Do Voter Registration Drives Increase Participation? For Whom and When?

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Most people interested in participating in the electoral process are registered to vote. This self-selection process creates two empirical puzzles. First, it is unclear whether voter registration drives introduce new voters into the electorate or simply facilitate a bureaucratic transaction that people registering would accomplish via other means in the absence of the drive. Second, estimating the causal effect of registration on turnout is difficult because the act of selection signals political interest and engagement that is correlated with turnout. This article utilizes field experiments to answer these two questions and the second question of the type of person mobilized by registration drives.[1] Across six cities, 620 streets were randomly assigned to receive face-to-face visits encouraging voter registration or a control group that received no attention from the campaign. On average, 10 more newly registered people appeared on treatment streets than control streets—an increase of 4.4%. This suggests that registration is a burden for a portion of the eligible population. Comparing the number of ballots cast by newly registered voters, treatment streets averaged two more votes than control streets. That is, 24% of the people registered as a direct result of the experiment voted. Disaggregating the results by socioeconomic status, the increase in registration is largest on relatively poor streets, but this difference is counterbalanced by higher turnout among new registrants on relatively affluent streets. Thus, the results of these six experiments suggest that electoral reforms reducing the costs associated with voter registration will assist a nontrivial portion of the electorate but not alter the overall composition of the electorate.

For nearly a century, most states have required eligible citizens to register before voting, but the 2008 Presidential campaign was unusual in its emphasis on voter registration. Beyond the partisan controversy over the 1.3 million registration cards collected by ACORN, the Obama campaign independently engaged in an ambitious 50-state voter registration strategy of its own. For instance, the 2008 Obama campaign was credited with registering 500,000 voters in Virginia (Shear and Gardner 2008), a state decided by only 234,000 votes. Since 85% of those registered actually vote,[2] it is tempting to view voter registration drives as decisive. However, it is possible that the type of people who participate in politics would register to vote on their own, so registration drives do nothing to increase the aggregate rates of participation. This logic is especially true during presidential elections where citizens are presented with multiple opportunities to register over the course of the campaign. This powerful self-selection process makes whether or not registration drives increase registration and subsequent participation a genuine puzzle. This article reports the results of six randomized field experiments that exogenously increased rates of voter registration in neighborhoods and traced the effect on voter turnout.

The contemporary debates on election laws are extremely heated, so accurate estimates of how many newly registered people will actually vote can help gauge the consequences of reforms involving voter registration. A large literature has examined spatial and temporal variation in election laws to estimate the percentage of newly registered people who actually vote. The estimates range from virtually none (e.g.,

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1. Supplementary material for this article is available at the "Supplements" link in the online edition and at http://www.nd.edu/~dnickers/data.php

2. According to the Current Population Survey November Supplement from 1960 to 2008, the only Presidential election where self-reported voter turnout among registered voters dropped below 85% was in 1996 when it was 82% (Census Department 2010).

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Gans 1990, 176; Martinez and Hill 1999, Table 1) to almost everyone (e.g., Brown, Jackson, and Wright 1999, Table 4; Brown and Wedeking 2006, Table 3; Mitchell and Wlezian 1998, Tables 3 and 4; Timpone 1998, Table 2) with some findings in between (e.g., Ansolabehere and Konisky 2006, Table 4, column 6; Knack 1995, Table 3 and p. 807), which spans the entire possible solution set and offers little guidance to both policy makers and political behavior theorists. This huge range of estimates is caused by the fact that individuals choose to register and states choose to change laws, and these selection processes are very hard to model. The field experiments reported in this article offer a new approach to estimating the relationship between registration and turnout by circumventing these selection processes.

A rapidly growing literature has employed field experiments to study mobilization campaigns to increase voter turnout (e.g., Arceneaux and Kolodny 2009; Dale and Strauss 2009; Gerber and Green 2000; McNulty 2005; Nickerson 2006a; Michelson 2006), but the experimental literature has focused on registered voters and ignored the logically prior question of how to register voters. Given the importance registration plays in elections by deciding who and who is not eligible to participate, this omission is unfortunate. The only published field experiments considering registration (Bennion and Nickerson 2011; Nickerson 2007a) examined the effect of email on voter registration and found no difference between treatment and control groups. Since the experimental email intervention did not raise registration rates, logic dictates that no increase in voter turnout could have resulted from the increase in registration rates. Thus, the six studies in this article constitute the only experimental measurements of the effect of door-to-door canvassing on voter registration and the first experiments capable of addressing the link between registration drives and turnout.

Since no definitive list of unregistered persons exist to randomize, treat, and track rates of voter registration, conducting experiments on voter registration is much more difficult than mobilization experiments. To construct a well-defined subject population, the experiments reported here focus on city streets. Streets in select cities were randomly assigned to receive visits from canvassers seeking to register voters or a control condition that received no attention from the campaign whatsoever. Because assignment to the treatment is random, in expectation streets are similar in every dimension—among both observed and unobserved variables—and differences in registration can be directly attributed to the campaign. Having created two sets of streets, one with exogenously higher registration rates than the other, voter turnout can then be compared across the streets.

The results demonstrate that door-to-door canvassing is an effective (albeit expensive) means of increasing voter registration rates. On average, the door-to-door canvassing generated 10 registrations on each treatment street—or about 4.4% of residents. These 10 additional registrations led to an average of two extra votes cast on these streets, leading to the conclusion that 24% of the people cast a vote once they were registered. Thus, registration does pose a barrier to participation, and voters are not entirely self-selected. Since the experiments were conducted across a range of cities and elections (ranging from Presidential to mayoral), the results appear to hold in a range of settings. Interestingly, the registration effect is largest in low socioeconomic neighborhoods, but those registered are more likely to participate in high-status socioeconomic neighborhoods. These two effects balance out, so registration drives are estimated to increase electoral participation in rich and poor neighborhoods equally.

BACKGROUND AND PRIOR FINDINGS

Elections are the chief mechanisms by which government officials are held accountable in democratic societies. Broad participation across socioeconomic strata and viewpoints is generally viewed as crucial to the health of a democracy. Registration laws add a bureaucratic cost to voting, so it is reasonable to think that they may lower participation (e.g., Schlozman et al. 2004, 53). This logic is starkly illustrated in the United States where, according to Powell, “registration laws make voting more difficult . . . than in almost any other democracy” (1986, 20–21). High levels of geographic mobility relative to other democracies mean that most voters will face the hurdle of registration not once but multiple times over a lifetime (Squire, Wolfinger, and Glass 1987). Not surprisingly, in Presidential elections between 1980 and 2004, 70% of eligible nonvoters were unregistered rather than registered abstainers, on average.[3] If increasing electoral participation is a worthy goal, examining the voter registration process is a logical place to begin.

Several scholars have utilized the temporal and spatial variation in voter registration laws to estimate their effect on registration and turnout. These studies do not directly address the effectiveness of voter registration drives, but can help to inform our expectations about the strength of selection processes in electoral participation. Of particular interest is the change in turnout attributed to the change in registration affected by the change in laws. Unfortunately, the academic literature has come to little consensus on this quantity of interest.

3. Figures calculated using the Current Population Survey.

Several studies have found that increases in registration from changes in electoral laws resulted in no increase in turnout (e.g., Gans 1990, 176; Martinez and Hill 1999, Table 1) or a very small boost in turnout (e.g., Hanmer 2009, Figure 3.6; Knack 2001, Abstract). Results such as these suggest that nearly everyone interested in participating has already registered, so changes that make registration easier have little or no effect on turnout. This state of the world has three implications for voter registration drives. First, voter registration drives may not find many unregistered and eligible citizens willing to be registered to vote. Second, the handful of people willing to register with the campaign probably would have registered on their own in the absence of the campaign. Finally, the marginal people that the campaign registers are unlikely to vote on Election Day. In short, the people interested in participating politically have already opted to register, and the drive will provide no marginal boost to participation.

On the other end of the spectrum, other observational studies have found that increases in registration rates because of changes in electoral laws result in proportionally large increases in voter turnout. Highton and Wolfinger (1998, 84), Knack (1995, Table 3, 806), and Mitchell and Wlezian (1995, Tables 3 and 4) all find that over 60% of boosts in registration rates are manifested in higher rates of voter turnout. That is, a sizable portion of the electorate will register once bureaucratic hurdles are reduced, and the majority of them will vote. This dynamic suggests that registration campaigns should meaningfully increase both registration and turnout rates.

These findings cover such a broad range of the possible solution space because three factors make the relationship between registration and turnout very difficult to estimate empirically.[4] First, individuals elect to register to vote, and this activity is likely to be correlated with political interest and notions of civic duty. The self-selection process is so strong that Erickson titled his 1981 article, "Why Do People Vote? Because they are Registered" and Squire, Wolfinger, and Glass proclaimed, "registration is virtually equivalent to voting" (1987, 47). Without knowing the data-generating process, estimating reliable selection models is impossible.[5] Second, examining changes in registration laws does not solve the selection problem since states write and implement the laws. As Hanmer (2009) points out, states had very different motives for adopting various registration laws. These motives are a symptom of different political cultures and suggest that implementation of and reactions to the laws will vary across states. That is, endogeneity may still be present in panel data, and there may be heterogeneity in treatment effects across states. Finally, dramatic changes in registration laws like Election Day registration are rare, so the results may be very case dependent and idiosyncratic. Thus, while registration laws have varied considerably over the past 40 years, self-selection and nonrandom variation have made empirical progress difficult to come by on this important topic.

Modeling Registration Drives

Careful experimental design can bypass these empirical problems and estimate precisely defined quantities of interest. The number of registered voters in an area, R, can be expressed as

$$R = C\eta \tag{1}$$

where C represents the number of citizens eligible to vote, and η is the percentage of people who elect to be registered. The number of registered voters in an area is the result of numerous processes (e.g., life-cycle and demographic shifts, the closeness of elections, civic group activity). Despite the complexity of the data-generation process, both C and η can be measured directly with observational data in the absence of a registration campaign to be studied.

Conducting a registration campaign potentially adds to the rate of voter registration. Call this amount t.

$$R = C\eta + t. \tag{2}$$

If registration laws no longer constitute a significant hindrance to participation so people interested in politics are already registered, then $t \approx 0$. Alternatively, it is also possible that the only people who speak to volunteers and fill out registration cards are the people who would participate even without the registration drive. If registration drives only successfully engage people who would become registered on their own, then $t \approx 0$. Under both of these scenarios, selection effects lead to the conclusion that registration drives do not alter the composition of registered voters appreciably.

4. As would be expected, many studies find results between the extreme estimate that no one will participate and most people will participate. For instance, Hanmer (2009) finds adopting Election Day Registration in Minnesota, Wisconsin, and Maine produced larger gains in turnout than in Idaho and Wyoming. Ansolabehere and Konisky (2006, Table 4, column 6) find that imposing registration requirements decreases turnout by 5 percentage points in US counties.

5. That said, most studies using two-stage models to estimate the link between registration and turnout find that nearly all increases in registration result in very large increases in voter turnout (e.g., Brown, Jackson, and Wright 1999: Table 4; Brown and Wedeking 2006: Table 3, column 3; Timpone 1998, Table 2).

While it is tempting to estimate t by counting up the registration cards collected by a campaign,[6] some of these people would have registered via another means. That is, the registration drive could simply serve as a substitute for the other processes generating the registration rate, η. Thus, η and t cannot be disentangled without a clear counterfactual to use as a baseline. A controlled experiment can provide that baseline by randomly selecting the areas where the registration drive takes place and the areas where the registration campaign is not present. Random assignment assures that in expectation, the number of eligible citizens, C, and the natural registration rate, η, are the same for treatment and control areas. Such experiments can provide an unbiased estimate of t by subtracting the registration rate in the control group (Equation 1), R_C, from the registration rate in the treatment group (Equation 2), R_T.

$$t = R_T - R_C = (C\eta + t) - C\eta. \tag{3}$$

Given the high correlation between political participation and socioeconomic status, the next step in the analysis is to consider for what social classes registration drives are effective. Suppose the electorate were partitioned into three categories: low SES; middle SES, and high SES. The number of registered voters could then be modeled as

$$R = C_L\eta_L + C_M\eta_M + C_H\eta_H, \tag{4}$$

where C_L, C_M, and C_H are the number of eligible low-, middle-, and high-SES citizens in the population, respectively, and η_L, η_M, η_H are the registration rates for each of the three SES levels. The effect of a registration drive on each of the three SES levels can then be expressed as

$$R = (C_L\eta_L + t_L) + (C_M\eta_M + t_M) + (C_H\eta_H + t_H), \tag{5}$$

where t_L, t_M, and t_H are the treatment effects for the low-, middle-, and high-SES subpopulations, respectively. Randomized, controlled experiments can provide estimates for t_L, t_M, and t_H, in the same manner that the average effect, t, can be estimated. In order to identify the treatment effect for each socioeconomic class, each subpopulation needs to be analyzed separately, but the logic and assumptions are the same:

$$\begin{aligned} t_L &= R_{LT} - R_{LC} = (C_L\eta_L + t_L) - C_L\eta_L; \\ t_M &= R_{MT} - R_{MC} = (C_M\eta_M + t_M) - C_M\eta_M; \\ t_H &= R_{HT} - R_{HC} = (C_H\eta_H + t_H) - C_H\eta_H. \end{aligned} \tag{6}$$

6. In the experiments described here, roughly 85% of the people who filled out cards were ultimately registered to vote. This success rate is higher than a typical registration campaign and represents the benefits of door-to-door work over site-based strategies and the high degree of quality control on the part of the managing organizations.

The key identifying assumption is that assignment to treatment is uncorrelated with the partition into social classes. As long as the definition of the socioeconomic status is defined without regards to the treatment, then subjects are equally likely to be assigned to treatment and control groups.

The literature offers little guidance as to the relative sizes of t_L, t_M, and t_H. Generally, socioeconomic status is positively correlated with political participation and interest (Campbell et al. 1960; Verba, Schlozman, and Brady 1995; Wolfinger and Rosenstone 1980). Registration is no exception where income is highly correlated with voter registration. This correlation may imply that higher SES neighborhoods may be more receptive to a campaign encouraging voter registration. That is, $t_L < t_M < t_H$. On the other hand, most residents in high-SES neighborhoods will have already registered to vote so a registration drive may find saturation is already achieved in high SES. If the correlation between registration and income still holds, it is possible that the middle-SES neighborhoods may possess a sweet spot where residents are receptive to the message of the registration drive but still contains residents who are not yet registered. That is, $t_H < t_L < t_M$. Finally, it is possible that the saturation effect dominates sufficiently that the lower registration rates in low-SES neighborhoods allow the registration drive to be successful. Furthermore, targets of the registration drive may be semivoluntary, since some people may agree to fill out cards simply to be rid of the volunteer regardless of their interest in elections. If these processes describe the dynamic, then low-SES neighborhoods are where registration drives should be most effective. That is, $t_H < t_M < t_L$.

Once the effect of a registration drive on overall rates of registration has been estimated, the next question is to ask how the drive affected voter turnout. Modeling the number of votes cast in a neighborhood, V, is a straightforward extension of the registration presented in Equation (1).

$$V = R\mu = C\eta\mu, \tag{7}$$

where μ is the rate of turnout among registered voters. A registration drive affects the number of votes cast by increasing the number of registered voters:

$$V = (C\eta + t)\mu. \tag{8}$$

Equation (8) makes explicit the assumption that the registration drive only affects the number of votes cast by introducing new registered voters into the electorate and not by directly altering the rate of turnout, μ. The assumption is unproblematic in the experiments described later in the article but could prove contentious in other settings (e.g., changes to the laws pertaining to registration and voting).

One assumption that is unlikely to be true is that previously unregistered citizens registered by the drive, t, are equally likely to vote as citizens who were previously registered, C_R where $C_R = R = C\eta$. Thus, when expanding Equation (8), we need to be mindful of this heterogeneity and model votes cast by:

$$V = C_R\mu_R + t\mu_U = C\eta\mu_R + t\mu_U, \tag{9}$$

where μ_R and μ_U are the rates of voter turnout among previously registered and unregistered citizens, respectively. If campaigns register people not really interested in elections, then registration may increase, $t > 0$, while turnout does not, $\mu_U \approx 0$. On the other hand, it is possible that registration drives are registering people who may become interested in elections after registration deadlines and therefore be prevented from voting. If this is the case, then registration drives can increase turnout, $\mu_U > 0$.

Using randomized, controlled experiments, an unbiased estimate of μ_U can be calculated. By subtracting the number of votes cast in the control group (Equation 8) from the number of votes cast in the treatment group (Equation 9) and dividing by the number of registered voters created by the treatment (Equation 3), the percentage of individuals who participate as a result of the treatment can be calculated.

$$\mu_U = \frac{V_T - V_C}{t} = \frac{V_T - V_C}{R_T - R_C}. \tag{10}$$

Equations (8–10) highlight three points worthy of note. First, in order to correctly estimate the amount that a registration drive increases participation, it is essential that the correct counterfactual be established for both registration, R_C, and turnout, V_C. Thus, experiments are needed to correctly estimate this quantity. Second, such an experiment will be silent with regards to how much of a hurdle registration is to participation for people not engaged by the campaign and only estimates a local average treatment effect by using a control group to cancel out the people who would register on their own. Finally, as a design principle, the precision of the estimates can be increased by looking only at newly registered individuals and omitting variance from people who were registered prior to the campaign.

Just as the registration rate can be partitioned by socioeconomic class, heterogeneity in voter turnout can also be examined. Maintaining the terminology developed, the number of citizens registered to vote who have low, middling, and high socioeconomic status is defined by C_{RL}, C_{RM}, and C_{RH}, respectively. Similarly, the rate of turnout among each group are then denoted by μ_{RL}, μ_{RM}, and μ_{RH}. The overall rate of voter turnout can then be disaggregated as

$$V = C_{RL}\mu_{RL} + C_{RM}\mu_{RM} + C_{RH}\mu_{RH}. \tag{11}$$

Turnout in the presence of a registration campaign can then be expressed as

$$\begin{aligned} V = {} & (C_{RL}\mu_{RL} + t_L\mu_{UL}) + (C_{RM}\mu_{RM} + t_M\mu_{UM}) \\ & + (C_{RH}\mu_{RH} + t_H\mu_{UH}), \end{aligned} \tag{12}$$

where μ_{UL}, μ_{UM}, and μ_{UH} represent the rates of voter turnout for citizens previously unregistered prior to the registration campaign. Combining the strategies employed to derive Equations (6) and (10), experimental estimates for the rate of participation are then calculated by

$$\begin{aligned} \mu_{UL} &= \frac{V_{TL} - V_{CL}}{t_L} = \frac{V_{TL} - V_{CL}}{R_{TL} - R_{CL}}, \\ \mu_{UM} &= \frac{V_{TM} - V_{CM}}{t_M} = \frac{V_{TM} - V_{CM}}{R_{TM} - R_{CM}}, \\ \mu_{UH} &= \frac{V_{TH} - V_{CH}}{t_H} = \frac{V_{TH} - V_{CH}}{R_{TH} - R_{CH}}. \end{aligned} \tag{13}$$

The next section of the article describes the experiments conducted.

METHOD AND DATA

Field experiments have three fundamental requirements: (1) a well-defined subject population that can be randomized; (2) the ability to administer the correct treatment to the correct subjects; (3) the ability to measure the outcome for all subjects regardless of treatment assignment. These requirements are easily satisfied in the case of voter mobilization experiments because the official list of registered voters defines the subject pool, can be randomized, provides addresses and phone numbers for the treatment to be administered, and the voter file can later be updated to record turnout for both the treatment and the control groups. Voter registration differs in an important regard—there is no official list of persons who are not registered to vote. Thus, all three requirements for field experiments are potentially violated.

Rather than construct a list of unregistered persons, the experiments presented here takes an alternative strategy by using streets as units of analysis. Streets were selected in each city and then randomly assigned to receive canvassing to increase registration rates or assigned to a control group that received no attention from the campaign. The rate of new voter registration on each street was then tracked, as was the number of votes cast on each street by newly registered voters. Because streets were randomly assigned to receive the treatment, on average, the streets should have the same number of unregistered persons residing on them. Any differences in registration on the treatment and control streets can be directly attributed to the success of the registration drive.[7] Subsequent differences in turnout are then a function of the increases in registration across the streets.

One assumption the strategy makes is that the initial treatment provided to increase registration only boosts turnout through increased registration (i.e., does not in and of itself increase turnout on the street). To avoid this possibility, the registration drives were conducted months in advance of Election Day (see Table 1, column 4). Experiments manipulating the timing of voter contact find no effect from contacts made more than three weeks before an election (Nickerson 2006b), so there is no reason to believe that the registration contacts would increase voter turnout. To prevent the possibility that persons registered by the experimental campaigns would later be mobilized by the organization conducting the registration, a very strict firewall was kept between the registration activities and later mobilization activities and databases. Moreover, with the exception of the experiment in Kalamazoo, the people organizing the experimental drives were not local and left immediately upon completion of the registration canvassing, taking all the paperwork and data with them. Thus, differences between treatment and control streets in voter turnout are almost assuredly the direct result of increases in voter registration.

The treatment provided in each of the experiments was very similar to each other and the bulk of grassroots registration drives conducted in the United States. Canvassers paid by a local nonpartisan organization walked down each one of the treatment streets knocking on every door. If someone was home and answered the door, the canvasser introduced herself and asked whether everyone in the household was registered to vote. If the person at the door responded that every resident was registered, the canvasser moved onto the next house. If someone was unregistered, the canvasser would help that individual fill out a voter registration card, and return the card to the county clerk's office. Since being home during a canvasser's visit is a haphazard occurrence, canvassers typically made at least two sweeps along the street to maximize contact with unregistered individuals on the street. Control streets never received visits from canvassers. In general, canvassers spoke to someone at 30–50% of the doors knocked on a given street.

To create variance in the socioeconomic status of streets included in the analysis, three experiments (Denver, Memphis, and Louisville) selected the universe of streets by an algorithm with the following steps. First, streets were given a score based on a factor analysis using the following variables (drawn primarily from the block group level of the 2000 Census): median household income, poverty rate, unemployment rate, percent of households with college educations, average years of education, percent home ownership, percent of housing units that are apartments, percent black residents, percent Hispanic residents, and voter turnout rates in prior elections. These items created an average socioeconomic status score (Cronbach's alphas for each city were: Denver = 0.80; Louisville = 0.87; Memphis = 0.86). Second, to ensure socioeconomic variance but limit the number of streets to be canvassed, only streets within the 15–30th (low), 46–54th (middle), and 70–85th (high) percentiles were targeted by the campaign. All other streets were not part of the experiment, but there is no prima facie reason to believe that people residing on streets in the 31–45th and 55–69th percentile streets would behave differently than the subjects included in the experiment.

The next series of steps in the selection algorithm used the number of registered voters on a street as a proxy for the number of people on the street. The third step dropped streets with fewer than 300 registered voters because they were unlikely to contain many treatable subjects (i.e., unregistered persons). Similarly, streets with more than 1,200 registered voters were omitted to avoid investing too much time on a single observation/street.[8] Finally, after all of these restrictions upon the sample, precincts with fewer than 300 registered voters left in the sample were excised from the experiment to provide canvassing density and ease the burden on organizers who had to monitor and drop off canvassers. Once these streets were selected for each city, two-thirds of the streets were randomly assigned to the treatment group and one-third was assigned to the control group.

The three experiments using the algorithm to select streets contain both poor and wealthy streets, so some concerns about external validity should be allayed. While the very wealthiest and poorest streets are not included in the experiment, a broad cross-section of these three cities is included. The biggest concern may be whether the results generalize to less urban areas. Given the difficulty of conducting large-scale door-to-door canvassing drives in areas

7. Many variables are available for the "algorithm" experiments, and balance checks confirm that none of the observable differences between streets were statistically significant (see online appendix). Comparable variables were not available for the Detroit, Tampa, and Kalamazoo experiments. However, there was no meaningful difference in the number of registered voters on the street or turnout among those individuals.

8. In Louisville, the lower cutoff was 200 and the upper 1,000.

Table 1. Description of the Experiments

Experiment	Year	Type	Administered	N	Percent Assigned Treatment	Previously Registered	Turnout of Previously Registered
Denver	2006	Algorithm	June–July	148	67%	329.5	47%
Memphis	2006	Algorithm	June	81	67%	252.0	43%
Louisville	2007	Algorithm	June–August	209	67%	190.7	40%
Detroit	2004	Targeted	March–May	63	51%	307.0	59%
Tampa	2004	Targeted	March–May	48	60%	141.7	77%
Kalamazoo	2006	Targeted	August–September	71	32%	60.5	27%
Total number of streets and average number of previously registered persons				620		224.9	

Note—Observations (N) are streets in cities.

with diffuse population, this particular experimental approach may not be feasible to understand the link between registration and voting in rural areas.[9]

The three experiments using the algorithm were all elections of moderate salience (Congressional elections and an off-cycle mayoral race). It is possible that registration drives perform differently in different electoral settings. Three additional experiments (Detroit, Tampa, and Kalamazoo) are included to provide a wider range of settings. These three experiments differ in research design because the organizations conducting the canvassing selected a universe of streets in neighborhoods where they thought a registration drive would be most effective. To work fertile ground, organizations targeted neighborhoods with a small number of registered voters relative to the Census Department's estimate of population in the area. In order to facilitate efficient canvassing, neighborhoods with a large percentage of secure apartment buildings where canvassers could not access doors were avoided. Short streets with few residents were also avoided to ensure that streets contained unregistered residents. As a result, the neighborhoods targeted by organizations to maximize the effect of the registration drive tended to be moderately dense, less affluent than average, and less likely to be majority white. Once the streets were selected by the organizations, they were randomly assigned to treatment and control conditions. The randomization assures internal validity for the estimates of the treatment effect, but the extent to which the results generalize to other parts of the city are a matter of speculation (which can be informed by the three experiments using the algorithm).

These neighborhoods with low rates of voter registration are precisely the areas where added effort to engage citizens is most necessary. So, these neighborhoods are extremely informative tests from normative and policy standpoints. By selecting neighborhoods where registration drives are likely to be successful, the groups can offer definitive proof that it is possible to increase rates of voter registration—even in the age of Motor Voter and during the extremely competitive 2004 Presidential elections. Registration experiments in these neighborhoods can also offer informative estimates of the link between registration and turnout—the neighborhoods with low rates of registration are the areas of greatest concern—but the generalizability of the findings to more affluent and rural populations is open to question.

Table 1 describes the six registration experiments. Conducting multiple experiments improves the external validity of the results considerably. Published field experiments often involve a single city (e.g., Alvarez, Hopkins, and Sinclair 2010; Gerber and Green 2000; Michelson 2003; Panagopoulos 2009), and many of the prior registration studies rely on the change of a law in a handful of Election Day Registration states (e.g., Knack 2000). The cities included in this study are spread across five states (three Southern, one Northern, and one Western) and range in population from less than 100,000 (Kalamazoo) to nearly 1 million (Detroit). The elections studied include Presidential (Detroit and Tampa), Congressional (Kalamazoo, Denver, and Memphis), and off-year Gubernatorial (Louisville). Detecting gains in registration during Presidential elections in battleground states (Florida and Michigan) is particularly difficult since the canvassing took place between March and

9. The total effort behind the six experiments was impressive. More than 90,000 doors were knocked on during the experiments (many of them twice) requiring more than 5,000 hours of canvassing.

May leaving more than three months for residents of control streets to be registered by the campaigns, independent organizations, or get inspired by the considerable media coverage of the election and register themselves. If registration effects can be detected even in settings such as these, this would constitute strong evidence that the electoral process is not engaging many people who could be convinced to participate. So not only do three of the experiments include socioeconomic variability, but across the six experiments important variation can be found. The remaining columns in Table 1 report: when the canvassing occurred; the number of streets included in the experiment; the percentage of streets assigned to the treatment condition; the number of previously registered voters residing on the street, and turnout among those previous registered voters.

The next section compares the number of newly registered voters on treatment and control streets. The analysis considers only people newly registered on the street and not the number of people registered on the street before the experiment. The next step is to compare the number of ballots cast by newly registered voters on treatment and control streets; again turnout by previously registered voters is irrelevant. The final piece of analysis estimates the effect the boost in registration had on turnout for the treatment streets. Since assignment to the treatment conditions is a good predictor of registration rates, can only affect turnout through registration, and is uncorrelated with all other observed and unobserved causes of registration and turnout, two-stage least squares using assignment as an instrument for registration will yield unbiased estimates of the rate of turnout among those individuals who registered to vote solely because of the registration campaign.[10]

RESULTS

Table 2 presents the results of each experiment. The first two rows report the number of people newly registered on each street for the control and treatment streets respectively. The fourth row reports the estimated effect of the registration drive with the associated standard errors. In five of the six cities, the increase in the number of registered voters on the street was statistically significant and substantively large ranging from 2.1 cards per street in Kalamazoo to 23.9 cards per street in Detroit. To account for population density and the lengths of the streets targeted, we can divide the number of cards per street by the number of preexisting registered voters on the street (see Table 1, column 7). After this adjustment, we see the rate of registration increase on each street ranged from roughly 3% in Denver to 10% in Detroit.[11] The Cochran's Q statistic for the six studies rejects the hypothesis that the six studies were drawn from a common distribution ($Q = 16.2$, df $=$ 5, $p < 0.02$), so the results from registration drives probably depend on neighborhood and electoral characteristics (see Table 3 for a breakdown by socioeconomic conditions). However, pooling across the six cities can provide a summary statistic to which future experiments can be compared. On average, the treatment street had nearly 10 more registered voters than the average control street (s.e. $=$ 2.6). Since the average street contained 225 registered voters prior to the experiment, the door-to-door canvassing increased voter registration by roughly 4.4%. These results are the first evidence that door-to-door canvassing can increase registration rates in the contemporary setting where voter registration is moderately easy.

Interpreting this result is somewhat difficult since the total number of unregistered persons residing on the street is unknown (hence, using streets as the unit of analysis rather than individuals). Considering that Martinez and Hill (1999) estimate that the National Voter Registration Act increased registration rates 6 percentage points, this increase in registration is not trivial—especially since the experiments occurred more than a decade after the implementation of the Motor Voter bill. The result also indicates that voter registration is a real bureaucratic hurdle for a portion of the electorate with low intrinsic motivation to vote.

The next question is whether increasing the registration rate increased voter turnout on treatment streets compared to control streets. Rows 5 and 6 report the average number of votes cast by newly registered voters on control and treatment streets, and row 7 presents the estimated effect of the treatment on the number of votes with standard errors. Once again, all six estimates are positive, and four

10. At first glance, the two-stage least-squares estimator may seem to be unnecessarily complicated but using the random assignment as an instrument for registration is necessary for valid inference. As the model demonstrates, simply looking at the percentage of new registrants who voted is potentially biased because some of those people would have registered and voted if left to their own devices. The control group provides an estimate on the number of people on each street who would register and vote on their own (or be mobilized by another organization). Since it is impossible to know which people would have registered independently and which people registered as a result of the experiment, the analysis must be conducted at the aggregate level, and the instrumental variable analysis can purge the registration effect of endogeneity and self-selection.

11. The statistically insignificant increase in Kalamazoo represented a substantively significant increase in registration rates of 3.5%.

Table 2. Registration and Turnout Increases on Streets by Treatment Condition for Each City

City	Denver	Memphis	Louisville	Algorithm Pooled	Detroit	Tampa	Kalamazoo	Pooled All
Control registrants	18	6.4	10.3		40.3	29.1	3.3	
	[49]	[27]	[69]		[31]	[19]	[48]	
Treatment registrants	27.7	14.8	20.5		64.3	43.8	5.4	
	[99]	[54]	[140]		[32]	[29]	[23]	
Registration effect	9.7	8.4	10.2	9.4	23.9	14.7	2.1	10.0
	(3.0)	(2.7)	(2.6)	(1.6)	(5.8)	(6.7)	(2.3)	(2.6)
New voters in control	8.1	2.9	0.4		24.5	16.6	0.3	
New voters in treatment	11.6	5.5	2.15		36.7	19.9	0.6	
Voter effect	3.5	2.5	1.8	1.9	12.3	3.3	0.3	2.0
	(1.4)	(1.1)	(0.3)	(0.3)	(4.7)	(3.0)	(0.2)	(0.7)
Ratio	0.36	0.30	0.17	0.26	0.51	0.23	0.14	0.26
	(0.07)	(0.07)	(0.03)	(0.06)	(0.15)	(0.12)	(0.09)	(0.05)

Note—Numbers in square brackets report the number of streets assigned to the condition. Numbers in parentheses represent standard errors. Registration effect is new registrants on treatment streets minus new registrants control streets. Voter effect is ballots cast by new registrants on treatment streets minus ballots cast by new registrants on control streets. Ratio = voter effect divided by registration effect; two-stage least squares provides the standard errors. Pooled estimates are calculated using a random effects estimator.

are statistically significant. While sums of 2 and 3 votes may not appear substantively large, they represent a 2–3% increase in the total number of votes cast on the street. Again, the Cochran's Q statistic rejects the notion that all six experimental results were drawn from the same distribution ($Q = 30.0$, df $= 5$, $p < 0.001$), so readers should be especially attentive to differences across cities and elections. If the reader feels the need to put a global summary statistic on the six experiments, however, streets assigned to the treatment group had two more votes cast by newly registered voters, on average, than control streets (s.e. = 0.7) when we pool across the experiments. Since the typical street has 225 previously registered voters and 40% cast ballots in an election, the 3.4 extra votes constitute 2% of the votes cast on the street. The magnitude of this effect is the equivalent of every registered voter receiving a volunteer voter mobilization call. Thus, increasing registration rates does increase voter turnout an appreciable amount.

The final row of Table 2 reports the direct estimate of how increasing voter registration affects turnout among those people registered because of the campaign. Across the six studies, the estimates range from 14% of the people registered by treatment actually voted (Kalamazoo) to 51% turnout among the newly created registrants (Detroit). In all six experiments, the effect of registration on turnout is lower than voter turnout rates among the people already registered to vote on the streets. This finding suggests that people brought into the pool of eligible voters by the experiment are less likely to participate. Just as there are differences in the effect of particular registration laws across states, the Cochran's Q-statistic reminds the reader to be attentive to differences across experiments ($Q = 12.6$, df $=$ 5, $p < 0.03$). Pooling across the six experiments, it is estimated that 26% of the people registered by the treatment

Table 3. Registration, Turnout, and Elasticity by Socioeconomic Strata

Socioeconomic Status	Low	Middle	High
Previously registered: N	261.1	222	277.1
Previously registered: Turnout	36%	41%	49%
	15.3	8.3	9.3
Control: New registrations	[53]	[44]	[48]
	31.7	16.4	12.6
Treatment: New registrations	[110]	[87]	[96]
	16.4	8.0	3.4
Registration effect	(2.9)	(2.4)	(1.8)
Control: New voters	4.1	2.5	4.2
Treatment: New voters	7.4	4.9	5.9
	3.3	2.4	1.7
Turnout effect	(0.9)	(0.8)	(1.0)
	0.20	0.30	0.50
Ratio	(0.04)	(0.06)	(0.11)

Note—Denver, Memphis, and Louisville are the experiments included in the analysis. Numbers in square brackets report the number of streets assigned to the condition. Numbers in parentheses represent standard errors. Registration effect is new registrants on treatment streets minus new registrants control streets. Voter effect is ballots cast by new registrants on treatment streets minus ballots cast by new registrants on control streets. Ratio = voter effect divided by registration effect; two-stage least squares provides the standard errors.

turned out to vote (s.e. = 0.05). That is, these experimental results demonstrate that there is a sizable population who would vote but are prevented by the hurdle of registration—even during hotly contested Presidential elections.

This particular estimate—that 26% of the people who registered as a result of the experiment voted—falls in the middle of the considerable spread of results from prior studies. The experimental estimate is sufficiently precise to suggest that studies showing few unregistered people would vote if given the opportunity (e.g., Gans 1990; Martinez and Hill 1999) are unlikely to be true. Similarly, studies claiming that the majority of unregistered persons would vote if they became registered (e.g., Brown and Wedeking 2006; Mitchell and Wlezian 1998) are also suspect.

There exists considerable heterogeneity across the six experiments (see Figure 1). It is unlikely that the results are drawn from a single distribution, so caution should be exercised when interpreting these findings. It is possible that the population studied and the political context matters a great deal. For instance, settings where the population studied is particularly interested in politics and faced with a salient election may be more likely to vote if they were eligible. Conversely, disengaged individuals in a low-salience election are probably less likely to vote when the registration hurdle is removed. Of course, one would expect engaged individuals in an exciting election to take the initiative to register, so the expectations regarding heterogeneity across political settings are not clear. Many more experiments would be required to develop well informed intuitions and test those intuitions.

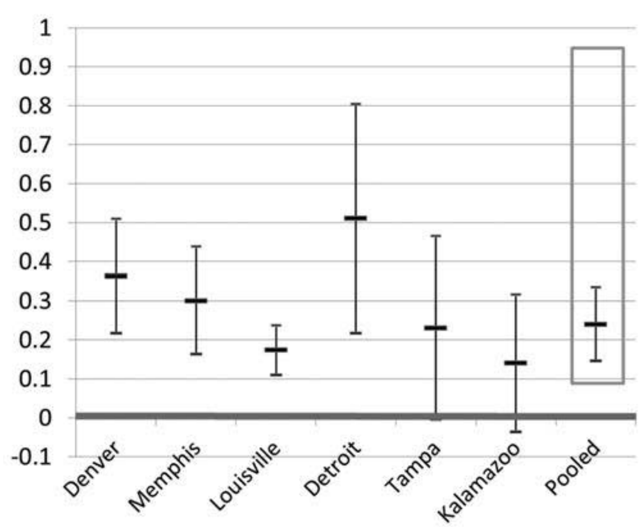

Figure 1. Estimates of registration to turnout linkage with 95% confidence interval. Figure 1 graphs the estimated yield of votes to registration for each experiment taken from the bottom row of Table 2.

Testing across different elections would require further experimentation, but the data collected allows for analysis of heterogeneity across types of neighborhoods. Three of the experiments were explicitly stratified by socioeconomic status in order to cleanly disaggregate the effects by socioeconomic status. Table 3 presents the results for each of the three SES groups. The top two rows present the number of previously registered voters on each type of street and their turnout. The next three rows present the number of new registrations on control and treatment streets and the average registration effect. Registration increased measurably across all three SES levels, but nearly five times as many people were registered on low socioeconomic status streets than on high socioeconomic-status streets as a result of the experiment (16.4 vs. 3.4). The door-to-door canvassing increased rates of registration by 6% on low-SES streets, 4% in middle-class streets, and 1% on high economic streets. These results reconfirm the fact that registration is a much bigger problem among the poor, but they also provide strong evidence that door-to-door canvassing can be a solution to the problem.

Rows 7–9 of Table 3 report the rates of voter turnout among newly registered voters on control and treatment streets and the resulting treatment effect. The number of votes created by the registration campaign is statistically significant in all three cases, but not large, and differences across strata do not approach statistical significance. The difference in the boost to registration between high and low SES may have been 500%, but that leads to only twice as many voters (3.3 vs. 1.7). As a percentage of the votes cast, the registration drive led to a 4% increase in votes in low-SES streets, 3% on middle-SES streets, and 1% on high-SES streets. While not eye-popping, the differences in turnout are substantively significant. That said, these turnout results suggest creating universal registration would alter the composition of the electoral minimally—a position argued in prior studies (e.g., Citrin, Schickler, and Sides 2003; Knack and White 1998; Nagler 1991).

The bottom row of Table 3 reports the strength of the link between registration and turnout for all three SES categories. The gradation across socioeconomic strata is striking, but not surprising, given the observed differences across strata for registration and turnout effects. Roughly 20% of the people who registered solely as a result of the door-to-door campaign voted on low-SES streets (s.e. = 4) compared to a 50% rate of turnout among people registered because of the experiment in high socioeconomic status neighborhoods (s.e. = 11). It is interesting to note that newly registered people vote at lower rates than the previously registered people in poor neighborhoods (20% vs.

36%), while the people registered as a result of the experiment in wealthier neighborhoods exhibit participation rates on par with their previously registered neighbors (50% vs. 49%). One possibility for this finding is that newly registered persons in high-status neighborhoods are exposed to more campaign activity and social pressure to vote. Another possibility is that the newly registered voters themselves differ across neighborhoods.[12] Much as responses vary to voter mobilization (Arceneaux and Nickerson 2009), substantial heterogeneity in response to voter registration drives is evident across socioeconomic strata. These differences suggest that the link between registration and turnout is contingent on context and personal characteristics.

DISCUSSION

These experiments provide an unbiased estimate of the link between turnout and registration among the people uniquely registered through six voter registration drives, but what does the experiment say about the unregistered people on treatment streets who did not register with the campaign? The campaign could have failed to register people in two primary ways. First, unregistered people simply may not have been home when the canvassers visited the house. If the likelihood of a particular unregistered person being home are haphazard and pseudo-random, then no bias is introduced. However, there is good reason to believe that people who are not contacted by campaigns are different from those contacted (Arceneaux, Gerber, and Green 2006). While most of the unregistered persons were simply not home and could therefore not be contacted by the campaign (rather than outright refusal to be registered), it is highly likely that these people would have lower rates of voter turnout than the people registered in the experiment.

The second form of failure to register comes from people who refuse to register to vote. The experiments can say nothing about people hostile to the idea of registering to vote, but it is reasonable to infer such people would exhibit very low rates of voter turnout even if registration barriers were removed entirely. Thus, the six experiments likely overstate the effect on turnout of passing universal registration laws. How large a portion of the population is opposed to voter registration? A very low percentage of people admitted to being unregistered and then refused to fill out a card (<2% in all experiments). However, a number of people may have falsely claimed to already be registered. Most of these people probably thought they were registered and did not realize that they needed to re-register when they moved or had been purged from the rolls, but a portion may have provided the answer simply to be rid of the canvasser on the doorstep. Given data limitations, it is impossible to know the precise proportion of the population that is averse to participating in politics, in part because such people are less likely to answer surveys, but the number is likely to be on small side. However, the estimates derived from the experiments on the link between registration and turnout should be treated as upper bounds rather than precise point estimates.

That said, the six experiments contained in the article offer an important contribution to our understanding of campaigns and voting behavior. The utility of door-to-door canvassing to increase turnout among registered voters across a range of contexts has been established by prior field experiments (e.g., Green, Gerber, and Nickerson 2003). The logically prior question of whether grassroots campaigns can register voters is answered here in the affirmative. In doing so, the studies establish a few basic facts about voter registration and turnout that were subject to debate. First, registration is not a trivial barrier for a sizable portion of the electorate. The number of registered voters increased by 4.4% as a direct result of the canvassing effort. Unsurprisingly, registration poses the biggest problem in low socioeconomic status neighborhoods. Even in extremely competitive electoral settings where media attention is intense, resident interest is high, and campaigns have a vested interest in mobilizing supporters, canvassing several months prior to the registration deadline increased registration rates appreciably.

The experiments also allow researchers to examine variation in the type of person whose behavior would be affected by electoral reforms like automatic registration. Newly registered persons will vote and in appreciable numbers; the six experiments here increased the number of votes cast on streets by 2% on average. However, these people brought into the electorate will not vote at rates as high as their previously registered neighbors. In all six experiments, the estimated ratio of turnout to registration for the people registered by the experiment was substantially lower than turnout among the people registered to vote on the street prior to the experiment. The strength of this finding is conditional on the socioeconomic status of the street in question where the gap is largest in moderately poor neighborhoods and smallest among moderately affluent neighborhoods. As a result, it appears that increasing registration rates would do little to alter the composition of the electorate.

These findings offer several practical implications for governments, campaigns, and civic organizations. First, policies that remove or reduce the barrier of voter registration

12. For starters, the newly registered voters differ with regards to socioeconomic status across the strata.

will expand the electorate in most settings. Not everyone will participate, but a sizable number of citizens are deterred from voting as a direct result of voter registration. However, advocates of Election Day Registration and similar registration reforms (such as automatic registration, which these experiments mimic) should not expect levels of turnout comparable to Australia, which has compulsory voting and turnout averages 90%. In fact, the estimates from these experiments suggest that automatically registering all the eligible unregistered citizens (roughly 20% of the potential electorate) would cause only one-quarter of them to vote. This 5 percentage point increase in voter turnout would constitute a large increase in turnout but would not radically transform the shape of the electorate.

Second, civic organizations seeking to increase rates of participation can do so through grassroots effort, but the effort is expensive. The average street across the experiments contained 225 registered voters and was canvassed twice to generate 10 people who would not be registered another way. If households contain on average 1.8 registered voters, that means there are 125 households with registered voters on a typical street in the experiment. Suppose there are 25% more households with no registered voters for a total of 156 households per street. If a canvasser can knock 20 doors an hour, then it would take 7.8 hours for each sweep through the neighborhood. At $10 an hour, each unique registration would cost $15.60. Under this calculation, appreciably raising rates of voter registration in a city is an expensive proposition.

Third, registering likely supporters is not a cost effective activity for campaigns to pursue. A dedicated registration effort would cost roughly $60 per vote ($15.60 per registration created divided by 0.26 to account for the drop off between registration and turnout). This sum is substantially higher than $13 for phone calls and $21 for canvassing or leafleting (Nickerson 2007b, Table 5). However, engaging in registration activities that are essentially costless is a very good idea. For instance, inquiring about registration status during an educational or persuasive canvass would make sense. If the cost of each registration could be reduced to around $5, then registration would be cost competitive with doors and leaflets with regards to dollars per vote. In situations where each registration costs $3.50, then registration drives are as cost effective as high-quality phone banks at increasing turnout. Thus, political campaigns are unlikely to pursue stand alone registration campaigns but should integrate registration into other outreach efforts.

The same logic applies to government policies that increase voter registration and sheds a very flattering light on the laws like the National Voter Registration Act. Government outreach solely devoted to voter registration is unlikely to be a cost effective means of increasing voter participation. However, the government has many routine interactions with citizens who may need to register to vote. Requiring government agencies like the Bureau of Motor Vehicles, public assistance, and disability offices to proactively attempt to register clients will increase voter registration with minimal expense since the targeted citizens are already in the office with all the required documentation. The same holds true for the provisions mandating that colleges and universities receiving federal dollars make an effort to register their students to vote. Registering people who enter official bureaucratic channels can be an especially cost effective means of increasing registration—especially among highly mobile populations who need to reregister frequently such as the poor and the young.

The results broken down by socioeconomic status reinforce an emerging view of the effects of electoral reform on the shape of the electorate (Berinsky 2005). For instance, Berinisky, Burns, and Traugott (2001) finds that vote by mail increases participation, but the people most likely to take advantage of that means of voting are relatively interested in politics and already engaged in the political process. Similarly, Gronke, Galanes-Rosenbaum, and Miller (2007) report that early voting reforms generally are used by people already voting and have at best a negligible effect on overall turnout (and sometimes negative). The registration experiments here echo these prior findings. While registration drives were much more effective in poor neighborhoods, residents of relatively affluent streets were far more likely to actually vote. The two counterbalancing effects were a wash, and the overall net effect of the registration drive did not change the average wealth of the electorate. This finding provides further evidence that electoral reforms designed to reduce the costs of participation are more often utilized by citizens with slightly higher propensities to participate. This regularity does not argue against the wisdom of reforms that make participation easier, but activists seeking to bring in traditionally underrepresented groups should not view such reforms as a panacea. Similarly, people who oppose electoral reforms that lower the costs of voting should not fear electoral repercussions should reform bills be enacted.

While subdividing the experiment by socioeconomic status provides a glimpse of the type of person signing up because of the canvass, it would be useful to get a more in-depth psychological profile of these people. The experiments in this article were designed to test the effect of canvassing on voter registration and subsequent turnout with no other contact from the campaign beyond the initial door knock.

Why those individuals voted in the election but did not bother to register previously is beyond the scope of the inquiry, but there are four possible reasons that spring to mind. First, perhaps the cost of voting is lower than the cost of registration and the reservation price of the individual falls in between. If true, then reforms such as online voter registration should be effective at increasing participation because the cost of registration will become comparable to the costs of voting. Second, it is also possible that some of the newly registered individuals may have been mobilized by other organizations once they appeared on the rolls. If this was the psychological mechanism, then changes to election laws will not engage these individuals—instead an increased commitment from political campaigns to engage low-income neighborhoods would be necessary to increase turnout in poor neighborhoods. Third, perhaps the person targeted by the registration campaign only became interested in the race closer to Election Day after the registration deadline has passed. That is, they never bothered to register because they weren't interested, but once they became interested, it was too late to register. If true, then reforms moving registration deadlines closer to Election Day will be effective at bringing new people into the electorate. Finally, newly registered people may begin to think of herself differently once registered and plan on voting. If this mechanism is at work, then electoral reforms that involve automatically registering people to vote may not serve to increase voter turnout at all since people will not see themselves in a different light. Each of these four mechanisms could account for the increase in voter registration and turnout observed in the experiments reported here, and they need not be mutually exclusive. Given how very different the policy implications for each mechanism are, future work should attempt to tease out the relative contribution of each mechanism.

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