A Get Out The Vote (GOTV) Experiment on the World's Largest Participatory Budgeting Vote in Brazil

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Abstract: Does non-partisan voter mobilization affect the popular vote? We use vote records from a state-level participatory budgeting vote in Brazil– the world's largest –to assess the impact of voter mobilization messaging on turnout and support for public investments. The government provided records as to how each ballot was cast and designed the tabulation process so that votes could be matched to treatment assignment without compromising the secrecy of the ballot. Citizens (n=43,384) were randomly assigned to receive non-partisan email and text messages designed to encourage voting. We document an impressive 4.7 percentage point increase in online voting in our treatment group. However, we found no effect of messaging on vote choice; voters in the treatment and control groups shared the same sectoral preferences and showed no difference in the average cost of public investment projects they supported. These results suggest non-partisan Get Out the Vote campaigns can increase citizen participation without skewing the outcome.

Main Text:

While academic studies of voter mobilization have taught us much about what drives turnout,[1] we know little about the effect Get Out the Vote (GOTV) campaigns has on election outcomes. Experimental work has shown that that there are heterogeneous treatment effects, in other words that encouragements have different turnout effects on different people.[2] However, there is little evidence from turnout experiments about whether mobilization efforts affect the actual outcome of the vote. One reason the GOTV literature primarily focuses on turnout is data availability. Individual vote choice is difficult to observe directly due to the secret ballot, while administrative records of turnout can be obtained in many places. The few previous studies of GOTV on vote choice look at partisan mobilization and rely on incomplete self-reported or aggregate vote choice.[3] As a result, we do not know whether GOTV campaigns actually mobilize people who vote differently than those who would otherwise have voted.

Several studies have looked at the relationship between turnout and policy outcomes at the national level and consistently find that higher turnout is associated with higher levels of redistribution.[4] However, these studies rely on observational data and cannot assign higher turnout experimentally.

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[1] Gerber and Green 2000; Gerber, Green, and Larimer 2008; Green, McGrath, and Aronow 2013.
[2] Enos, Fowler, and Vavreck 2014; Imai and Strauss 2010.
[3] Arceneaux 2005; Arceneaux and undefined 2007; Pons 2014; Rogers and Middleton 2015.
[4] Filer, Kenny, and Morton 1993; Larcinese 2007; Mahler 2008.

In this study there are two main outcomes of interest. First, we examine the vote encouragement treatment effects on online turnout in a participatory budgeting vote. This part of the study is similar to much of the established GOTV literature where the outcome of interest is whether an eligible voter turns out to vote on Election Day. Second, we explicitly study the effect of GOTV treatments on the actual vote using administrative records of vote choice. To our knowledge, this has never been studied before using real vote data. The nature of the participatory budgeting (PB) vote in our case allows us to study both the average cost of proposals selected by voters as well specific sectoral (health, education, environment etc.) preferences.

To test whether increased turnout from voter mobilization affects the popular vote, we conducted a randomized controlled trial during the June 2014 participatory budgeting vote in Rio Grande do Sul (RS) in southern Brazil.[5] Note that this is not a regular election where votes are cast for candidates and parties. In a PB vote voters are asked to choose between specific public investment projects that they can indicate their support for. We devised a procedure in close collaboration with the implementing government agency that retained an indicator of individual-level treatment assignment with the actual vote choice record. The procedure anonymizes all other individual attributes, thus preserving the secrecy of the ballot.

[5] For more information about the PB process in Rio Grande do Sul, *see* Spada et al. 2016.

The list of experimental subjects consists of 43,384 voters who took part in the annual online PB vote in the past two years. The list includes only individual voters who voluntarily provided both an email address and a phone number when voting in the past. The population we study appears to have a higher propensity to vote than the rest of the online voting population. Examining historical online PB vote data we find that 22.4 percent of voters for whom we have an email address take part in the online vote both 2012 and 2013. Among all online voters the equivalent number is 18.2 percent.

We randomly assigned subjects, in equal proportions, to one of four groups: control, informational, public benefit of voting, and private benefit (lottery reward) using a simple random allocation scheme.[6] All of the messages were non-partisan in nature and were focused on increasing turnout. The sender of the email was *Gabinete Digital*, the Governor's digital engagement unit. Figure 1 shows the email that subjects in the informational treatment group received in the morning on the first day of online voting on June 2. Each experimental subject were sent three emails: a voter registration email on May 30, a message about voting starting on June 2, and on June 4 a message about it being the last chance to vote. In addition we sent out a mobile phone text message (SMS) on June 3.

[6] The lottery reward was a pre-existing voter encouragement scheme that all voters were eligible for. The lottery message merely provided information about this existing scheme.

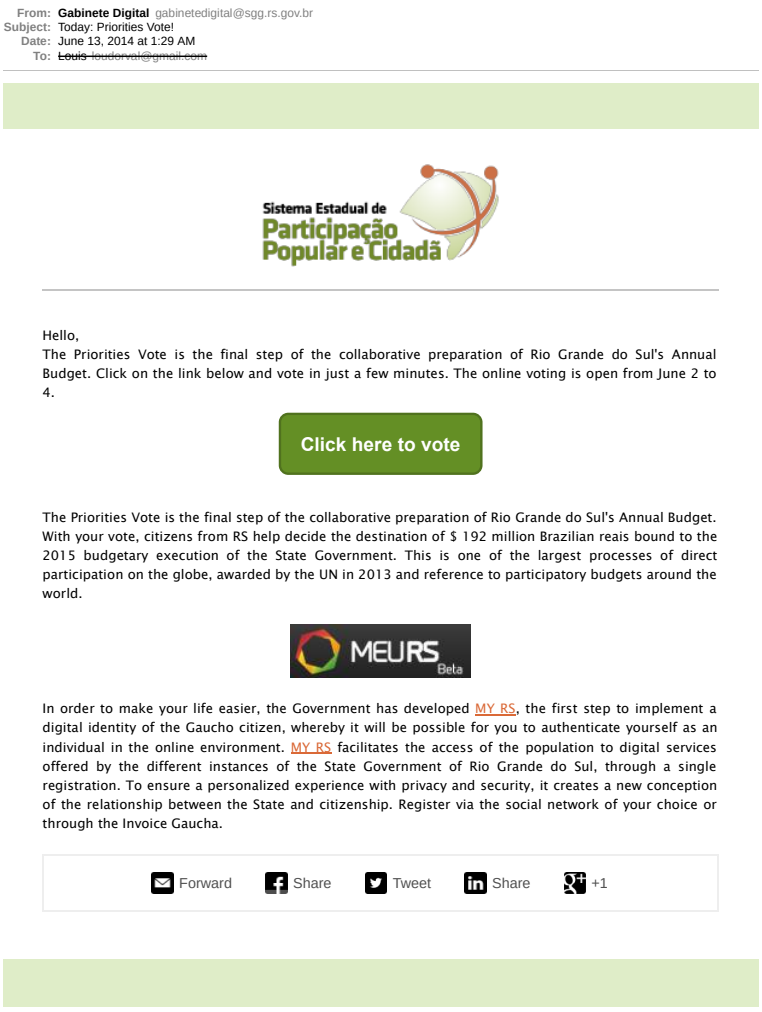

Fig. 1. Informational Treatment message on Election Day 1 on June 2. More details about the treatments in the supplementary material.

The content of the message was the same throughout the process for each treatment condition, albeit differing in exact wording due to space limitations of the SMS format. On June 2, the first treatment group was sent an informational message stating basic information about this year's online voting taking place from June 2-4. The message subject line read 'Today: Priorities Vote!' and the body text contained a direct link to the voting site. The two other groups were also sent the

informational message, but in addition, the public benefit motivation condition was assigned the following message: 'This is your chance to make government work on your priorities.' Also, the subject line for the public benefit condition included the words: 'Make a difference'. The private benefit condition instead got the following message: 'If you vote in via Citizen Login you can win two extra tickets for the raffle at R$ 1 million occurring in March 2015.'

In this GOTV experiment we specifically study the final stage of an annual three-month long budget consultation process that culminates in citizens voting on their priorities for a pre-screened set of public investment projects and policy sectors. In the PB vote in RS the ballot is split into two sections: a list of up to 20 possible public investment projects with specific costs attached to each of them, where a voter can pick up to four projects, and a list of five possible regional sectoral (health, education etc.) priorities, where a voter can choose up to two options. The precise items on the ballot vary across the 28 PB areas depending on the projects suggested within the participatory budgeting meetings held prior to voting day and a few of the regions do not have costs attached directly.[7] However, on the ballot, each of these items is coded into one of the same 14 possible thematic areas allowing us to compare vote choices across these electoral areas.

Because the secrecy of the vote is a key tenet of electoral integrity, the GOTV literature has had to rely on self-reported or aggregate-level voting when studying vote choice. In this process, it was vital to avoid receiving vote choice data with information that could identify an individual voter. To achieve this, a vector with

[7] For an example of how the ballot looks like, see the online appendix.

the treatment assignment was merged into the vote records data by the state agency that administered the online vote (PROCERGS) prior to decrypting an individual's vote choice. After the vote verification process was completed (checking for duplicate voters, voters voting in the wrong area, etc.), the vote record had individual identifiers stripped, while leaving in the treatment assignment vector. Only at this point were the votes decrypted. The vote records could then be transferred to the research team safely, without compromising individuals' privacy. At no point in the voting process are decrypted votes and personally identifiable information kept on the same server. This approach is analogous to voters being given a different colored ballot when their identification is checked at the polling station depending on their treatment group. The analysis is then conducted solely in terms of differences in votes cast using different colored ballots. Since the number of experimental groups is extremely small compared to the number of voters, the different ballots do not meaningfully increase the posthoc identifiability of voters.

Prior to treatment assignment, we submitted a pre-analysis plan to Experiments in Governance and Politics. As specified in the plan, we estimate the intent-to-treat (ITT) effect by using a difference-in-means estimator. Compliance with treatment, defined as the recipient having read the subject line or the body text, is difficult to determine since we do not know whether the mail possibly ended up in the junk folder, but the bounce rate is a very low 1.6 percent. With non-compliance this low, effectively the estimand is the Average Treatment Effect (ATE). In the linear model we include dummy variables for each of the 28 regions (COREDES) since the ballot

varies across regions. The multiple comparisons problem is addressed by using Benjamini and Hochberg's method for adjusting *p*-values[8] to control the false discovery rate for each dependent variable section in line Sun et al.[9]

The first substantive finding is that different GOTV treatments have a turnout increasing effect (see **Figure 1**). Taken together, voters assigned to receive any of the GOTV messages were 4.7 percentage points more likely to vote in the online poll (ITT, difference in means linear estimation, $p<0.001$, see **Table 1**). Turnout in the control group was 22.6 percent. Each of the separate mobilization treatments shows significant differences from the control group that received no email. The content of the mobilization messages does not matter much, a finding that is broadly consistent with the literature.[10] However, the private benefit (lottery) treatment did perform significantly worse than the informational treatment (-1.6 percentage points, difference in means linear estimation, FDR adjusted $p=0.01$).

Figure 1. Online turnout in control, pooled treatment group, and the three treatments separately: informational, public benefit, and private benefit. 95% Confidence Intervals are displayed.

[8] Benjamini and Hochberg 1995.
[9] Sun et al. 2006.
[10] Gerber and Green 2000.

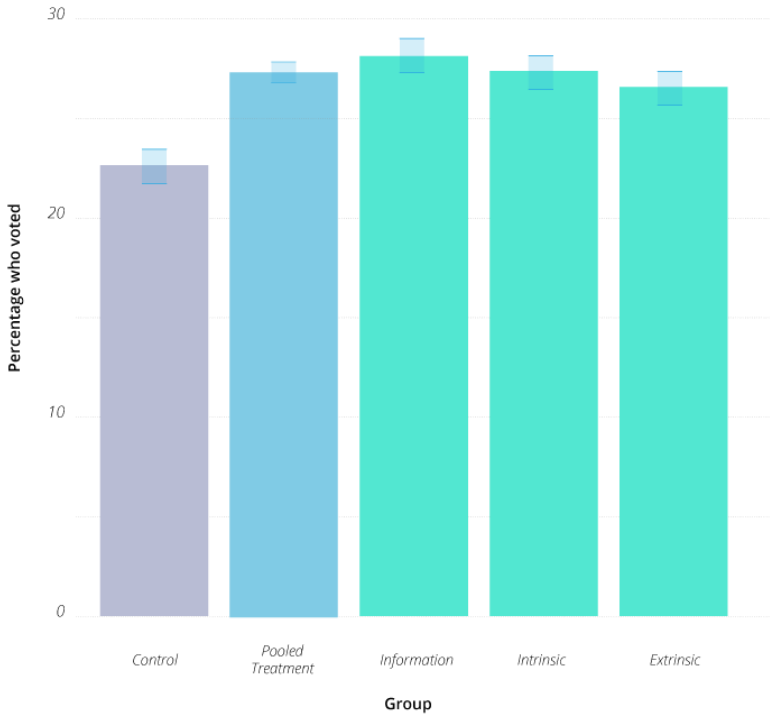

Table 1. Treatment Effects on Turnout – Main Model and Comparisons Across Different Treatment Subsets.

Comparison	N	Estimate	Std.Error	T-value	Unadjusted p-value	FDR adjusted p-value
Pooled treatments	43,384	0.047	0.005	9.634	0.00000	0.00000
Information	43,384	0.055	0.006	9.248	0.00000	0.00000
Public benefit	43,384	0.047	0.006	7.810	0.00000	0.00000
Private benefit	43,384	0.039	0.006	6.542	0.00000	0.00000
Info vs. Public	21,692	-0.009	0.006	-1.411	0.15840	0.19008
Public vs. Private	21,692	-0.008	0.006	-1.255	0.20942	0.20942
Info vs. Private	21,692	-0.016	0.006	-2.666	0.00768	0.01153

The turnout effect we document here is substantially larger than that found in most previous GOTV studies,[11] particularly those focusing on the effect of new technologies, like email and SMS.[12] While we cannot test the reasons for this performance directly, it is likely due to repeated messages and multi-mode contact, and the direct ability to vote through a link in the message. In addition, while this is a low salience election, almost a quarter of the subjects targeted are habitual voters where large effects have been found.[13] The high performance of the informational message suggests that one of the primary ways in which the GOTV message is working is simply through informing voters that there is an election at all. The lower performance of the private benefit message could be for a number of reasons. A private benefit may undermine public benefit motivation,[14] it may be more likely to be caught by spam filters, or voters may simply be less trusting of such a message. Regardless of the reason, these results suggest that large lottery rewards are not an especially effective way of mobilizing voters.

Let us now turn to the second part of the study, where we examine unique administrative vote records. Previous studies have shown that non-voters differ from voters,[15] which implies that additional voters brought in by GOTV efforts will have an effect on election outcomes by altering the preferences represented. We test this implication directly by examining vote choice across the different treatment groups. As specified in the pre-analysis plan, we first test whether our

[11] Green, McGrath, and Aronow 2013; Nickerson 2007.
[12] Bhatti et al. 2015; Malhotra et al. 2011; Nickerson 2007.
[13] Malhotra et al. 2011.
[14] Fehr and Falk 2002; Fehr and Fischbacher 2002.
[15] Enos, Fowler, and Vavreck 2014.

treatments affect the average cost of proposals selected by voters across different groups. Each proposal was listed on the ballot with a Brazilian Real (R$) cost attached to it. While the total amount of money spent is not directly affected by the choices a voter makes, we view a lower total cost on a ballot as reflecting a voter's preference for lower spending. We operationalize cost as the average cost of the items a voter chose on the ballot expressed as a proportion of the maximum cost they could have picked on their ballot. In the control group that number was 65.1 percent. As can be seen from **Table 2**, the total costs do not differ significantly between the control and treatment groups (-0.2 percentage points, difference in means linear estimation, FDR adjusted p=0.79). Similarly, none of the individual treatments show differences from the control group or each other. These results show that encouraged voters do not differ from natural voters in terms of their preferences for higher or lower costing public investments.

Table 2. Treatment Effects on Cost – Main Model and Comparisons Across Different Treatment Subsets. Average cost of options chosen on a respondent's ballot as a percentage of the maximum cost they could have chosen.

Comparison	N	Estimate	Std.Error	T-value	Unadjusted p-value	FDR adjusted p-value
Pooled treatments	43,384	-0.002	0.006	-0.260	0.794	0.794
Information	43,384	-0.008	0.007	-1.120	0.263	0.717
Public benefit	43,384	-0.003	0.007	-0.437	0.662	0.773
Private benefit	43,384	0.007	0.007	0.994	0.320	0.717
Info vs. Public	21,692	0.004	0.007	0.656	0.512	0.717
Public vs. Private	21,692	0.010	0.007	1.507	0.132	0.717
Info vs. Private	21,692	0.004	0.007	0.679	0.497	0.717

As well as the cost of proposals, we also consider whether encouraged and un-encouraged voters differ in their preferences for different public policy sectors. We

compare the proportion of voters who vote for an item in the three most popular issue areas displayed on the second part of the ballot. In the 2014 PB vote these were health, infrastructure and security. In addition we also include a 'other' category for the proportion who vote for any of the other categories. We analyze only the votes on the second section of the ballot where a voter can choose up to two priorities for their own region (*Prioridades Regionais*).

Table 3. Treatment Effects on Vote Choice – Main Model and Comparisons Across Different Treatment Subsets. Average proportion of voters choosing the top three sectoral categories: health, infrastructure or security, and a 'other' category combining all other categories.

Treatment	Issue Area	N	Estimate	Std.Error	T-value	Unadjusted p-value	FDR adjusted p-value
Pooled	health	11,350	-0.003	0.011	-0.286	0.775	0.984
Pooled	infrastructure	11,350	0.006	0.011	0.513	0.608	0.984
Pooled	security	11,350	-0.003	0.009	-0.343	0.731	0.984
Pooled	other	11,350	-0.008	0.008	-1.086	0.278	0.984
Information	health	11,350	-0.012	0.014	-0.879	0.379	0.984
Public	health	11,350	0.002	0.014	0.112	0.911	0.984
Private	health	11,350	0.001	0.014	0.071	0.943	0.984
Information	infrastructure	11,350	0.015	0.013	1.120	0.263	0.984
Public	infrastructure	11,350	0.002	0.013	0.123	0.902	0.984
Private	infrastructure	11,350	0.000	0.014	0.020	0.984	0.984
Information	security	11,350	-0.005	0.011	-0.463	0.643	0.984
Public	security	11,350	-0.007	0.011	-0.630	0.529	0.984
Private	security	11,350	0.003	0.011	0.246	0.806	0.984
Information	other	11,350	-0.007	0.009	-0.744	0.457	0.984
Public	other	11,350	-0.010	0.009	-1.111	0.267	0.984
Private	other	11,350	-0.008	0.009	-0.871	0.384	0.984
Info v Public	health	6,015	0.014	0.013	1.089	0.276	0.984
Info v Public	infrastructure	6,015	-0.013	0.013	-0.991	0.322	0.984
Info v Public	security	6,015	-0.002	0.010	-0.217	0.828	0.984
Info v Public	other	6,015	-0.003	0.009	-0.358	0.721	0.984
Public v Private	health	5,840	-0.001	0.013	-0.064	0.949	0.984
Public v Private	infrastructure	5,840	-0.002	0.013	-0.123	0.902	0.984
Public v Private	security	5,840	0.010	0.011	0.924	0.356	0.984
Public v Private	other	5,840	0.002	0.009	0.244	0.807	0.984
Info v Private	health	8,894	0.014	0.013	1.108	0.268	0.984
Info v Private	infrastructure	8,894	-0.014	0.013	-1.076	0.282	0.984
Info v Private	security	8,894	-0.002	0.010	-0.200	0.842	0.984
Info v Private	other	8,894	-0.003	0.009	-0.354	0.723	0.984

Table 3 shows the proportions choosing each area across the control and treatment conditions. As with the total cost, we find no differences between the control and Pooled treatment groups in their preferences for health (FDR adjusted p=0.98), infrastructure (FDR adjusted p=0.98), security (FDR adjusted p=0.98) and other spending (FDR adjusted p=0.98). We also tested whether voters in the different treatment groups differed from each other if, for instance, voters encouraged to participate by a public benefit message might be more inclined to support health projects. In all cases, we found no differences in sectoral preferences according to treatment condition. Taken together, these results provide no evidence that our GOTV treatments had any impact on outcome of the online vote.

Based on the size of the GOTV effect and the proportions of voters in the treatment and control groups, we can look at what the implied size of the difference in preferences between regular (voters who would have voted anyway) and encouraged (voters who only voted because of the GOTV treatment) voters (although none of these differences is close to significant as we discussed above). Applying the method outlined by Fowler[16], the largest implied difference is voting for "other", which we estimate as 4.8 percentage points less likely among encouraged voters, see **Table 4**. However, all of these differences have confidence intervals encompassing zero. In the supplementary statistical material, we show a power analysis that suggests that we would expect to be able to detect a 19 percentage point difference 81% of the time. This means that we can be reasonably confident that there are not large differences in sectoral preferences between

[16] Fowler 2015.

regular and encouraged voters. We cannot for instance rule out the possibility that differences of the size seen here 1.8-4.8 percentage points might remain and become significant with a larger sample size.

Table 4. Estimated differences in sectoral preference between regular and encouraged voters and difference in chosen cost. Confidence intervals estimated using bootstrapping.

Issue	Estimate	Lower CI	Upper CI
Infrastructure	0.041	-0.083	0.164
Health	-0.018	-0.135	0.080
Security	-0.026	-0.139	0.042
Other	-0.048	-0.151	0.113
Proposal cost	-0.015	-0.081	0.049

An important implication of our study is that email messaging can be powerful tools for mobilizing people to take part in a PB vote, especially if the type of engagement allows for online modes of participation. This is particularly vital in the context of Brazil, which has experienced a wave of protests related to, among other reasons, how the government spends its money. Democratic innovations that allow citizens to participate directly in budgeting are seen as an important way of increasing the engagement of citizens and the legitimacy of government, and high rates of participation is furthermore considered very important for the credibility and sustainability of these initiatives.[17]

Future research should focus on replicating these results in other contexts. As the world moves towards more online voting, analyzing vote choice may become more

[17] Goldfrank and Schneider 2006.

feasible in more cases. However, we would encourage researchers to think carefully about voter privacy. The focus in this study, as in most of the literature, was on a non-partisan voter mobilization. Needless to say, we still expect partisan GOTV campaigns targeting its own core supporters or particular demographic groups would shift the election outcome by increasing their own supporters' turnout. However, this would be the result of differential targeting not heterogeneous treatment effects.

This research calls into question the assumption that changing the pool of voters by increasing turnout will inevitably affect the distribution of votes cast. Unlike observational studies, we do not find any effect of higher turnout on the policy preferences chosen by voters and can be confident that the new voters brought in do not have drastically different sectoral preferences. There are a number of possible reasons for these divergent findings. One possibility is that another factor explains both higher turnout and redistribution across different countries. However, there are also important contextual differences that could explain our findings. In a low salience and non-partisan election, the voters who are mobilized through GOTV may be sufficiently similar to those who were already turning out to vote that the choices of voters are not distinguishable. It is also possible that endogenous increases in turnout are more likely to bring in voters who vote differently to existing voters than increases in turnout generated through untargeted GOTV. Finally, the choices in a PB vote may be less correlated with turnout than in partisan elections, where there are more established signals for voters to use. Nonetheless it is striking that the experimental evidence of turnout's

effect on policy choices comes to such a different conclusion than previous observational evidence.

While it might initially seem that our results imply that there is no value in using non-partisan GOTV to increase turnout, this, however, assumes that the only value of voting is for changing the outcome. Many arguments have been made for increasing turnout, including legitimization of the political system, decreasing the alienation of the populace[18] and even as a form of education.[19] In fact, these results should encourage non-partisan GOTV campaigns precisely because they are unlikely to greatly affect the results.

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[18] Powell Jr 1986; Warren 1999.
[19] Pateman 1970.

the original research who agreed that it adhered to the research and ethical standards of World Bank research.

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