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Articles

The Effects of Voter ID Notification on Voter Turnout: Results from a Large-Scale Field Experiment

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ABSTRACT

State voter identification (ID) laws have proliferated in the past ten years. Political campaigns remain divided about whether and how to address identification requirements when encouraging voter turnout. This article reports results from a direct mail get-out-the-vote (GOTV) experiment, conducted during the run-up to the 2012 general election in counties along the Tennessee-Virginia border and in heavily African American precincts in Roanoke and Knoxville. Results indicate that informing low-propensity voters of a new identification requirement raises turnout by approximately one percentage point. Messages providing details about ID requirements and offering to help recipients obtain acceptable ID appear somewhat more effective than messages only pointing out the need to bring proof of identification. These mailings, which have similar effects in both states, also appear to raise turnout among others in the recipients' households. Overall, we find no evidence that calling attention to voter identification requirements dissuades voters from voting.

Advocated as a safeguard for electoral integrity and excoriated as voter suppression akin to Jim Crow-era poll taxes,[1] state voter identification (voter ID) laws have proliferated over the last ten years. Of the thirty voter ID laws on the books,[2] the seven "strict" statutes, which disqualify provisional ballots cast if the voter fails to furnish the required identification,[3] have predictably drawn the most fire. Of these, four (Georgia, Indiana, Kansas, and Tennessee) require photo identification while three others (Arizona, Ohio, and Virginia) accept other proof of identity and address as evidence of one's eligibility to vote.[4]

The debate about voter ID laws is polarized along familiar partisan and ideological fault-lines (Hood and Bullock 2008; Biggers and Hanmer 2011). Promoted by conservative advocacy groups such as the American Legislative Exchange Council (Mayer 2012), voter ID laws have been adopted almost exclusively in states controlled by Republicans.

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[1]Speaking against Texas' voter ID law, Attorney General Eric Holder said "we call [strict photo ID laws] poll taxes" (*New Hampshire Union Leader* 2012). A year earlier, Representative Steny Hoyer remarked at a congressional forum on the impacts of voter ID laws, "A poll tax by any other name would smell as vile" (quoted in Dropp 2012).

[2]Three more have been passed and are either pending Department of Justice Section 5 preclearance or awaiting further judicial scrutiny. The National Council of State Legislatures tracks voter ID laws and associated legislative and judicial activity at <http://www.ncsl.org/legislatures-elections/elections/voter-id.aspx#>.

[3]Other laws permit a ballot to be counted as long as the voter signs an affidavit affirming his or her identity and eligibility to vote.

[4]Of the additional six efforts to implement strict voter ID laws with photo identification requirements, two (Pennsylvania and Wisconsin) have been enjoined by courts, two (Texas twice and Mississippi) were denied preclearance by the Department of Justice under Section 5 of the Voting Rights Act, and two (Virginia's new law and Arkansas's) have yet to take effect.

Proponents argue that voter ID requirements enjoy wide public support (Washington Post Poll, July 2012) even among minority groups (cf. Pew Hispanic Center National Survey of Latinos, September 2012) and help combat electoral fraud that taints electoral legitimacy (von Spakovsky 2012). Citing research showing impersonation-based voter fraud is exceedingly rare (Minnite 2010; Hasen 2012), Democrats, along with various civil rights organizations, counter that voter ID laws' principal effect is to disenfranchise minority voters, the poor, the young, and the elderly.

Many adult citizens, especially among putatively vulnerable groups, indeed lack accepted forms of identification, and obtaining identification may be time-consuming and confusing even in states that make voting IDs available for free (Barreto, Nuno, and Sanchez 2007; Rogowski and Cohen 2012; Gaskins and Iyer 2012).[5] Yet the vast majority of Americans, including minorities, claim to have necessary identification (e.g., 87% of Latinos interviewed by the Pew Hispanic Center National Survey of Latinos, September 2012), and it is unclear how many of those potentially affected by ID laws would otherwise vote. Evidence about the effects of voter ID laws on turnout is mixed, with some studies showing a moderate decline in turnout among groups least likely to have identification (Alvarez, Bailey, and Katz 2008; Vercellotti and Anderson 2006), others finding no effect (Ansolabehere 2009; Mulhausen and Sikich 2007; Mycoff, Wagner, and Wilson 2009; Milyo 2007; Pastor et al. 2010), and at least one purporting to show that voter ID laws can boost turnout by promoting confidence in the integrity of the electoral system (Lott 2006; but see Ansolabehere and Persily 2008). The range of estimates in part reflects the methodological challenges of inferring the effects of voter ID laws. Cross-state comparisons raise concerns about whether states that adopt different ID requirements are otherwise comparable in terms of unobserved causes of turnout (Erikson and Minnite 2009).[6] More recent research using difference-in-differences estimators (e.g., Alvarez et al. 2011; Dropp 2012) seems to be on firmer methodological footing, but even its findings (small negative effects on turnout) are subject to concerns about omitted variables bias.

Another source of uncertainty in discerning the effects of ID laws on turnout is the fact that these laws are themselves the subject of campaign messages and mobilization activity, and the strategic response to voter ID laws continues to evolve. Campaign strategists seeking to mobilize voters currently wrestle with the question of whether and how to address ID requirements. As we explain below, opinions differ about the effects of different messages regarding voter ID laws, especially about the influence of simply notifying subjects of the laws' requirements. Raising the specter of identification requirements could discourage turnout; alternatively, a message that encourages voters to bring appropriate identification to the polls could raise turnout. The relative strength of these competing hypotheses is an empirical question, one that sheds light on how voter ID laws are likely to affect turnout in years to come.

Here we report results from a large-scale field experiment—the first, we believe, of its kind—evaluating a direct mail get-out-the-vote (GOTV) campaign conducted in two states, Tennessee and Virginia, which enacted strict voter ID laws after the 2010 general election. Tennessee's law requires voters to present one of several approved photo identifications; Virginia's law allows for a wider array of IDs, including bank statements and utility bills showing the voter's name and address. Subjects in the experiment are drawn from two settings: (1) counties along the Tennessee-Virginia border that have very similar demographic and partisan profiles and (2) heavily African American precincts in Roanoke City, Virginia and Knoxville, Tennessee, two endpoints of a major corridor of Interstate-81.

The experiment compares voter turnout in an untreated control group to randomly assigned groups that received one of three mailings: (1) a simple *reminder* to vote on Election Day ("Reminder"), (2) a terse *warning* that voters are required to show identification at the polls ("Warning"), and (3) a message that both warns of the need for ID and also provides *help* in the form of information about which identification documents are required and how voters can obtain help in acquiring them ("Help"). These treatments, described

[5]For example, in Tennessee, obtaining a "free" identification card requires visiting a Department of Motor Vehicles office with a birth certificate and proof of address.

[6]This literature features other designs as well. Ansolabehere (2009), for example, asks survey respondents whether they were prevented from voting by an identification requirement, finding tiny effects.

more fully below and displayed in Appendix B, therefore enable assessment of whether a mere reminder to vote, a simple warning about voter ID requirements, and information about specific identification requirements combined with an offer of help affect turnout. This design, coupled with the use of stratified sampling, allows us to assess whether treatment effects vary by political context or demographic profile.

HYPOTHESES

The experimental research literature on the mobilizing effects of direct mail has become one of the most extensive in all of social science, comprising dozens of experiments based on more than one million subjects. A recent meta-analysis describing this literature (Green, McGrath, and Aronow 2013) suggests two conclusions. First, mere reminders to vote have very little effect on turnout, with estimates on the order of one-tenth of one percentage point. Second, turnout effects of GOTV tend to be weaker in high salience elections than low salience elections, arguably because messaging in high salience contests is drowned out by other campaign hoopla. Accordingly, in our current field experiment we expect to find at most a weak positive effect of a mailing that simply reminds people to vote in the 2012 presidential election. Like other direct mail experiments that attempt to isolate the effects of specific messages (Panagopoulos 2011), we include a simple "Reminder" treatment primarily to verify that the active ingredient in our voter ID-focused messages is something other than a conventional message about the election accompanied by an exhortation to vote.

There are several reasons to expect that advising voters of the need for acceptable identification would raise turnout. Most straightforwardly, notification could counteract the potentially demobilizing effect of the law by reminding voters what they may need at the polls and giving them the lead time necessary to obtain the documentation they may lack. Some advocates hope it might fuel turnout by inspiring backlash against a policy perceived as aimed at disenfranchisement, spurring voters to obtain ID if they lack it and to vote once they obtain it (Issenberg 2012). There is even speculation that awareness of voter ID laws may raise turnout by enhancing voters' confidence in the integrity of the electoral system (Lott 2006).

Alternatively, certain types of notification may discourage turnout, especially those that provide little information about the nature of the new requirement. Voters made newly aware of the costs of compliance may come away with an inflated notion of how difficult it is to obtain ID or an erroneous belief that identification documents they already possess are insufficient. Notification messages—even well-intentioned attempts to provide detailed instructions about appropriate forms of identification—might also cause voters to become anxious about the possibility of hostile scrutiny at the polls and the discomfort of having to prove their eligibility in front of others. References to penalties may exacerbate this tendency. The threatening potential of communications regarding voter eligibility made headlines when billboards posted predominantly in poor, heavily minority neighborhoods in Ohio and Wisconsin warned that "VOTER FRAUD IS A FELONY," and displayed, next to a picture of a gavel, a red banner stating "UP TO 3½ YRS & $10,000 Fine" (O'Toole 2012).[7] Yet even simple advisories that lack explicit reference to punishment but fail to provide clear information or help could evoke similar concerns. Billboards in Pennsylvania, displayed even after the state's photo ID law was enjoined for the 2012 election, displayed the slogan "SI QUIERES VOTAR, MUESTRALA" ("If you want to vote, show it") and featured a woman with a stern expression holding up a driver's "Got ID?" public outreach campaign could be taken as a light-hearted way of engaging voters via a well-known advertising slogan but also might seem ominous to those wondering whether they have the proper ID and what might happen to them if they attempt to vote without it.

Beyond the potential for certain forms of voter ID notification to arouse rational concerns about the cost of voting, the likelihood of being able to do so, and the consequences of attempting to vote without an accepted form of ID, notification could also suppress turnout by activating other psychological processes. Simple notices could function as aversive communications that convey a sense of suspicion

[7]Lambasted as voter intimidation and targeted by multiple petitions, the billboards were eventually removed (O'Toole 2012).

about whether the recipient is qualified to vote. Low-propensity voters could succumb to the so-called "Golem Effect," wherein an authority figure's low performance expectations for a subordinate become self-fulfilling prophecy as they are internalized and become a benchmark for the subordinate (Babad, Inbar, and Rosenthal 1982). In the case of minority voters especially, stereotype threat (Steele and Aronson 1995) might compound the tendency to withdraw from voting in the face of aversive communication that evokes fears of confirming a negative stereotype about one's group. While stereotype threat is usually linked to performance on intellectual tasks, some research finds that internalization of negative stereotypes is associated with reduced effort and preparation as well (Charles et al. 2009, pp. 178–181).

Given the many ways that notification could discourage voting, it is not surprising that a major concern for civic organizations advocating voter participation is *how* to talk about these laws without depressing turnout. As an official at the League of Women Voters noted in personal correspondence with us, their efforts target "the very communities that might not have ID and might easily be discouraged from voting if there appears there will be any obstacle to their participation."[8]

As a result, our expectations regarding the likely effects of voter ID notification hinge on how the notification is framed. Basic information that a new law requires all voters to show identification at the polls, especially coming without information about specific requirements or how to obtain necessary documentation, might raise or lower turnout, depending which of the mechanisms described above prevails. However, providing more information and articulating a positive tone that signals a motivation to include subjects in the electoral process may overcome the potentially aversive tone of the Warning message. Our "Help" mailings provide details about what is needed and information about where to obtain assistance. From a theoretical perspective, the Help treatment reduces the transaction and psychological costs that voters may experience from a mere notification. In addition to giving directions, the Help mailing provides encouragement and, implicitly, the message that others are there to help. Hence, our expectation is that the Help treatment will, on balance, raise turnout.

EXPERIMENTAL DESIGN

Site selection

The field experiment was conducted in the run-up to the 2012 general election in Tennessee and Virginia. These states were selected primarily because each introduced a different type of strict voter ID law after the 2010 general election (Tennessee in 2011 and Virginia in early 2012). The two new laws differed in that Tennessee's required a photo identification and Virginia's allowed a wider range of proofs of identity, including utility bills. Appendix A, Table A1 provides details on Tennessee's and Virginia's voter ID requirements.

Four geographic strata were selected so as to maximize sample comparability of the experimental subjects across different jurisdictions (Keele and Minozzi 2013). Two strata (one from each state) were composed of the ten counties along the Tennessee-Virginia border. Virginia jurisdictions included were Lee, Scott, Washington, and Grayson Counties and Bristol City ("VA border sample"). Tennessee jurisdictions included were Johnson, Sullivan, Hawkins, Hancock, and Claiborne Counties ("TN border sample"). As shown in Table 1, these counties have strikingly similar demographic and partisan profiles and are almost completely located within the Tri-Cities TN-VA media market that received an estimated $1.1 million in presidential campaign advertising during 2012 (Baum 2012). Although Virginia residents were much more likely to receive campaign contacts associated with a closely contested presidential contest and U.S. Senate race, border sample residents of both states received similar doses of mass media advertising. [9]

Two heavily African American strata were drawn from zip codes and precincts identified as having large African American populations in Roanoke City, Virginia ("Roanoke sample") and Knoxville, Tennessee ("Knoxville sample"), the largest city in Knox County. These two cities are linked by a busy corridor of Interstate-81 and also have similar

[8]E-mail correspondence with Jeanette Senecal (Senior Director, Elections and e-Democracy, League of Women Voters of the United States) received May 15, 2013.

[9]Because Tennessee was not a battleground state, it was largely ignored by the presidential campaigns. Tennessee also had an uncompetitive race for U.S. Senate, and none of the three House seats in our coverage area was closely contested.

TABLE 1. GEOGRAPHIC STRATUM DEMOGRAPHICS

	Population	*Under 18*	*65+*	*White non-Hispanic*	*Black*	*Hispanic*	*Y-Y same residence*	*HS grad*	*BA+*	*Home ownership*	*Median HH inc*	*Poverty rate*	*08 Dem pres vote*
Virginia	*8,096,604*	*22.9%*	*12.5%*	*64.5%*	*19.8%*	*8.2%*	*83.8%*	*86.1%*	*33.8%*	*68.9%*	*61,406*	*10.3%*	*52.7%*
Border Counties													
Lee	25,146	20.2%	15.4%	93.1%	4.0%	1.7%	88.1%	71.5%	12.0%	73.8%	31,352	21.2%	35.0%
Scott	23,126	19.0%	20.8%	97.3%	0.8%	1.1%	92.3%	73.6%	10.3%	77.0%	34,250	18.4%	27.7%
Washington	54,827	19.3%	18.1%	95.8%	1.5%	1.4%	87.9%	80.4%	20.5%	74.2%	40,422	14.7%	33.0%
Grayson	15,328	18.6%	21.3%	93.8%	2.4%	2.8%	89.7%	73.4%	10.3%	81.3%	32,178	15.6%	34.5%
Bristol City	17,750	20.2%	18.8%	89.9%	5.9%	1.5%	79.8%	79.8%	19.7%	61.8%	32,079	23.4%	36.3%
Roanoke City[1]	96,714	21.8%	14.3%	61.9%	28.5%	5.5%	80.6%	80.9%	21.9%	56.0%	36,422	20.9%	61.0%
Tennessee	*6,403,353*	*23.3%*	*13.7%*	*75.4%*	*16.9%*	*4.7%*	*83.8%*	*82.5%*	*22.7%*	*69.6%*	*43,314*	*16.5%*	41.8%
Border Counties													
Johnson	18,231	18.1%	18.6%	95.0%	2.2%	1.6%	89.5%	69.1%	10.1%	76.4%	29,949	23.8%	27.9%
Sullivan	157,419	20.3%	19.0%	94.1%	2.4%	1.6%	85.2%	82.1%	20.0%	75.8%	39,957	15.9%	28.7%
Hawkins	56,671	21.9%	17.1%	95.5%	1.6%	1.3%	85.3%	78.1%	12.4%	76.1%	35,392	18.3%	28.2%
Hancock	6,737	21.2%	17.6%	97.7%	0.4%	0.3%	85.2%	68.3%	7.4%	71.5%	23,125	30.3%	27.0%
Claiborne	32,172	20.6%	16.6%	96.2%	1.0%	0.9%	87.8%	69.2%	11.1%	77.3%	31,353	19.3%	29.5%
Knoxville City[1]	180,761	19.1%	12.6%	74.2%	17.1%	4.6%	77.6%	84.2%	29.0%	51.6%	32,756	23.4%	37.6%[2]

Sources: U.S. Census Bureau, American Factfinder Quickfacts 2010–11 American Community Survey and Virginia and Tennessee Secretary of State Web sites.

[1]Knoxville, Tennessee, and Roanoke, Virginia, are adjoined by a major corridor of highway I-81. Knoxville City is part of Knoxville County. Roanoke is an independent city separate from Roanoke County. As described in the Experimental Design section, subjects were selected only from zip codes and precincts with high African American populations.

[2]Turnout data are shown for Knoxville County. Precincts located mostly in Knoxville City had Democratic vote shares comparable to Roanoke City, and our procedure entailed selecting from among the most heavily Democratic precincts in both samples.

Y-Y, year-over-year change; HS, high school; HH inc, household income.

demographic profiles, although they are located in different media markets.

Sample selection

Registered voter lists were obtained for the twelve counties and independent cities comprising these four geographic strata. In all four strata, vote history records provided in the registered voter lists were used to exclude registered voters who had voted in both the 2008 and the 2010 general elections. In the TN and VA border samples, one member of each household was then selected at random. In the TN border counties this procedure left a subject pool of 64,205 registered voters. Of these, 21,796 were randomly selected to the sample, pending National Change of Address (NCOA) screening. In the VA border counties, this procedure left a subject pool of 34,688, of whom 25,000 were randomly selected to the sample pending address verification.[10]

Further steps were taken in the Knoxville and Roanoke samples to obtain heavily African American subject pools. In the Knoxville sample, subjects were retained only if they resided in zip code 37914 or 37915, which were 39% black and 73% black, respectively, according to the 2010 Census. Registered voters in these geographic blocks were retained only if they resided in a precinct that had voted at least 60% for the Obama ticket in 2008, a restriction that serves as a proxy for registrants' race. After one member from each household was selected at random, this selection procedure yielded a Knoxville subject pool of 5,449 registered voters. In the Roanoke sample, subjects were retained only if they resided in zip code 24017, which was 66% black in the 2010 Census. Since all precincts spanning this zip code voted at least 60% for the Obama ticket in 2008, no further registered voters were dropped from the Roanoke sample. After selecting one member from each household, we were left with a subject pool of 6,997 registered voters.

The resulting lists of registered voters from each region, as well as out-of-sample household members within the selected households, were then submitted for NCOA screening so as to minimize undeliverable mail. (Of the thousands of mailings we sent, fewer than two dozen were returned as undeliverable.) Due to budget constraints, we opted to construct samples in each state that were 20% drawn from the black oversample and 80% drawn from the border counties. Since the Knoxville pool was left with 4,541 voters after NCOA screening—the smallest number left from any of the four strata—we randomly retained 4,541 of the remaining registered voters in Roanoke and four times this number from those remaining in each of the border samples pools. Specifically, each registered voter left after NCOA screening in each stratum was assigned a random number. The first 11% of the randomly ordered registered voters remaining were assigned to a "Reminder" treatment, 22% each to the "Warning" and "Help" treatments, and the remaining 45% to a control condition (no mailing). As expected, random allocation to the four experimental groups produces groups that are balanced in terms of background covariates. A multinomial regression of treatment assignment on voter turnout in 2008 and 2010 produces a non-significant likelihood-ratio statistic of 12.7 ($p=0.39$), which is consistent with the null hypothesis of random assignment.

Treatments

In keeping with prior studies of nonpartisan voter mobilization (e.g., Gerber and Green 2000), we partnered with the League of Women Voters. Postcards were labeled with the League's Washington, D.C. return address. Subjects in the control group received no mailing. Those in the Reminder group received a postcard with the following message:

> Our democracy only works if you vote.
> Dear Registered Voter,
> Election Day is coming up on November 6.
> Visit Vote411.org to find your polling place and learn who will be on your ballot.
>
> Our democracy only works if people make their voices heard. VOTE ON NOVEMBER 6.

Virginia subjects receiving the Warning treatment received an identical postcard except that the bolded phrase "Please be aware that a new law requires all voters in Virginia to show proof of their identity at the polls in order to vote" appeared after the "Visit Vote411.org" exhortation. In Tennessee, the Warning treatment's additional bolded phrase was

[10]The larger number of subjects sent for address verification in the VA border sample was due to our having encountered larger-than-expected drop-off in the TN border sample.

"Please be aware that a new law requires all voters in Tennessee to show photo identification at the polls in order to vote." The message to subjects in the Help treatment group in Virginia followed the bolded "Please be aware" clause in the Warning Treatment with:

> You may show several kinds of documents, including a driver's license, bank statement, utility bill, or voter registration card.
>
> To learn about all the ways you can show proof of identity, call us toll-free at 1-866-OUR-VOTE or visit www.sbe.virginia.gov/votinginperson.html. If you don't yet have an accepted proof of identity, contact your local registrar of voters.

In the Tennessee Help treatment, this text was replaced with:

> You may show several documents that have your name and photo on them, including a driver's license, US passport, US military ID, or federal or state employee ID.
>
> To learn about accepted photo identifications, call us toll-free at 1-866-OUR-VOTE or visit www.tn.gov/safety/photoids.shtml. If you don't yet have an accepted state-issued piece of photo identification, a free ID can be obtained for voting from a Tennessee Driver Service Center.

It should be noted that the Help treatment adds two components to the Warning message, information about valid forms of identification and a help line number. We cannot distinguish the distinct effects of each component; instead, we estimate the joint effect of these two helpful pieces of information.

Each mailer type is displayed in Appendix B. All postcards were mailed on Tuesday, October 23, and the authors and mail vendors received them at the latest on October 26th.

Outcome measures

In February 2013, we obtained voter turnout information from registrars in each of the sampled Virginia and Tennessee jurisdictions. This information allows us to tabulate turnout rates by treatment condition for subjects in each geographic stratum.[11] In addition, because we randomly selected members from each household, we are also able to analyze our mailings' "spillover effects" on registered voters living in the same households as our addressees (Sinclair, McConnell, and Green 2012). Spillover effects might occur through verbal communication about the voter ID law or the postcard. Alternatively, non-addressees might have read the postcards addressed to their housemates. Whereas all addressees were by design low-propensity voters (i.e., those who failed to vote in the general election of 2008, 2010, or both), other household members included both low- and high-propensity voters (who voted in both the 2008 and 2010 elections), making possible a comparison of spillover effects on these two types.

Statistical model

Our statistical analysis is greatly simplified by the fact that the probabilities of assignment to each of the three treatment groups are constant across the four geographic strata in our study. Unbiased point estimates for the three treatment effects in each of the four strata can be obtained simply by comparing average outcomes in each stratum, and the strata may be pooled in order to generate estimates of the overall average treatment effect. The precision of these estimates may be improved by controlling for background covariates that predict voter turnout. From the voter files, we have information about whether subjects were registered to vote and participated in the November elections of 2008 and 2010. We also know the number of people who are registered to vote at each subject's address. And, of course, we can control for the geographic strata within which the experiment occurred. Although the estimated coefficients for these control variables have no causal interpretation, the inclusion of covariates eliminates any incidental imbalance in background attributes across experimental groups and reduces the standard errors of our estimated treatment effects. In keeping with our pre-analysis plan, we report the multivariate

[11] Another potential outcome measure is the casting of a provisional ballot. However, provisional balloting turned out to be rare. In Roanoke and the five border counties, only 95 such ballots were cast; presumably, this figure would be much smaller if we were to focus on provisional ballots cast by subjects in our experiment. We do not have corresponding information from Tennessee, but it appears that the upper bound of any treatment effect is small.

results using ordinary least squares (OLS) regression, but in Appendix A, we report probit estimates of the treatments' marginal effects, which are very similar.

RESULTS

Table 2 displays voter turnout rates for each experimental condition and for each of four geographic strata. Looking first at the results pooled over all regions, we see that turnout in the Reminder condition (40.08%) was only slightly higher than turnout in the control condition (39.95%). Turnout in the Warning condition was 40.92%, approximately one percentage point higher than in the control group, which runs counter to the conjecture that warnings about the need for proper identification demobilize voters. Turnout in the Help condition was slightly higher (41.08%) than under the Warning condition, suggesting that the extra encouragement in the Help message may have nudged turnout upwards.

Breaking down the results by state, we again find little support for the hypothesis that notification of ID requirements depresses turnout. In Tennessee, which has a more stringout ID requirement than Virginia, the Warning message boosts turnout. In Virginia's border counties, the Warning message appears to have a weak positive effect, while in Roanoke, the effect appears to be weakly negative. In each of the four regions, the Help message appears to raise turnout, with positive effects ranging from 0.66 to 2.03 percentage points.

Using regression to control for past voter turnout allows us to estimate these treatment effects more precisely and to estimate their accompanying standard errors. For completeness, we present the regression results in Table 3 both with and without controlling for turnout in past elections. After adjusting for covariates, the Reminder treatment is found to have a near zero effect; for the full sample, its estimated effect is -0.19 percentage points with a 0.68 percentage point standard error. The Warning mailing is found to increase turnout in the full sample by 0.49 percentage points but with a standard error (SE) of 0.53. Although our best guess is that this treatment raises turnout slightly, we cannot rule out the null hypothesis that the true effect is zero (two-tailed p-value $= 0.35$). The strongest evidence of a positive treatment effect comes from the Help treatment, which appears to raise turnout by 0.91 percentage points (SE $= 0.53$, two-tailed $p = 0.08$). Disaggregating the data by state, we find that the estimated effect of the Help treatment is almost identical in the two states. By contrast, the Warning treatment appears to work better in Tennessee (1.51 percentage points with a SE of 0.73) than in Virginia (-0.50 percentage points with a SE of 0.75) after controlling for covariates. A possible implication is that Warning-type messages are more effective where ID laws are more stringent, but we interpret this result cautiously given the high probability of obtaining a false positive from multiple subgroup comparisons (Benjamini and Hochberg 1995).

Further evidence that the notifications raised turnout comes from an examination of turnout among non-addressees living at the same address as those who received the experimental mailings. These regression estimates are presented in Table 4. Looking first at non-addressees as a whole, we see that they seem altogether unaffected by the Reminder. The Warning mailing produces a non-significant increase of 0.40 percentage points (SE $= 0.62$, two-tailed $p = .52$), but the Help message is associated with a marginally significant increase

Table 2. 2012 General Election Turnout by Sample and Treatment Condition, Addressees Only

	Tennessee samples			*Virginia samples*			
Condition	*Knoxville*	*TN border*	*Total TN*	*Roanoke*	*VA border*	*Total VA*	*Total both states*
Control	40.57%	29.69%	31.87%	50.71%	47.35%	48.02%	39.95%
	(2,041)	(8,163)	(10,204)	(2,041)	(8,163)	(10,204)	(20,408)
Reminder	41.20%	29.70%	32.00%	51.80%	47.25%	48.16%	40.08%
	(500)	(2,000)	(2,500)	(500)	(2,000)	(2,500)	(5,000)
Warning	41.10%	31.40%	33.34%	50.50%	48.00%	48.50%	40.92%
	(1,000)	(4,000)	(5,000)	(1,000)	(4,000)	(5,000)	(10,000)
Help	42.60%	30.35%	32.80%	52.60%	48.55%	49.36%	41.08%
	(1,000)	(4,000)	(5,000)	(1,000)	(4,000)	(5,000)	(10,000)
Total *N*	4,451	18,163	22,704	4,541	18,163	22,704	45,408

Note: N's in parentheses; includes only subjects addressed in the mailings.
TN, Tennessee; VA, Virginia.

Table 3. Ordinary Least Squares Estimates of Treatment Effects on Addressees, by State

	(1) Full sample	*(2) Full sample*	*(3) TN*	*(4) TN*	*(5) VA*	*(6) VA*
Reminder	0.0003	−0.0019	0.0008	0.0023	−0.0004	−0.0055
	(0.0076)	(0.0068)	(0.0103)	(0.0094)	(0.0111)	(0.0097)
Warning	0.0104	0.0049	0.0159*	0.0151*	0.0049	−0.0050
	(0.0059)	(0.0053)	(0.0080)	(0.0073)	(0.0086)	(0.0075)
Help	0.0103	0.0091	0.0084	0.0096	0.0122	0.0088
	(0.0059)	(0.0053)	(0.0080)	(0.0073)	(0.0086)	(0.0075)
2+ Residents	0.1132	0.0653	0.0998	0.0527	0.1264	0.0769
	(0.0045)	(0.0041)	(0.0062)	(0.0057)	(0.0066)	(0.0058)
TN Border	−0.1088	−0.1150	−0.1090	−0.1061		
	(0.0080)	(0.0072)	(0.0077)	(0.0070)		
Roanoke	0.0921	0.1015				
	(0.0101)	(0.0090)				
VA Border	0.0558	0.0854			−0.0364	−0.0203
	(0.0080)	(0.0072)			(0.0082)	(0.0073)
2008NV		−0.1029		−0.0250		−0.1875
		(0.0075)		(0.0101)		(0.0111)
2008V		0.3526		0.3973		0.2929
		(0.0078)		(0.0103)		(0.0118)
2010NV		−0.1922		−0.1351		−0.2468
		(0.0091)		(0.0122)		(0.0137)
2010V		0.2813		0.2526		0.2896
		(0.0113)		(0.0160)		(0.0159)
Constant	0.3589	0.4359	0.3639	0.3381	0.4454	0.6420
	(0.0078)	(0.0090)	(0.0081)	(0.0106)	(0.0088)	(0.0113)
N	45,408	45,408	22,704	22,704	22,704	22,704

*$p < .05$ (two-tailed); significance notations provided only for treatment variables.
Standard errors in parentheses. Sample reference categories are Knoxville (Models 1–4) and Roanoke (Models 5 and 6). The 2+ residents dummy identifies households in which the addressee lived with at least one other person. 2008 and 2010 status reference category is those not registered to vote in the county in the specified year; NV refers to those who were registered but did not vote in the specified year, and V refers to those who voted.

of 1.00 percentage points (SE = 0.60, two-tailed $p = .10$). Closer inspection of the pattern of treatment effects among non-addressees reveals that the bulk of this spillover effect may be traced to high-propensity voters, among whom the Help mailing was associated with a 1.77 percentage point increase in turnout (SE = 0.67, two-tailed $p < .01$). Ironically, high-propensity voters were excluded from our mailing targets on the grounds that they tend to be unresponsive to GOTV appeals in presidential elections (Arceneaux and Nickerson 2009). Additional research is needed to verify that the Help message has special resonance among voters who are otherwise strongly predisposed to vote.

CONCLUSION

Election reform is back on the front burner in American politics. Some changes, such as the liberalization of absentee voting procedures, early voting, and later registration deadlines are designed to make voting easier. State voter ID laws, which have been at the center of partisan controversy for nearly a decade, stand in contrast to such reforms. Not only do they make voting more difficult; these laws also present tactical challenges to campaigns seeking to mobilize low-propensity voters. Voter ID laws also place special burdens on public officials to notify voters of the new requirements.

This article considered the potential effects of a direct mail GOTV campaign that informs the electorate about state voter ID requirements. Given that voter turnout tends to respond weakly to most forms of direct mail, especially mailings that arrive in the run up to a closely contested presidential election, nonpartisan direct mail campaigns that notify voters about identification requirements might be thought futile—or perhaps even counterproductive insofar as they cause voters to feel unprepared or

Table 4. Ordinary Least Squares Estimates of Effects on Low- and High-Propensity Non-Addressees, by State

	(1) Full sample	*(2) Full sample (with controls for 2008 and 2010 turnout, not shown)*	*(3) Full sample: LP*	*(4) Full sample: HP*	*(5) TN: LP*	*(6) TN: HP*	*(7) VA: LP*	*(8) VA: HP*
Reminder	−0.0021	−0.0020	−0.0045	0.0035	−0.0025	0.0033	−0.0061	0.0032
	(0.0085)	(0.0077)	(0.0119)	(0.0094)	(0.0169)	(0.0231)	(0.0166)	(0.0096)
Warning	0.0079	0.0040	0.0073	0.0095	0.0118	0.0185	0.0037	0.0059
	(0.0068)	(0.0062)	(0.0097)	(0.0070)	(0.0138)	(0.0168)	(0.0135)	(0.0072)
Help	0.0100	0.0100	0.0063	0.0177*	0.0098	0.0309*	0.0034	0.0123
	(0.0066)	(0.0060)	(0.0093)	(0.0067)	(0.0134)	(0.0158)	(0.0130)	(0.0069)
TN Border	−0.1128	−0.1248	−0.1064	−0.1190	−0.1065	−0.1186		
	(0.0109)	(0.0091)	(0.0150)	(0.0115)	(0.0150)	(0.0115)		
Roanoke	0.0606	0.0680	0.0960	−0.0154				
	(0.0130)	(0.0109)	(0.0183)	(0.0114)				
VA Border	0.0191	0.0467	0.0294	−0.0016			−0.0665	0.0138
	(0.0103)	(0.0085)	(0.0150)	(0.0082)			(0.0132)	(0.0090)
High-Propensity	0.4640	−0.2145						
	(0.0048)	(0.0156)						
Constant	0.4522	0.5225	0.4428	0.9346	0.4409	0.9295	0.5403	0.9213
	(0.0108)	(0.0124)	(0.0148)	(0.0083)	(0.0158)	(0.0106)	(0.0135)	(0.0088)
N	34,576	34,576	22,515	12,061	10,149	3,428	12,366	8,633

*$p < .05$ (two-tailed); significance notations provided only for treatment variables.

Standard errors, clustered by household, in parentheses. HP refers to high-propensity voters (i.e., those who voted in both the 2008 and 2010 general elections). LPs (low-propensity voters) either were registered and did not vote in 2008 or 2010 or were not yet registered to vote in 2010.

anticipate that their votes might be challenged at the polls.

Nevertheless, our experimental results suggest that notifications about voter identification requirements may increase turnout. Warnings about the need for ID seem to raise turnout slightly, although not to a statistically significant extent. We find no evidence that they have a net demobilizing effect. Warnings coupled with encouragement to seek help in obtaining identification seem to produce somewhat stronger effects that border on statistical significance both among addressees and others registered at the same address. Despite important differences between Virginia's and Tennessee's ID laws, we find relatively similar effects in both states. A possible exception is the stronger effect of the Warning message in Tennessee, the state with the more stringent ID requirements.

Various mechanisms may underlie the overall pattern of positive effects. The mailings may have encouraged people who lacked accepted identification to obtain one or reminded people who had an accepted proof of identity to bring it to the polls. Perhaps the mailings provoked a backlash against laws perceived as attempts to disenfranchise eligible voters. It is even possible that notification of voter ID laws elevated turnout by bolstering confidence in the integrity of the electoral system. Future research along these lines might use surveys administered after the mail is received to assess whether attitudes such as confidence increase in the wake of these treatments.

Although the effects associated with the Help mailing are not large, they compare favorably with other GOTV campaigns in terms of cost effectiveness. The printing and distribution of the Help mailings cost $6,100 and produced 91 additional votes among addressees and 78 additional votes among non-addressees, for an overall cost-per-vote of $36. This degree of cost-effectiveness exceeds that of the typical nonpartisan direct mail campaign, many of which were evaluated in the context of low salience elections (Green and Gerber 2008, p. 139). A question for future research is whether notifications akin to the Help mailing are equally or possibly more effective in low salience elections. On the one hand, voters may be less motivated to secure the necessary ID when the stakes are lower; on the other hand, the notification message is more likely to attract attention when the overall volume of campaign communications is reduced. Clearly, this experiment warrants replication in other regions and electoral settings.

Although we interpret our findings cautiously pending replication, the results in hand have important implications for how government officials may implement the notification requirements that accompany many voter ID laws. Further experimentation is needed to verify that the mailings we tested produce similar results when sent by government officials rather than the League of Women Voters, yet it seems plausible that the Help message would tend to produce positive turnout effects if it were to accompany state-issued sample ballots or registrars' routine address verification postcards. Given the low marginal cost of including the help message as part of other official communications, the results presented here offer some assurance that a public notification campaign can inexpensively reach large numbers of voters without suppressing turnout.

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APPENDIX A

Table A1. Virginia and Tennessee Voter ID Law Summaries

	Virginia	*Tennessee*
Accepted forms of ID	VA voter registration card; VA valid driver's license or any other card issued by state or local VA government agency; social security card or other U.S. government-issued ID; employee ID card; valid student ID card from institution of higher education in VA; copy of current utility bill or bank statement; government-issued check; paycheck that shows name and address; concealed handgun permit	Valid voter ID card issued by TN Department of Safety; TN or other state photo driver's license (current or expired); valid photo ID card issued by any state; valid U.S. passport or military ID; gun permit card
Free ID for Voting	All registered voters mailed a VA voter registration card	Show proof of citizenship (e.g., birth certificate) and two proofs of Tennessee residency at most Driver Service Centers
State public education requirements	None	None
Exemptions	None	Those voting at nursing homes or who are hospitalized; those with a religious objection to obtaining a photo ID; those swearing at the polls that they are indigent and unable to obtain a free ID for voting

Note: Tennessee's law has since been tightened to exclude ID cards issued by other states and explicitly excluding cards issued by counties and municipalities. Tennessee's law is currently facing a court challenge. Virginia has since passed a photo ID law similar to Tennessee's. If it survives court challenges, it will be implemented in spring of 2014.

Table A2. Probit Estimates of Marginal Effects of Treatments on Addressees, by State

	(1) Full sample	*(2) Full sample*	*(3) TN*	*(4) TN*	*(5) VA*	*(6) VA*
Reminder	0.0004	−0.0027	0.0010	0.0023	−0.0004	−0.0072
	(0.0078)	(0.0083)	(0.0104)	(0.0107)	(0.0112)	(0.0122)
Warning	0.0109	0.0060	0.0162*	0.0179*	0.0049	−0.0059
	(0.0061)	(0.0064)	(0.0081)	(0.0084)	(0.0087)	(0.0094)
Help	0.0106	0.0113	0.0084	0.0115	0.0124	0.0115
	(0.0061)	(0.0064)	(0.0081)	(0.0083)	(0.0087)	(0.0094)
Control for 2008 and 2010 turnout	No	Yes	No	Yes	No	Yes
N	45,408	45,408	22,704	22,704	22,704	22,704

*$p < .05$ (two-tailed).

Standard errors in parentheses. Each model includes the same covariates as the model with the same number in Table 3. See notes in Table 3 for details. Probit coefficients (not shown but available from the authors) were converted into estimates of marginal effects on the probability of turnout using the *margins* command in Stata 12, holding all other covariates at their means.

TABLE A3. PROBIT ESTIMATES OF MARGINAL EFFECTS OF TREATMENTS ON LOW- AND HIGH-PROPENSITY NON-ADDRESSEES, BY STATE

	(1) Full sample	*(2) Full sample*	*(3) Full sample: LP*	*(4) Full sample: HP*	*(5) TN: LP*	*(6) TN: HP*	*(7) VA: LP*	*(8) VA: HP*
Reminder	−0.0015	−0.0019	−0.0047	0.0037	−0.0028	0.0046	−0.0061	0.0033
	(0.0105)	(0.0111)	(0.0121)	(0.0092)	(0.0170)	(0.0225)	(0.0166)	(0.0096)
Warning	0.0106	0.0074	0.0075	0.0092	0.0121	0.0183	0.0037	0.0060
	(0.0083)	(0.0088)	(0.0098)	(0.0068)	(0.0139)	(0.0164)	(0.0135)	(0.0072)
Help	0.0140	0.0158	0.0064	0.0171*	0.0098	0.0304	0.0034	0.0125
	(0.0080)	(0.0084)	(0.0095)	(0.0065)	(0.0134)	(0.0157)	(0.0131)	(0.0069)
Control for 2008 and 2010 turnout	No	Yes	No	No	No	No	No	No
N	34,576	34,576	22,515	12,061	10,149	3,428	12,366	8,633

*$p < .05$ (two-tailed).

Standard errors, clustered by household, in parentheses. Each model includes the same covariates as the model with the same number in Table 4. HP refers to high-propensity voters (i.e., those who voted in both the 2008 and 2010 general elections). LPs (low-propensity voters) either were registered and did not vote in 2008 or 2010 or were not yet registered to vote in 2010. Probit coefficients (not shown but available from the authors) were converted into estimates of marginal effects on the probability of turnout using the *margins* command in Stata 12, holding all other covariates at their means.

APPENDIX B

FIG. B1. Reminder mailing sent to voters in both Tennessee and Virginia.

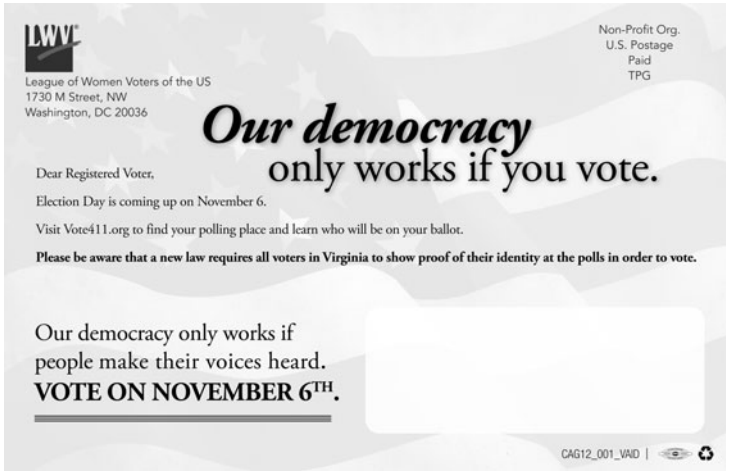

FIG. B2. Warning mailing sent to voters in Virginia.

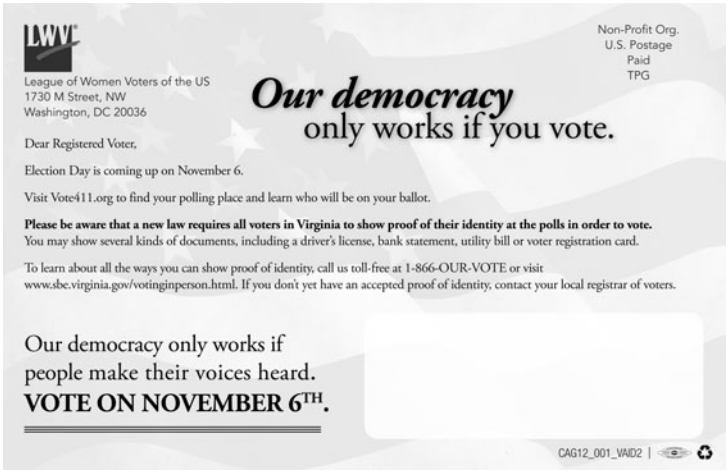

FIG. B3. Help mailing sent to voters in Virginia.

FIG. B4. Warning mailing sent to voters in Tennessee.

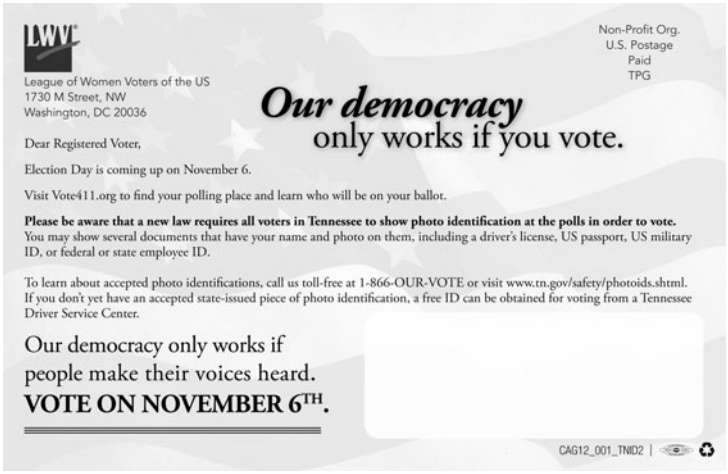

FIG. B5. Help mailing sent to voters in Tennessee.