Persistent Polarizing Effects of Persuasion: Experimental Evidence from Turkey

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Abstract

I analyze the electoral effects of randomly assigned information campaigns implemented before a referendum on weakening constraints on the executive in Turkey. Using administrative electoral data, I show that voter responses to identical information campaigns increased policy polarization over the referendum and partisan polarization in presidential, general, and local elections over the next two years. The evidence supports the claim that in a setting where voters disagree on whether strengthening the state is a good policy, combating censorship can backfire. I conclude that because information campaigns have both positive and negative effects, their impact on civil society is underestimated.

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1 Introduction

Although the number of democracies has rapidly increased following the fall of the Berlin Wall, civil liberties and political rights around the globe are deteriorating (Figure A1). Nearly half of democracies today are characterized by low levels of political accountability and civil liberties, including media censorship and the political exclusion of targeted groups (Bidner et al., 2015; Mukand and Rodrik, 2020). It has been well documented that in these environments state censorship is an important channel for swaying electoral support and consolidating power under the incumbent (Guriev and Treisman, 2019). This raises the question of whether disseminating non-state-generated information can counterbalance this power and reduce voter support for measures weakening democratic institutions.

In this study, I use experimental field evidence to analyze the impact of exposure to non-state-provisioned information on voter behavior and ideology in Turkey. I evaluate two randomized door-to-door information campaigns that were delivered to approximately a quarter million voters before a constitutional referendum. The incumbent leader initiated the referendum: a "Yes" or "No" vote on an institutional policy reform that would weaken constraints on the executive branch and strengthen the state under the incumbent. The information campaigns were organized by members of the largest party opposing the referendum and carried out by party volunteers.[1] I find that rather than uniformly countering electoral support for the proposed policy, exposure to identical information campaigns increased policy polarization. Moreover, I find that this transitory exposure to the same information campaigns had persistent polarizing effects along partisan lines according to administrative election data gathered approximately one and two years later, resulting in increased partisan polarization. These longer-term results provide supporting evidence that in a setting where voters disagree on whether strengthening the state is a good policy, combating censorship can increase support for the incumbent.

Relative to liberal democracies where voters have access to diverse information sources, information campaigns are assumed to have a large average effect on vote share in countries where media censorship is high (Enikolopov et al., 2011). I show to the contrary that the information campaigns in Turkey had a zero average effect on vote share across the ideological spectrum. A null result is a common outcome of information interventions designed to affect consumer or voter behavior, but I use a proxy measure of baseline ideology and stratified randomization design to show that the zero average effect masks both intended and backfiring effects of the information campaigns on vote share. Overall, the results in this study suggest that i) the effects of information campaigns, or media, on vote share in existing studies may be underestimated because potential backfiring effects were not accounted for in the empirical designs and ii) information transmission is an important mechanism for explaining persistent political polarization.

[1]The opportunity to conduct this evaluation was an outcome of a nonpartisan study that I had done earlier with the involvement of all the political parties in parliament. Due to ethical considerations, I offered to evaluate data for all of the parties, but only the main opposition party took up the offer for systemic reasons discussed in Baysan (2018).

This study circumvented a number of challenges that have hindered research on reducing censorship and voter support for the state, both in terms of implementation and experimental design. First, such a study has to be done in a context where democratic institutions are already weak. Generally, publicly available data are limited in such settings. This is also true for Turkey with the exception of ballot box-level vote share and voter turnout data, which have been publicly available since 2009. In addition, referendums on proposed changes to democratic institutions are irregular and initiated by incumbent leaders during tumultuous periods. Therefore, the referendum presented a rare opportunity to obtain experimental evidence on factors driving voter policy preferences for a stronger state. Indeed, the timing and context of this particular referendum are especially significant. In the decade preceding the referendum, Turkey experienced one of the sharpest deteriorations in democratic norms in the world. As of 2016, its ranking in terms of civil liberties and political rights was below that of Pakistan, Bangladesh, and Kenya and it was the leading jailer of journalists in absolute numbers (Freedom House, 2017; Beiser, 2018). A low point in this decline was after the coup attempt in July 2016 when there was an escalation in mass arrests. The coup attempt catalyzed the referendum, which was held less than a year later. This study took place at this significant juncture in Turkey's history and in the history of representative democracy globally.

Second, conducting this study during a period of declining democratic norms, when social tensions along ideological lines were highly politicized, affected the experimental design. I expected that party affiliation would be predictive of how voters react to the information campaigns. This expectation required a careful experimental design incorporating subgroup analysis along partisan lines (as the researcher, I could only suggest an experimental design and had no control over the content of the information campaigns). Specifically, randomization was conducted within strata of quartiles of the past average vote share for the main opposition party in two 2015 general elections. Empirically, I confirm that pre-existing support for the opposition party is a good predictor of the "No" vote share in the referendum. The intention to estimate heterogeneous treatment effects by stratum was specified in a registered pre-analysis plan.[2] Pre-specification can be important for inference not only because it eliminates the need to search over many possible partitions of the data, but also because it can significantly strengthen results when heterogeneity in outcomes across subgroups is considered a key focus of a study (Athey and Imbens, 2016; Duflo et al., 2020).

Identical information campaigns were conducted in neighborhoods randomly selected to be in the treatment group. In one treatment arm, voters received information on past policy outcomes related to the deteriorating economy and increasing terrorist attacks under the incumbent. In the other treatment arm, voters received information on the longer-term implications of the proposed constitutional changes without any mention of the incumbent. Voters in neighborhoods randomly assigned to the control group were not visited by canvassers. Following the pre-analysis plan, I estimate the average treatment effect on vote share and voter turnout across quartiles and by quartile. The outcome variables are observed at the ballot-box level for all elections. Taken together, the two information campaigns had a zero

[2]Registered at osf.io/hhqej and retrieved from osf.io/8394u (Baysan, 2017).

average effect on vote share. However, they significantly increased the "No" vote share by 1.2 percentage points (1.7%, p-value .03) and .8 percentage points (1%, p-value .031) in the third and fourth quartiles, respectively, where the opposition was stronger. The campaigns significantly decreased the "No" vote share by 3.4 percentage points (5.3 percent, p-value .001) in the second quartile and, insignificantly, by .6 percentage points (1.1%, p-value .764) in the first quartile, where the opposition was weaker. Since the negative effects were in quartiles where the "No" vote share was low and the positive effects were in quartiles where the "No" vote share was high, the information campaigns resulted in policy polarization. The difference in the average vote share between the second and third quartiles increased by more than 5 percentage points (50%). While I cannot directly rule out a mobilization effect stemming from differential mobilization within quartiles, I find that the information campaigns had a zero average treatment effect on voter turnout overall and by quartile.

Conducting the analysis in this polarized period helped to uncover the duration of the underlying heterogeneous effects. Three elections occurred within two years after the referendum, allowing me to estimate the persistence of campaign induced polarization. General and presidential elections were held on June 24, 2018, and a local election was held on March 31, 2019. The relatively rare opportunity of having both multiple elections after a single campaign experiment and publicly available electoral data at the level of treatment alleviates the need to rely on follow-up surveys and self-reported voting data. I find that there was no weakening in the magnitude or significance of the treatment effects in either 2018 election. Local election data shows that the polarizing effects of the information campaigns persisted into 2019. The estimated negative and positive effects are similar in magnitude to those of previous elections, and the negative effect remains statistically significant. Altogether, these findings suggest that policy views on the issue of a stronger state remained salient in regular elections and that the information campaigns therefore increased partisan polarization.

The experimental design rules out the possibility that the increase in polarization is driven by voters self-selecting into different ideological sources of information, a common mechanism explored in the literature (e.g., Gentzkow and Shapiro, 2011; Martin and Yurukoglu, 2017). My results suggest that voters interpreted either the same information or the quality of the opposition party (information source) differently. In Turkey, this may relate to survey evidence showing that voters have different views on why policy outcomes have deteriorated, ranging from external threats and terrorist attacks to poor incumbent performance. If this, in turn, translates into differences in ideological positions on whether more, or less, constraint on the executive is the best policy option then voters could have become more polarized in response to the same information campaigns. I refer to this policy dimension as authoritarianism or state strength. Put differently, voters face an identification problem (e.g., Piketty, 1995; Dixit and Weibull, 2007; Andreoni and Mylovanov, 2012; Benoit and Dubra, 2016; Loh and Phelan, 2019) and, even under the assumption of Bayesian learning and common exposure to information, disagreement over the optimal policy can persist (Acemoglu et al., 2016; Gentzkow et al., 2021). Using this framework, the information campaigns caused the opposition party to unintentionally lose votes among moderate voters who previously supported them on one dimension, e.g. religious identity (secularists versus Islamists), but not on the proposed policy for more authoritarianism. These secular authoritarians not

only changed their vote to "Yes" in the referendum because of the information campaigns, but continued to support the incumbent party in subsequent elections. This persistence of treatment induced polarization on to regular elections is consistent with the evidence on the longer run impacts of divisive referenda. A common feature of such referenda, e.g. Brexit, is that the state proposes a new policy dimension that cuts across a traditional political cleavage, e.g. left-right dimension, and causes a realignment in political affiliation (Hobolt and Rodon, 2020).

My study contributes to the empirical literature on the political economy of persuasion. Experimental political information campaigns have been evaluated primarily in countries with low media censorship (e.g., Pons, 2018; Kalla and Broockman, 2018).[3] The estimated vote share effects in these settings are typically smaller than what I find; for example, a meta-analysis of 49 information campaign experiments in the U.S. found that the campaigns had a zero average effect on vote share (Kalla and Broockman, 2018). My results are therefore consistent with the hypothesis that information campaigns have larger effects on vote share in weak democracies (Enikolopov et al., 2011). My study also speaks to quasi-experimental and experimental studyies that evaluate whether exposure to state media increases electoral support for the incumbent (Adena et al., 2015; Enikolopov et al., 2011; Knight and Tribin, 2019; Chen and Yang, 2019).

A less explored hypothesis is whether information transmission is more likely to lead to a persistent increase in political polarization in illiberal democracies. The majority of empirical studies find that exposure to the same information campaign or media outlet has a uniform effect on vote share or voter behavior and, in the U.S., exposure to counter-attitudinal media can even reduce attitudinal polarization (Levy, 2020).[4] The exceptions are studies that evaluate this relationship in weak democracies (Adena et al., 2015; Peisakhin and Rozenas, 2018). Consistent with these papers, I provide experimental evidence that exposure to the same information outlet increases electoral polarization in a weak democracy. Moreover, I show that the effects of this one-off campaign persists.[5]

The remainder of this paper is organized as follows. In Section 2, I provide background information on political polarization in Turkey and the 2017 referendum. In Section 3,

[3]A related literature studies the impact of nonpartisan information campaigns or the media on voter behavior in middle-income and lower-income countries. Examples include Ferraz and Finan (2008); Banerjee et al. (2010); Casey (2015); Chong et al. (2015); Cruz et al. (2020); Platas and Raffler (2021); Dunning et al. (2019).

[4]In the literature on media and persuasion, exposure to certain media outlets is modelled as consumption of ideological slant (e.g., DellaVigna and Kaplan, 2007) and consumers have preferences for media that are slanted toward their own ideology (Martin and Yurukoglu, 2017). Therefore, self-selection of voters with different ideological views into separate media outlets can increase political polarization; however, the degree of selective exposure is found to be small (Gentzkow and Shapiro, 2011; Flaxman et al., 2016). While the existing empirical analyses in this literature find that media has a uniform effect on vote share, it's unclear whether polarizing effects of exposure to the same media outlet can be ruled out.

[5]Voter polarization in response to the same information has also been investigated in lab or online settings in liberal democracies, but these studies typically observe self-reported views over a short period (e.g., Lord et al., 1979; Andreoni and Mylovanov, 2012; Fryer et al., 2019) and do not necessarily observe backfiring effects (Alesina et al., 2018).

I discuss the information campaigns and the experimental design. Section 4 presents the results. In Section 5, I discuss various explanations of the results. Section 6 concludes.

2 Background

2.1 Polarization in Turkey and the 2017 Referendum

After the founding of Turkey, religious parties were marginalized until the Justice and Development Party (AK Party) gained power in 2003 under the leadership of Erdoğan (Tuğal, 2019). As discussed by Tuğal (2021), the AK Party did not seek to dismantle the existing "authoritarian-militaristic structures of the Turkish state," from which they had been ostracized, but instead sought to infiltrate and lead them. They also positioned themselves as opposing the "secular elites" who were characterized as having contempt for the uneducated and rural masses. Throughout, Erdoğan asserted his shared identity with "Black Turks" as opposed to "White Turks" in his speeches (Acemoglu et al., 2013) and referred to his followers as the "real people" (Temelkuran, 2019, p. 20) as opposed to the "establishment" or "elite" (Aydin-Düzgit, 2019).

Figure A2, which uses data from VDem, shows that after gaining power, the AK Party gradually increased repression of the opposition and media censorship (Coppedge et al., 2021). Consistent with episodes of democratic backsliding in other countries, the Turkish state contextualized targeted arrests and repression as a response to national security concerns and the need to control the "enemy" and secure the country from "foreign threats" (Levitsky and Ziblatt, 2018). External threats were salient to voters. Starting in 2010, and especially in 2015, there was a dramatic and unprecedented rise of terrorist attacks, which is clearly visible in data from the Global Terrorism Database (see Figure A3a). This period also saw a resurgence of civil conflict and state repression in Kurdish-majority areas, and finally the coup attempt of July 2016. During the coup attempt, millions of citizens were within earshot of air strikes, thousands took to the streets to prevent the coup, and hundreds were killed. In parallel to worsening national security, there were also indications of a worsening economy, including a devaluation in the Turkish Lira. As shown in Figure A3b, this devaluation started in 2010 and escalated starting in 2015.

Just five months after the coup attempt, the AK Party proposed a referendum, which asked for a "Yes" or "No" vote on switching from a parliamentary system to a presidential system and on eighteen amendments to the constitution. Around the same time, there was also a record devaluation in the local currency. The AK Party's main pro-referendum argument was that the proposed institutional changes would increase national security by increasing state strength (Esen and Gumuscu, 2017). As an example, if the referendum passed, the president could efficiently increase national security by unilaterally "determining the national security policies and taking the necessary measures" (proposed Article 104(13) for the constitution as translated in (Commission, 2017)) Therefore, the AK Party's position suggested that a stronger state would also contribute to economic stability.

Independent international agencies, such as the European Commission for Democracy through

Law, however, warned that the "proposed constitutional amendments would introduce in Turkey a presidential regime which lacks the necessary checks and balances required to safeguard against becoming an authoritarian one" (Commission, 2017).[6] Therefore, winning the referendum would institutionalize weakened democratic norms and constraints on the executive. Using data from VDem on democratic indices, Figure A4 shows that the referendum marked a low point in the deterioration of democratic norms in Turkey.

Ultimately, however, support for the referendum depended on voters' perceiving the worsening of national security, and concurrent deteriorating economic conditions, as external threats beyond the control of the state. If so, voters would be willing to support a stronger state under the incumbent, despite a weakening in democratic institutions. Nationally representative survey evidence collected prior to the referendum is informative on the nature of voter perceptions regarding external threats. Figure 1 includes graphs constructed using voter responses to two survey questions from this survey. In Figure 1a, we see that irrespective party affiliation the majority of voters agreed that the devaluation of the Turkish lira had a negative impact on their personal life. However, responses significantly vary when it comes to attribution for the devaluation. Figure 1b shows that a majority of those supporting the opposition blame the incumbent while a majority of those supporting the incumbent instead blame external threats. The survey data also suggest that party affiliation and differing views on attribution, which presumably predicts differing support for a stronger state, is correlated, but imperfectly. The results of the experiment suggest that this correlation became stronger in response to the information campaigns by causing moderate voters to switch party affiliation, and cross "identity" lines, to the one aligned on their ideological position regarding a stronger state. Overall, this resulted in increased ideological polarization. More general, this period of significant ideological polarization, where polarization is defined as differences in views on a major political issue, is evident in VDem data on polarization (Figure 1c).

3 Voter Campaign Experiment

3.1 Setting

The administration of the door-to-door information campaigns occurred in Izmir, the third most populous province out of 81 provinces in Turkey. Historically, the opposition party had longstanding high electoral support in Izmir, implying that support for the referendum would be low. Figure A5 illustrates this by showing the neighborhood-level distribution of

[6] As an example, the referendum granted the president a new power to bypass the parliament completely and introduce legislation by issuing decrees with the force of law (Jenkins, 2016). The referendum also introduced ambiguous procedures by which a president could serve up to three five-year terms. With respect to Erdoğan, the successful adoption of the constitutional amendments permit him to potentially extend his position of power, which started in 2003, to 2033. Erdoğan served as prime minister from 2003 to 2014, stepping down just before his term limit in order to be the first nationally elected president of Turkey as of 2014. Therefore, at the time of the 2017 referendum, Erdoğan was president in a parliamentary system, but with the introduction of a presidential regime could potentially serve until 2033 as both the head of state and the head of government.

the 2017 "No" vote share for the whole country and for the experimental sample. Operating in an opposition stronghold facilitated the team's ability to immediately organize a group of party volunteers who were willing to canvass during a state of emergency. Recruiting volunteers on short notice was generally difficult because of an environment of suspicion and potentially high repercussions from being reported to the authorities. The volunteers had prior experience in canvassing and were all based in Izmir. The funding and details of the content of the campaigns were determined by a campaign manager and staff from the opposition party who were managing operations in Izmir. The overall strategy was selected by this campaign team and was not a part of a centralized party campaign.

3.2 Sampling

I selected neighborhoods for the experiment using neighborhood-level voter data from the November 2015 general election. This was the most recent election prior to the referendum. An important consideration in sampling was the campaign team's capacity constraint for outreach. Since randomization would be at the neighborhood-level, the compliance rate within each neighborhood needed to be sufficiently high and a minimum number of neighborhoods needed to be reached to increase the statistical power of the experiment. Therefore, before conducting the randomization, I excluded from the sample neighborhoods that would be too difficult to reach or would take too long to completely cover. Following the definition of "rural" used by survey companies in Turkey, I classified neighborhoods as "rural" if they had 500 or fewer registered voters in 2015. Then, I classified a district as "rural" if more than 50% of the neighborhoods in it were rural and dropped these districts. I then dropped neighborhoods where the number of registered voters was in the top 7% or bottom 5% of the distribution. Large neighborhoods were also dropped because they require extra time to cover all households. Finally, to further decrease large geographic dispersion, I dropped districts that were in the bottom 15% in terms of the number of remaining neighborhoods.[7]

In the end, the sampling selection, prior to randomization, included 14 out of 30 districts and 550 out of 1294 neighborhoods in Izmir. The sampling selection procedure increased the average and median number of registered voters per neighborhood, relative to the population of neighborhoods in Izmir, from 2403 and 679 to 2690 and 1545, respectively. The range of the number of registered voters also changed from 15–28,134 to 113–10,946. Figures A9 and A10 show the geographic information for the experimental sample and the spatial correlation in the dropped districts. The sampling selection procedure did not meaningfully change the average 2015 vote share for the opposition party. It increased from .42 (standard deviation .17) to .44 (standard deviation .16).

3.3 Experimental Design

The experiment involved two separate information campaign treatment arms. In one treatment arm, voters were exposed to information on past policy outcomes (PO campaign). The campaign was on negative incumbent performance. It included information on deteriorating

[7]The campaign team requested dropping 15 neighborhoods in which neither of the two main parties had at least 30% of the vote share in the previous general elections prior to randomization.

economic conditions and increased terrorist activity under the current leadership. In general, the information was meant to convey that power should not be consolidated under the current president, Erdoğan, because of poor performance. The other treatment arm included information on the implications of the proposed referendum, which would weaken the system of checks and balances (CB campaign). The focus was on telling voters that the referendum would lower the accountability of any president elected in the future and not just the current leadership. In contrast to the PO campaign, the CB campaign did not mention the current leadership; rather, the message was that the referendum would involve weakened democratic institutions. Overall, the information campaigns were a mechanism by which the campaign team could combat state censorship and directly deliver information on the referendum to voters.

In treated neighborhoods, messages were conveyed to voters both verbally, if they opened their door, and in a pamphlet. The original text of the pamphlets for each campaign can be seen in Figures 2a and 2b. The canvassers also received training from the campaign team on how to deliver the information outlined in the pamphlet orally and in a personable manner. In both campaigns, the canvassers informed voters that they were volunteers from the opposition party.

I used a stratified randomization design for the experiment. Prior to randomization, I calculated the stratifying variable using the average vote share for the CHP, the main opposition party, across two general elections held in 2015.[8] I calculated quartiles of this variable and then, within each quartile, the neighborhoods were randomly allocated into one of the two information campaign treatment arms or the control group. The distribution and quartiles of the stratifying variable are shown in Figure 3. I used the vote share for the main opposition party because it has a strong negative correlation with the incumbent party's vote share in the sample. Figure A6 shows that the 2015 vote share for the main opposition party is a strong predictor of the "No" vote share in the 2017 referendum. Within each quartile there are 25 treatment neighborhoods, with 12 treatment neighborhoods that were covered in the PO campaign and 13 covered in the CB campaign. Therefore, altogether, of the 550 neighborhoods sampled for the experiment, 48 were assigned to the PO campaign treatment group, 52 to the CB campaign treatment group, and 450 to the control group. The control neighborhoods were not exposed to either campaign. The probability of a neighborhood being assigned to the treatment group was less than 50% because it was unlikely that the campaign team could reach more than a total of 100 neighborhoods.

3.4 Implementation

A number of challenges specific to an illiberal democracy affected the planned implementation of the campaigns. First, given the state of emergency, it was possible that voters would be hesitant to open their doors. Second, the party was constrained in terms of the number of volunteers that were willing to canvass. To address the first issue, every household in a treated neighborhood was visited to increase the likelihood that a sufficient share of voters

[8]The first general election in 2015 was held in June and, as the AK Party failed to form a coalition, a snap general election was held in November 2015.

opened their doors and engaged with the canvassers at the neighborhood level.[9] In-person conversation is considered to be one of the most effective methods of affecting voter behavior (Pons, 2018; Green and Gerber, 2015). Reaching more neighborhoods, but only partially covering them, would have reduced the statistical power of the experiment.

Once canvassing started, I observed whether the implementation of the information campaigns varied by quartile or whether there was selective canvassing within neighborhoods. I had geocoded every street in each neighborhood and provided the canvassers with an optimal route for each neighborhood. In turn, they recorded the number of people they completed a conversation with per street. This procedure allowed me to ensure that the canvassers were not selecting certain streets within a neighborhood. In addition, I combined these results with data on the number of registered voters per street to calculate the average share of voters that interacted with the canvassers in each neighborhood. According to the canvassers' records, out of the 100 treatment neighborhoods, 20 could not be canvassed because the party volunteers reported that they received threats (aggressive behavior, warnings to call the police, etc.). In the 80 neighborhoods where the volunteers did not receive any threats, all streets were recorded as being canvassed. Table A1 shows the average share of registered voters per neighborhood that canvassers could complete a conversation with (reach rate). These descriptive statistics show that the average reach rate is not correlated with the quartiles of past vote share.

Even though the volunteers recorded threatening neighborhoods as being unreachable, it is still possible that the neighborhoods were partially canvassed or some voters briefly saw canvassers. Therefore, I only estimate the intent-to-treat (ITT) effect.

3.5 Empirical Strategy and Pre-analysis Plan

A. Data

My analysis draws on voter data for the two 2015 general elections, the 2017 referendum, the 2018 presidential election, the 2018 general election, and the 2019 local election that were scraped from the Supreme Election Council's website.[10] The data are available at the ballot box-level with neighborhood names. Ballot boxes cannot be matched across time, but district and neighborhood names were used to match observations across elections.

B. Empirical Strategy and Pre-analysis Plan

I estimate the effect of a neighborhood being assigned to the treatment group (ITT effect of the information campaigns) on vote share and voter turnout using the following OLS

[9]The experimental design did not affect the total number of households the campaign team planned to target. This number was determined by the number of volunteers available to the team. However, there was an explicit discussion between myself and the campaign team that there would be more statistical power in the experiment if they targeted all households in a neighborhood instead of maximizing the number of neighborhoods and limiting the number of households visited.

[10]https://sonuc.ysk.gov.tr

specification:

$$Y_{bnq} = \alpha_1 + \beta_1 T_{nq} + X'_{nq}\lambda_1 + \delta_q + \epsilon_{bnq}, \quad (1)$$

where Y_{bnq} is the ballot-box level vote share or voter turnout. The outcome variable for vote share in the 2017 referendum is the "No" vote share. The analogous outcome variable for vote share in the 2018 presidential election relative to the referendum is the share that voted for a candidate other than Erdoğan. Similarly, the relevant outcome variables for vote share in the 2018 general election and the 2019 local election are the vote share for the opposition parties.[11] In the general election, members of parliament were elected to represent Izmir for a five-year term by a system based on closed list proportional representation according to the D'Hondt method. In the local elections, metropolitan mayors, district municipal mayors, and municipal councilors were elected. T_{nq} is an indicator for whether the neighborhood was assigned to one of the two treatment groups and δ_q are strata (quartile) fixed effects.

Following the pre-analysis plan, I estimate the effect of the information campaigns on vote share and voter turnout both across quartiles and within each quartile.[12] I also pre-specified, the vector of control variables measuring past voter data from each election in 2015 in Equation (1) (X'_{nq}). The control variables include the number of registered voters, the number of valid votes, the number of votes for the CHP, vote share for the CHP, and voter turnout. I show the balance between the treatment and control groups for these variables across quartiles in Table A2. Since I specified estimating heterogeneous treatment effects by quartile and by campaign (CB and PO) in the pre-analysis plan, I also show the balance of the pre-specified covariates across the control group by treatment arm (CB and PO, separately) and within each quartile. These results are shown in Tables A6 and A7. I chose these control variables because, in general, voting data from past elections in Turkey explain a significant amount of variation in voter turnout and vote share of future elections. Therefore, including them in the regression improved the statistical power in estimating the heterogeneous treatment effects and in correcting for imbalance between the CB campaign and control group in quartile 3.

In the pre-analysis plan, the main empirical specification is equivalent to Equation (1) except that all variables are at the neighborhood level. In the main results presented in this paper, following Equation (1), the control variables are also at the neighborhood level, but the dependent variables are at the ballot-box level. Standard errors are clustered at the neighborhood level.

In addition to the ballot box-level analysis, I show the results analyzing the outcome variables at the neighborhood level, with or without population weights (number of registered voters).[13] There are efficiency gains from including population weights in the neighborhood-level

[11] Some members of the MHP started a new party called the Iyi Party in October 2017. In February 2018, the MHP formed an electoral alliance with the AK Party before the presidential and general elections. Therefore, the MHP is not counted as an opposition party for the 2018 and 2019 elections, which is also verified empirically in the data.

[12] Registered at osf.io/hhqej and retrieved from osf.io/8394u (Baysan, 2017).

[13] The number of registered voters at the ballot-box level does not vary significantly. Therefore, I do not

analysis because of population size-related heteroskedasticity. Including population weights also affects the point estimates because there are heterogeneous treatment effects by population size (Solon et al., 2015). When comparing the ballot box-level to the neighborhood-level analysis with population weights, I find that the results are nearly identical. This is because the number of ballot boxes in a neighborhood is correlated with the number of voters in a neighborhood, although it is a coarser measure of population size relative to the number of registered voters in a neighborhood. I present the results for each empirical strategy in order to illustrate the implications of incorporating population size in the analysis (Solon et al., 2015). Most importantly, though, the results of all the empirical strategies show that the information campaigns increased policy polarization.

4 Results

4.1 Average Treatment Effect versus Heterogeneous Treatment Effects on Vote Share

Panel A of Table 1 shows the results from estimating the ITT using Equation (1). Across quartiles (labeled "Overall"), the combined average treatment effect of the two campaigns on the "No" vote share in the April 2017 referendum is zero. Conducting the analysis by quartile reveals that the zero average effect is masking substantial heterogeneous effects. The "No" vote share decreased by .6 percentage points (1.1%, p-value .764) and 3.4 percentage points (5.3%, p-value .001) in quartiles 1 and 2, respectively. By contrast, the campaigns increased the "No" vote share by 1.2 percentage points (1.7%, p-value .03) and .8 percentage points (1%, p-value .031) in quartiles 3 and 4, respectively. Since the information campaigns simultaneously increased the "No" vote share in quartiles where average support for the referendum was lower and decreased the "No" vote share in quartiles where average support for the referendum was higher, the information campaigns increased policy polarization on average. This increase in policy polarization is illustrated in Figure 4. The results from estimating Equation (1), but using the neighborhood-level measure of the dependent variable, is shown in Table A3. Panel A shows the results when observations are weighted by the number of registered voters in a neighborhood and Panel B shows the results without weights. Overall, analyzing the data at the neighborhood level also provides clear evidence of policy polarization.

I conduct randomization inference exercises for all quartiles in the 2017 referendum to calculate an exact p-value under the sharp null of no treatment effect and without making assumptions on the distribution of errors (Imbens and Rubin, 2015). To implement these randomization inference exercises, I run 10,000 permutations of the treatment on the full sample of neighborhoods to generate a distribution of coefficients and calculate the randomization inference p-values. Using two similar methods, I find that the p-value for quartile 2 is approximately .006 or .005 depending on the program that is used. For quartile 3, I find that the p-value is approximately .058 or .061 depending on the program that is used. The p-value for quartile 4 is .058 or .112 depending on the program that is used. The p-value

weight observations by the number of registered voters at the ballot-box level.

for quartile 1 is high regardless of the program that is used (above .7). These results are summarized in Table A4.[14] The calculated p-values are described as approximate because they are sensitive to the seed used for the randomization, as the authors of both programs attest to. Table A5 shows the estimated average treatment effect across quartiles with and without the pre-specified covariates.

4.2 Persistence

I show the persistent effects of the information campaigns across quartiles and by quartile in 2018 and 2019 in Panels B and C of Table 1. I chose the 2018 presidential election and the 2019 metropolitan mayoral election for these results. The equivalent results for the 2018 general election and the other two 2019 local elections are shown in the Appendix. Table 1 shows that the estimated effects by quartile in 2018 are nearly equivalent in comparison in magnitude and statistical significance to those of 2017. In the 2019 elections, the statistical significance of the effect in the second quartile remains high, but the magnitude diminishes. The magnitude of the estimated effect remains high in the third quartile, but the result is less precise.

Table 2 shows the same set of results as Table 1 except that it shows the estimated average treatment effect of each information campaign separately.[15] The table includes a test of the null hypothesis that the effect of the two information campaigns is the same. I can only reject the null hypothesis in two out of 24 regressions (for each quartile and six elections). The two specifications for which I can reject the null hypothesis are in the second quartile of the 2019 elections for municipal mayors and municipal councilors. As shown in Table 2, the estimated effects are most persistent for the CB campaign. However, overall, the analysis provides evidence that the two information campaigns do not have differential effects on vote share.

4.3 Voter Turnout

Table 3 shows the esimated effect of the information campaigns on voter turnout across quartiles and by quartile in 2017, 2018, and 2019. The average voter turnout for the control group is also shown in the table and ranges from 85%–87% across quartiles in the 2017 and 2018 elections. In 2017 and 2018, the magnitude of the estimated treatment effects by quartile is small. For example, the estimated treatment effect in quartile 2 is .6 to .7 percentage points, which is insufficient to explain the large effect we see on vote share in the 2017 and 2018 elections. In 2019, the estimated treatment effects remain small except for the second quartile.

In Tables A10 and A11, I directly compare the estimated effect of the information campaigns on vote share and voter turnout, using a seemingly unrelated regressions framework.[16] In

[14] I use two STATA commands, `randcmd` (Young, 2019) and `ritest` (Hess, 2017).

[15] I also show the estimated effects by campaign and within each quartile with and without covariates in Table A8.

[16] Figures XX show this test by campaign separately.

these regressions, I calculate the vote share by dividing the number of "No" votes by the number of registered voters instead of by the number of valid votes. Since the number of registered voters is the same variable used in the denominator to calculate voter turnout, I can directly compare the treatment effect of each of the information campaigns on the numerator: the number of valid votes versus the number of "No" votes. When testing the difference between coefficients, I use the absolute value of each coefficient. It can be seen that in the 2017 referendum, voter turnout can explain approximately 23% of the effect on vote share in quartile 2, 7.5% in quartile 3, and 24% in quartile 4. I am able to reject the null of no difference in the coefficients for voter turnout and vote share with a p-value of .04 for quartile 2, .03 for quartile 3, and .04 for quartile 4. I cannot reject the null of no difference in the coefficients for quartile 1. The results are similar in 2018, but the results for the 2019 local election suggest that the entire estimated treatment effect in quartile 2 could be explained by a change in voter turnout. Average voter turnout in the control group is lower in the 2019 local election than in the other elections. Therefore, it is possible that the information campaigns mitigated an overall decrease in voter turnout in quartile 2.

Figure A11 shows the estimated heterogeneous treatment effects on voter turnout for different numbers of quantiles across the 2017 and 2018 elections. Here, I run the regression at the neighborhood level, weight the obervations by the number of registered voters, and estimate the treatment effect with election fixed effects. In each quantile, there is no statistically significant treatment effect on voter turnout and the coefficients are close to zero. This is in contrast to the estimated treatment effects on vote share in different quantiles, as shown in Figure A12.

Overall, the results provide evidence that the effects of the information campaigns on vote share cannot be entirely explained by voter turnout. The average treatment effects across quartiles and by quartile are precisely estimated and they are small. It is possible that there are differential effects within each quartile and I discuss this possibility in further detail in Section 5.

4.4 Persuasion Rates

I calculate the persuasion rate of the information campaigns by quartile and under the following conservative assumptions: i) all individuals in a neighborhood assigned to the treatment group were exposed to the information campaigns and ii) the treatment response is monotonic within each quartile. The first assumption is conservative because in 20% of the neighborhoods assigned to the treatment group, canvassers received threats and could not complete the campaigns. In addition, they only completed conversations with voters in a fraction of households in the treatment neighborhoods. However, I use the first assumption because I do not have information on how much of the canvassing they were able to complete in threatening neighborhoods. More generally, I cannot isolate the role of in-person conversations, pamphlets, or the presence of the canvassers in a neighborhood in explaining the estimated treatment effects on vote share. Jun and Lee (2021) labels calculating the persuasion rates under the first assumption as being consistent with a "sharp persuasion design." The authors refer to the second assumption as the "monotonic treatment response"

assumption. While this is a natural assumption because it is consistent with the objective of persuasive efforts, it is an unrealistic assumption in this context and so the calculated persuasion rates are underestimates.

To calculate the persuasion (and dissuasion) rates under a sharp persuasion design, I divide the ITT estimate from Table 1 for each quartile by the share of voters who would take the (unintended) action of interest, voting "Yes" (quartiles 1 and 2) or "No," (quartlies 3 and 4) without having been exposed to the persuasive message. Therefore, the point estimates are divided by the average "No" vote share of the control group in quartiles 1 and 2 and by the average "Yes" vote share of the control group in quartiles 3 and 4. The resulting persuasion rates are 1.1%, 5.3%, 4.3%, and 4.6% for quartiles 1, 2, 3, and 4, respectively. Recall that the point estimate for quartile 1 is highly noisy and so the corresponding persuasion rate is also imprecise.

The persuasion rates of the door-to-door information campaigns are higher in this study than in studies where the authors estimate the impact of information campaigns on vote share in liberal democracies. In the meta-analysis of 49 campaign field experiments in the U.S., the persuasion rate is zero (Kalla and Broockman, 2018). A meaningful comparison to other experimental door-to-door campaign experiments is not possible since I can only compare θ_L, an overly stringent measure. However, to provide an example, the equivalent of θ_L for the door-to-door information campaign experiment analyzed by Pons (2018) is approximately 1.2%.[17]

5 Potential Mechanism

In the following discussion, while non-exhaustive and limited to theories based on Bayesian updating, I outline a few potential mechanisms to explain the results. I predominantly focus on marginal voters who switched their intended vote in response to the information campaigns ("switchers"). In the discussion, I argue that the relative share of different voters matters in terms of geographic "ideological segregation," where voters are ideologically aligned with their neighbors (social network) in their policy views (Gentzkow and Shapiro, 2011). This helps explain why the quartiles predict whether the net effect of the information campaigns on the "No" vote share is positive or negative. Regarding polarization, I primarily focus on "ideological polarization," in which individuals disagree more about a policy after being exposed to the same information (see, e.g., Lord et al., 1979; Benoit and Dubra, 2016).

Information in the PO campaign: Consider two groups of voters who choose their vote in the referendum depending on their party allegiance and beliefs about whether weakening constraints on the executive branch is a good policy. The group with high beliefs that it is a good policy are those who think that external threats to national security are more important than incumbent quality. The other group with low beliefs think that incumbent quality is more important. These different views along identity lines (e.g., party affiliation

[17] I derive this persuasion rate by dividing the ITT point estimate of a precinct being randomly allocated to canvassers by the fraction of voters who would have supported candidates other than François Hollande.

is determined by Islamists versus secularists) can arise from selective exposure to signals.[18] Consider that information about the state of the economy is initially imperfect and everyone agrees that the economy is determined by external threats and incumbent quality. The intervention informs everyone that the economy is doing poorly, but incumbent voters will blame outside forces and to desire more power for the incumbent; and opposition voters will blame the incumbent and increase support for retaining constraints on the incumbent's power.[19] In both groups, voters switch from their intended vote and cross party lines. Figure 1b shows these potential "switchers," i.e., voters who self-report as supporters of the incumbent (opposition) party, but whose ideological views are aligned with the average voter supporting the opposition (incumbent) party (see Figure 1b for these switchers). With this new information, ideological polarization increases and voters switch their vote in the referendum accordingly and vote more like their neighbors, leading to ideological polarization on whether a stronger state is a good policy. In subsequent elections, voters also switch their affiliation to the party closest to their policy position and again vote more like their neighbors, leading to partisan polarization. The results that I obtain using ballot box-level vote share can be mapped to this individual-level behavior if there is pre-existing geographic ideological segregation regarding the policy. So, switchers who typically vote for the opposition, but have views that are more similar to incumbent voters, live in neighborhoods where there are relatively more incumbent voters. Geographic ideological segregation is a natural assumption because voters update their views based on experiences or feedback they receive directly, e.g., dominant views discussed in parks or the arrest of a neighbor (Gentzkow et al., 2021). During a period in which civil liberties are low, it may also be safer to adopt the views of the local majority.[20]

Information in the CB campaign: Like the PO campaign, the CB campaign also increased ideological polarization. In this case, however, voters received a more precise signal on how the referendum would change the level of constraints on the executive branch or learned more about the opposition party's position on the referendum. As in the case of the PO campaign, the two groups of voters respond differently depending on whether they have high or low beliefs on whether weakening constraints on the executive branch is a good policy. After receiving a more precise signal, voters either put more weight on the policy dimension of their utility function relative to the consumption value they get from shared identity, or change their affiliation to the party that they learn is closest to their policy position.

Information source: Voters may have reacted to the fact that the information was coming from a biased source. If voters are rational, they will filter out the bias and not be persuaded

[18]Similar frameworks are discussed by Acemoglu et al. (2016), Andreoni and Mylovanov (2012), Benoit and Dubra (2016), Loh and Phelan (2019), among others.

[19]Figure 1a shows that incumbent or opposition supporters agree that a depreciation of the lira (a poor economic outcome) had a negative impact on their life. The bottom panel shows that voters have partisan views on why the value of the lira dropped. Opposition voters predominantly blamed the current leadership. Incumbent voters blamed external factors beyond the control of the leadership, such as the coup attempt, the global economic crisis, and the U.S. presidential election.

[20]Ideological segregation among social networks may be more severe in weak democracies in which there is state repression of politically excluded groups.

by the information (Bray and Kreps, 1987; DellaVigna and Kaplan, 2007). While this study rules out such an explanation, it is possible that voters reacted to the slant of the information and then updated their belief on the quality of the information source (the opposition party) (Gentzkow and Shapiro, 2006). This mechanism is similar to updating on the information itself; however, rather than become more polarized in their policy views, voters switch to the party that they now learn is aligned with their priors and policy preferences.

Peer effects and turnout: Alternative mechanisms that can explain the results include peer pressure or mobilization. For example, suppose that all voters underestimated the quality of the opposition party or the likelihood that they would win. The information campaigns would have caused voters to revise this view positively. Consequently, supporters of the opposition party would have mobilized in quartiles 3 and 4 and supporters of the incumbent party would have mobilized in quartiles 1 and 2. However, I find that the information campaigns have a zero average effect on voter turnout by quartile (or octile, as shown in Figure A11). Therefore, if the effects on vote share are explained by voter turnout then there must be differential mobilization within quartiles. This would require opposition voters to have mobilized in quartiles 3 and 4, incumbent voters to have demobilized in the same quartiles, and that the reverse occur in quartiles 1 and 2. A potential explanation for this mechanism is peer effects. For example, the information campaigns may have caused voters in quartiles 3 and 4 who were going to vote for the opposition party regardless to be more likely to pressure their neighbors. Under this interpretation, we might expect higher voter turnout effects in quartile 4 relative to quartile 3, for example, but this is not the case. In addition, these mechanisms would require that reactions to the perceived strength of the opposition persist for more than a year in regular elections and in an opposition stronghold where voters know that they are not pivotal. For example, in the 2015 and 2018 general elections, fourteen candidates from the main opposition party were elected into parliament in comparison to eight from the incumbent party.

6 Conclusion

Access to non-state-provisioned information is considered to be important for countering electorally motivated state censorship and the consolidation of power under an incumbent. This study uses experimental field evidence to show that exposure to non-state-provisioned information can instead be polarizing. I use a stratified randomization design to directly test for a polarized electorate in the context of a constitutional referendum where voters were choosing whether to weak constraints on the executive. The average effect is zero, but this outcome masks both a significant increase and decrease in voter support for the proposed institutional policy reform. Moreover, the polarizing effects persist in regular elections 1 and 2 years later, resulting in partisan polarization. These results are in contrast to many empirical studies examining whether trends of increasing ideological polarization are driven by voters self-selecting into different information sources. Instead, the results of my study provide evidence that exposure to the same information campaigns increases ideological polarization.

Further research is needed on whether the results are driven by voters facing an identification

problem: due to the salience of external threats, voters are not certain why economic conditions are bad. As in, voters have different views on whether external threats or the incumbent is most important in explaining poor economic conditions. These views are then correlated with disagreement on whether weakening constraints on the executive and strengthening the state is a good policy. Voters who blame external threats prefer a stronger state so that the incumbent is better able to counter external threats. Therefore, the opposition party's efforts to circumvent state censorship and directly deliver information on the proposed policy and poor economic conditions under the incumbent can backfire and increase support for a stronger state under the incumbent.

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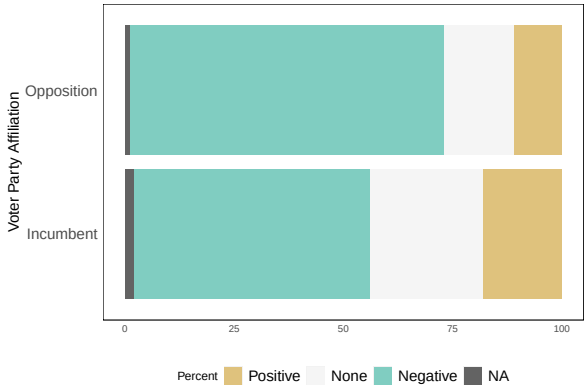
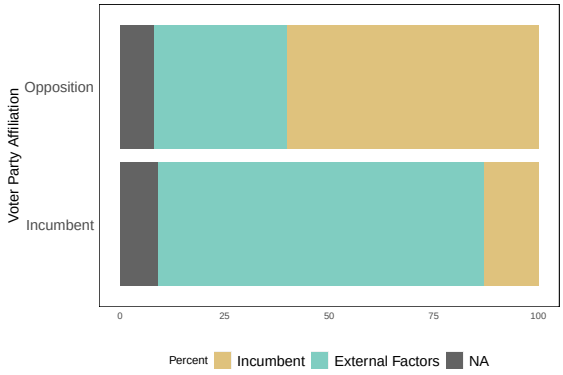
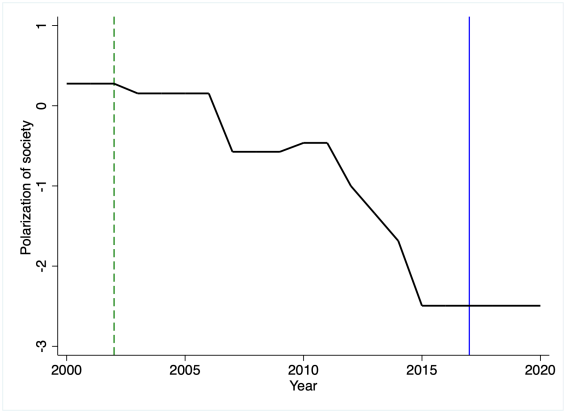
Figure 1: Voter Perception and Polarization



(a) 2016 Survey evidence: Does the drop in the value of Turkish Lira have any impact on your personal life?



(b) 2016 Survey evidence: Who is most responsible for the latest devaluation of Turkish Lira?



(c) VDem: Polarization of Society

Panels a and b: The data for these figures are from a survey conducted by a U.S. based firm. The name of the firm cannot be disclosed. The sample is nationally representative and includes 1,215 voters. The survey was conducted between December 25th, 2016 through January 11, 2017, which is after the coup attempt and before the April 2017 Referendum. The survey also took place after a record devaluation of the local currency in Turkey. Opposition refers to citizens who self-reported support for the main opposition party. Incumbent refers to citizens who self-reported support for the incumbent party. External factors include the coup attempt, global economic crisis, and U.S. elections.

Panel c: This figure shows a time series plot of an indicator relating to polarization from the V-Dem database (Coppedge et al., 2021). Polarization is defined as differences of opinions on major political issues in society. This indicators has only been made available since 2000. The green vertical line indicates the year that the AKP came into power and the blue vertical line indicates the year of the referendum.

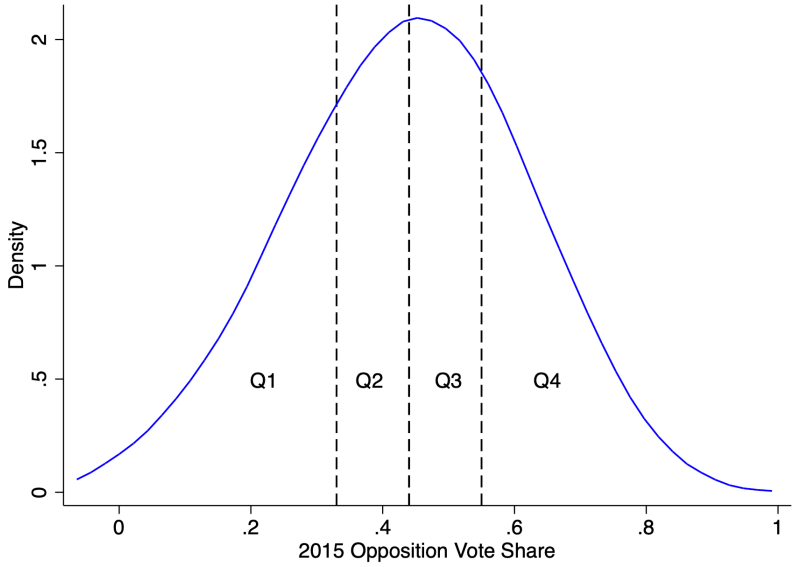
Figure 2: Pamphlets

(a) Policy Outcomes

(b) Checks and Balances

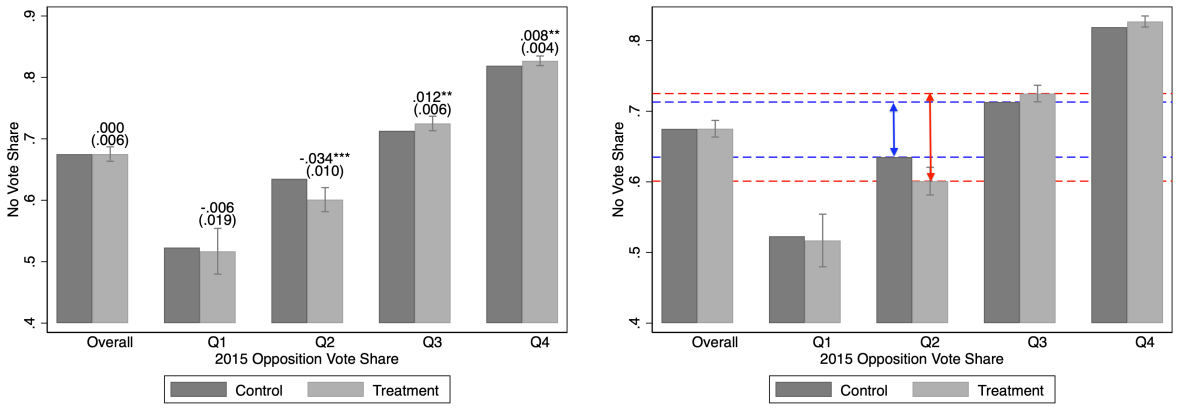
These are the copies of the original pamphlets that were used in the information campaigns. I have pasted English translations over the original Turkish text. The graphic in Figure 2b says "For my future, No (Hayir)."

Figure 3: Distribution of Stratifying Variable: 2015 Opposition Vote Share



This figure shows the distribution of the stratifying variable: the average neighborhood-level vote share for the opposition party in 2015. The average vote share was calculated using two general elections that were held in 2015. The vertical lines indicate the quartiles of vote share. Randomization was stratified by quartile. The distribution is weighted by the number of registered voters in a neighborhood.

Figure 4:
Overall and Heterogeneous Average Treatment Effect of Both Door-to-Door Campaigns on 2017 Referendum "No" Vote Share



This figure displays the ITT estimated effects that are also shown in Tables **??** (column 1) and 1. The y-axis is the "No" vote share and the x-axis shows each quartile of the stratifying varible: the 2015 vote share for the opposition. The figure on the right shows the polarization result in terms of an increase in the average vote share difference for the opposition between the control and treatment groups in the second and third quartile.

Table 1:
Average Treatment Effect on Vote Share Overall and Across Elections

Panel A	Referendum 2017 "No"				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.000	-0.006	-0.034***	0.012**	0.008**
	(0.006)	(0.019)	(0.010)	(0.006)	(0.004)
Mean	.675	.523	.635	.713	.819
N Ballot	3992	919	983	1058	1032
R squared	.785	.279	.416	.409	.664
Panel B	Presidential 2018				
	Overall	Q1	Q2	Q3	Q4
Treatment	-0.000	-0.014	-0.034***	0.017**	0.007*
	(0.007)	(0.018)	(0.012)	(0.007)	(0.004)
Mean	.658	.51	.612	.693	.809
N Ballot	4406	1015	1093	1160	1138
R squared	.766	.281	.441	.43	.626
Panel C	Metropolitan Mayor 2019				
	Overall	Q1	Q2	Q3	Q4
Treatment	-0.000	-0.008	-0.028***	0.014	0.006
	(0.007)	(0.019)	(0.011)	(0.009)	(0.006)
Mean	.602	.459	.555	.626	.759
N Ballot	4793	1096	1191	1274	1232
R squared	.751	.199	.325	.321	.666
N Nbhd	550	138	137	138	137

All dependent variables are the ballot box level. The dependent variable for Panel A is 2017 "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. I show the estimated treatment effect across all strata and within each strata. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 2:
Average Treatment Effects on Vote Share by Quartile and Campaign for the 2017 Referendum and 2018 Elections

Panel A	Referendum 2017			
	Q1	Q2	Q3	Q4
Policy Outcomes	0.013	-0.038***	0.017**	0.004
	(0.028)	(0.011)	(0.007)	(0.004)
Checks & Balances	-0.024	-0.030**	0.007	0.013**
	(0.028)	(0.014)	(0.007)	(0.006)
Mean	.523	.635	.713	.819
N Ballot	919	983	1058	1032
R squared	.284	.416	.41	.665
PO=CB p-value	.325	.596	.246	.153
Panel B	Presidential 2018			
	Q1	Q2	Q3	Q4
Policy Outcomes	0.005	-0.037***	0.019**	0.006
	(0.028)	(0.013)	(0.009)	(0.004)
Checks & Balances	-0.033	-0.031*	0.014*	0.008
	(0.025)	(0.017)	(0.008)	(0.006)
Mean	.51	.612	.693	.809
N Ballot	1015	1093	1160	1138
R squared	.286	.441	.43	.626
PO=CB p-value	.298	.743	.685	.701
Panel C	Metropolitan Mayor 2019			
	Q1	Q2	Q3	Q4
Policy Outcomes	-0.006	-0.016	0.013	0.000
	(0.027)	(0.010)	(0.013)	(0.007)
Checks & Balances	-0.011	-0.041***	0.015*	0.012
	(0.024)	(0.016)	(0.008)	(0.008)
Mean	.459	.555	.626	.759
PO=CB p-value	.894	.130	.863	.240
N Ballot	1096	1191	1274	1232
R squared	.199	.329	.321	.667
N Nbhd	138	137	138	137

All dependent variables are the ballot box level. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. Each column shows the estimation result within each strata. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

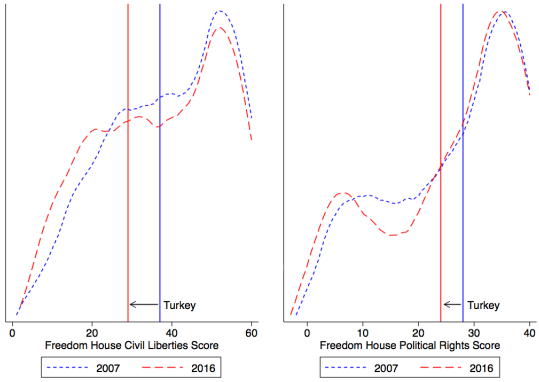
Table 3:
Treatment Effects on Voter Turnout by Quartile and Election

	Referendum 2017 Turnout				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.002	-0.000	0.006***	0.001	0.002
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)
Outcome Mean	.872	.857	.87	.879	.882
N Ballot	3992	919	983	1058	1032
R squared	.401	.403	.385	.301	.361
	Presidential 2018 Turnout				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.001	0.003	0.007*	-0.004	0.001
	(0.001)	(0.004)	(0.004)	(0.003)	(0.002)
Outcome Mean	.87	.853	.865	.879	.882
N Ballot	4406	1015	1093	1160	1138
R squared	.318	.334	.305	.199	.243
	Metropolitan Mayor 2019				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.002	0.001	0.016**	0.004	-0.008**
	(0.003)	(0.006)	(0.007)	(0.005)	(0.003)
Outcome Mean	.815	.81	.816	.823	.811
N Ballot	4793	1096	1191	1274	1232
R squared	.357	.383	.384	.343	.338
N Nbhd	550	138	137	138	137

The dependent variable in each column is voter turnout at the ballot box level. Each column shows the estimation result within each strata. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

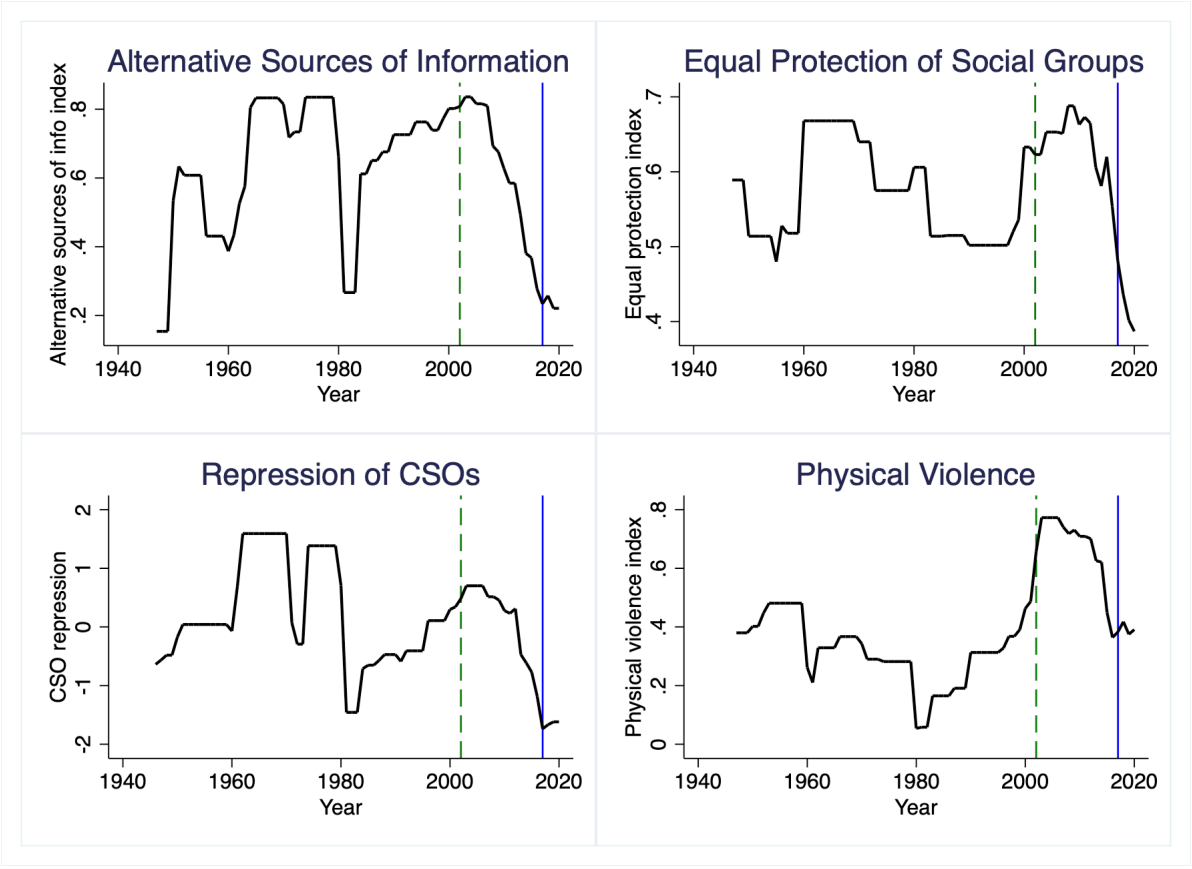
A For Online Publication: Appendix

Figure A1: Distribution of Civil Liberties and Political Rights Scores Across Countries



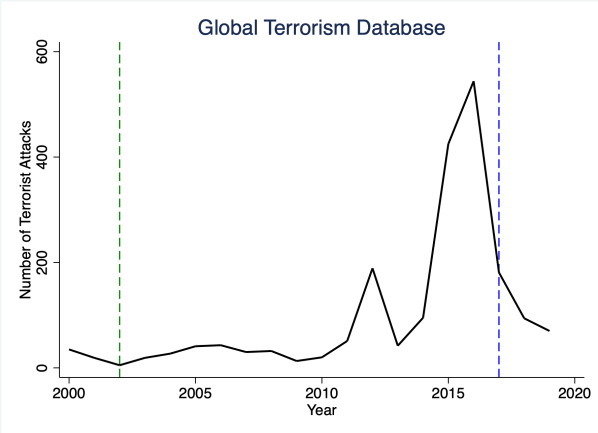
This figure shows the distribution of aggregate scores for civil liberties and political rights in 2007 and 2016 across countries. 193 countries are included in 2007 and 198 in 2016. The figure also shows Turkey's position within the distribution. There was a decline in both measures across all countries and particularly in Turkey between 2007 and 2016.
Source: Freedom House (2017)

Figure A2: Polarization and State Repression



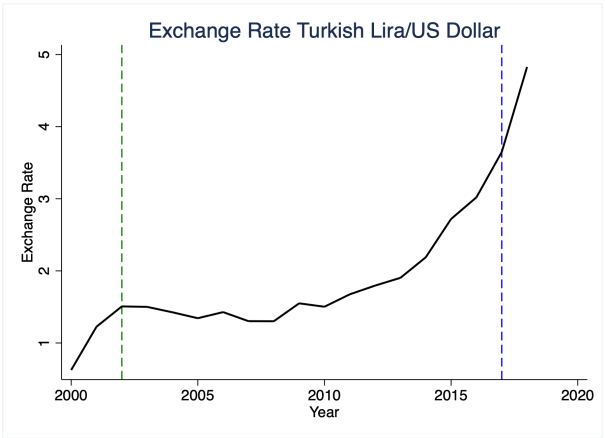
These figures show time series plots of indicators relating to media cenosrship and state repression from the V-Dem database. Access to alternative information is defined as the extent to which the media is (a) un-biased in their coverage (or lack of coverage) of the opposition, (b) allowed to be critical of the regime, and (c) representative of a wide array of political perspectives. Equal protection of social groups is defined as the protection of rights and freedoms across social groups by the state. Repression of CSOs is defined as the degree to which the government attempts to repress civil society organizations. Physical violence index is defined as the degree to which physical integrity respected, where physical integrity is the freedom from political killings and torture by the government. I restrict the time series to after 1946 because this is the year when Turkey transitioned to a multi-party democracy. In each figure, the dashed vertical green line indicates the year that the AK Party came into power and the solid blue vertical line indicates the year of the constitutional referendum. For all variables, lower numbers indicate worse outcomes. To provide a concrete example, according to a report by the Committee to Protect Journalists (CPJ), the highest number of jailed journalists across all countries ever recorded since the CPJ starting tracking such incidents in 1992 was between 2015 and 2018 (Beiser, 2018). For those three years, Turkey was the leading jailer in the world in absolute numbers. According to a report by the Media Ownership Monitor, 7 out of 10 news portals and 9 out of 10 of the most watched television channels belonged to owners that were affiliated to the government (Media Ownership Monitor, 2019).
Source: V-Dem (Coppedge et al., 2021)

Figure A3: National Security and the Economy



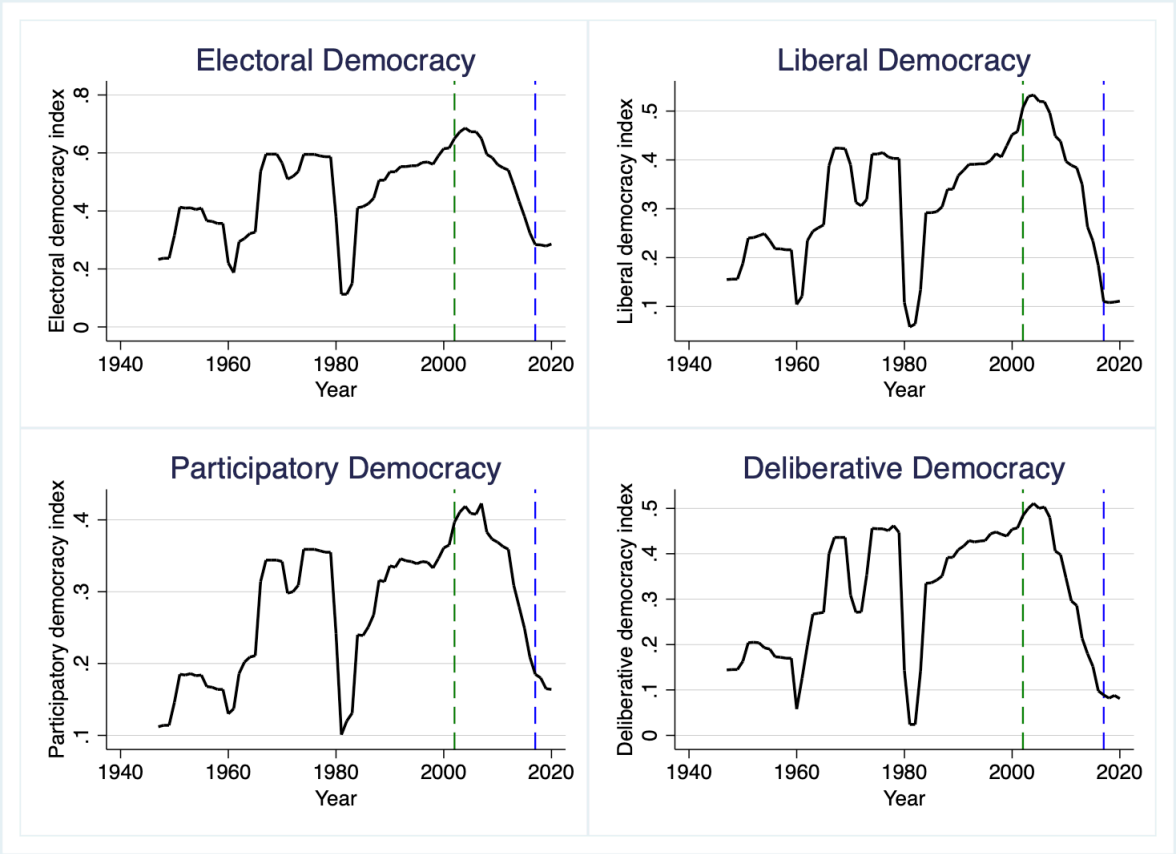
(a) Terrorist Attacks in Turkey

This figure shows the number of terrorist attacks in Turkey using data from the Global Terrorism Database (GTD). According to the GTD, a terrorist attack is defined as the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation.



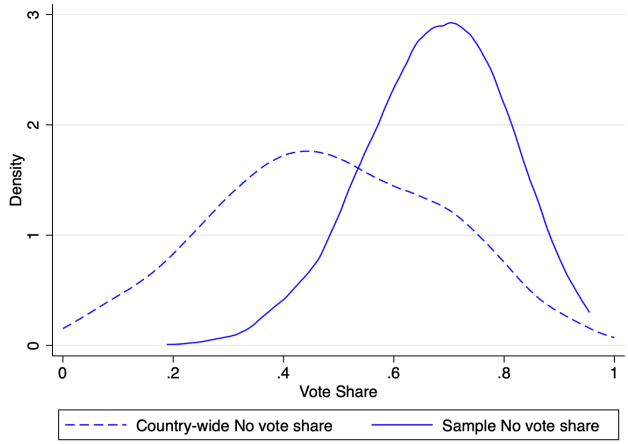
(b) OECD Data Exchange Rate

Figure A4: Features of Democracy



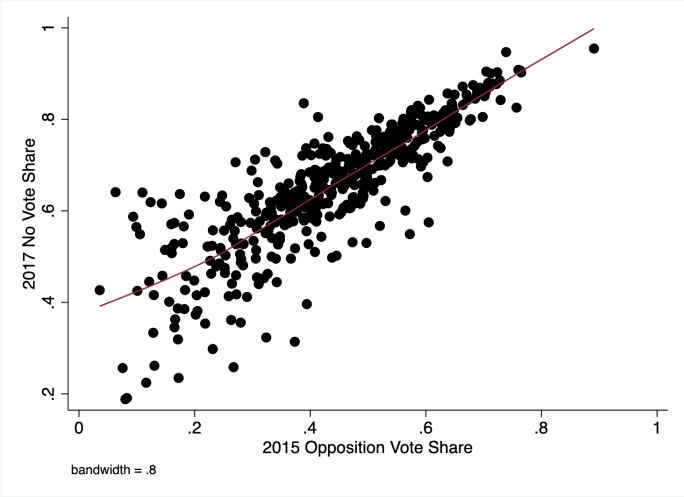
This figure shows a time series plot of macro-level indices that describe features of democracy at the highest level from the V-Dem database. The green vertical line indicates the year that the AKP came into power and the blue vertical line indicates the year of the referendum.
Source: V-Dem (Coppedge et al., 2021)

Figure A5: "No" Vote Share Distribution Across Country and Sample



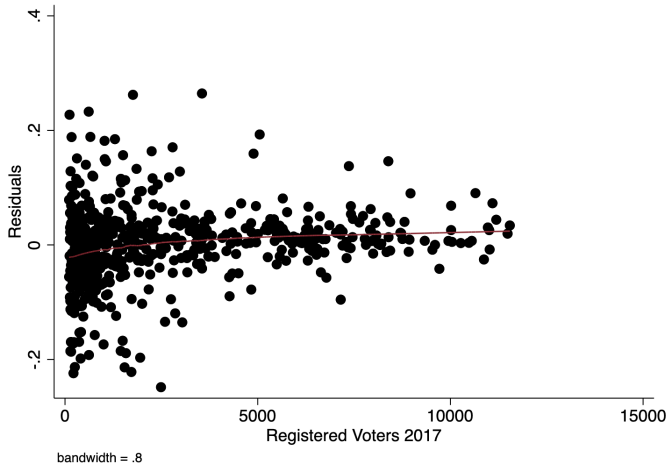
This figure shows the distribution of the neighborhood-level "No" vote share for Turkey in blue and for the experimental sample among the control group. The distributions are weighted by the number of registered voters in a neighborhood.

Figure A6: Correlation Between 2015 and 2017 Vote Shares



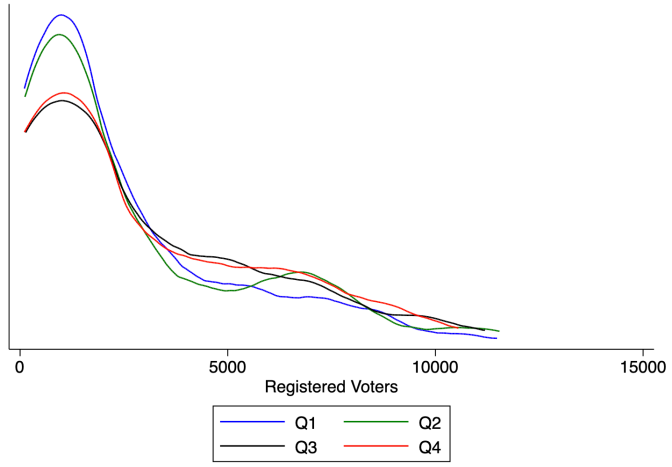
This figure shows the correlation between the vote share for the opposition party from the 2015 general elections and the "No" vote share in the control group of the experimental sample.

Figure A7: Residuals vs. Neighborhood Size



This figure shows the residuals from regressing the "No" vote share on the November 2015 vote share for the opposition party plotted against neighborhood size. The bottom figure shows the distribution of registered voters in a neighborhood for each quartile.

Figure A8: Distribution of Neighborhood Size



This figure shows the distribution of registered voters in a neighborhood for each quartile.

Figure A9: Map of Sample Within Turkey and with Province Borders

Figure A10: Map of Sample Within Izmir and with District Borders

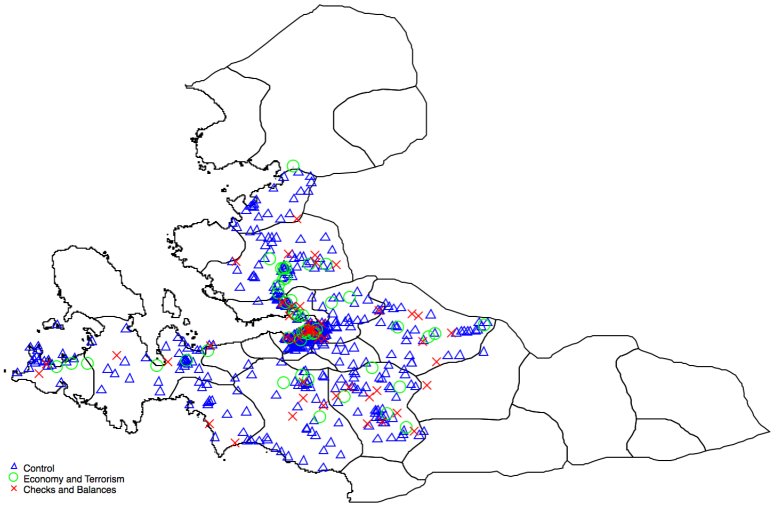
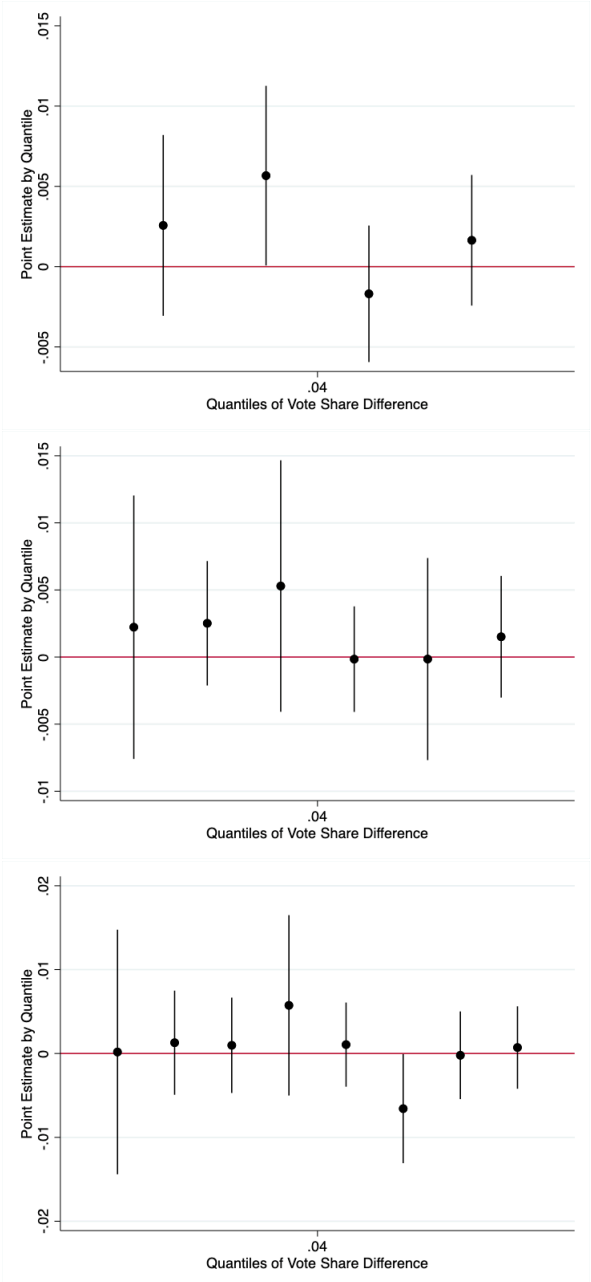


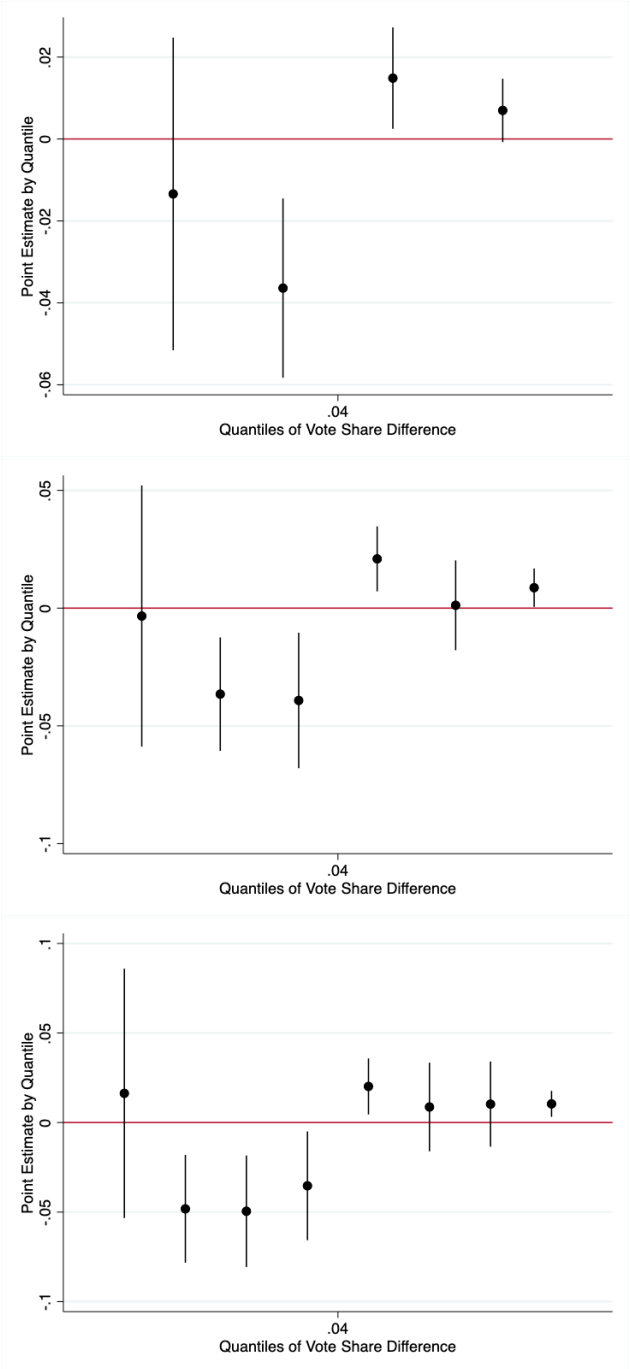
Figure A9 shows the location of the neighborhoods in the experimental sample within Turkey. Figure A10 shows the location of neighborhoods in each treatment group within the province of Izmir.

Figure A11:
Treatment Effects on Voter Turnout by Quantile Across the Distribution



These figures show the estimation results for different numbers of quantiles of the stratifying variable (the average vote share for the main opposition party in the 2015 elections). The dependent variable is at the neighborhood level and observations are weighted by the number of registered voters in a neighborhood. The outcome variable for each election is voter turnout. Election fixed effects and pre-specified control variables are included in all regressions. Standard errors are clustered at the neighborhood level.

Figure A12: Treatment Effects on Vote Share by Quantile Across the Distribution



These figures show the estimation results for different numbers of quantiles of the stratifying variable (the average vote share for the main opposition party in the 2015 elections). The dependent variable is at the neighborhood level and observations are weighted by the number of registered voters in a neighborhood. The outcome variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdoğan. In the 2018 general election, the outcome variable is the vote share for the opposition parties. Election fixed effects and pre-specified control variables are included in all regressions. Standard errors are clustered at the neighborhood level.

Table A1: Number of Neighborhoods Reached and Share of Voters Conversation Completed (Weighted)

	All		Not Threatened	
Quartiles	**Mean**	**N**	**Mean**	**N**
1	0.08	25	0.10	21
2	0.10	25	0.10	20
3	0.09	25	0.10	20
4	0.06	25	0.07	19
Total		100		80

Quartiles refers to the four quantiles of the variable used for stratification (the average vote share for the main opposition party in the 2015 elections). Column 1 shows the average share of registered individual voters who opened their doors and completed a conversation with the canvassers (conversation completion rate) in neighborhoods assigned to the treatment group. Column 2 shows the total number of neighborhoods assigned to the treatment group. Column 3 also shows the mean conversation completion rate, but excludes neighborhoods where the party volunteers faced threat and aggression and in which the canvassers did not share information on the number of voters they completed a conversatino with. Column 4 shows the number of neighborhoods where canvassers did not face threat and aggression. Estimates are weighted by the number of registered voters in a neighborhood. In a previous version of this paper, I reported unweighted averages and a higher conversation completion rate in the fourth quartile. This was because the conversation completion rate was above 100% in one of the neighborhoods and I had capped it at 100%. I now replace the conversation completion rate as "missing," but include the neighborhood in columns 2 and 4.

Table A2: Balance on Pre-Specified Variables

	Aggregate		
	Control Mean	Coefficient	Standard Error
Reg Voters Nov	2593.430	-37.853	547.182
Valid Casts Nov	2250.080	-29.280	473.658
Opp Votes June	1020.180	38.123	220.028
Opp Votes Nov	1064.650	31.146	235.802
Opp Share June	0.430	0.000	0.008
Opp Share Nov	0.430	-0.003	0.009
Turnout Nov	0.871	0.001	0.003
N	550		

Balance test across the treatment and control groups on all pre-specified variables. These variables are measured at the neighborhood level, which is the level of randomization. Balance is tested across the whole sample. Strata fixed effects are included and observations are weighted by the number of registered voters. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A3:
Average Treatment Effect on Neighborhood Level Vote Share by Quartile: 2017 Referendum

Panel A	Referendum 2017: Weighted			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	-0.008	-0.035***	0.013**	0.008*
	(0.019)	(0.011)	(0.006)	(0.004)
Mean	.526	.635	.714	.82
N	138	137	138	137
R squared	.358	.625	.679	.842
Panel B	Referendum 2017: Unweighted			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	0.011	-0.027**	0.020**	0.013**
	(0.021)	(0.012)	(0.009)	(0.007)
Mean	.496	.628	.694	.798
N	138	137	138	137
R squared	.402	.398	.489	.682

The dependent variable in each column is the "No" vote share at the neighborhood level. Each column shows the estimation result within each strata. Pre-specified controls are included at the neighborhood level, which is the level of randomization. In Panel A, observations are weighted by the number of registered voters in a neighborhood. In Panel B, observations are not weighted. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A4: Randomization Inference Based P-values

	Table 2 p-values (ballot)			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
p-values	.764	.001	.031	.042
RI p-values (Young)	.782	.006	.058	.058
RI p-values (Hess)	.752	.005	.061	.112

This table shows p-values calculated with and without using randomization inference exercises. The calculations using randomization inference are under the sharp null of no treatment effect and without making assumptions on the distribution of errors. To implement these randomization inference exercizes, I run 10,000 permutations of the treatment on the full sample of neighborhoods within each quartile to generate a distribution of coefficients and calculate the p-values. I run two programs to calculate randomization inference based p-values using STATA: `randcmd` (Young, 2019) and `ritest` (Hess, 2017). For `randcmd` (Young, 2019), I report the p-values calculated using the "randomization-t based" statistic.

Table A5:
Average Treatment Effect on Vote Share and Voter Turnout: 2017 Referendum With and Without Covariates

	No Vote Share		Voter Turnout	
	(1)	(2)	(3)	(4)
Treatment	-0.0063	0.0002	0.0025	0.0017
	(0.0090)	(0.0062)	(0.0032)	(0.0011)
Mean	.675	.675	.872	.872
N Ballot	3992	3992	3992	3992
N Nbhd	550	550	550	550
R squared	.673	.785	.0688	.401
Controls	No	Yes	No	Yes

The dependent variables are at the ballot box level. The dependent variable in columns 1 and 2 is the "No" vote share. The dependent variable in columns 3 and 4 is voter turnout. Pre-specified controls are included at the neighborhood level, which is the level of randomization, in columns 2 and 4. Standard errors are clustered at the neighborhood level. Strata fixed effects are included in all specifications. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A6: Balance on Pre-Specified Variables by Campaign and Quartiles 1 and 2

PO Campaign	Q1			Q2		
	Control Mean	Coef	SE	Control Mean	Coef	SE
Reg Voters Nov	2593.430	604.328	2434.540	2593.430	-532.754	903.217
Valid Casts Nov	2250.080	516.021	2107.968	2250.080	-504.082	769.782
Opp Votes June	1020.180	278.290	563.432	1020.180	-197.514	281.252
Opp Votes Nov	1064.650	290.866	608.838	1064.650	-266.754	267.848
Opp Share June	0.430	0.027*	0.015	0.430	0.007	0.018
Opp Share Nov	0.430	0.024	0.018	0.430	-0.003	0.018
Turnout Nov	0.871	-0.006	0.007	0.871	-0.004	0.012
CB Campaign	Q1			Q2		
	Control Mean	Coef	SE	Control Mean	Coef	SE
Reg Voters Nov	2593.430	-1609.801	1076.551	2593.430	1040.344	1897.910
Valid Casts Nov	2250.080	-1387.595	952.384	2250.080	773.720	1602.391
Opp Votes June	1020.180	-362.629	279.670	1020.180	369.106	668.856
Opp Votes Nov	1064.650	-368.183	324.895	1064.650	396.283	739.312
Opp Share June	0.430	-0.031	0.031	0.430	0.004	0.013
Opp Share Nov	0.430	-0.035	0.033	0.430	-0.000	0.018
Turnout Nov	0.871	-0.004	0.012	0.871	-0.015***	0.006

Balance test across the treatment and control groups across all pre-specified variables. These variables are measured at the neighborhood level, which is the level of randomization. Balance is tested by strata (quartile of the average vote share for the main opposition party in the 2015 elections). Observations are weighted by the number of registered voters in a neighborhood. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A7: Balance on Pre-Specified Variables by Campaign and Quartiles 3 and 4

PO Campaign	Q3			Q4		
	Control Mean	Coef	SE	Control Mean	Coef	SE
Reg Voters Nov	2593.430	449.155	1440.571	2593.430	610.983	1323.919
Valid Casts Nov	2250.080	441.799	1287.778	2250.080	566.586	1150.964
Opp Votes June	1020.180	120.587	542.656	1020.180	507.839	817.930
Opp Votes Nov	1064.650	108.614	579.450	1064.650	527.718	866.490
Opp Share June	0.430	-0.007	0.013	0.430	0.020	0.016
Opp Share Nov	0.430	-0.012	0.012	0.430	0.020	0.019
Turnout Nov	0.871	0.007	0.008	0.871	0.009	0.006
CB Campaign	Q3			Q4		
	Control Mean	Coef	SE	Control Mean	Coef	SE
Reg Voters Nov	2593.430	-1200.008	1256.877	2593.430	23.680	573.495
Valid Casts Nov	2250.080	-1077.271	1070.367	2250.080	109.233	522.961
Opp Votes June	1020.180	-582.050	459.287	1020.180	12.402	333.531
Opp Votes Nov	1064.650	-646.357	480.524	1064.650	39.103	367.210
Opp Share June	0.430	-0.016	0.011	0.430	-0.003	0.026
Opp Share Nov	0.430	-0.024**	0.012	0.430	0.005	0.031
Turnout Nov	0.871	-0.003	0.005	0.871	0.015	0.010

Balance test across the treatment and control groups across all pre-specified variables. These variables are measured at the neighborhood level, which is the level of randomization. Balance is tested by strata (quartile of the average vote share for the main opposition party in the 2015 elections). Observations are weighted by the number of registered voters in a neighborhood. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A8: Treatment Effect on Vote Share by Quartile and Campaign With and Without Controls

	Q1		Q2		Q3		Q4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Policy Outcomes	0.027	0.013	-0.057*	-0.038***	0.006	0.017**	0.016	0.004
	(0.022)	(0.028)	(0.030)	(0.011)	(0.013)	(0.007)	(0.015)	(0.004)
Checks & Balances	-0.036	-0.024	-0.025	-0.030**	-0.024**	0.007	0.026	0.013**
	(0.035)	(0.028)	(0.032)	(0.014)	(0.011)	(0.007)	(0.022)	(0.006)
Mean	.523	.523	.635	.635	.713	.713	.819	.819
N Ballot	919	919	983	983	1058	1058	1032	1032
N Nbhd	138	138	137	137	138	138	137	137
R squared	.0158	.284	.0398	.416	.015	.41	.0187	.665
Controls	No	Yes	No	Yes	No	Yes	No	Yes

The dependent variable is the "No" vote share and is observed at the ballot box-level. In columns 2, 4, 6 and 8 pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A9:
Treatment Effects on Voter Turnout by Quartile and Campaign in the 2019 Local Elections

Panel A	Municipal Mayor 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Policy Outcomes	0.001	0.007	-0.000	-0.015***
	(0.007)	(0.007)	(0.008)	(0.004)
Checks & Balances	-0.001	0.024**	0.009	-0.001
	(0.010)	(0.011)	(0.006)	(0.004)
Mean	.808	.814	.821	.809
R squared	.373	.397	.34	.335
PO=CB p-value	.89	.208	.335	.00256
Panel B	Municipal Councillors 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Policy Outcomes	-0.001	0.008	0.001	-0.014***
	(0.006)	(0.007)	(0.007)	(0.004)
Checks & Balances	-0.002	0.022*	0.008	0.001
	(0.009)	(0.011)	(0.006)	(0.004)
Mean	.804	.81	.819	.807
R squared	.367	.376	.336	.33
PO=CB p-value	.901	.285	.42	.00261
N Ballot	1096	1191	1274	1232
N Nbhd	138	137	138	137

The dependent variable in each column is voter turnout at the ballot box level. Each column shows the estimation result within each strata. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A10: Vote share versus Voter turnout

	Referendum 2017			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
T (1)	-0.000	0.006***	0.001	0.002
Turnout	(0.003)	(0.002)	(0.002)	(0.002)
T (2)	-0.005	-0.026***	0.012**	0.009**
Vote share	(0.016)	(0.009)	(0.006)	(0.004)
Ratio (1/2)	.0264	.225	.0751	.24
p-value	.785	.0434	.0274	.0395
F-stat	.0749	4.16	4.97	4.32
N Ballot	919	983	1058	1032
	Presidential 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
T (1)	0.003	0.007*	-0.004	0.001
Turnout	(0.004)	(0.004)	(0.003)	(0.002)
T (2)	-0.011	-0.026**	0.012*	0.007
Vote share	(0.015)	(0.010)	(0.006)	(0.004)
Ratio (1/2)	.269	.251	.318	.107
p-value	.61	.109	.286	.063
F-stat	.262	2.61	1.14	3.51
N Ballot	1015	1093	1160	1138
	General 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
T (1)	0.004	0.007**	-0.003	0.002
Turnout	(0.003)	(0.003)	(0.003)	(0.002)
T (2)	-0.012	-0.028***	0.012*	0.008*
Vote share	(0.015)	(0.010)	(0.006)	(0.004)
Ratio (1/2)	.343	.243	.208	.251
p-value	.625	.0532	.220	.0939
F-stat	.239	3.8	1.52	2.85
N Ballot	1015	1093	1160	1138

This table compares the effect of the campaign on voter turnout and a different measure of vote share. The denominator for vote share is now the number of registered voters instead of the number of valid votes. The two effects are estimated using a seemingly unrelated regressions framework. The table also includes the ratio between the effects on turnout and on vote share. The results for p-value and F-statistic are from a test of the null hypothesis that the two effects are equal. All dependent variables are the ballot box level. The outcome variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. Pre-specified controls are included in all regressions at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A11: Vote share versus Voter turnout

Panel A	Metropolitan Mayor 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
T (1)	0.001	0.016**	0.004	-0.008**
Turnout	(0.006)	(0.007)	(0.005)	(0.003)
T (2)	-0.008	-0.014*	0.014*	-0.002
Vote share	(0.014)	(0.008)	(0.007)	(0.006)
Ratio (1/2)	.0703	1.08	.308	4.51
p-value	.631	.916	.222	.224
F-stat	.232	.0112	1.51	1.49
Panel B	Municipal Mayor 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
T (1)	0.000	0.015**	0.004	-0.008***
Turnout	(0.006)	(0.007)	(0.005)	(0.003)
T (2)	-0.007	-0.004	0.017*	0.008
Vote share	(0.016)	(0.012)	(0.010)	(0.007)
Ratio (1/2)	.0208	4.04	.219	1.03
p-value	.68	.412	.262	.975
F-stat	.171	.676	1.27	.00097
Panel C	Municipal Councillors 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
T (1)	-0.001	0.015**	0.004	-0.007**
Turnout	(0.006)	(0.007)	(0.005)	(0.003)
T (2)	-0.008	-0.005	0.016*	0.007
Vote share	(0.016)	(0.010)	(0.010)	(0.006)
Ratio (1/2)	.143	2.97	.251	.978
p-value	.685	.411	.274	.984
F-stat	.165	.679	1.21	.000402
N Ballot	1096	1191	1274	1232

This table compares the effect of the campaign on voter turnout and a different measure of vote share. The denominator for vote share is now the number of registered voters instead of the number of valid votes. The two effects are estimated using a seemingly unrelated regressions framework. The table also includes the ratio between the effects on turnout and on vote share. The results for p-value and F-statistic are from a test of the null hypothesis that the two effects are equal. All dependent variables are the ballot box level. The outcome variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. Pre-specified controls are included in all regressions at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A12: Vote share versus Voter turnout

Panel A	Referendum 2017			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
PO (1)	0.004*	0.004*	-0.000	-0.001
Turnout	(0.002)	(0.002)	(0.003)	(0.003)
PO (2)	0.013	-0.031***	0.016**	0.002
Vote share	(0.024)	(0.011)	(0.008)	(0.004)
Ratio	.278	.124	.0289	.396
p-value	.694	.0287	.12	.817
F-stat	.155	4.89	2.44	.0538
Panel B	Presidential 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
PO (1)	0.006	0.001	-0.008**	-0.000
Turnout	(0.004)	(0.003)	(0.004)	(0.003)
PO (2)	0.006	-0.032***	0.012	0.005
Vote share	(0.023)	(0.012)	(0.009)	(0.004)
Ratio	.932	.0292	.702	.0336
p-value	.986	.0189	.737	.438
F-stat	.000326	5.64	.113	.605
Panel C	General 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
PO (1)	0.007*	0.002	-0.007*	0.002
Turnout	(0.004)	(0.003)	(0.004)	(0.003)
PO (2)	0.008	-0.031***	0.012	0.005
Vote share	(0.024)	(0.011)	(0.009)	(0.004)
Ratio	.84	.0625	.648	.383
p-value	.957	.0201	.708	.41
F-stat	.00293	5.53	.141	.682

This table compares the effect of the campaign on voter turnout and a different measure of vote share. The denominator for vote share is now the number of registered voters instead of the number of valid votes. The two effects are estimated using a seemingly unrelated regressions framework. The table also includes the ratio between the effects on turnout and on vote share. The results for p-value and F-statistic are from a test of the null hypothesis that the two effects are equal. All dependent variables are the ballot box level. The outcome variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. Pre-specified controls are included in all regressions at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A13: Vote share versus Voter turnout

Panel A	Metropolitan Mayor 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
PO (1)	-0.000	0.009	0.001	-0.015***
Turnout	(0.006)	(0.007)	(0.007)	(0.004)
PO (2)	-0.006	-0.008	0.012	-0.012*
Vote share	(0.022)	(0.008)	(0.010)	(0.006)
Ratio	.074	1.04	.115	1.29
p-value	.814	.977	.401	.535
F-stat	.0558	.000815	.71	.388
Panel B	Municipal Mayor 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
PO (1)	0.001	0.007	-0.000	-0.015***
Turnout	(0.007)	(0.007)	(0.008)	(0.004)
PO (2)	-0.015	0.014	0.013	0.001
Vote share	(0.020)	(0.018)	(0.016)	(0.007)
Ratio	-.062	.534	-.0192	-14.4
p-value	.489	.715	.427	.0829
F-stat	.481	.133	.634	3.05
Panel C	Municipal Councillors 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
PO (1)	-0.001	0.008	0.001	-0.014***
Turnout	(0.006)	(0.007)	(0.007)	(0.004)
PO (2)	-0.013	0.008	0.012	-0.001
Vote share	(0.021)	(0.013)	(0.015)	(0.006)
Ratio	.0395	1.04	.0706	20.1
p-value	.587	.979	.535	.0337
F-stat	.297	.000718	.386	4.6

This table compares the effect of the campaign on voter turnout and a different measure of vote share. The denominator for vote share is now the number of registered voters instead of the number of valid votes. The two effects are estimated using a seemingly unrelated regressions framework. The table also includes the ratio between the effects on turnout and on vote share. The results for p-value and F-statistic are from a test of the null hypothesis that the two effects are equal. All dependent variables are the ballot box level. The outcome variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. Pre-specified controls are included in all regressions at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A14: Vote share versus Voter turnout

Panel A	Referendum 2017			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
CB (1)	-0.004	0.008***	0.002	0.006**
Turnout	(0.003)	(0.003)	(0.003)	(0.002)
CB (2)	-0.022	-0.021*	0.007	0.017***
Vote share	(0.022)	(0.012)	(0.007)	(0.006)
Ratio	.172	.374	.349	.339
p-value	.45	.277	.42	.0142
F-stat	.574	1.19	.654	6.18
Mean	.652	.711	.753	.802
Panel B	Presidential 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
CB (1)	-0.000	0.012**	0.001	0.002
Turnout	(0.005)	(0.005)	(0.004)	(0.004)
CB (2)	-0.028	-0.020	0.013	0.009
Vote share	(0.020)	(0.015)	(0.008)	(0.007)
Ratio	.00196	.627	.109	.188
p-value	.202	.657	.0716	.138
F-stat	1.64	.198	3.3	2.23
Mean	.644	.697	.744	.798
Panel C	General 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
CB (1)	0.001	0.012**	0.003	0.002
Turnout	(0.004)	(0.005)	(0.004)	(0.004)
CB (2)	-0.032	-0.025*	0.013*	0.011
Vote share	(0.021)	(0.015)	(0.007)	(0.007)
Ratio	.0446	.474	.271	.191
p-value	.092	.411	.0909	.0754
F-stat	2.88	.681	2.9	3.21
Mean	.645	.694	.739	.795

This table compares the effect of the campaign on voter turnout and a different measure of vote share. The denominator for vote share is now the number of registered voters instead of the number of valid votes. The two effects are estimated using a seemingly unrelated regressions framework. The table also includes the ratio between the effects on turnout and on vote share. The results for p-value and F-statistic are from a test of the null hypothesis that the two effects are equal. All dependent variables are the ballot box level. The outcome variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. Pre-specified controls are included in all regressions at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A15: Vote share versus Voter turnout

Panel A	Metropolitan Mayor 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
CB (1)	0.001	0.022**	0.008	0.001
Turnout	(0.009)	(0.011)	(0.006)	(0.004)
CB (2)	-0.009	-0.021*	0.017**	0.009
Vote share	(0.017)	(0.012)	(0.008)	(0.008)
Ratio	.162	1.09	.476	.0653
p-value	.656	.913	.176	.194
F-stat	.199	.012	1.85	1.7
Mean	.59	.634	.669	.714
Panel B	Municipal Mayor 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
CB (1)	-0.001	0.024**	0.009	-0.001
Turnout	(0.010)	(0.011)	(0.006)	(0.004)
CB (2)	0.002	-0.021*	0.023**	0.016
Vote share	(0.020)	(0.013)	(0.010)	(0.010)
Ratio	.424	1.12	.382	.0543
p-value	.966	.893	.101	.219
F-stat	.0018	.0182	2.73	1.53
Mean	.59	.628	.663	.709
Panel C	Municipal Councillors 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
CB (1)	-0.002	0.022*	0.008	0.001
Turnout	(0.009)	(0.011)	(0.006)	(0.004)
CB (2)	-0.003	-0.018	0.022**	0.016*
Vote share	(0.020)	(0.013)	(0.009)	(0.008)
Ratio	.538	1.22	.373	.0514
p-value	.947	.822	.086	.0419
F-stat	.00443	.0509	2.99	4.22
Mean	.591	.629	.665	.71

This table compares the effect of the campaign on voter turnout and a different measure of vote share. The denominator for vote share is now the number of registered voters instead of the number of valid votes. The two effects are estimated using a seemingly unrelated regressions framework. The table also includes the ratio between the effects on turnout and on vote share. The results for p-value and F-statistic are from a test of the null hypothesis that the two effects are equal. All dependent variables are the ballot box level. The outcome variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for a candidate other than Erdogan. In the 2018 general election and 2019 local election, the outcome variable is the vote share for the opposition parties. Pre-specified controls are included in all regressions at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.