Online Appendix

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Figure 1: Control flier.

ПОДДЕРЖИВАЕТЕ НЕЗАВИСИМОГО КАНДИДАТА?

У НАС ЕСТЬ ШАНС НА ПОБЕДУ!

В 2013 году на выборах мэра
оппозиционному кандидату
до первого места в одном из районов
нашего округа не хватило всего 208 голосов.

На приближающихся выборах в Думу
ваш голос может стать решающим!

ЮЛИЯ Яблоко
ГАЛЯМИНА

18 сентября

ГОЛОСУЙТЕ

ЗА ЖИВОЕ!

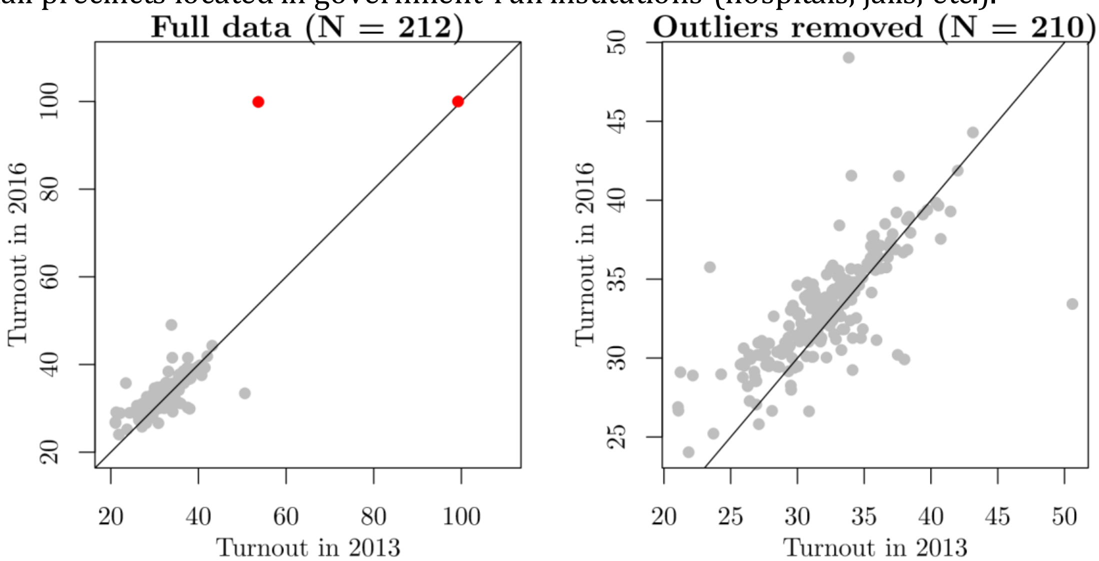
Figure 2: Flier with the closeness of election appeal.

Figure 3: Flier with the policy impact appeal.

2 Outliers in election data

The figure below plots the turnout rate by precinct in 2013 and in 2016 for all 212 precincts that were included in the experiment (left panel). We clearly see that there are two precincts that are outliers.

Precinct No. 335 is a state-run home for children with severe developmental disorders (*internat*), and it had turnout rate of 100% in both the 2013 and 2016 elections. Such turnout rates (mostly in favor of government) are quite typical in elections that take place in small precincts located in government-run institutions (hospitals, jails, etc.).



Precinct No. 469 had a turnout rate of 60% in the 2013 mayoral election and 99% in the 2016 parliamentary election. The precinct is home to the headquarters of the Main Intelligence Directorate (GRU), the foreign intelligence arm of the Russian Army. Given the nature of these two precincts, it appears reasonable to exclude them both from the analyses.

3 Power calculations

To calculate the statistical power of the experimental design, we use simulations. Let n_i denote the number of eligible voters in a precinct, $i = 1,...,N$, where N is the number of precincts in the district that were subject to the experiment. Let π_i denote the baseline turnout rate in precinct i under the control condition. We then simulate the turnout rate, T_i, in precinct i as follows:

(1) $$V_i \sim \text{Binomial}(n_i, \pi_i + \alpha_1 \text{Closeness}_i + \alpha_2 \text{Policy Impact}_i),$$

(2) $$T_i = V_i / n_i,$$

where $Closeness_i$ is an indicator equal to one if precinct i received the 'closeness of election' treatment and $Policy\ Impact_i$ is an indicator equal to one if precinct i received the 'policy impact' treatment. Thus, α_1 and α_2 capture the true effects of the two treatments. After simulating turnout rates in each precinct, $i = 1,...,N$, we then fit the OLS regression

(3) $$T_i = \beta_0 + \beta_1 \text{Closeness}_i + \beta_2 \text{Policy Impact}_i + \epsilon_i.$$

We use the turnout rate in the 2013 mayoral election in place of π – the turnout rate that would obtain in each precinct without any informational treatment. Very similar results are obtained if turnout in 2012 or 2011 elections is used in place of π. We set $\alpha_1 = \alpha_2 = 0.02$, which means that each treatment increases the turnout rate by two percentage points relative to the baseline.

We then simulate the data and run the above regression repeatedly 1,000 times. In Table 1, we show the proportion of times a null hypothesis of no effect has been rejected given that it is false (statistical power) assuming variable levels of statistical significance. Since we have two treatments, we investigate the power of the design with respect to three types of null hypotheses: one where we look to detect the effect of both treatments ($\beta_1 = 0$ and $\beta_2 = 0$), one where we look to detect an effect of a single treatment ($\beta_1 = 0$), and one where we look to detect an effect of at least one treatment ($\beta_1 = 0$ or $\beta_2 = 0$). Note that the first null hypothesis is most difficult to reject and the last one is easiest to reject.

Null hypothesis	95% confidence	90% confidence

$\beta_1 = 0$ AND $\beta_2 = 0$	0.73	0.84
$\beta_1 = 0$	0.83	0.91
$\beta_1 = 0$ OR $\beta_2 = 0$	0.94	0.97

Table 1: Power calculations

The simulation results in Table 1 indicate that the design is quite powerful. The most conservative null hypothesis that both treatments are effective is rejected with the probability 0.73 given 95 percent confidence and with the probability 0.84 given 90 percent confidence. The least stringent null hypothesis that at least one treatment is effective is rejected with the probability 0.94 given 95 percent confidence and with the probability 0.97 given 90 percent

confidence.

4 OLS estimates

	Turnout		Candidatevote		Partyvote	
	(1)	(2)	(3)	(4)	(5)	(6)
Closeness	0.79	−0.12	0.62	−0.38	45 . . .	0.33
	(0.59)	(0.40)	(0.84)	(0.70)	(0·86)	(0.67)
Impact	0.36	−0.08	−0.35	−0.83	85	0.31
	(0.59)	(0.40)	(0.84)	(0.69)	(0 86)	(0.67)
Turnout in 2013		0.63^{**}		0.70^{**}		0.78^{**}
		(0.04)		(0.07)		(0.07)
Observations	210	210	210	210	210	210
Adjusted R^2	-0.001	0.56	-0.003	0.33	0.004	0.40

$^*p<0.05$; $^{**}p<0.01$.

Dependent variables are measured in percentages.

Table 2: OLS estimates of the campaign effects.

5 Additional robustness checks

We considered the possibility that adjusting for a specific set of covariates could bias our results in a particular direction. As a robustness check, we did the following exercise: we considered all possible combinations of pre-treatment variables (used in our balance tests) as covariates in the regression. The figure below shows the coefficient estimates and the 95 percent confidence intervals from a series of these regressions. We see that the coefficient estimates remain small and insignificant irrespective of which set of pre-treatment variables we adjust for.

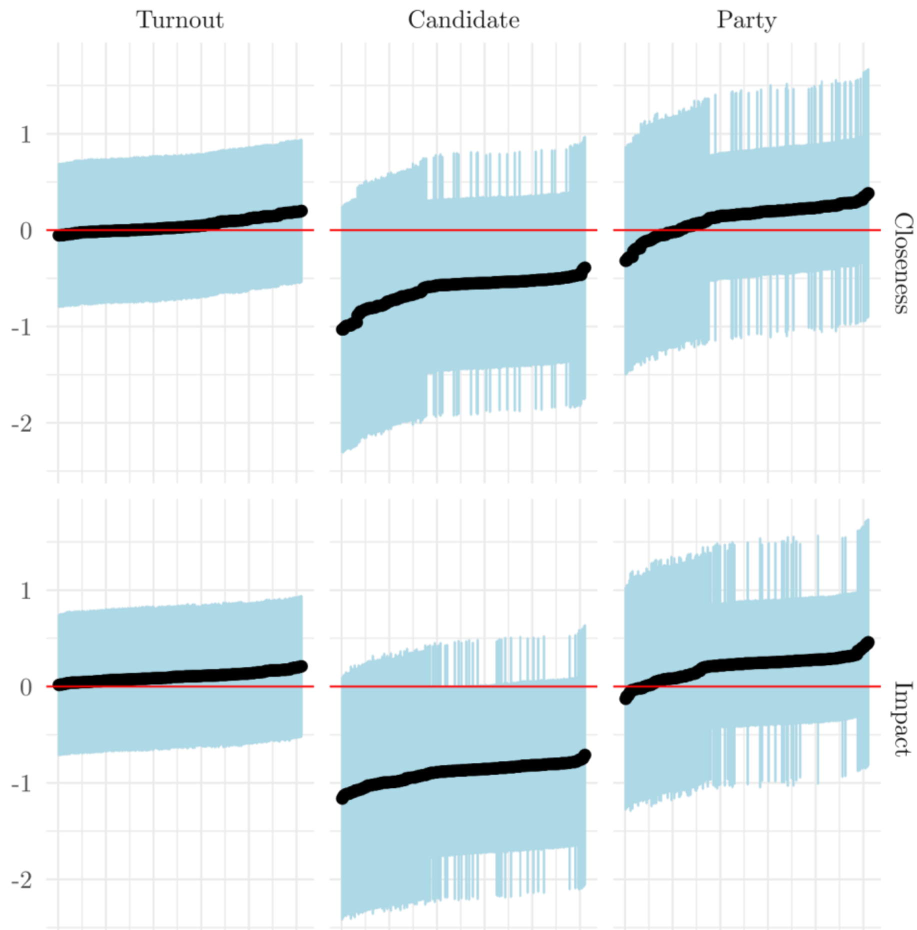


Figure 4: Coefficient estimates and 95 percent confidence intervals given each possible combination of pre-treatment covariates (arranged from smallest to largest).

6 Alternative outcomes

The table below shows coefficient estimates for campaign effects on the joint vote-share of all opposition candidates, which include Galiamina (Yabloko), Makarov (Parnas), Yurchenko (Civic Platform), Onishchenko (Patriots of Russia), Obedkov (Greens), Nechaev (Partiia Rosta), and Guskov (Grazhdanskaia Sila).

		Alloppositioncandidates		
		(1)	(2)	
			−0.34	Closeness
0.71			.07)	
	Impact		−1.13	(1.24) (1
−0.72			.06)	
	Turnout in 2013		0.93**	(1.23) (1
			(0.11)	
	Observations	210	210	
	Adjusted R^2	-0.003	0.25	
	*p<0.05; **p<0.01.			

Dependent variables are measured in percentages.

Table 3

The results do not indicate either substantively large or statistically significant effects of either campaign on the votes for all opposition candidates; if anything, most coefficients are actually negative.

7 Interactive effects

The figure below shows the marginal effects of experimental treatments on turnout at different levels of support for Navalny in 2013 (estimated from an interactive regression model). We don't see substantially meaningful or statistically significant effects of either treatment for any level of prior opposition support. Very similar results emerge if we use vote-share for Yabloko in 2014 local elections as an alternative measure of opposition's support in a precinct.

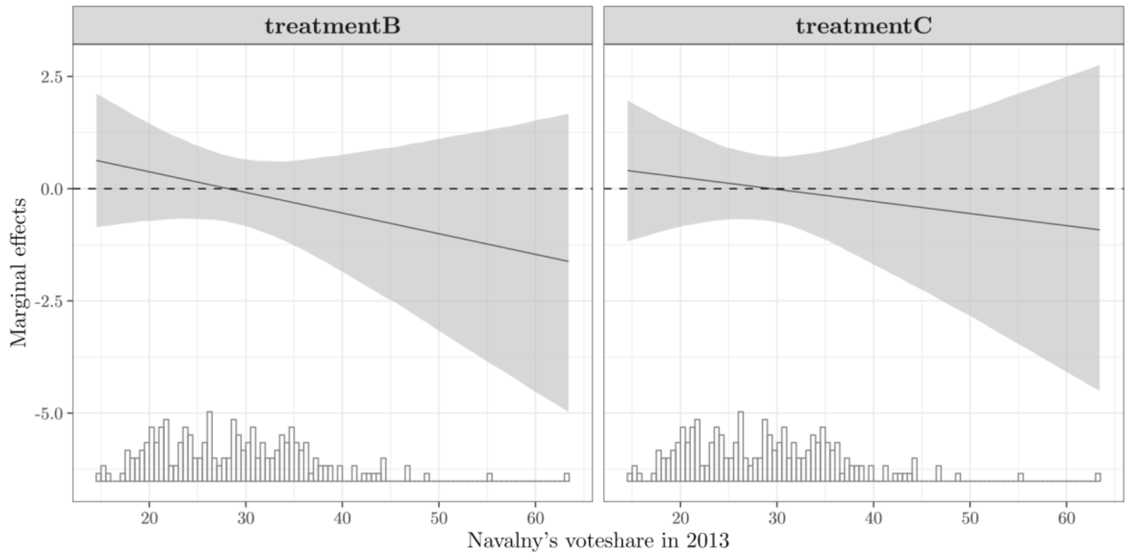


Figure 5: Marginal effects of each treatment conditional on Navalny's vote-share in 2013. "TreatmentB" = Closeness, "TreatmentC" = Impact

8 Effects at different saturation rates

Galiamina vote

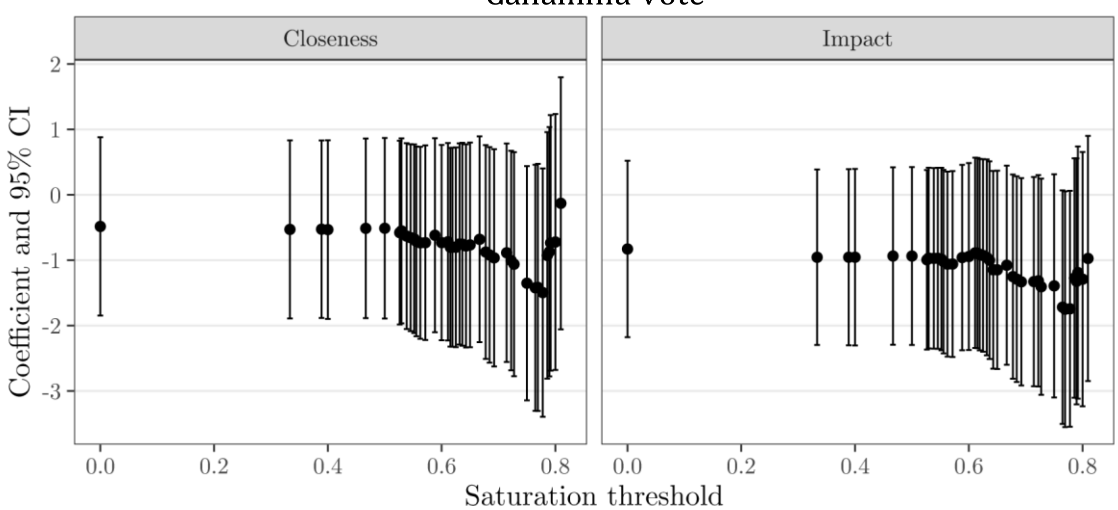


Figure 6: Treatment effects on the candidate's vote at different levels of treatment saturation.

Yabloko vote

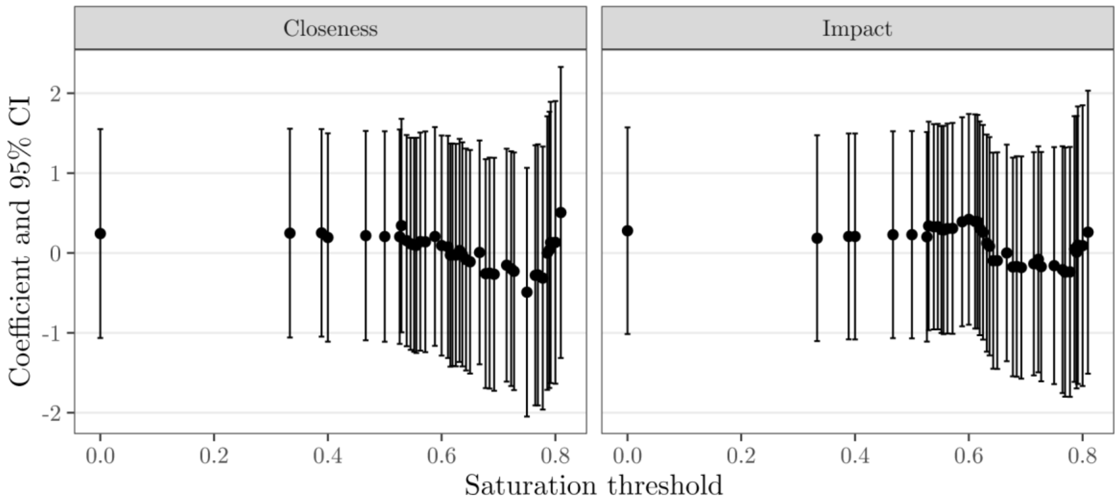


Figure 7: Treatment effects on the candidate's party vote at different levels of treatment saturation.

9 Election Fraud

9.1 The Extent of Fraud

Elections in Russia in the recent past are known to be fraudulent (Enikolopov et al., 2013; Klimek et al., 2012; Kalinin and Mebane, 2012; Mebane, 2013; Kobak et al., 2016; Kobak, Shpilkin and Pshenichnikov, 2018; Rozenas, 2017). However, according to the report by the *Committee of Civic Initiatives*, a non-governmental civic organization, given that elections in Russia are typically manipulated by inflating turnout, the 2016 elections have fewer signs of fraud and other "non-electoral means" of influence, because official turnout figures in 2016 were lowest in Russia's post-1991 history (and especially so in Moscow) [1]. There were very few reports of election fraud in Moscow, especially in comparison to the infamous 2011 parliamentary election (Kynev, Lyubarev and Maksimov, 2017).

To evaluate the extent of fraud in district 198 where the experiment was conducted, we reached out to Dr. Sergey Shpilkin, one of the leading specialists on election fraud in Russia. According to Dr. Shpilkin's analysis, there is no evidence of fraud in district 198. Consider Figure 8: in a typical Russian district with fraud, the conditional distribution of support for

[1] https://komitetgi.ru/analytics/2969/

candidates varies significantly depending on turnout. However, that is not the case in district 198, where the conditional distribution for the first-ranked (pro-government) candidate and the conditional distribution for the second-ranked candidate (Galiamina) have very similar shapes. These distributions look quite different in districts with election fraud.

9.2 *Effect Attenuation Due to Regime Fraud Conditional on the Treatment Assignments*

The biggest challenge to our results is that the government conducted fraud conditional on the distribution of experimental treatments. We believe that this is extremely unlikely, because the campaign took place only a few days prior to election, and even then it is not clear why or how the government would decide to incentivize more fraud in the control versus

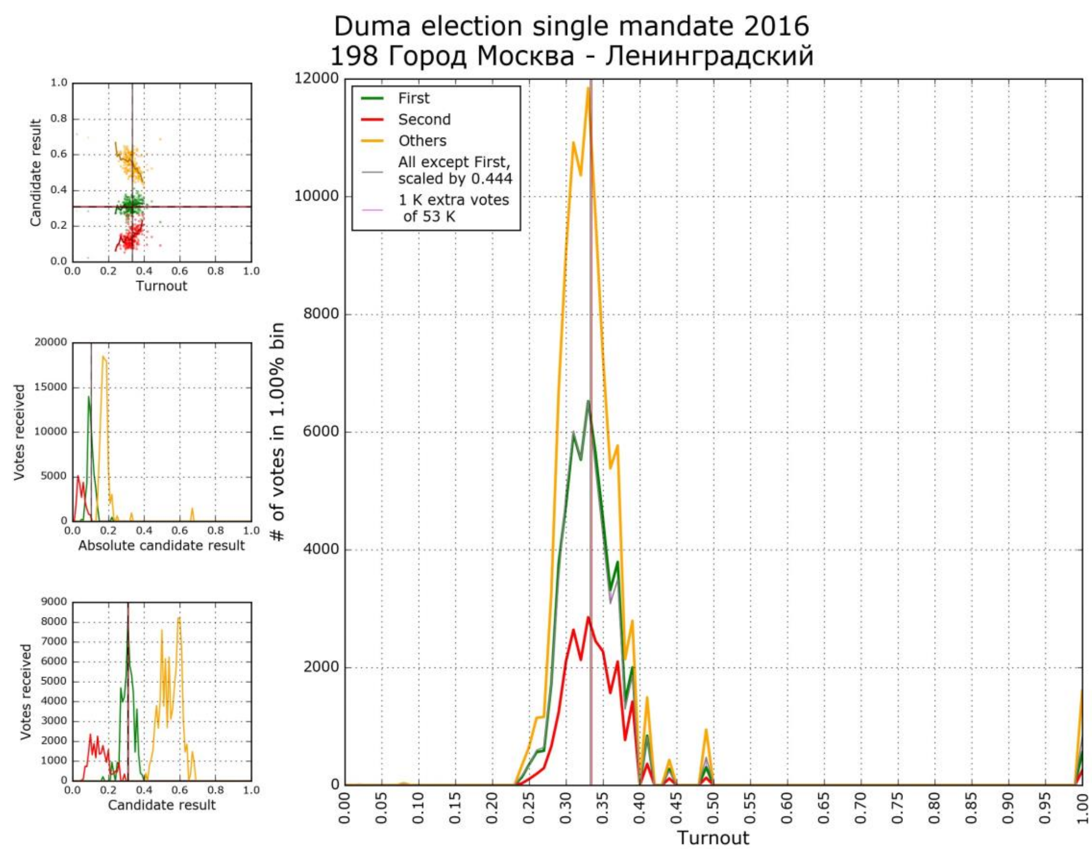


Figure 8: Distribution of support for the first-ranked, second-ranked, and other-ranked candidates across precincts conditional on turnout. Source: Sergey Shpilkin.

treatment precincts. Furthermore, even if there was fraud conditioned on the treatment, we have to consider how exactly the fraud had to be conducted to produce null experimental results.

Suppose that the treatments had in fact increased turnout. For the fraud to drive the estimated treatment effects so close to zero, one of the two things must have happened. First, either the regime artificially reduced turnout in precincts randomly assigned to the two mobilizational messages or it inflated turnout in precincts assigned to the control condition. The second possibility seems far more plausible, because the existing studies suggest that Russian government predominantly uses turnout inflation to manipulate election results (Klimek et al., 2012; Frye, Reuter and Szakonyi, 2014). Now, if the government inflated turnout in precincts randomized to the control condition then we would expect that those inflated votes should have gone to pro-regime candidates and parties, and so the support for the Galiamina and Yabloko party should be lower in the two treatment conditions. But that is not what we observe.[2]

9.3 Effect Attenuation Due to Orthogonal Fraud

Another, far more reasonable, possibility is that election fraud occurred, but it was not conditioned on the experimental conditions, i.e. it was orthogonal to the distribution of treatments. Even then, it is possible that fraud led to biased estimates of treatment effects due to the attenuation bias, because it introduced an asymmetric measurement error in the outcome variable.

To evaluate the extent of such attenuation bias we conduct a simulation study. We simulate election data where each informational treatment has a known effect and then we

[2] Another sign of fraud could be that the government has invalidated more ballots in the treatment precincts, but our empirical estimates show no evidence to that effect. In the regression where the dependent variable is the percentage of invalid ballots, the coefficients for the closeness and the impact treatments are -0.07 and -0.10, respectively.

contaminate these data to see how the estimated effects would change under different assumptions about election fraud. Our simulations are for turnout, but the same ideas extend to electoral support.

To stay as closely as possible to our study context, we generate data for $i = 1,...,210$ precincts assuming the overall voting population in each precinct i to be N_i, where N_i is the number of registered voters actually registered in that precinct. The number of registered voters in Russia's precincts is based on registration of residential addresses with local police units, and it is not subject to manipulation (Enikolopov et al., 2013). We then simulate turnout data as follows:

(4) $$Y_i^* \sim \text{Binomial}(N_i, 1/3 + 0.01 \cdot \text{Closeness}_i + 0.02 \cdot \text{Impact}_i),$$

(5) $$f_i \sim \text{Binomial}(N_i - Y_i^*, F),$$

(6) $$y_i = 100(Y_i^* + f_i)/N_i.$$

Y_i^* represents the true number of voters in precinct i who turned out to vote. We assume that the turnout rate in the baseline condition is 1/3 (roughly the empirical average), whereas it is higher, on average, by 1 percentage point in precincts treated with the "closeness' flier, and 2 percentage points higher in precincts treated with the "impact" flier. We deliberately chose quite small treatment effects to favor the attenuation effect hypothesis. The distribution of the treatments across the precincts is assumed to be exactly the same as in the experiment.

Equation 4 shows how the true turnout is then contaminated: for each of the $N_i - Y_i^*$ who did not show up, the regime's officials/representatives at the precinct polling station take an average share of F ballots and stuff them into the ballot boxes to inflate turnout. Finally, the observed turnout percentage is calculated by adding the true turnout percentage and the artificially inflated turnout percentage, as shown in equation 6.

After election data are simulated, for a given F, we estimate the WLS regression of simulated turnout on the closeness and impact treatments, with the control flier as the reference category. We simulate the datasets by setting F to be 0, 0.05, 0.25, 0.5, and 0.75, and for each value of F we simulate 1,000 datasets, and so estimate 1,000 coefficient values for each treatment.

The results are shown in Figure 9. When the election data are simulated without fraud F = 0, expectedly, the average estimated coefficient corresponds to the ground truth for each treatment. As we increasingly contaminate election data by inflating turnout, the coefficients attenuate, but the rate of attenuation is very slow. If turnout is inflated by 5% (F = 0.05), the distribution of the coefficients is only slightly closer to zero relative to clean election

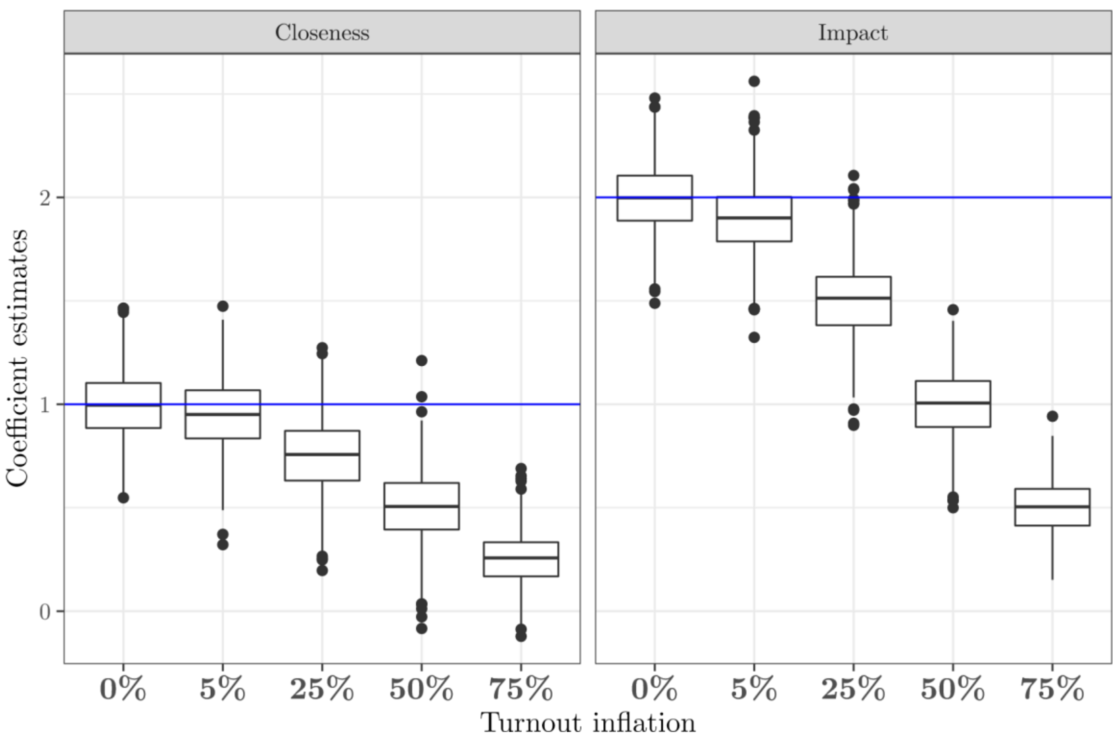


Figure 9: Distributions of the simulated coefficient estimates at different levels of turnout inflation. The horizontal blue lines show the assumed true effects of each treatment.

data. Even when the turnout rate is inflated by 25 % (F = 0.25), all simulated coefficients are positive and substantively larger then the actual coefficients we estimate in the paper. Only when the turnout inflation rates increase above 50% do we start seeing substantial attenuation effects that would be required to explain our null results. The existing studies put the estimate on turnout inflation at around five percentage points (Enikolopov et al., 2013), which is considerably below the degree of turnout inflation that would be reguired to generate the null results we observe in the experiment.

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