

Can Raising the Stakes of Election Outcomes Increase Participation?
Results from a Large-Scale Field Experiment in Local Elections

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Abstract

Political campaigns frequently emphasize the material stakes at play in election outcomes in seeking to motivate voters. But field experimental academic work has given greater attention to other aspects of the voter's decision to participate, perhaps because the dominant theoretical model of turnout implies material incentives matter only when one's vote might decide an election. We identify three classes of treatments that may increase the material incentive to participate and test these messages in a large scale placebo-controlled field experiment in which approximately 38,000 treatment letters were delivered during Connecticut's 2013 municipal elections. We find clear evidence that these messages are effective in increasing participation, and also that some of these messages appear more effective than non-partisan GOTV material in motivating participation. These findings have important implications for our understanding of how voters decide whether to participate and how best to mobilize citizens who otherwise sit out elections.

Word Count: 9,781 words

(Nuclear explosion takes place on screen)

President Johnson: "These are the stakes. To make a world in which all of God's children can live, or to go into the dark. We must either love each other, or we must die."

Narrator: "Vote for President Johnson on November 3rd. The stakes are too high for you to stay home."

"Daisy" Campaign Advertisement, 1964

As this epigraph illustrates, a common theme in campaign communications is that who wins the upcoming election is a matter of enormous consequence. Campaign appeals invoking the critical importance of the election are frequently dramatic and often stretch credibility, but are clearly viewed as an essential ingredient by nearly all successful campaigns. Creating a sense that the stakes are important is perhaps the central feature of campaigns and mobilization efforts (e.g. Sides and Vavreck 2013; Vavreck 2009). By contrast, academic work on voter turnout has traditionally discounted the role of perceived stakes in influencing the voter's decision to participate. Historically the most influential framework for analyzing turnout and organizing empirical results, the "calculus of voting" model (Downs 1957; Riker and Ordeshook 1968) posits that voters' turnout decisions follow from assessments of the costs versus the benefits of participation, where the core instrumental benefit of participation is calculated as the expected consequences of altering the election outcome for the individual ("B" for net benefits) multiplied by the probability that the voter's participation in the election changes the outcome ("P"). However, the miniscule probability any vote affects the election outcome in mass politics implies that unless the perceived electoral consequences are unrealistically large or the beliefs about one's chances for deciding the election are dramatically overstated, the product of these quantities (P times B) cannot be the source of substantial turnout and, consequently, changing the perceived stakes of the election is not an effective method for producing more than a minute increase in turnout levels.

This standard model therefore naturally turns attention away from explaining turnout based on factors related to the specific political context, such as the candidates running for office and the importance of the contested office, and toward factors like voters' values and beliefs that may explain participation for reasons unrelated to the material stakes of the election. This has led to great attention to how a citizen's general engagement with politics and desire to perform their civic and social duty may explain participation, because these obligations and rewards—unlike the stakes of the election—are relevant regardless of whether an election is close (that is, they are independent of P ; Downs 1957; Riker and Ordeshook 1968).² Numerous field experimental studies have subsequently shown that appeals to civic duty, social norms, and other factors unrelated to the stakes of an election have modest but incremental effects on turnout (see Green and Gerber 2015 for an overview).

But focusing on the motivational power of appeals to democratic citizenship is in stark contrast to what actually unfolds in most campaigns, where turnout appeals are typically

² An alternative line of work avoids the problem of the vanishingly small “ P ” term by arguing that voting is expressive, which means that merely voting confers a variety of individual psychological benefits regardless of the effect of the vote on the outcome. These expressive benefits may stem, for example, from affirmation of membership in the community or nation, expression of group and partisan solidarity, or expression of approval of a politician (e.g., Rogers et al. 2013; Schuessler 2000). Our treatments focus on the effects of varying the salience of the electoral stakes, and in the conclusion we discuss expressive voting as one of several possible explanations for our findings. For those who favor expressive explanations, our work shows how expressive benefits change by making more salient key features of what is at stake in elections.

designed to create a sense of urgency due to the importance of the election contest (why this race matters a lot) rather than emphasizing broad themes of civic engagement or democratic responsibility (e.g., Fowler et al. 2016). In the language of the calculus of voting model, campaign appeals often work to mobilize by increasing the salience of the “B” term. However, systematic studies of these sorts of appeals, and comparisons of their relative effectiveness to standard non-partisan get-out-the-vote (GOTV) messages emphasizing non-material reasons to participate, are not widespread. This paper presents the first field test (to our knowledge) of the turnout effects from increasing the salience of the stakes of an election. To do so, we designed and implemented a large-scale voter mobilization field experiment in which we mailed roughly 38,000 letters to registrants a few days before the November 2013 local general elections in the state of Connecticut. The main aim of our three classes of experimental treatments, which provide various pieces of information about local spending and taxation or the composition of the electorate, is to remind citizens of the stakes of voting in municipal elections. These treatments (1) emphasize the general power of control of government, (2) implicate poor government performance, or (3) imply the electorate is potentially unrepresentative of the registrant’s preferences. Additionally, we build into our design a standard non-partisan GOTV civic duty appeal so that we estimate the incremental effectiveness of providing information about the material stakes of the election. We focus on local elections because they provide an environment where the dissemination of non-partisan factual information can alter the perceived stakes of voting.

Prior observational work has noted the correlation between numerous features of the individual (usually measured using survey data) and voting.³ A general finding in research focusing on differences across individuals is that those who vote are more informed about and interested in the race, have more of a vested self-interest in its outcome, and see greater benefit to one candidate winning versus another (e.g., Adams et al. 2006; Leighley and Nagler 2014; Lewis-Beck et al. 2008; Rosenstone and Hansen 1993). Scholars have speculated that the failure to appreciate the consequences of voting explain low turnout, a pattern that is exacerbated in non-presidential contests (e.g., Hajnal 2010; Leighley and Nagler 2014). But testing these arguments is difficult using observational data because, for example, being informed and engaged may be both a consequence and a cause of political participation. Additionally, prior research suggests reasons for skepticism—if those who are not engaged have low efficacy or trust and therefore believe the political system is beyond their control or comprehension (e.g., Verba et al. 1995), then merely knowing much is at stake would be unlikely to change patterns of participation. By contrast, our approach allows us to isolate the causal linkage between increasing the salience of election stakes and turnout and estimate the magnitude of the effect produced by these interventions compared to standard non-partisan voter mobilization messages.

We find that messages emphasizing the budgetary stakes of local elections, tax burdens, and the unrepresentativeness (age bias) of the local electorate are all effective in increasing participation in local contests. Compared to an untreated control group, these effects are

³ A related literature has examined differences in participation across types of election (e.g., by office) or contests (e.g., comparing voting in competitive vs. non-competitive races; e.g., Holbrook and McClurg 2005).

somewhat large. A single letter emphasizing the budgetary resources that are controlled by local government increases turnout by between 2 and 3 percentage points; a letter highlighting the comparative tax burden in a citizen's town is estimated to increase voting by about 4 points; and messages emphasizing the age bias in the electorate increase voting by about 2 points. By contrast, a simple non-partisan GOTV letter, which is the base to which these novel treatments are added, has a point estimate of 1.3 points, meaning that many of these effects are substantively important compared to standard mobilization efforts. Overall, these results support the argument that, despite the very small probability a voter will tip the election, communication emphasizing the stakes of the election can have a substantial effect on turnout (particularly when providing information about tax burden).⁴ Assuming these results replicate, our findings suggest the use of these messages may be a powerful and underutilized tool for increasing engagement in local contests.

Our results have a number of important implications. First, they provide causal evidence that inattention to the stakes of electoral contests reduces turnout. Although prior work has hypothesized this relationship using survey data about perceived differences between the candidates and reported reasons for abstention (e.g., Adams et al. 2006; Leighley and Nagler 2014), our findings rule out standard concerns about omitted variable bias from observational analyses and survey response misreports (e.g., post-hoc rationalizations or social desirability bias). Second, the results highlight that the perceived stakes of an election pertain not just to its outcome but also to who decides that outcome. Numerous studies note that those who do and do

⁴ Note that we cannot rule out the possibility that individuals believe p is reasonably large in local elections.

not vote have distinct policy preferences, with nonvoters generally at least moderately more liberal on economic matters (e.g., Ansolabehere et al. 2006; Bennett and Resnick 1990). Our findings, however, suggest that abstainers may not recognize that voters fail to represent their interests, which means that those who stay home perceive lower-than-warranted stakes in participation. Finally, the findings signal a potentially more successful approach to engage nonvoting registrants than many existing strategies. Our experiment shows that effective mobilization is not simply a matter of reminding registrants of an upcoming contest or telling them that it is their civic duty to vote, but rather convincing them of an election's stakes and informing them about how well the electorate reflects their preferences.

Can Invoking Election Stakes Increase Turnout?

Scholars have long noted that low and unequal turnout plagues American elections. Participation rates in presidential contests have barely reached 60% of the voting eligible population in the past several decades (United States Election Project 2014), and those who vote in these races are older, wealthier, less likely to be racial or ethnic minorities, more educated, and more informed about politics. These patterns tend to be exacerbated in less salient elections, with lower and more unequal turnout in midterm, primary, and local contests (e.g., Leighley and Nagler 2014; Oliver 2012).

Among the many proffered explanations for these low and differential turnout rates is variation across individuals in perceptions of how an election's outcome affects their self-interest. Resource-based, psychological, and rational-choice models of participation all attribute a prominent role in the decision to vote to such perceived stakes, whether they are financial, psychological, or social benefits an individual expects to derive from one candidate winning versus another (e.g., Campbell et al. 1960; Riker and Ordeshook 1968; Verba et al. 1995). These

theories anticipate that those who are more informed about and interested in politics, perceive greater policy differences between the candidates and/or parties, or care more about the outcome should be more likely to vote. In contrast, nonvoters should have greater difficulty seeing the importance of a particular electoral outcome. This should not be surprising; if the stakes are unclear, then an individual has much less of an incentive to vote.

A rich descriptive survey literature is consistent with this perspective,⁵ but leaves open several critical questions. Most centrally, it is unclear whether perceiving greater stakes of voting is causal in explaining participation. As with any observational analysis, the central problem is that omitted variables could cause both participation and perceptions, a problem that is exacerbated in the survey context where measurement error is ubiquitous. The clearest evidence that the stakes of an election matter in driving turnout is that those whose jobs or family well-being are directly tied to an election, like teachers or parents of school-aged children voting in municipal elections, are most likely to vote (Anzia 2011; Berry 2009; Moe 2006), although this

⁵ For example, higher levels of political information and interest are associated with a higher propensity to vote (e.g., Campbell et al. 1960; Leighley and Nagler 2014; Lewis-Beck et al. 2008). Wealthier individuals, who participate at higher rates than poorer Americans, perceive greater policy differences between presidential candidates (Leighley and Nagler 2014). Relatedly, reported indifference between candidates reduces the inclination to vote in presidential (Adams et al. 2006) and midterm elections (Plane and Gershtenson 2004). Finally, survey respondents frequently justify decisions to abstain in terms of failing to see the stakes of the election (e.g., Verba et al. 1995).

work cannot distinguish between greater concern for the outcome, greater knowledge, or group mobilization as driving participation.

Additionally, even if abstainers stay home because they believe little is at stake, this does not mean that such beliefs are remediable—these individuals may be altogether alienated, believe the political system is too complicated for them to understand or control (low efficacy), or have difficulty figuring out for whom they ought to vote. In these cases, merely knowing government is important may do little to bring people who know they ought to care to the polls.

Altering the Perception of Electoral Stakes

If nonvoters' lack of motivation to vote stems in part from failing to perceive why a corresponding electoral outcome might matter, one seemingly fruitful manner to increase participation is to change that perception. But there is scant prior work testing the possibility of making the stakes of participation higher to increase voting, and the work that does exist often tests messages that bundle multiple appeals, making it difficult to discern the relative importance of altering perceptions of the importance of voting (e.g., Nickerson 2007).⁶ Instead, political science has mostly focused on institutional reforms that reduce the costs of participation (such as making registration and voting more convenient; see e.g., Hanmer 2009) or evaluating the effects of messages emphasizing the non-instrumental returns to voting (such as civic duty or social norms; see e.g., Gerber and Green 2000; Gerber et al. 2008; Panagopolous 2011). Other

⁶ A related body of work focuses on altering beliefs about the pivotality of one's vote rather than the perceived stakes of who wins. These studies seek to heighten perceived pivotality by stressing the closeness of a particular electoral contest or elections in general (Enos and Fowler 2014; Gerber and Green 2000).

proposed solutions to low levels of participation have extremely long timelines, such as trying to inject politics into the lives of people at an early age through general education or more tailored civics and service-learning programs (e.g., Patterson 2003; Schlozman et al. 2012). Many of these programs are costly and have not yet been shown to be effective.

Those alternative approaches, however, neglect the possibility that for at least some individuals, and in some electoral contexts, failing to vote may stem not from systematic ignorance about the importance of politics or the cost of voting, but rather from an election-specific solvable informational issue about the stakes of participation. These effects are likely particularly important in lower-salience elections where candidate- and campaign-level factors do less to engage these potential voters. Simply put, given that Americans tend to be poorly informed about political matters in general and know even less about state and local affairs (e.g., Delli Carpini and Keeter 1996; Hopkins 2018), one might be able to increase participation just by making citizens aware of the consequences of abstaining.

Given this possibility, we identify three classes of messages that may be potentially useful in increasing participation and have not been employed in prior field experiment work. The first is a simple message emphasizing what is a stake in control of government. This is equivalent to increasing perceptions of the value of B in the calculus-of-voting model. The second is information about relative government performance. Both academic work on retrospective voting (e.g., Arceneaux 2003; Fiorina 1978; Niven 2006) and candidate campaign communications emphasizing an incumbent's good or bad performance directly point to the prospects of increasing the motivation to vote by stressing how well or poorly current government performs. Failures in particular have potential to motivate previous abstainers to understand the value of voting (once again, this can be understood as changing the value of B in

the calculus-of-voting model). The third class of messages informs voters about the stakes of allowing electoral choices to be made by others. Some nonvoters may not recognize the value in voting because they perceive, correctly or incorrectly, that those who do vote are like them (demographically and/or socioeconomically) and/or share their preferences for government policy. To the extent that nonvoters believe that voters represent their political interests and preferences, then voting can be safely left to others. That belief may be misplaced, and in fact an extensive literature concludes that the documented demographic and socioeconomic distinctions between those who do and do not vote correspond to different policy preferences in at least some (generally economic) areas (e.g., Ansolabehere et al. 2006; Leighley and Nagler 2014). If people come to understand such differences, then the stakes of allowing those who do not share one's policy preferences to make electoral decisions on their behalf could serve to make them more likely to vote, perhaps in part by making the likely distributive consequences of voting more salient.

Experimental Design

We designed and implemented a large-scale field experiment during the November 2013 local elections cycle in Connecticut to test the three classes of messages introduced above. The experiment targeted active, eligible registrants in towns that held municipal elections at that time. After excluding invalid and out-of-state mailing addresses, we randomly selected a single person from each (potentially multi-person) household for inclusion in the sampling frame.⁷ We then constructed 16 assignment strata, which were composed of the cross between 3 age categories

⁷ We additionally removed from our sampling frame households with selected journalists and elected state and local officials to avoid media coverage of the experiment in progress.

(less than 35, 35-54, 55+) and 5 past participation patterns, based on meeting the first criteria as listed: New registrant (first registered after November 1, 2010), Ever municipal voter (had ever voted in a local election), Ever midterm voter (had ever voted in a midterm election, Ever voter (any record of voting), and Never voter, as well as one additional stratum (composed of approximately 9000 individuals who we recruited to participate in a separate survey project).

We then assigned treatments at different rates across strata so as to achieve adequate power to compare across these groups. Setting aside the survey group (from which we assigned 4000 to treatments with the goal of being able to merge survey data to treatment effectiveness assessments in later work), this involved undersampling ever municipal voters (because they had already displayed a propensity to vote in local elections) and oversampling ever midterm and ever voters (with the goal of testing whether these groups could be induced to participate in municipal elections). Panel A of Table 1 lists the counts of selected registrants in the initial sampling frame and overall treatment assignment rates (any treatment assigned) for each stratum. Our sample is composed of 962,118 registrants and we sent a total of 37,983 treatment mailings.

In addition to varying the rate at which we treated the strata, the rate at which each treatment was assigned within strata differed across treatments (but was the same across strata). We assigned our “placebo” treatment, modeled after a standard GOTV treatment letter, at higher rates because we compare multiple treatments to this baseline. We also allocated additional resources to treating two different electorate-composition treatments (described below) to enable us to make precise comparisons for treatments we thought might differ modestly in their comparative effectiveness. Treatment assignment rates (which are the same conditional on any

Table 1A. Sampling Frame and Overall Treatment Assignment Rates

Registration Strata	Number of Registrants	Number with Any Treatment Assigned	Any Treatment Assigned Rate
Survey	9,014	3,999	0.44
New Registrant, <=35	61,619	1,416	0.02
New Registrant, 35-55	31,819	1,416	0.04
New Registrant, >=55	15,857	1,416	0.09
Ever Local Voter, <=35	28,543	2,832	0.10
Ever Local Voter, 35-55	149,340	2,832	0.02
Ever Local Voter, >=55	292,600	2,832	0.01
Ever Midterm Voter, <=35	26,864	2,832	0.11
Ever Midterm Voter, 35-55	65,765	2,832	0.04
Ever Midterm Voter, >=55	66,760	2,832	0.04
Ever Presidential Voter, <=35	49,486	2,832	0.06
Ever Presidential Voter, 35-55	58,562	2,832	0.05
Ever Presidential Voter, >=55	35,442	2,832	0.08
Never Voter, <=35	23,517	1,416	0.06
Never Voter, 35-55	26,548	1,416	0.05
Never Voter, >=55	20,382	1,416	0.07

Note: Initial universe is all active Connecticut registrants listed in an October 2013 voter file residing in a town with a local election in November 2013 and who had a valid mailing address in the state, with a limit of one registrant selected at random from multi-person households. Additionally removed from the sampling frame are households with selected journalists or elected state and local officials.

Table 1B. Treatment Assignment Rates, by Treatment

Treatment	Number of Treatments Assigned	Proportion of Assigned Treatments
GOTV Treatment	8,933	0.24
Budget Own Spending Treatment	2,611	0.07
Budget Average Spending Treatment	2,603	0.07
Budget Comparative Spending Treatment	2,608	0.07
Taxes Own Mill Rate Treatment	1,566	0.04
Taxes Low State Average Treatment	1,566	0.04
Taxes High State Average Treatment	1,562	0.04
Taxes Comparatively Higher Treatment	1,566	0.04
Taxes Comparatively Lower Treatment	1,566	0.04
Age Bias Smaller Treatment	6,706	0.18
Age Bias Larger Treatment	6,696	0.18

treatment being applied across stratum) are summarized in panel B of Table 1.⁸ Our subsequent analysis accounts for the different rates of assignment across treatments and strata using inverse probability weights and indicators for stratum.

Treatment letters were mailed via US first-class mail from a Connecticut location under cover of “Connecticut Votes (CTV)” and timed to be delivered 3 to 5 days before the election. We measured turnout in the 2013 election as recorded in a Connecticut voter file dated February 11, 2014. Individuals were coded as having voted if they were listed in the file and had a turnout record for November 2013. Individuals who were listed as not voting or who were no longer present in the file were coded as not voting. 31% of registrants in our initial sampling frame voted in November 2013.

Treatment Descriptions

We designed treatments to mirror the three classes of messages described above. The basic template for all treatments is the text from our standard non-partisan GOTV mobilization letter that both informs respondents about the upcoming election (provides information) and invokes civic duty norms. The treatment letters hold constant the GOTV message but provide additional content. This approach allows us to assess whether simple non-partisan communication can increase participation in local elections and to benchmark our other

⁸ For each stratum we assessed whether observed covariates (age, gender, years since registration, and town) explain treatment assignment using a multinomial logit and fail to reject the null that they are jointly insignificant in all cases (the smallest p-value is .16, the next smallest is .27, and the mean is .61).

treatments to this standard campaign mobilization tactic.⁹ The text of the GOTV treatment read as follows:

Dear <FIRSTNAME> <MIDDLENAME> <LASTNAME>,

This letter is to remind you that a Municipal Election will be held on Tuesday, November 5th, 2013. Polls will be open from 6 AM to 8 PM on Election Day. Connecticut records show that as of September 15th, 2013 you were registered to vote.

Voting is a right and responsibility. Whichever candidate or party you prefer, we want to remind you to exercise your right to vote this November. The right to vote is an important American tradition. The whole point of democracy is that citizens are active participants in government and democracy functions best when everyone takes part in the voting process. This November, remember your rights and responsibilities as a citizen.

If you have any questions about the voting process, please visit the Secretary of State's website (<http://www.sots.ct.gov>) or call your local Registrar of Voters.

Sincerely,

The Connecticut Votes Team

Budget Treatments

Our first class of treatments sought to emphasize the material resources at stake in local government control by discussing local budgets. The three versions of this treatment told recipients that local elections affected the allocation of local spending and informed them about spending levels in different ways (thereby informing them of the large amount of money controlled by local governments). Across all Connecticut municipalities, average spending per capita in 2012 was approximately \$3,400, with a range from \$1,600 to \$7,100. The *Budget Own Spending* treatment text, which followed the GOTV language described above and emphasized what was at stake in the respondent's own locality, read as follows:

Your voice starts with your vote: In Connecticut, local elected officials make many important decisions that affect your life. One of the most important things they do is decide what to spend money on. **In 2012, your city/town's budget was \$XXXX per**

⁹ Examples of all treatment letters appear in the supplemental appendix (pages 2-11).

resident. By voting, you make sure local elected officials are thinking about you when they decide how to spend your money. Make sure your voice is heard!

The *Budget Average Spending* treatment is identical except that it replaces the town-level figure with the average across all towns (“In 2012, the average city/town budget in Connecticut was \$3,354 per resident.”). Unlike the town-level treatment, therefore, this treatment is homogenous across all towns (i.e., it does not present different information depending on actual spending levels in a town/city). If individuals use the statewide average to make inferences about spending levels in their own locality, this should also increase the motivation to participate.

Finally, the third material resources treatment, *Budget Comparative Spending*, provides both pieces of information simultaneously along with a direct comparison of these amounts (“In 2012, your city/town’s budget was \$XXXX per resident, which is ZZ% [higher/lower] than the \$3,354 average in all Connecticut cities/towns.”). Theoretically, how this treatment might work is somewhat ambiguous: it provides the same information about the average and local stakes of voting, but in comparing the two amounts it may make the stakes in a particular city/town appear larger or smaller. If a locality’s spending is above the state average, the stakes of controlling those resources are arguably higher. On the other hand, if a locality spends less than other towns, citizens may infer that their city should raise and spend more, which may increase the motivation to participate. As such, we do not have clear ex ante predictions about the effectiveness of this treatment, which may be heterogeneous.

Tax Treatments

Our next set of five treatments focused on tax rates rather than spending behavior. In Connecticut, as with many other states, local property taxes are an important source of revenue and tax rates vary substantially due to differences in the relative wealth and homogeneity of

localities. Our treatments of primary interest present individuals with a comparison of their own tax rates to either a low or high state tax baseline, which provides information about relative government performance and, depending on the person's policy preferences, may imply a current failure of government. Our primary expectation is that telling people their tax rates are comparatively higher than elsewhere in the state will increase turnout more. For some individuals, however, even a comparison to a higher baseline will make their taxes appear high, in which case the same mechanism may increase turnout.¹⁰ Additionally, it may also be the case that simply informing individuals about their relative tax burden, even if it is less onerous than elsewhere in the state, induces them to participate because it makes clear the stakes of local elections and leads citizens who prefer higher spending levels to vote in order to seek increases in local tax effort.

To construct the *Taxes Comparatively Higher* and *Taxes Comparatively Lower* treatments without engaging in deception, we calculated two measures of the average tax burden in Connecticut. The first, which yields a lower baseline level of taxation, is the average taxes calculated on a town-by-town basis across the state. In 2012, the average mill rate across Connecticut localities was 23.90. Our second measure is the average mill rate individuals experienced in Connecticut, which is higher at 28.18 because more populous towns tend to have

¹⁰ Approximately 38% of Connecticut residents have tax rates above the average under either comparison, 38% have rates below average under either comparison, and the remaining 24% have rates above average under the low baseline but below average under the high baseline. We note that this is an observational partitioning.

higher tax rates.¹¹ By constructing two measures of relative taxes we can expose all respondents to messages that make their taxes appear higher or lower, whereas if we relied only on variation across localities we would have to be concerned that places with higher (lower) taxes would respond to the comparative treatments differently because of some correlation between underlying citizens preferences and observed local tax rates. The complete treatments (1) describe how taxes are calculated in Connecticut, (2) provide information about the resident's local tax (mill) rate, and (3) provide information about either the higher or lower baseline described above. The following is an example of the treatment text, which again is added to the baseline GOTV content:

Your voice starts with your vote: In Connecticut, local elected officials make many important decisions that affect your life. One of the most important things they do is set the property tax rate, which affects the taxes that people who own real estate pay each year. This tax rate is called the mill rate. Each mill is the number of dollars that a person has to pay for each \$1,000 in assessed value for their property (by state law, property is assessed at 70% of its fair market value). Here is some information about local property taxes:

- **Local taxes fund more than 72% of local spending in Connecticut.**
- **[Higher Treatment] In 2012, the average city/town's mill rate in Connecticut was 23.90.**
- **[Lower Treatment] In 2012, the average person in Connecticut had a mill rate of 28.18.**
- **In 2012, your city/town's mill rate was XX.XX.**

By voting, you make sure local elected officials are thinking about you when they are setting your tax rates. Make sure your voice is heard!

We note that in this treatment we do not directly calculate a relative tax burden. Rather, recipients must perform this calculation themselves.

¹¹ This reflects a variety of sources, including a greater preponderance of property exempt from taxation, greater services demand and delivery, and more wealth inequality.

This treatment has many different components apart from the comparison of local tax rates to these different baselines, and so we also implemented three treatments that include each of those components in isolation. The *Taxes Own Mill Rate* is the above treatment without any reporting of statewide taxes. The *Taxes High State Average* and *Taxes Low State Average* are the above treatment without the reference to the recipient's local taxes.

We did not have strong theoretical expectations about how these different component parts would operate in isolation. The mill rate is a relatively abstract concept without any comparison, although this treatment does mention that local taxes are an important source of resources. For the *Taxes High State Average* and *Taxes Low State Average*, the inclusion of this information in isolation allows us to test whether simply telling people about a higher or lower state baseline alters turnout (though without any reference point subjects likely do not recognize that these baselines should be seen as "high" or "low"). If simply telling people taxes in the state are lower increases turnout more than telling them taxes are higher (without a comparison to one's own taxes) then it is inappropriate to interpret the comparison of the *Taxes Comparatively Higher* to *Taxes Comparatively Lower* as operating via the comparison rather than the different state baselines. Our design allows us to directly test for this assumption violation.

Electoral Composition Treatments

Our final set of treatments present respondents with information about the unrepresentativeness of local electorates, namely the fact that older registrants are overrepresented relative to younger registrants among those voting in local contests. This allows us to assess whether making (younger) registrants aware that older voters, who they may believe hold different policy preferences, are comparatively overrepresented in the electorate increases

participation, and whether these effects are enhanced when information is presented that makes these differences seem larger.

As with the tax treatments, we constructed two versions of this treatment without engaging in deception. To do so, we calculated in each locality the age gap in turnout between those registrants age 55 and older and those less than 35 in two elections: The 2011 municipal elections (where, on average, turnout was lower and older voters were more likely to vote than younger voters) and the 2012 presidential election (in which overall turnout was higher and age differences tended to be more muted). For each locality, we then used whichever calculation produced a larger (smaller) difference in turnout for our *Age Bias Larger (Age Bias Smaller)* treatment. The average turnout gap in the larger and smaller bias treatments is 33 and 21 points, respectively. Potential treatment intensity (the difference between the two figures) ranged from 0 to 33 points. Treatment text, with the appropriate comparison of younger and older turnout, appears below. As before, this content is added to the baseline GOTV treatment content:

Your voice starts with your vote: In Connecticut, local elected officials make many important decisions that affect your life. One of the most important things they do is decide what to spend your money on. Here are some facts about voters in your city/town:

- **In a recent election in your city/town, [2012 Turnout]% of registrants voted.**
- **In a recent election in your city/town, registered voters over 55 were XX percentage points more likely to vote than registered voters under 35.**

By voting, you make sure local elected officials are thinking about you when they decide how to spend your money. Make sure your voice is heard!

Note that in these two treatments we hold constant municipality turnout (we always report 2012 turnout) but manipulate whether the age gap is smaller or larger. We also report the gap in participation rather than rates, because rates are generally lower in elections where the age gap is larger and we do not want absolute rates of participation to be collinear with treatment.

Furthermore, in addition to providing information about the unrepresentativeness of the electorate, these messages may also convey information about descriptive norms. That is, the message that makes local electorates appear less representative makes it seem like older voters are more likely to vote than younger voters. In this case, these norm effects (Cialdini et al. 2006; Gerber and Rogers 2009) may actually depress turnout among younger voters and increase it among older voters. In light of this possibility, one can examine whether the messages are effective among a group that is not included in the comparison set—voters 35 to 54—who nonetheless learn about the unrepresentativeness of the electorate without their own behavior being described and therefore being subject to norm compliance.

Data Analysis and Results

Our analysis proceeds in two steps. We first assess the average effect on turnout of receiving any treatment and the comparative effectiveness of the GOTV message. We report these analyses both for our entire sample and by past patterns of voter participation. Next, we examine the effectiveness of each of our three sets of treatments, reporting results for the entire sample as well as theoretically-motivated subgroups. As part of this presentation, we compare treatment effectiveness both to the uncontacted control group and the baseline GOTV message.

Baseline Effect of Receiving any Treatment or GOTV Message

We begin by examining the effect of being sent any of our treatment messages.¹² Our analysis strategy, which we repeat throughout, uses OLS regression with controls for the 16

¹² These effects are for assignment to treatment, as we cannot guarantee that subjects received the assigned message (e.g., mail may have been discarded, etc.).

assignment strata discussed above (as indicators) and town-level fixed effects (as indicators).¹³

Where applicable, we also use weights to account for the different rates of assignment to treatment across treatments and strata (in this analysis we have only a single treatment, and so weights account only for different average treatment rates across strata). Our specification is therefore

$$(1) Y = G*Strata + H*Town + B0 + B1*Any Treatment + e.$$

In addition to this analysis, we also subset our data to those assigned to either the control group or the GOTV message (dropping all observations from other treatments) and estimate the effect of the GOTV message relative to not being contacted as

$$(2) Y = G*Strata + H*Town + B0 + B1*GOTV Treatment + e.$$

Results of this analysis appear in Table 2. Per column (1), we see that being sent any message is associated with an increase in turnout of 1.7 points ($p < .01$). This effect is precisely estimated because of our large sample size (more than 962,000 registrants) and large number of assigned treatments (almost 38,000). In substantive terms, baseline turnout (see second from bottom row of table) is 31%, which means the average effect of any treatment is to increase participation by about 5.5% (see bottom row of table). Column (2) reports analysis restricted to the GOTV treatment, which has a statistically significant effect of 1.4 points ($p < .05$) that is modestly smaller, but not distinguishable from, the pooled estimate in column (1). Taken at face value, the latter estimate implies one additional vote is generated for every 72 pieces of mail that are sent. Assuming a cost of production and delivery of \$.50 per treatment, generating each additional vote costs about \$36.

¹³ Results using logistic regression are available upon request.

Table 2. Effect of Mail Treatment about Election Stakes on Turnout, Pooled Analysis and GOTV Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Entire Sample	Entire Sample	Vote History Group = New registration	Vote History Group = Ever Municipal voter	Vote History Group = Ever Midterm voter	Vote History Group = Ever voter	Vote History Group = Never voter	Vote History Group = New registration	Vote History Group = Ever Municipal voter	Vote History Group = Ever Midterm voter	Vote History Group = Ever voter	Vote History Group = Never voter
Assigned any Treatment (1=yes)	0.017 [0.003]		0.017 [0.005]	0.024 [0.006]	0.018 [0.004]	0.006 [0.003]	0.002 [0.002]					
GOTV Treatment (1=yes)		0.014 [0.007]						0.021 [0.011]	0.019 [0.013]	0.008 [0.008]	0.006 [0.005]	0.003 [0.003]
Constant	0.348 [0.023]	0.371 [0.033]	0.159 [0.048]	0.499 [0.041]	0.257 [0.042]	0.092 [0.024]	0.028 [0.014]	0.134 [0.075]	0.570 [0.050]	0.181 [0.057]	0.171 [0.066]	0.011 [0.006]
Observations	962,118	933,068	109,295	470,483	159,389	143,490	70,447	106,046	463,985	152,891	136,992	67,198
R-squared	0.280	0.285	0.069	0.083	0.049	0.029	0.023	0.092	0.099	0.065	0.045	0.095
Total Number Treated	37,983	8,933	4,248	8,496	8,496	8,496	4,248	999	1,998	1,998	1,998	999
Mean of DV	0.310	0.310	0.120	0.530	0.140	0.050	0.010	0.120	0.540	0.140	0.050	0.010
Proportional Increase in Turnout Associated with Treatment	5.5%	4.5%	14.2%	4.5%	12.9%	12.0%	20.0%	17.5%	3.5%	5.7%	12.0%	30.0%

Note: OLS regression estimates with robust Huber/White standard errors in brackets. Dependent variable is whether the individual voted in the 2013 November municipal election (1=yes, 0=no). Strata and town-level fixed effects not reported to save space.

Columns (3) through (12) repeat these analyses by voter history partitions of the sample, first for any treatment and then comparing the GOTV message to no contact. Focusing on the pooled analysis, we find the largest point estimates for the ever local voters group (2.4 points, $p < .01$), followed by ever midterm voters (1.8 points, $p < .01$), and new registrants (1.7 points, $p < .01$). The effect for ever voters is substantively smaller at .6 points ($p < .05$), and while the effect for never voters is positive, it is small at .2 points with a standard error of the same size. We note that despite these differences in absolute effect sizes, relative effect estimates, which account for differences in baseline turnout, are more similar across voter history groups at between 12 and 20% (with the exception of ever municipal voters, who have a much higher baseline propensity to vote). The largest relative estimate is for the never voters group, which is not statistically significant given we allocated fewer treatments to this group and estimate a much smaller (absolute) treatment effect. Thus, with the exception of never voters, our combination of treatments successfully increased turnout regardless of prior levels of participatory engagement.

Shifting to the GOTV treatment, we estimate this standard mobilization message increased turnout by 2.1 points for new registrants, 1.9 points for ever local voters, 0.8 points for ever midterm voters, 0.6 points for ever voters, and 0.3 points for never voters. Only the effect for new registrants is statistically distinguishable from 0 ($p < .05$), though this stems in part from small treated counts (less than 2,000 per vote history group) leaving the analysis underpowered. Relative differences range from 3.5% to 30%.

Complete Treatment Analysis

Next we analyze whether our novel treatments are effective at increasing participation by raising the apparent stakes of voting in local elections. To conduct this analysis, we repeat our earlier specification from equation (1) but replace the pooled treatment indicator with a separate

indicator for each treatment and use weights that account for different rates of assignment across treatment and strata. Complete regression results appear across a variety of specifications in Table 3.

Column (1) shows results from a model that excludes town-level fixed effects. Because some of the treatments rely on town-level figures, one concern is that the treatments take on different meanings across towns. For this reason, as well as to account for unobserved features of town-level elections that might explain turnout, our preferred specification appears in column (2) and includes town-level fixed effects. Finally, column (3) is a robustness specification in which we include pre-treatment measures of registrant characteristics available in the voter file (age, gender, partisanship, year of registration). As is obvious from direct inspection of the coefficients across columns, the choice of specification has relatively minor effects on the interpretation of our results, and so we focus our discussion on results from column (2).

Emphasizing the Budgetary Implications of Local Elections Increases Turnout

We first focus on the comparative effectiveness of the three treatments emphasizing the control of local resources at stake in local elections. As a reminder, the *Budget Average Spending* treatment is identical across localities, while the *Budget Own Spending* treatment reports a different figure depending on where people live. The *Budget Comparative Spending* treatment compares these two figures, and so varies in content (and meaning) across localities.

Per column (2), both the *Budget Own Spending* and *Budget Average Spending* treatments are estimated to meaningfully increase participation in local elections by roughly similar magnitudes of 2.6 ($p < .05$) and 3.0 ($p < .05$) points. Notably these coefficients are at least twice as large as the effect of the baseline GOTV treatment (1.3 points), the content of which is included in these treatments. A linear combination-of-coefficients test of whether these differences are

Table 3. Effect of Mail Treatment about Election Stakes on Turnout, Individual Treatment Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Entire Sample	Entire Sample	Entire Sample	Budget Treatments Only	Taxes Less than Both Comparisons	Taxes Between Two Comparisons	Taxes Above Both Comparisons	Those <=35 Only	Those 35 to 54 Only
GOTV Treatment	0.013 [0.007]	0.013 [0.007]	0.013 [0.007]		0.021 [0.010]	0.010 [0.011]	0.000 [0.014]	0.004 [0.006]	0.003 [0.009]
Treatment=GOTV, low town				0.016 [0.011]					
Treatment=GOTV, avg town				0.010 [0.012]					
Treatment=GOTV, high town				0.013 [0.013]					
Budget Own Spending Treatment	0.022 [0.013]	0.026 [0.012]	0.026 [0.012]		0.018 [0.019]	0.023 [0.021]	0.044 [0.025]	0.032 [0.013]	0.012 [0.017]
Treatment=Own Town Budget per capita, low town				0.043 [0.018]					
Treatment=Own Town Budget per capita, avg town				0.021 [0.021]					
Treatment=Own Town Budget per capita, high town				0.004 [0.024]					
Budget Average Spending Treatment	0.032 [0.013]	0.030 [0.012]	0.031 [0.012]		0.032 [0.018]	0.015 [0.021]	0.046 [0.027]	-0.006 [0.009]	0.024 [0.017]
Treatment=Average Budget per capita, low town				0.027 [0.018]					
Treatment=Average Budget per capita, avg town				0.021 [0.021]					
Treatment=Average Budget per capita, high town				0.051 [0.024]					
Budget Comparative Spending	0.001 [0.013]	0.001 [0.013]	0.003 [0.012]		-0.016 [0.019]	0.023 [0.022]	0.003 [0.026]	-0.003 [0.011]	0.021 [0.017]
Treatment=Own Town Budget per capita vs. State, low town				0.012 [0.020]					
Treatment=Own Town Budget per capita vs. State, avg town				-0.006 [0.021]					
Treatment=Own Town Budget per capita vs. State, high town				-0.012 [0.024]					
Taxes Own Mill Rate Treatment	-0.012 [0.017]	-0.012 [0.016]	-0.014 [0.015]		-0.042 [0.025]	-0.001 [0.026]	0.031 [0.030]	-0.002 [0.013]	-0.007 [0.021]
Taxes Low State Average Treatment	0.004 [0.016]	0.005 [0.015]	0.004 [0.015]		0.002 [0.022]	-0.021 [0.027]	0.050 [0.032]	0.000 [0.015]	-0.021 [0.021]
Taxes High State Average Treatment	0.036 [0.016]	0.035 [0.015]	0.038 [0.015]		0.054 [0.022]	0.009 [0.026]	0.032 [0.033]	-0.016 [0.012]	0.030 [0.022]
Taxes Comparatively Higher Treatment	0.043 [0.016]	0.041 [0.015]	0.042 [0.015]		0.057 [0.024]	0.024 [0.026]	0.035 [0.032]	0.013 [0.016]	0.074 [0.021]
Taxes Comparatively Low Treatment	0.015 [0.017]	0.019 [0.016]	0.018 [0.015]		0.024 [0.023]	0.009 [0.028]	0.024 [0.032]	0.013 [0.014]	-0.003 [0.022]
Age Bias Smaller Treatment	0.013 [0.008]	0.015 [0.008]	0.015 [0.008]		0.013 [0.012]	0.018 [0.013]	0.013 [0.016]	0.015 [0.008]	0.017 [0.011]
Age Bias Larger Treatment	0.025 [0.008]	0.023 [0.008]	0.022 [0.008]		0.023 [0.012]	0.020 [0.013]	0.026 [0.017]	0.004 [0.007]	0.024 [0.011]
Constant	0.226 [0.008]	0.325 [0.049]	3.885 [1.001]	0.307 [0.065]	0.057 [0.125]	0.297 [0.050]	0.232 [0.040]	0.157 [0.042]	0.397 [0.064]
Observations	962,118	962,118	962,115	940,890	443,908	307,154	211,056	192,330	335,234
R-squared	0.251	0.293	0.304	0.289	0.288	0.296	0.306	0.163	0.255
Total Number Treated	37,983	37,983	37,980	16,755	17,161	12,551	8,271	12,398	12,726
Mean of DV	0.310	0.310	0.310	0.310	0.330	0.300	0.280	0.110	0.280
Town Fixed Effects?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: OLS regression estimates with robust Huber/White standard errors in brackets. Dependent variable is whether the individual voted in the 2013 November municipal election (1=yes, 0=no). Covariates (age, age squared, gender, party identification, registration year, and number of registrants in household) used in column (3) analysis reported in Table SA1 (page 12). Strata fixed effects not reported to save space. Data weighted to account for different rates of treatment assignment across treatment and strata.

statistically distinguishable from the baseline GOTV effect in isolation or when averaging these estimates (which improves statistical power) yields one-sided p-values of .18, .12, and .09 for comparisons to *Own*, *Average*, and Pooled *Own/Average* treatments.

In stark contrast to the clear effectiveness of emphasizing the resources at stake in local elections, the *Budget Comparative Spending* treatment appears totally ineffective with a point estimate near 0 (.1 points), which is both substantively unimportant and worse than the baseline GOTV treatment delivered in isolation. In column (4), we investigate whether the average ineffectiveness of this treatment masks theoretically informative heterogeneity across towns by portioning our treatment analysis for the budget treatments by town-level spending (categorized as below average, average, or above average). This is an observational partitioning, and so to avoid threats to inference we allow the effect of all treatments included in this specification (*GOTV*, *Budget Own*, *Budget Average*, and *Budget Comparison*) to vary with town-level spending. This shows that for no subgroup of towns does the comparison of local to state-average spending appear more effective than either of those pieces of information presented in isolation or the GOTV treatment.

As we note above, emphasizing a comparison may invoke different considerations for different people such that any mobilization effect is offset by a demobilization effect for other potential voters. Unfortunately we lack any theoretically-relevant measures of underlying local tax policy preferences with which to explore this potential heterogeneity. By contrast, the simple presentation of local or state average spending does not involve comparison but instead simply signals that local government controls a great deal of money, which is a more homogenous message emphasizing the stakes of local elections.

Emphasizing High Local Tax Burdens Increase Turnout

If highlighting the spending side of the budgetary implications of local elections is an effective way to increase turnout, are treatments that emphasize the implications of local elections for taxes similarly effective? The results presented in column (2) provide clear evidence that they are.

In particular, the *Taxes Comparatively Higher* treatment, which compares taxes to a lower state baseline and therefore makes local taxes appear higher, is associated with a 4.1 point increase in turnout ($p < .01$). This effect is 2.8 points larger than the effect of the baseline GOTV treatment ($p = .05$, one-tailed test of difference of coefficients) and 2.2 points larger than the 1.9 point effect of the *Taxes Comparatively Lower* treatment. The comparison of the latter treatment to the GOTV message is not significant, while the difference in the effect of the two tax treatments has a one-sided p-value of .16 (reflecting the smaller number of individuals assigned to each tax treatment than to the GOTV message).

Overall, a message emphasizing high local tax burdens appears very effective in increasing turnout—it produces an increase in turnout of approximately 13% over baseline voting rates and a cost per vote of \$12 if each treatment costs \$.50. One concern though is that either one's own mill rate (although constant across the two comparative tax treatments) or the different baseline statewide tax rates explain the combined effect of the mill rate and the comparison to a low state benchmark. However, the relative effectiveness of those treatment components in isolation shows these concerns are not warranted. Simply telling someone their mill rate, without any contextual information about what it means for taxes or whether that figure is high or low (*Taxes Own Mill Rate*), depresses turnout by 0.1 points relative to no contact at all. More interesting is that telling people average taxes are lower rather than higher appears to

depress turnout (*Taxes Low State Average* – *Taxes High State Average* = -3 points, two-sided p-value of .16), despite the fact that comparing their own taxes to the lower state figure increases turnout. Thus, we can easily reject the argument that telling people average taxes are low is the reason comparing their relative value to this lower baseline increases turnout.¹⁴

As before, we can conduct an observational analysis across towns to assess whether the *Taxes Comparatively Higher* treatment is especially effective in places where local taxes are higher. We partition the sample in columns (5) through (7) by whether local tax rates are below both comparative benchmarks, between the two benchmarks, or above both benchmarks. The results show that compared to the GOTV message, the *Taxes Comparatively Higher* treatment increases turnout by 3.6 points in low-tax municipalities (one-sided p-value .09), 1.4 points in moderate tax places (not significant), and 3.5 points in high tax places (one sided p-value. .16). Actual local tax rates therefore do not appear to matter in a consistent way, but instead the *Taxes Comparatively Higher* treatment is a relatively effective message in all three subgroups.¹⁵

¹⁴ Accordingly, if we estimate a difference-in-differences linear combination of coefficients test of (Own Tax Rate vs. Low State Avg. - Own Tax Rate vs. High State Avg.) minus (Low State Avg. – High State Avg.) we obtain an estimate of 5.1 points with a one-sided p-value less than .05.

¹⁵ We can also compare the effect of the *Taxes Comparatively Higher* treatment to the *Taxes Comparatively Lower* treatment. These differences are 3.3, 1.5, and 1.1, but none are significant at $p < .05$. In high tax places these modest differences are not surprising: In both treatment arms, local taxes are high. More interesting is that in low tax places, the low baseline appears more effective despite local taxes being lower than both benchmarks.

Emphasizing Age Bias in Participation Increases Participation

Our final set of treatments present individuals with information about the age bias of local electorates and varies the comparison across elections to make this difference appear either larger or smaller. Per column (2), the *Age Bias Larger* treatment induces a 2.3 point increase in turnout ($p < .01$), which is larger than the *Age Bias Smaller* treatment effect of 1.5 points ($p < .10$). Both effects are larger than the 1.3 point effect of the standard GOTV message, although neither comparison to the GOTV message is statistically significant at conventional levels (the difference for the *Age Bias Larger* treatment is 1 point, with a one-sided p-value of .17).

In light of concerns that the *Age Bias Larger* treatment might actually depress turnout among the younger registrants by describing a past pattern of low participation by that group (a descriptive norm), we repeat this analysis separately for younger voters included in the comparison (≤ 35) in column (8) as well as for older registrants age 35-54 in column (9). Registrants aged 35-54 are less likely than those 55+ to vote but their behavior is not described directly in this treatment, therefore reducing concerns that descriptive norm compliance would decrease their participation.

These results provide some evidence to validate concerns about the power of descriptive norms. Per column (8), for registrants 18-35 the *Age Bias Larger* treatment is no more effective than the GOTV message (both are insignificant effects of .4 points), and the *Age Bias Smaller* treatment increases participation by 1.5 points ($p < .10$, difference with GOTV or *Age Bias Larger* treatments not significant). By contrast, among registrants aged 35-54, per column (9), the *Age Bias Larger* treatment is substantially more effective—at 2.4 points ($p < .05$)—than the GOTV message (estimated effect .3 points, difference in effects p-value = .07 one-tailed) and is slightly

more effective, by .8 points, than the *Age Bias Smaller* treatment (estimated effect 1.7 points, difference with *Age Bias Larger* not significant [one-sided p-value .30]).¹⁶

Overall, the age bias treatments appear to increase turnout more effectively among groups that are not implicated in the comparison. On average the messages appear more effective than a GOTV message, although the effect of describing a group's past participation as relatively poor may counteract the incentive to mobilize by learning one's group is underrepresented.

Conclusion

A central message of political campaigns is the substantial stakes of the contest in question. Campaigns, candidates, and parties all go to great lengths to emphasize the crucial significance of the election's outcome as a means to highlight the importance of showing up to the polls on Election Day. Yet despite this strategy being a ubiquitous approach taken by political actors, scholars have generally ignored this approach when assessing the utility of various means to increase turnout. As such, there is little field experimental work testing the effectiveness of these types of messages, and none that attempt to benchmark the impact of these messages against a standard GOTV message.

We address this empirical oversight by conducting, to our knowledge, the first large-scale voter mobilization field experiment that tested the extent to which efforts to alter the perceived stakes of an election can successfully activate registrants. Our findings strongly suggest that such

¹⁶ Analysis (not reported) finds the *Age Bias Larger* treatment is about 1.1 points more effective than the *Age Bias Smaller* treatment among registrants 55+. It is also about .2 points more effective than the GOTV message. None of these comparisons, however, are statistically significant.

outreach does engage citizens. Combined, our set of treatments, which emphasized the resources controlled by local government (i.e., their budget), the tax burdens imposed upon residents, or the unrepresentativeness of the local electorate, increased turnout compared to receiving no contact, and did so regardless of prior vote history (with the exception of those registrants who had never voted). Individually, a number of these messages exerted substantively important effects that were noticeably larger than a simple non-partisan GOTV letter (though often those differences were not statistically significant, due in part to power considerations).

Our results have a number of important implications. Perhaps most notably, they provide causal evidence that messages emphasizing the (perceived) stakes of an electoral contest influence the decision to vote. Considerable prior work speculates that ignorance of an election's consequences raises the probability of abstention, but those analyses are limited in their ability to demonstrate a causal link in this relationship. Our approach, in contrast, shows that messages that should lead to perceiving greater electoral stakes increased participation. Furthermore, our experiment reveals that these perceptions are remediable. Simple outreach can clarify why a contest's outcome matters, and that clarification not only enhances one's understanding of why what government does is important, but also translates into a higher propensity to vote. That many of these messages exert a larger impact on turnout than a standard GOTV message signals that affecting these perceptions is a powerful strategy to raise participation rates.

That said, the exact mechanism through which these messages increase turnout is still unclear. Our messages highlight the stakes of voting, but that can cause greater participation through a variety of causal pathways that implicate different theoretical mechanisms and inform different models of the decision to vote. One possibility is that increasing the stakes of voting increases participation because people behave as the canonical rational-choice turnout model

would predict, but have sufficiently large assessments of the chances that their vote is pivotal or sufficient non-material incentives to vote that they are otherwise near indifference regarding participation. A related class of accounts also stresses the importance of material stakes but allows that voters do not condition their evaluations on the chances their votes are decisive. This could arise if voting is expressive and raising the stakes makes expressing one's point of view more important, or if higher stakes cause people to perceive a greater normative or group return to voting and turn out on those grounds. Distinguishing among these competing accounts is beyond the scope of this paper, but we note that doing so would require additional measurement of what sets of beliefs and motivations are implicated by our treatment. For the moment, the key point is that such material incentives appear to matter for voting in a way that prior field-experimental work has neglected.

While our results are derived from a large-scale field experiment, there are nonetheless reasons for caution in interpreting our results. Some relate to standard caveats about whether effects would arise in different electoral contexts, but we also note that despite the size of our experiment, the large number of treatments we tested means that we still have imprecision in some estimates. These apprehensions, which raise the possibility of false positives, are a greater concern when comparing the effects of our novel treatments to the baseline GOTV messages because the difference in effects are smaller. For this reason, it would be desirable to replicate these classes of treatments on an even larger scale and in different contexts with sufficient power to accurately discern relative treatment differences of perhaps 1 point (a proportionally large effect that is at least 75% of the effect of the GOTV treatment).

Those concerns aside, our approach provides a model upon which future tests of this relationship can build. The scope and design of the study (10 distinct treatments paired with a

placebo GOTV message administered to almost 38,000 registrants) provides a method not just for assessing the impact of a wide array of theoretically-driven messages, but also for determining if that outreach has greater mobilization success than commonly employed messages. That set-up should serve as the model for other new and innovative messages testing theories yet to be subjected to field experimental assessments.

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Supporting Information for:

**Can Raising the Stakes of Election Outcomes Increase Participation?
Results from a Large-Scale Field Experiment in Local Elections**

This Supporting Information contains the following material:

Samples of all Treatment Letters [Pages 2-11]

Table SA1. Effect of Mail Treatment about Election Stakes on Turnout, Individual Treatment Analysis (Full Model Results) [Page 12]



October 2013

FIRST NAME LAST NAME

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CITY, CT ZIP

Dear FIRST NAME LAST NAME,

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Voting is a right and responsibility. Whichever candidate or party you prefer, we want to remind you to exercise your right to vote this November. The right to vote is an important American tradition. The whole point of democracy is that citizens are active participants in government and democracy functions best when everyone takes part in the voting process. This November, remember your rights and responsibilities as a citizen.

If you have any questions about the voting process, please visit the Secretary of State's website (<http://www.sots.ct.gov>) or call your local Registrar of Voters.

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Your voice starts with your vote: In Connecticut, local elected officials make many important decisions that affect your life. One of the most important things they do is decide what to spend money on. **In 2012, your city/town's budget was \$XXXX per resident.** By voting, you make sure local elected officials are thinking about you when they decide how to spend your money. Make sure your voice is heard!

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Your voice starts with your vote: In Connecticut, local elected officials make many important decisions that affect your life. One of the most important things they do is decide what to spend money on. **In 2012, the average city/town budget in Connecticut was \$3,354 per resident.** By voting, you make sure local elected officials are thinking about you when they decide how to spend your money. Make sure your voice is heard!

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- **Local taxes fund more than 72% of local spending in Connecticut.**
- **In 2012, your city/town's mill rate was XX.XX.**

By voting, you make sure local elected officials are thinking about you when they are setting your tax rates. Make sure your voice is heard!

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- **Local taxes fund more than 72% of local spending in Connecticut.**
- **In 2012, the average city/town's mill rate in Connecticut was 23.90.**
- **In 2012, your city/town's mill rate was XX.XX.**

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- **Local taxes fund more than 72% of local spending in Connecticut.**
- **In 2012, the average person in Connecticut had a mill rate of 28.18.**
- **In 2012, your city/town's mill rate was XX.XX.**

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- **In a recent election in your city/town, XX% of registrants voted.**
- **In a recent election in your city/town, registered voters over 55 were XX percentage points more likely to vote than registered voters under 35.**

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Table SA1. Effect of Mail Treatment about Election Stakes on Turnout, Individual Treatment Analysis (Full Model Results)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Entire Sample	Entire Sample	Entire Sample	Budget Treatments Only	Taxes Less than Both Comparisons	Taxes Between Two Comparisons	Taxes Above Both Comparisons	Those <=35 Only	Those 35 to 54 Only
GOTV Treatment	0.013 [0.007]**	0.013 [0.007]**	0.013 [0.007]**		0.021 [0.010]**	0.010 [0.011]	0.000 [0.014]	0.004 [0.006]	0.003 [0.009]
Treatment=GOTV, low town				0.016 [0.011]					
Treatment=GOTV, avg town				0.010 [0.012]					
Treatment=GOTV, high town				0.013 [0.013]					
Budget Own Spending Treatment	0.022 [0.013]*	0.026 [0.012]**	0.026 [0.012]**		0.018 [0.019]	0.023 [0.021]	0.044 [0.025]*	0.032 [0.013]**	0.012 [0.017]
Treatment=Own Town Budget per capita, low town				0.043 [0.018]**					
Treatment=Own Town Budget per capita, avg town				0.021 [0.021]					
Treatment=Own Town Budget per capita, high town				0.004 [0.024]					
Budget Average Spending Treatment	0.032 [0.013]**	0.030 [0.012]**	0.031 [0.012]**		0.032 [0.018]*	0.015 [0.021]	0.046 [0.027]*	-0.006 [0.009]	0.024 [0.017]
Treatment=Average Budget per capita, low town				0.027 [0.018]					
Treatment=Average Budget per capita, avg town				0.021 [0.021]					
Treatment=Average Budget per capita, high town				0.051 [0.024]**					
Budget Comparative Spending	0.001 [0.013]	0.001 [0.013]	0.003 [0.012]		-0.016 [0.019]	0.023 [0.022]	0.003 [0.026]	-0.003 [0.011]	0.021 [0.017]
Treatment=Own Town Budget per capita vs. State, low town				0.012 [0.020]					
Treatment=Own Town Budget per capita vs. State, avg town				-0.006 [0.021]					
Treatment=Own Town Budget per capita vs. State, high town				-0.012 [0.024]					
Taxes Own Mill Rate Treatment	-0.012 [0.017]	-0.012 [0.016]	-0.014 [0.015]		-0.042 [0.025]*	-0.001 [0.026]	0.031 [0.030]	-0.002 [0.013]	-0.007 [0.021]
Taxes Low State Average Treatment	0.004 [0.016]	0.005 [0.015]	0.004 [0.015]		0.002 [0.022]	-0.021 [0.027]	0.050 [0.032]	0.000 [0.015]	-0.021 [0.021]
Taxes High State Average Treatment	0.036 [0.016]**	0.035 [0.015]**	0.038 [0.015]**		0.054 [0.022]**	0.009 [0.026]	0.032 [0.033]	-0.016 [0.012]	0.030 [0.022]
Taxes Comparatively Higher Treatment	0.043 [0.016]**	0.041 [0.015]**	0.042 [0.015]**		0.057 [0.024]**	0.024 [0.026]	0.035 [0.032]	0.013 [0.016]	0.074 [0.021]**
Taxes Comparatively Low Treatment	0.015 [0.017]	0.019 [0.016]	0.018 [0.015]		0.018 [0.023]	0.009 [0.028]	0.024 [0.032]	0.013 [0.014]	-0.003 [0.022]
Age Bias Smaller Treatment	0.013 [0.008]	0.015 [0.008]*	0.015 [0.008]**		0.013 [0.012]	0.018 [0.013]	0.013 [0.016]	0.015 [0.008]*	0.017 [0.011]
Age Bias Larger Treatment	0.025 [0.008]**	0.023 [0.008]**	0.022 [0.008]**		0.023 [0.012]**	0.020 [0.013]	0.026 [0.017]	0.004 [0.007]	0.024 [0.011]**
Age November 2013 (years)				0.011 [0.002]***					
Age squared/100				-0.009 [0.001]***					
Gender = Female				-0.006 [0.007]					
Gender = Unknown				-0.012 [0.016]					
Party=Democrat				0.057 [0.008]***					
Party=Republican				0.060 [0.010]***					
Registration year, missing=1900				-0.002 [0.000]***					
Registration year missing				-0.134 [0.050]***					
Num. registrants in HH=2				0.051 [0.008]***					
Num. registrants in HH=3 or 4				0.017 [0.010]*					
Num. registrants in HH>=5				0.027 [0.024]					
Constant	0.226 [0.008]***	0.325 [0.049]***	3.885 [1.001]***	0.307 [0.065]***	0.057 [0.125]	0.297 [0.050]***	0.232 [0.040]***	0.157 [0.042]***	0.397 [0.064]***
Observations	962,118	962,118	962,115	940,890	443,908	307,154	211,056	192,330	335,234
R-squared	0.251	0.293	0.304	0.289	0.288	0.296	0.306	0.163	0.255
Total Number Treated	37,983	37,983	37,980	16,755	17,161	12,551	8,271	12,398	12,726
Mean of DV	0.310	0.310	0.310	0.310	0.330	0.300	0.280	0.110	0.280
Town Fixed Effects?	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: OLS regression estimates with robust Huber/White standard errors in brackets. Dependent variable is whether the individual voted in the 2013 November municipal election (1=yes, 0=no). Strata fixed effects not reported to save space. Data weighted to account for different rates of treatment assignment across treatment and strata. *p < 10%; **p < 5%; *** p < 1%