	0.161329**	0.020061
Youth X Raleigh	0.108049**	0.01925
Youth X St. Paul	0.011248	0.024457
Bridgeport	-0.38439**	0.018961
Detroit	-0.35665**	0.014381
Minneapolis	-0.3356**	0.017717
Raleigh	-0.32161**	0.015896
St. Paul	-0.28564**	0.028117
Constant	0.398953**	0.006396
N	45835	
R-sq	0.1495	

Note: Assignment to treatment or control group is used as an instrument for contact in the first stage.

APPENDIX C

Two-Stage Least Squares Model Including an Interaction Term

	New Haven	Bridgeport	Detroit	Minneapolis	St. Paul	Raleigh	Pooled W/O Raleigh	Pooled W/ Raleigh
Contact	0.097** (-0.025)	0.152** (-0.051)	0.082* (-0.036)	0.100 (-0.079)	0.115 (-0.06)	0.000 (-0.03)	0.088** (0.020)	0.073** (0.017)
Contact X Youth	-0.035 (-0.04)	-0.005 (-0.055)	-0.018 (-0.042)	0.044 (-0.054)	0.001 (-0.065)	0.009 (-0.044)	0.075 (0.057)	0.067 (0.048)
Youth	-0.254** (-0.008)	-0.042* (-0.019)	-0.123** (-0.017)	-0.122** (-0.017)	-0.264** (-0.025)	-0.165** (-0.018)	-0.218** (0.007)	-0.213** (0.007)
History	0.092** (-0.007)	0.199** (-0.015)	0.630** (-0.012)	0.379** (-0.015)	0.378** (-0.027)	0.333** (-0.014)	0.224** (0.006)	0.236** (0.005)
Constant	0.382** (-0.016)	-0.049* (-0.025)	0.053* (-0.022)	0.148** (-0.031)	0.139** (-0.037)	0.129** (-0.022)	0.368** (0.020)	0.204** (0.020)
Neighborhood	X	X	X	X	X	X	X	X
N	29380	1806	4954	2827	2208	4660	41175	45835

Numbers in parentheses represent standard errors.

* means coefficient significant at < 0.05; ** means coefficient significant at < 0.01.