Persistent Polarizing Effects of Persuasion: Experimental Evidence from Turkey

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Abstract

I evaluate randomly varied neighborhood exposure to information campaigns regarding either executive performance, or increases in executive power, prior to a Turkish referendum on weakening checks and balances on the executive. The campaigns increased voter polarization over the referendum, and subsequently changed party affiliation in national and local elections over the next two years, leading to partisan polarization. My results suggest that, when voters disagree on whether increasing executive power is a good policy, more information can increase voter polarization. Finally, I conclude that because potential polarization is often ignored, the impact of information campaigns 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, democratic norms around the globe are deteriorating. Nearly half of democracies today are characterized by low levels of political accountability and civil liberties (Bidner, Francois and Trebbi, 2015; Mukand and Rodrik, 2020), coupled with political polarization. It is well-documented that state censorship is an effective tool for swaying electoral support and consolidating power under the incumbent in these environments (Guriev and Treisman, 2019).

Whether providing non-state-authorized information can undermine such censorship remains an open question. If voters interpret the information, or the trustworthiness of its source, similarly, then we may anticipate a weakening of the state's political position and more homogeneous policy preferences among voters. However, if some voters interpret the information, or the trustworthiness of its source, differently, then this information may instead polarize citizens and increase policy and partisan segregation. Gaining credible evidence on the relative strength of these countervailing effects is hard. Such evidence must come from settings with already weak democratic institutions, where incumbent leaders can unilaterally and on short notice initiate measures to further weaken democratic institutions. It is challenging to both predict these junctures of institutional change and to operate a research program in such an environment.

This paper addresses these challenges in the context of Turkey's 2017 constitutional referendum. In the preceding decade, Turkey had experienced the sharpest deterioration in democratic norms globally (Freedom House, 2017). Just months after a coup attempt in 2016, the incumbent initiated a referendum on an institutional reform that would weaken checks and balances on the executive branch and increase executive power. I use administrative voter turnout and vote share data to analyze the neighborhood-level impact of a large-scale non-state-authorized information experiment on support for the referendum in one Turkish province. Following Gentzkow and Shapiro (2011), I consider a geographic measure of polarization. Specifically, I define neighborhood-level policy polarization to be increasing when both the "No" vote share in neighborhoods where average support for the referendum was lower increases and the "No" vote share in neighborhoods where average support for the referendum was higher decreases. I use subsequent national and

local elections results to observe whether increased policy segregation at the neighborhood-level persists and translates into increased partisan segregation (Brown and Enos, 2021).

The field experiment involved two door-to-door information campaigns that were randomized at the neighborhood level and delivered to approximately a quarter million voters before the referendum. The information campaigns were organized by members of the largest party opposing the referendum and carried out by party volunteers.[1] Neighborhoods included in the experiment were divided into two treatment groups and a control group. In one treatment group, voters were provided with information on the referendum itself, including how it would weaken political accountability and consolidate power under the presidency. In the second treatment group, voters were provided with information on recent executive performance, including how it had worsened economic conditions and national security. Voters in neighborhoods randomly assigned to the control group were not visited by canvassers.

Since the information campaigns could increase voter polarization, I incorporated this possibility into the experimental design. (As the researcher, I suggested an experimental design, but had no control over the content of the information campaigns.) As neighborhood-level party vote share is predictive of different voter policy views, I stratified randomization by quartiles of the past average vote share for the main opposition party in two 2015 general elections. I specified estimating heterogeneous treatment effects by stratum in a registered pre-analysis plan (Athey and Imbens, 2016; Duflo et al., 2020).[2]

The two information campaigns had a zero average effect on vote share at the ballot-box level. However, they increased the "No" vote share by 1.2 percentage points (1.7%, p-value .030) and 0.8 percentage point (1%, p-value .031) in the third and fourth quartiles, respectively, where the opposition was stronger. The campaigns decreased the "No" vote share by 3.4 percentage points (5.3%, p-value .001) in the second quartile and, insignificantly, by 0.6 percentage point (1.1%, p-value .764) in the first quartile, where the opposition was weaker. Since the negative effects were in quartiles with a low

[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).

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

"No" vote share and the positive effects were in quartiles with a high "No" vote share, the campaigns increased policy polarization. The two campaigns separately had similar polarizing effects. Finally, while I cannot directly rule out differential mobilization effects within quartiles, I find that the information campaigns had a zero average treatment effect on voter turnout overall and by quartile. This result is consistent with the idea that the information campaigns caused voters to switch their intended vote. Under this interpretation, the marginal voter, who was planning to vote based on one dimension, e.g., social identity, switched because the information campaigns increased the salience of a secondary cross-cutting dimension related to the referendum, e.g., checks and balances. Policy polarization increased because the marginal voter affected by the campaigns voted more like their neighbors with whom they already shared similar policy views.

National and local elections occurred within two years after the referendum, allowing me to estimate the persistence of campaign-induced polarization. I find that the effects persisted in 2018 and 2019. Therefore, the campaigns also increased voters' support for the party aligned with their vote choice in the referendum, leading to increased partisan polarization.

My study contributes to the empirical literature on the political economy of persuasion. First, the experimental design rules out the possibility that increased polarization is driven by voters self-selecting into different ideological sources of information, i.e., "echo chambers," a commonly explored mechanism (e.g., Sunstein, 2001; Gentzkow and Shapiro, 2011; Martin and Yurukoglu, 2017). Instead, voter polarization increased despite exposure to the same information source. This can result from voters interpreting either the same information in the campaigns or the quality of the opposition party (information source) differently.[3] Second, experimental political information campaigns have been evaluated primarily in countries with low media censorship (e.g., Pons, 2018; Kalla and Broockman, 2018).[4] The estimated

[3]A theoretical literature argues that voter polarization can increase, even with Bayesian updating, despite common exposure to the same information or information source (e.g., Dixit and Weibull, 2007; Andreoni and Mylovanov, 2012; Benoit and Dubra, 2019; Loh and Phelan, 2019) and that this increase in voter polarization can persist (Acemoglu, Chernozhukov and Yildiz, 2016; Gentzkow, Wong and Zhang, 2021; Piketty, 1995).

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

vote share effects are typically smaller than what I find. My results are consistent with the hypothesis that information campaigns have larger effects on vote share in weak democracies (Enikolopov, Petrova and Zhuravskaya, 2011). My study also relates to the literature on whether exposure to state or non-state media affects state support in authoritarian environments (Adena et al., 2015; Enikolopov, Petrova and Zhuravskaya, 2011; Knight and Tribin, 2022; Chen and Yang, 2019). Finally, a less explored hypothesis is whether exposure to the same information campaign or media is more likely to increase political polarization in weak democracies. Most empirical studies find a uniform effect on vote share and voter behavior, or even a reduction in attitudinal polarization (e.g., DellaVigna and Kaplan, 2007; Martin and Yurukoglu, 2017; Levy, 2021).[5] The exceptions are studies that evaluate this relationship in weak democracies (Adena et al., 2015; Peisakhin and Rozenas, 2018; Caprettini et al., 2022).

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, Robinson and Torvik, 2013) and referred to his followers as the "real people" (Temelkuran, 2019, p. 20) as opposed to the "establishment" or "elite" (Aydin-Düzgit, 2019). This is common rhetoric among populist leaders (Guriev and Papaioannou, 2020).

[5]Voter polarization in response to the same information source 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, Ross and Lepper, 1979; Andreoni and Mylovanov, 2012; Fryer, Harms and Jackson, 2019) and do not observe backfiring effects (Alesina, Stantcheva and Teso, 2018).

Figure A1, which uses data from V-Dem, 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 an unprecedented rise in terrorist attacks, which is visible in data from the Global Terrorism Database (see Figure A2a). 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 indications of a worsening economy, including a devaluation of the Turkish lira. Figure A2b shows that this devaluation started in 2010 and escalated starting in 2015. After the coup attempt, there was a record devaluation of the Turkish lira.

Just five months after the coup attempt, and shortly after the record devaluation of the Turkish lira, the AK Party proposed a referendum on switching from a parliamentary system to a presidential system and consolidating executive power under the presidency via the introduction of eighteen amendments to the constitution. 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) of the constitution as translated in Commission (2017).) Therefore, the AK Party's position suggested that a stronger state would also contribute to economic stability.

In response to the proposed referendum, independent international agencies, such as the European Commission for Democracy through Law, 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]

[6]As an example, the referendum granted the president a new power to bypass the

Therefore, winning the referendum would institutionalize weakened democratic norms and constraints on the executive. Using data from V-Dem on democratic indices, Figure A3 shows that the referendum marked a low point in the deterioration of democratic norms in Turkey.

Ultimately, however, support for the referendum depended on whether voters perceived the worsening national security, and the concurrent deteriorating economic conditions, as external threats beyond the control of the state. If so, voters would be willing to support increased executive power, despite its weakening of democratic institutions. Nationally representative survey evidence collected prior to the referendum is informative of voter perceptions regarding external threats. The survey results indicate that irrespective of 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. The majority of those supporting the opposition blamed the incumbent while a majority of those supporting the incumbent instead blamed external threats. The data also suggest that one's party affiliation and one's view on attribution, which presumably predicts one's support for a stronger state, are correlated, albeit imperfectly. Foreshadowing my findings, the experimental results suggest that the information campaigns strengthened this correlation and increased ideological polarization over the proposed referendum for a stronger state.

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 A4

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. 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.

illustrates this by showing the neighborhood-level distribution of 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. The volunteers had prior experience in canvassing and were all based in Izmir. They also received training by a campaign team manager before canvassing started. The funding and details of the content of the campaigns were determined by the campaign manager and staff who were managing operations in Izmir. This was the only campaign team operating in Izmir, and so there is no risk that other campaign teams from the opposition party implemented a campaign in control neighborhoods. 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]

[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.

In the end, the sampling selection, prior to randomization, included 14 out of 30 districts and 550 out of 1,294 neighborhoods in Izmir. The sampling selection procedure increased the average and the median number of registered voters per neighborhood, relative to the population of neighborhoods in Izmir, from 2,403 and 679 to 2,690 and 1,545, respectively. The range of the number of registered voters also changed from 15–28,134 to 113–10,946. Figures A5 and A6 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 three arms: one control arm and two information campaign treatment arms. In one treatment arm, voters were exposed to information on past policy outcomes (PO campaign). The campaign was on negative executive performance. It included information on deteriorating economic conditions and increased terrorist activity under the current leadership. In general, the information was meant to convey that executive power should not increase 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.

In treated neighborhoods, messages were conveyed to voters both verbally, if they opened their door, and in a pamphlet. The pamphlet text for each campaign can be seen in Figures A7a and A7b. The canvassers also received training from the campaign team on how to deliver the information outlined in the pamphlet orally, without guessing the ideological position of voters, 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 Republican People's Party (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 groups or the control group. The distribution and quartiles of the stratifying variable are shown in Figure 1. 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 Izmir. 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 a weak 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 would open their doors and engage 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

[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.

[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.

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 (conversation completion rate). These descriptive statistics show that the average conversation completion rate is not correlated with the quartiles of past vote share and that implementation of the campaigns was similar across quartiles.

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

3.5 Empirical Strategy and Pre-analysis Plan

3.5.1 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 three 2019 local elections that were scraped from the Supreme Election Council's website (Supreme Election Council, 2021).[10] The data are available at the ballot-box level. Ballot boxes cannot be matched across time, but district and neighborhood names were used to match observations across election years.

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

3.5.2 Empirical Strategy and Pre-analysis Plan

I estimate the pooled effect of a neighborhood being assigned to either treatment arm on vote share and voter turnout using the following specification:

$$Y_{bnq} = \alpha + \beta T_{nq} + X'_{nq}\lambda + \delta_q + \epsilon_{bnq}, \quad (1)$$

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 elections 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. Standard errors are clustered at the neighborhood level.

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 a test of whether I can reject the null hypothesis that the two treatment campaign arms have the same effect on vote share and voter turnout across quartiles and by quartile.[13] Finally, I pre-specified the vector of control variables measuring past voter data from 2015 in Equation (1) (X'_{nq}). The control variables, which are at the neighborhood level, 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. Tables A3 and A4 show the

[11] The opposition parties include the Iyi Party, the CHP, and the Peoples' Democratic Party (HDP). Some members of the Nationalist Movement Party (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] In the do-file uploaded for the pre-analysis plan, the variable for the quartiles is `chp_pct_n_avg_quart`.

balance of the pre-specified covariates across the control group by treatment arm (CB and PO, separately) and within each quartile.

In addition to the ballot box-level analysis, I show the results for the outcome variables at the neighborhood level, with or without population weights (number of registered voters).[14] There are efficiency gains from including population weights in the neighborhood-level 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, Haider and Wooldridge, 2015). When comparing the ballot box-level to the neighborhood-level analysis with population weights, I find that the results are nearly identical.[15] This is because the number of ballot boxes in a neighborhood is correlated with the number of 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, Haider and Wooldridge, 2015).

4 Results

4.1 Treatment Effects on Vote Share

Figure 2 shows the results from estimating the ITT using Equation (1). I also show these results in Panel A of Table A5. Across quartiles (labeled "Overall"), the combined average treatment effect of the two campaigns on the "No" vote share in the 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 point (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 point (1%, p-value .031) in quartiles 3 and 4, respectively. In Panel A of Table 1, I show the results of estimating the ITT for each campaign treatment arm. By quartile, the estimated effects are similar for the two treatment arms, and so I cannot reject the null hypothesis that the effects are the same.

[14] In the pre-analysis plan, I specified conducting the analysis at the neighborhood level.

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

In summary, the information campaigns increased the "No" vote share in quartiles where average pre-existing support for the opposition party was higher and decreased the "No" vote share in quartiles where average pre-existing support for the opposition party was lower. Since the 2015 opposition party vote share is a strong predictor of the "No" vote share, these results show that the information campaigns increased policy polarization on average.[16]

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 by Young (2019) and Hess (2017), I find that the p-value for quartile 2 is approximately .006 or .005 depending on the method.[17] For quartile 3, I find that the p-value is approximately .058 or .061 depending on the method that is used. The p-value for quartile 4 is .058 or .112 depending on the method that is used. The p-value for quartile 1 is high regardless of the method that is used (above .7). These results are summarized in Table A7. The calculated p-values are described as approximate because they are sensitive to the seed used for the randomization, as the authors of both methods attest to. Table A8 shows the estimated average treatment effect across quartiles with and without the pre-specified covariates. Table A9 shows the same results, but by quartile and campaign treatment arm.

The expected magnitude of the treatment effects by quartile depends on the share of persuadable ("moderate") voters and the net result of countervailing effects. Presumably, there are more persuadable voters where the vote share differential between the incumbent and opposition parties is smaller, but this is also where the net result of countervailing effects would be zero. To explore this empirically, I show the estimated effects of the information campaigns by four, six, and eight quantiles in Figure A8. The figure shows

[16]The results from estimating Equation (1), but using the neighborhood-level measure of the dependent variable, is shown in Table A6. 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.

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

that the average treatment effect in the first quartile is small because in the tail end of the distribution (first octile) there is a very noisy, but positive, effect on vote share. The treatment effects in the second through fourth octiles are similar in size and are all negative. Thus, the magnitude of the average treatment effect is not uniformly higher in the second quartile than in the first quartile. Figure A9 also shows that small neighborhoods have a small sample bias and Figure A10 illustrates that there are a disproportionate number of small neighborhoods in the first quartile. This could explain the outlier in the first octile. By contrast, in comparing the magnitude of the treatment effects in the third and fourth quartiles, the results in Figure A8 suggest that there are more persuadable voters in the third quartile.

4.2 Persistence

I show the persistent effects of the information campaigns across quartiles and by quartile in Figure 3. I also show these results in Panels B and C of Table A5. I chose the 2018 presidential election and the 2019 metropolitan mayoral election to be included in the main results because they both pertain to higher-level leadership. Equivalent results for the 2018 general election and the other two 2019 local elections are shown in Table A10. The results for both elections in 2018 are similar and the results for all three elections in 2019 are also similar. Figure 3 shows that the estimated effects by quartile in 2018 are nearly equal in magnitude and statistical significance to those in 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.

I show the persistent effects of each information campaign separately in Panels B and C of Table 1.[18] The table includes a test of the null hypothesis that the effect of the two information campaigns is the same. Across all elections from 2017 through 2019, I reject the null hypothesis in only two out of 24 regressions (for each quartile and six elections): the second quartile of the 2019 elections for municipal mayors and municipal councilors. As shown in Table 1, 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.

[18]Equivalent results for the other elections are shown in Table A11.

4.3 Voter Turnout

Table 2 shows the estimated effect of the information campaigns on voter turnout across quartiles and by quartile in the 2017 referendum, the 2018 presidential election, and the 2019 metropolitan mayoral elections.[19] 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 between .6 and .7 of a percentage point, 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 in the second quartile.

In Tables A13 and A14, I directly compare the estimated effect of the information campaigns on vote share and voter turnout, using a seemingly unrelated regressions framework.[20] In 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. 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 elections 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 elections 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. In each quantile, there is no statistically significant treatment effect on voter

[19] Equivalent results for the other elections are shown in Table A12.

[20] Tables A15, A16, A17, and A18 show this test by campaign separately.

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 A8. Overall, the results suggest that the effects of the information campaigns on vote share cannot be entirely explained by voter turnout. The average treatment effects across quantiles and by quantile are precisely estimated and they are small. In Section 5, I discuss the possibility that there can be differential effects on turnout within each quartile.

5 Discussion

I observe an increase in polarization at the population level: the information campaigns increased the "No" vote share in neighborhoods with many pro-opposition supporters and decreased it in neighborhoods with many pro-incumbent supporters. This effect persisted in subsequent national and local elections measured using the opposition vote share. Therefore, the information campaigns caused the marginal voter to vote more like their neighbors for at least two years. I propose two potential explanations related to changes at the individual level. The first is based on marginal voters who switch their vote choice and the second on marginal voters who switch their turnout choice.

Regarding the first explanation, consider that all voters choose their vote according to two dimensions. One dimension captures authoritarianism. For example, voters with pro-authoritarian views believe that external threats to national security are more important than incumbent quality in explaining economic conditions and support increased executive power when the economy worsens. By contrast, voters with anti-authoritarian views believe that incumbent quality is more important in explaining economic conditions and support constraints on the executive branch to hold the incumbent accountable when the economy worsens. The second dimension is based on social identity, e.g., religion, and is independent of the first.

Within each neighborhood, views on authoritarianism tend to be strongly correlated across voters. This could be because voters influence each other, have exposure to the same experiences, or engage in motivated reasoning (e.g., Gentzkow, Wong and Zhang, 2021; Iyengar, Sood and Lelkes, 2012). Before the intervention, in pro-government neighborhoods, most people are pro-authoritarian. Among them, some nonetheless support the opposition because of their social identity: their identity is at odds with their views on

authoritarianism (type-1 switchers). In pro-opposition neighborhoods, most people are anti-authoritarian. Similarly, among them, some nonetheless support the government because of their social identity (type-2 switchers). Both campaigns increase the salience of authoritarianism by conveying information related to authoritarianism (or the opposition party's position on authoritarianism), leading type-1 switchers to start supporting the government in the referendum and subsequent regular elections and type-2 switchers to start supporting the opposition in the referendum and subsequent regular elections. Overall, switchers end up voting based on authoritarianism, thus putting them in agreement with most other voters in their neighborhood, whose views on this dimension they share.

The second explanation for changes at the individual level is related to voter turnout. A typical mechanism is that in response to campaigns, voters revise their view to one where the canvassing party will win and therefore they become mobilized, leading to higher turnout. In my study, this mechanism would have been consistent with mobilization increasing among mostly marginal opposition supporters in quartiles 3 and 4 and among mostly marginal incumbent supporters in quartiles 1 and 2. I rule this out because the information campaigns have a zero average effect on voter turnout by quartile (or octile, as shown in Figure A8). Therefore, if the effects on vote share are explained by mobilization there must also be peer effects, or other mechanisms like expressive voting, that cause differential turnout within quartiles. This would require mobilization among marginal opposition voters in quartiles 3 and 4, and demobilization among marginal incumbent voters in the same quartiles, and vice versa in quartiles 1 and 2. This could happen, for example, if voters who always support either the opposition or incumbent party ("always" voters) not only independently revised their views to one where the "No" campaign will win, but also pressured their neighbors to turn out and vote similarly.

While differential turnout is a plausible mechanism, it is not strongly supported by the results. Under this mechanism, for example, we would expect larger voter turnout effects, and larger vote share effects, in quartile 4. Presumably, the pool of marginal opposition voters that can be pressured and mobilized is larger in quartile 4 than in quartile 3, and the pool of pro-government voters that can be cross-pressured and demobilized is smaller in quartile 4 than in quartile 3, and so we should expect a more positive turnout effect overall. This is not what I find. Moreover, while the polls correctly predicted that the referendum would be a close election, the differ-

ential turnout mechanism requires the voters' revised beliefs (to an outcome where the opposition party is successful) to persist in the 2019 metropolitan mayoral elections. This is unlikely because Izmir is an opposition stronghold where all voters know that they are not pivotal in the metropolitan mayoral elections.

The potential explanations for the results of this study stand in contrast to the most common mechanism examined in the empirical literature on voter polarization. Most studies have focused on voters self-selecting into different information sources, e.g., Fox News versus CNN, and being influenced by the slant of the information, i.e., "echo chambers." The experimental design used in this study rules out this explanation. Instead, I show that even with common exposure to the same information source, voter polarization can increase in a persistent manner. This finding has important implications for understanding the role of information in strengthening political accountability in contexts where state censorship levels are high. While exposure to non-state-authorized information can counter state censorship among some voters, it can simultaneously backfire, thereby increasing voter polarization.

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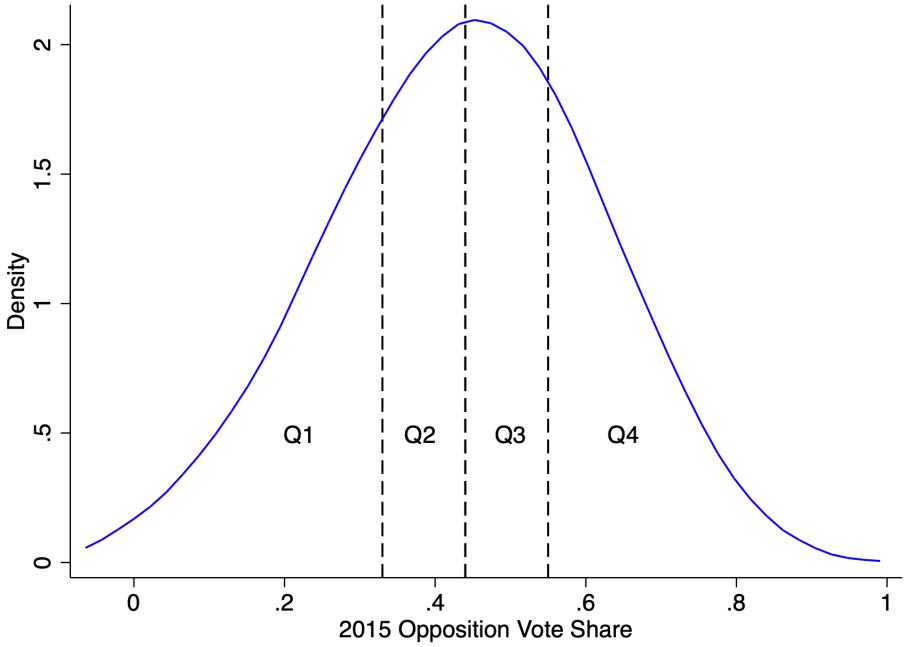
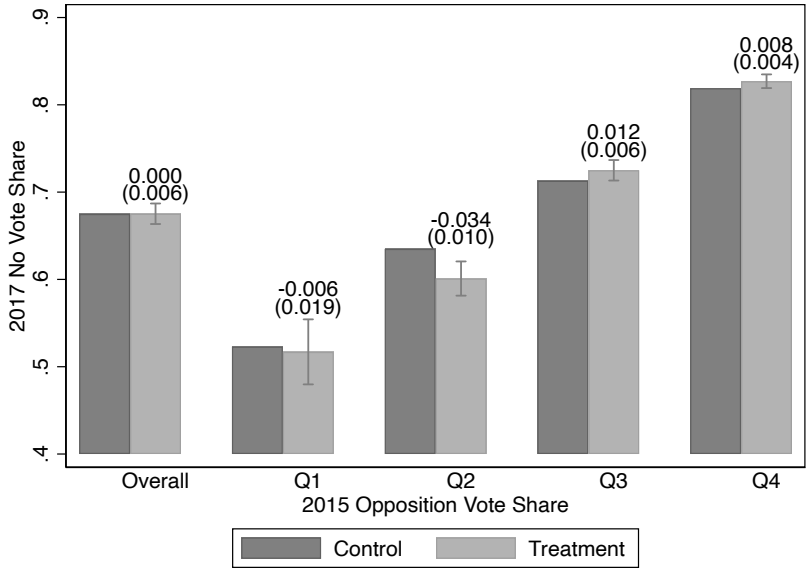
Figure 1 Distribution of Stratifying Variable: 2015 Opposition Vote Share



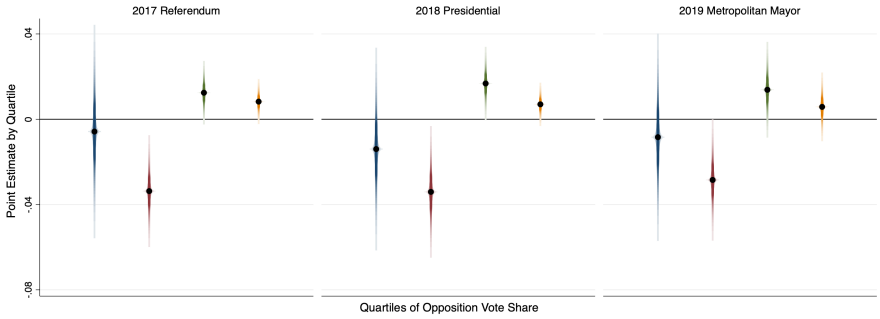
This figure shows the neighborhood-level distribution of the stratifying variable (the average vote share for the main opposition party in the 2015 elections). Observations are weighted by the number of registered voters in a neighborhood. The vertical lines indicate the quartiles of vote share. Randomization was stratified by quartile.

Figure 2
Treatment Effects on the 2017 Referendum "No" Vote Share



This figure displays the ITT estimated treatment effects of the information campaigns on the "No" vote share in the 2017 referendum. The dependent variable is at the ballot-box level. I show the estimated treatment effect across all strata ("Overall") and by stratum (Q1-Q4). Strata are the quartiles of the average vote share for the main opposition party in the 2015 elections. Strata fixed effects are included in the regression for "Overall." Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level. Overall, there are 550 neighborhoods (100 treatment and 450 control) and 3992 ballot-boxes included in the analysis.

Figure 3 Persistence of Treatment Effects



This figure displays the persistence of the treatment effects across elections. All dependent variables are the ballot-box level. The dependent variable for the 2017 referendum is the "No" vote share. The outcome variable for the 2018 presidential election is the vote share for all candidates other than Erdoğan. In the 2019 metropolitan mayoral election, the outcome variable is the vote share for all opposition parties. I show the estimated treatment effect across all strata ("Overall") and by stratum (Q1-Q4). Strata are the quartiles of the average vote share for the main opposition party in the 2015 elections. Strata fixed effects are included in the regression for "Overall." Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level.

Table 1
Treatment Effect on Vote Share by Quartile and Campaign

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	0.523	0.635	0.713	0.819
N Ballot	919	983	1058	1032
R squared	0.284	0.416	0.410	0.665
PO=CB p-value	0.325	0.596	0.246	0.153
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	0.510	0.612	0.693	0.809
N Ballot	1015	1093	1160	1138
R squared	0.286	0.441	0.430	0.626
PO=CB p-value	0.298	0.743	0.685	0.701
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	0.459	0.555	0.626	0.759
N Ballot	1096	1191	1274	1232
R squared	0.199	0.329	0.321	0.667
PO=CB p-value	0.894	0.130	0.863	0.240
N Nbhd	138	137	138	137

All dependent variables are the ballot-box level. The dependent variable for Panel A is the 2017 "No" vote share. The outcome variable for the 2018 presidential election is the vote share for all candidates other than Erdoğan. In the 2019 local election, the outcome variable is the vote share for all opposition parties. I show the estimated treatment effect by stratum (Q1-Q4). Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level.

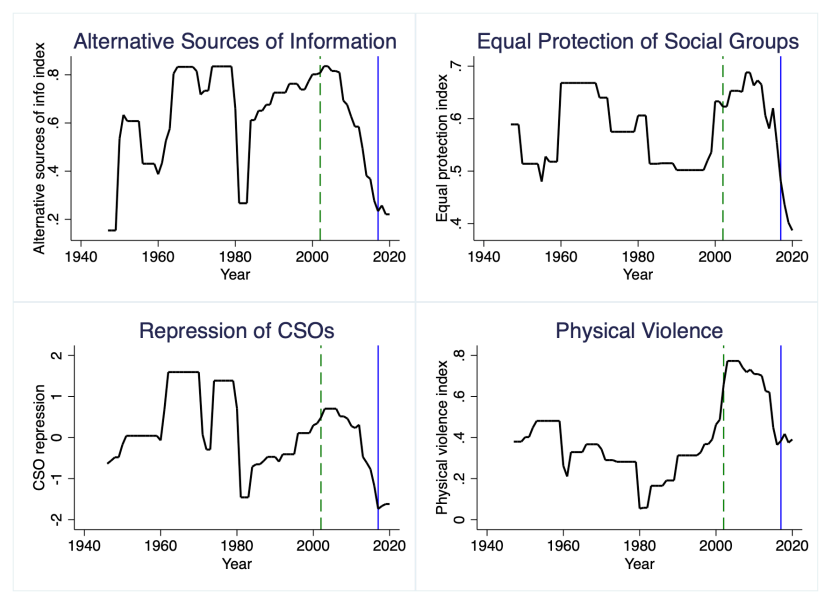
Table 2
Treatment Effect on Voter Turnout Overall and by Quartile

	Referendum 2017				
	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)
Mean	0.872	0.857	0.870	0.879	0.882
N Ballot	3992	919	983	1058	1032
R squared	0.401	0.403	0.385	0.301	0.361
	Presidential 2018				
	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)
Mean	0.870	0.853	0.865	0.879	0.882
N Ballot	4406	1015	1093	1160	1138
R squared	0.319	0.334	0.305	0.199	0.243
	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)
Mean	0.815	0.810	0.816	0.823	0.811
N Ballot	4793	1096	1191	1274	1232
R squared	0.360	0.383	0.384	0.343	0.338
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. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level.

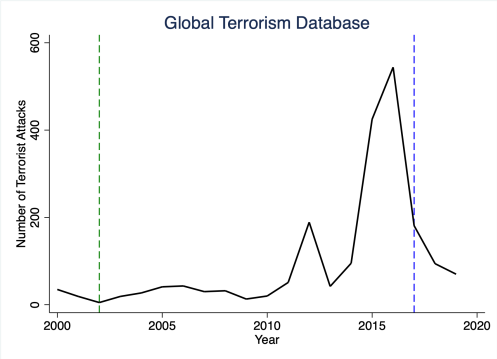
A For Online Publication: Appendix

Figure A1 Media Censorship and State Repression

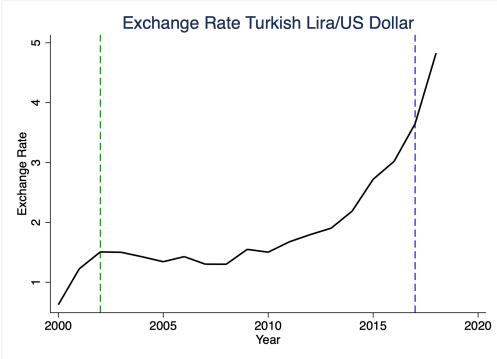


[Source] These figures show time series plots of indicators relating to media cenosrship and state repression from the V-Dem database. Access to alternative information is defined as the extent to which the media is (a) un-biased in their coverage (or lack of coverage) of the opposition, (b) allowed 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. A specific example of censorship is that the highest number of jailed journalists across all countries ever recorded since the Committee to Protect Journalists (CPJ) started 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 Turkish government (Media Ownership Monitor, 2019). (Source: V-Dem (Coppedge et al., 2021))

Figure A2 National Security and the Economy



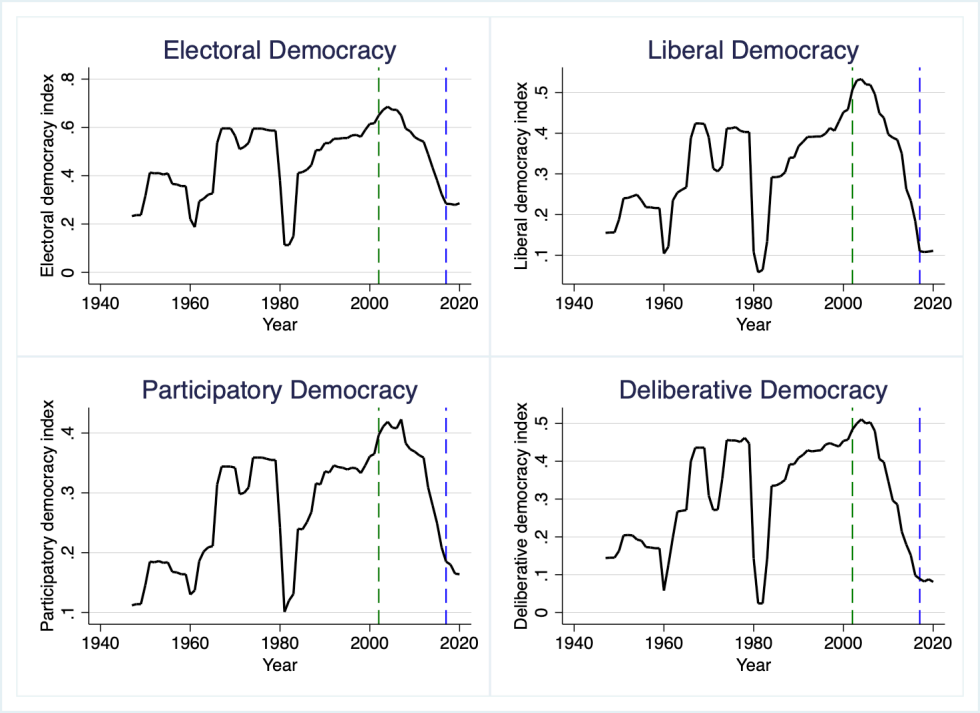
(a) Terrorist Attacks in Turkey



(b) OECD Data Exchange Rate

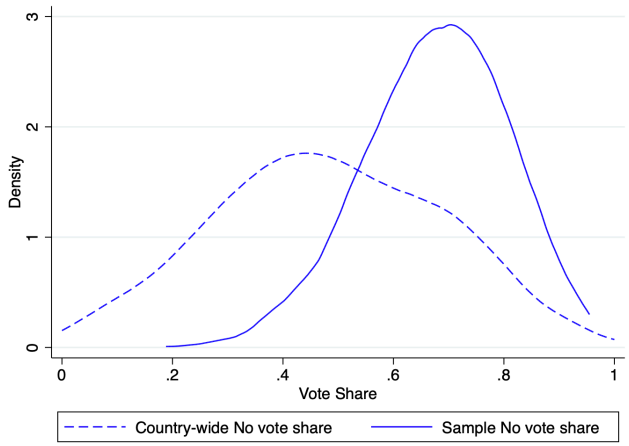
The figure on the left shows the number of terrorist attacks in Turkey using data from the Global Terrorism Database (Global Terrorism Database, 2021). According to Global Terrorism Database (2021), a terrorist attack is defined as the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation. The figure on the right shows the exchange rate between the Turkish Lira and US Dollar (OECD, 2021).

Figure A3 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 AK Party came into power and the blue vertical line indicates the year of the referendum. (Source: V-Dem (Coppedge et al., 2021)).

Figure A4 “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 A5 Map of Sample Within Turkey and with Province Borders

Figure A6 Map of Sample Within Izmir and with District Borders

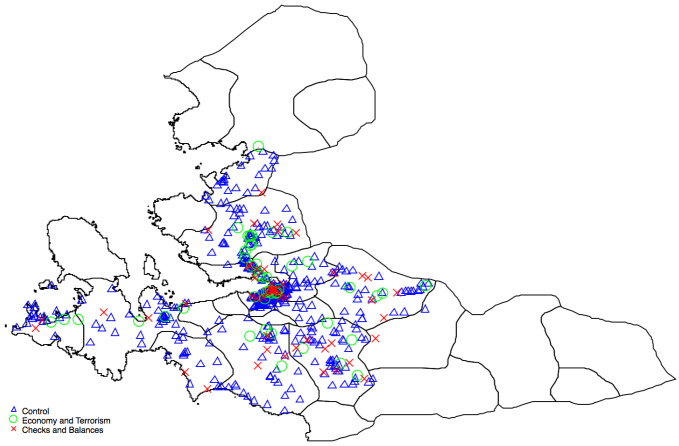


Figure A5 shows the location of the neighborhoods in the experimental sample within Turkey. Figure A6 shows the location of neighborhoods in each treatment group within the province of Izmir.

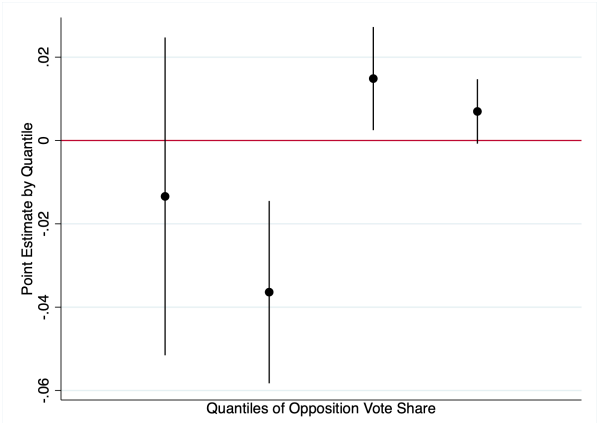
Figure A7 Pamphlets

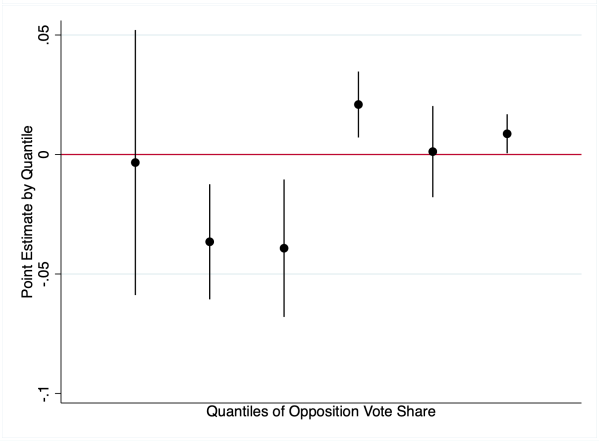
(a) Policy Outcomes

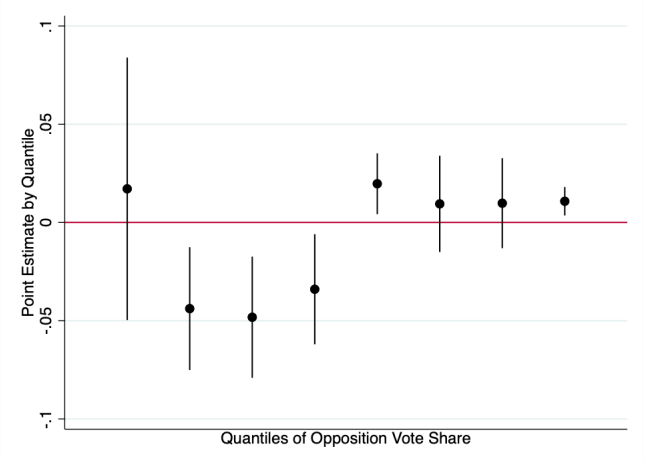
(b) Checks and Balances

These are 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 A7b says "For my future, No (Hayir)."

Figure A8 Treatment Effects on Vote Share by Quantile Across the Distribution

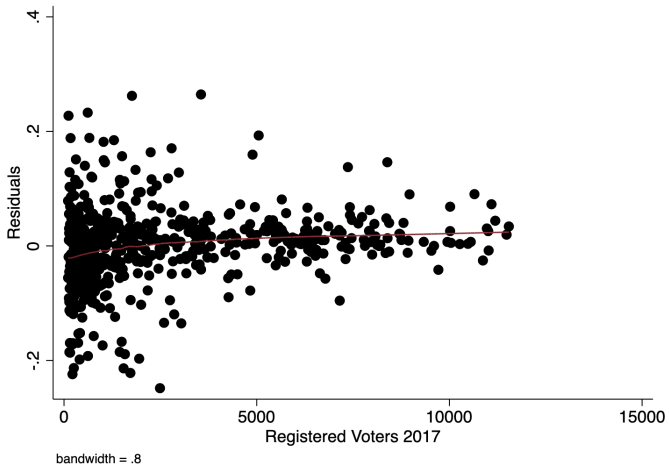






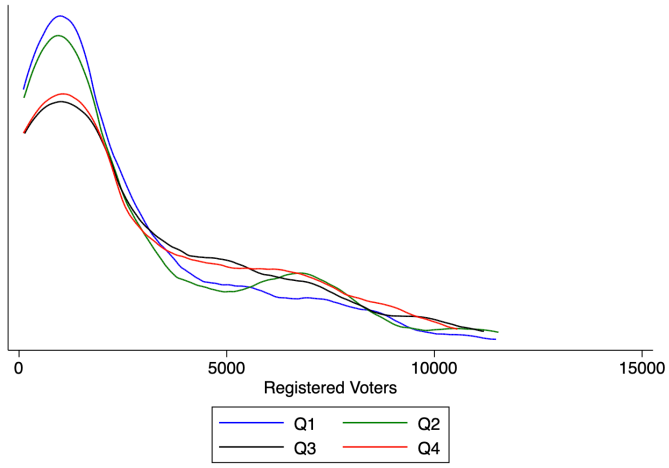
These figures show the estimation results for different numbers of quantiles of the stratifying variable (the average vote share for the main opposition party in the 2015 elections). The dependent variable is at the ballot-box level. The outcome variable for the 2017 referendum is the "No" vote share.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

Figure A9 Residuals vs. Neighborhood Size



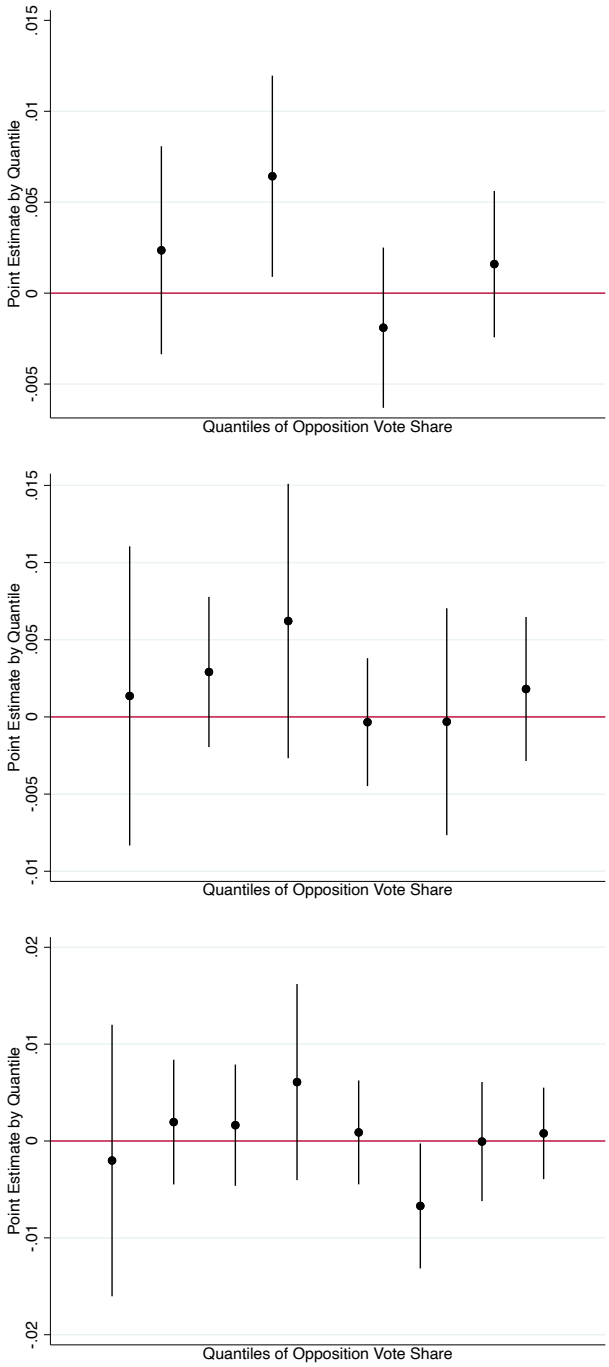
This figure shows the residuals from regressing the "No" vote share on the November 2015 vote share for the opposition party plotted against neighborhood size, which is defined as the number of registered voters in a neighborhood.

Figure A10 Distribution of Neighborhood Size



This figure shows the distribution of registered voters in a neighborhood for each strata. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections.

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 ballot-box level. The outcome variable for each election from 2017 to 2018 is voter turnout. 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 Conversations 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. Canvassers did not share information on the number of voters they completed a conversation in these neighborhoods. Column 4 shows the number of neighborhoods assigned to the treatment group, but excluding neighborhoods where canvassers faced 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" for this neighborhood, but include this neighborhood in columns 2 and 4.

Table A2 Balance on Pre-Specified Variables

	Aggregate		
	Control Mean	Coefficient	Standard Error
Reg Voters Nov	5486.421	-37.853	547.182
Valid Casts Nov	4759.285	-29.280	473.658
Opp Votes June	2079.003	38.123	220.028
Opp Votes Nov	2199.444	31.146	235.802
Opp Share June	0.440	0.000	0.008
Opp Share Nov	0.452	-0.003	0.009
Turnout Nov	0.866	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. Strata are the quartile of the average vote share for the main opposition party in the 2015 elections.

Table A3 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	5121.790	604.328	2434.540	5439.362	-532.754	903.217
Valid Casts Nov	4407.089	516.021	2107.968	4718.222	-504.082	769.782
Opp Votes June	1005.546	278.290	563.432	1779.321	-197.514	281.252
Opp Votes Nov	1056.871	290.866	608.838	1868.805	-266.754	267.848
Opp Share June	0.235	0.027	0.015	0.388	0.007	0.018
Opp Share Nov	0.237	0.024	0.018	0.395	-0.003	0.018
Turnout Nov	0.859	-0.006	0.007	0.867	-0.004	0.012
CB Campaign	Q1			Q2		
	Control Mean	Coef	SE	Control Mean	Coef	SE
Reg Voters Nov	5121.790	-1609.801	1076.551	5439.362	1040.344	1897.910
Valid Casts Nov	4407.089	-1387.595	952.384	4718.222	773.720	1602.391
Opp Votes June	1005.546	-362.629	279.670	1779.321	369.106	668.856
Opp Votes Nov	1056.871	-368.183	324.895	1868.805	396.283	739.312
Opp Share June	0.235	-0.031	0.031	0.388	0.004	0.013
Opp Share Nov	0.237	-0.035	0.033	0.395	-0.000	0.018
Turnout Nov	0.859	-0.004	0.012	0.867	-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.

Table A4 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	5734.712	449.155	1440.571	5614.352	610.983	1323.919
Valid Casts Nov	4994.065	441.799	1287.778	4883.714	566.586	1150.964
Opp Votes June	2388.448	120.587	542.656	3053.813	507.839	817.930
Opp Votes Nov	2528.924	108.614	579.450	3248.519	527.718	866.490
Opp Share June	0.492	-0.007	0.013	0.629	0.020	0.016
Opp Share Nov	0.506	-0.012	0.012	0.654	0.020	0.019
Turnout Nov	0.871	0.007	0.008	0.867	0.009	0.006
CB Campaign	Q3			Q4		
	Control Mean	Coef	SE	Control Mean	Coef	SE
Reg Voters Nov	5734.712	-1200.008	1256.877	5614.352	23.680	573.495
Valid Casts Nov	4994.065	-1077.271	1070.367	4883.714	109.233	522.961
Opp Votes June	2388.448	-582.050	459.287	3053.813	12.402	333.531
Opp Votes Nov	2528.924	-646.357	480.524	3248.519	39.103	367.210
Opp Share June	0.492	-0.016	0.011	0.629	-0.003	0.026
Opp Share Nov	0.506	-0.024	0.012	0.654	0.005	0.031
Turnout Nov	0.871	-0.003	0.005	0.867	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 (quartiles 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.

Table A5
Treatment Effect on Vote Share Overall and by Quartile

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	0.675	0.523	0.635	0.713	0.819
N Ballot	3992	919	983	1058	1032
R squared	0.785	0.279	0.416	0.409	0.664
Panel B	Presidential 2018				
	Overall	Q1	Q2	Q3	Q4
Treatment	-0.001	-0.014	-0.034	0.017	0.007
	(0.006)	(0.018)	(0.012)	(0.007)	(0.004)
Mean	0.658	0.510	0.612	0.693	0.809
N Ballot	4406	1015	1093	1160	1138
R squared	0.769	0.281	0.441	0.430	0.626
Panel C	Metropolitan Mayor 2019				
	Overall	Q1	Q2	Q3	Q4
Treatment	-0.001	-0.008	-0.028	0.014	0.006
	(0.006)	(0.019)	(0.011)	(0.009)	(0.006)
Mean	0.602	0.459	0.555	0.626	0.759
N Ballot	4793	1096	1191	1274	1232
R squared	0.757	0.199	0.325	0.321	0.666
N Nbhd	550	138	137	138	137

All dependent variables are the ballot-box level. The dependent variable for Panel A is the 2017 "No" vote share. The outcome variable for the 2018 presidential election is the vote share for all candidates other than Erdoğan. In the 2019 local election, the outcome variable is the vote share for all opposition parties. I show the estimated treatment effect across all strata ("Overall") and by stratum (Q1-Q4). Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Strata fixed effects are included in the regression for "Overall." Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level.

Table A6
Treatment Effect on Neighborhood-Level 2017 "No" Vote Share by Quartile

Panel A	Referendum 2017				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.000	-0.008	-0.035	0.013	0.008
	(0.006)	(0.019)	(0.011)	(0.006)	(0.004)
Mean	0.677	0.526	0.635	0.714	0.820
R squared	0.867	0.358	0.625	0.679	0.842
Panel B	Referendum 2017: Unweighted				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.008	0.011	-0.027	0.020	0.013
	(0.007)	(0.021)	(0.012)	(0.009)	(0.007)
Mean	0.654	0.496	0.628	0.694	0.798
R squared	0.782	0.402	0.398	0.489	0.682
N	550	138	137	138	137

The dependent variable in each column is the "No" vote share at the neighborhood level. I show the estimated treatment effect across all strata ("Overall") and by stratum (Q1-Q4). Strata are the quartile of the average vote share for the main opposition party in the 2015 elections. Strata fixed effects are included in the regression for "Overall." 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.

Table A7 Randomization Inference Based
P-values

	Table 1 Panel A p-values			
	Q1	Q2	Q3	Q4
p-values (Table 1)	0.764	0.001	0.031	0.042
RI p-values (Young)	0.782	0.006	0.058	0.058
RI p-values (Hess)	0.752	0.005	0.061	0.112

This table shows p-values calculated with and without using randomization inference exercises for the results shown in Table 1 Panel A (Referendum 2017). 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 A8
Treatment Effect on Vote Share and Voter Turnout:
2017 Referendum With and Without Controls

	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	0.675	0.675	0.872	0.872
N Ballot	3992	3992	3992	3992
N Nbhd	550	550	550	550
R squared	0.673	0.785	0.069	0.401
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. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections.

Table A9 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	0.523	0.523	0.635	0.635	0.713	0.713	0.819	.819
N Ballot	919	919	983	983	1058	1058	1032	1032
N Nbhd	138	138	137	137	138	138	137	137
R squared	0.016	0.284	0.040	0.416	0.015	0.410	0.019	0.665
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. I show the estimated treatment effect within each strata (Q1-Q4). Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Standard errors are clustered at the neighborhood level.

Table A10
Treatment Effect on Vote Share Overall and by Quartile

Panel A	General 2018				
	Overall	Q1	Q2	Q3	Q4
Treatment	-0.002	-0.016	-0.037	0.016	0.007
	(0.006)	(0.018)	(0.012)	(0.007)	(0.004)
Mean	0.652	0.512	0.604	0.682	0.803
N Ballot	4406	1015	1093	1160	1138
R squared	0.753	0.260	0.423	0.419	0.637
Panel B	Municipal Mayor 2019				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.007	-0.007	-0.016	0.018	0.019
	(0.008)	(0.020)	(0.017)	(0.013)	(0.008)
Mean	0.595	0.463	0.544	0.614	0.753
N Ballot	4793	1096	1191	1274	1232
R squared	0.713	0.142	0.157	0.239	0.641
Panel C	Municipal Councillors 2019				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.005	-0.009	-0.017	0.017	0.016
	(0.007)	(0.020)	(0.014)	(0.012)	(0.007)
Mean	0.604	0.471	0.554	0.623	0.761
N Ballot	4793	1096	1191	1274	1232
R squared	0.726	0.145	0.208	0.280	0.651
N Nbhd	550	138	137	138	137

The dependent variable for all elections is the ballot box-level vote share for the opposition parties. I show the estimated treatment effect across all strata ("Overall") and within each strata (Q1-Q4). Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Strata fixed effects are included in the regression for "Overall." Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level.

Table A11
Treatment Effects on Vote Share by
Quartile and Campaign

Panel A	General 2018			
	Q1	Q2	Q3	Q4
Policy Outcomes	0.006	-0.037	0.019	0.004
	(0.028)	(0.012)	(0.010)	(0.004)
Checks & Balances	-0.038	-0.037	0.013	0.011
	(0.025)	(0.017)	(0.007)	(0.006)
Mean	0.512	0.604	0.682	0.803
N Ballot	1015	1093	1160	1138
R squared	0.267	0.423	0.420	0.637
PO=CB p-value	0.231	0.989	0.637	0.354
Panel B	Municipal Mayor 2019			
	Q1	Q2	Q3	Q4
Policy Outcomes	-0.017	0.011	0.015	0.016
	(0.026)	(0.022)	(0.021)	(0.009)
Checks & Balances	0.003	-0.043	0.022	0.021
	(0.027)	(0.016)	(0.010)	(0.011)
Mean	0.463	0.544	0.614	0.753
N Ballot	1096	1191	1274	1232
R squared	0.143	0.170	0.239	0.641
PO=CB p-value	0.578	0.045	0.745	0.714
Panel C	Municipal Councillors 2019			
	Q1	Q2	Q3	Q4
Policy Outcomes	-0.015	0.004	0.013	0.013
	(0.027)	(0.017)	(0.019)	(0.007)
Checks & Balances	-0.003	-0.038	0.021	0.019
	(0.027)	(0.018)	(0.009)	(0.009)
Mean	0.471	0.554	0.623	0.761
N Ballot	1096	1191	1274	1232
R squared	0.146	0.216	0.280	0.651
PO=CB p-value	0.742	0.075	0.695	0.568
N Nbhd	138	137	138	137

The dependent variable for all elections is the ballot box-level vote share for the opposition parties. I show the estimated treatment effect by stratum (Q1-Q4). Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level.

Table A12
Treatment Effects on Voter Turnout by Quartile and Election

Panel A	General 2018				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.002	0.004	0.007	-0.003	0.002
	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)
Mean	0.870	0.853	0.865	0.878	0.881
N Ballot	4406	1015	1093	1160	1138
R squared	0.323	0.341	0.310	0.209	0.248
Panel B	Municipal Mayor 2019				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.001	0.000	0.015	0.004	-0.008
	(0.003)	(0.006)	(0.007)	(0.005)	(0.003)
Mean	0.813	0.808	0.814	0.821	0.811
N Ballot	4793	1096	1191	1274	1232
R squared	0.358	0.373	0.394	0.339	0.331
Panel C	Municipal Councillors 2019				
	Overall	Q1	Q2	Q3	Q4
Treatment	0.002	-0.001	0.015	0.004	-0.007
	(0.003)	(0.006)	(0.007)	(0.005)	(0.003)
Mean	0.810	0.804	0.810	0.819	0.807
N Ballot	4793	1096	1191	1274	1232
R squared	0.349	0.367	0.374	0.335	0.325
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 by stratum. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Standard errors are clustered at the neighborhood level.

Table A13 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)	0.026	0.225	0.075	0.240
p-value	0.785	0.043	0.027	0.040
F-stat	0.08	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)	0.269	0.251	0.318	0.107
p-value	0.610	0.109	0.286	0.063
F-stat	0.26	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)	0.343	0.243	0.208	0.251
p-value	0.625	0.053	0.220	0.094
F-stat	0.24	3.80	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 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 all candidates other than Erdogan. In the 2018 general 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. Each column shows the estimation result by stratum. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Standard errors are clustered at the neighborhood level.

Table A14 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)	0.070	1.080	0.308	4.510
p-value	0.631	0.916	0.222	0.224
F-stat	0.23	0.01	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)	0.021	4.045	0.219	1.033
p-value	0.680	0.412	0.262	0.975
F-stat	0.17	0.68	1.27	0.00
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)	0.143	2.969	0.251	0.978
p-value	0.685	0.411	0.274	0.984
F-stat	0.17	0.68	1.21	0.00
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 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 all 2019 local elections 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. Each column shows the estimation result by stratum. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Standard errors are clustered at the neighborhood level.

Table A15 Vote share versus Voter turnout for PO campaign

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	0.278	0.124	0.029	0.396
p-value	0.694	0.029	0.120	0.817
F-stat	0.16	4.89	2.44	0.05
N Ballot	919	983	1058	1032
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	0.932	0.029	0.702	0.034
p-value	0.986	0.019	0.737	0.438
F-stat	0.00	5.64	0.11	0.61
N Ballot	1015	1093	1160	1138
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	0.840	0.063	0.648	0.383
p-value	0.957	0.020	0.708	0.410
F-stat	0.00	5.53	0.14	0.68
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 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 all candidates other than Erdogan. In the 2018 general election,

Table A16 Vote share versus Voter turnout for PO Campaign

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	0.074	1.041	0.115	1.290
p-value	0.814	0.977	0.401	0.535
F-stat	0.06	0.00	0.71	0.39
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	0.062	0.534	0.019	14.362
p-value	0.489	0.715	0.427	0.083
F-stat	0.48	0.13	0.63	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	0.040	1.045	0.071	20.068
p-value	0.587	0.979	0.535	0.034
F-stat	0.30	0.00	0.39	4.60
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 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 all 2019 local elections 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. Each column shows the estimation result by stratum. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Standard errors are clustered at the neighborhood level.

Table A17 Vote share versus Voter turnout for CB Campaign

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	0.172	0.374	0.349	0.339
p-value	0.450	0.277	0.420	0.014
F-stat	0.57	1.19	0.65	6.18
N Ballot	919	983	1058	1032
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	0.002	0.627	0.109	0.188
p-value	0.202	0.657	0.072	0.138
F-stat	1.64	0.20	3.30	2.23
N Ballot	1015	1093	1160	1138
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	0.045	0.474	0.271	0.191
p-value	0.092	0.411	0.091	0.075
F-stat	2.88	0.68	2.90	3.21
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 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 all candidates other than Erdogan. In the 2018 general election,

Table A18 Vote share versus Voter turnout for CB Campaign

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	0.162	1.089	0.476	0.065
p-value	0.656	0.913	0.176	0.194
F-stat	0.20	0.01	1.85	1.70
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	0.424	1.116	0.382	0.054
p-value	0.966	0.893	0.101	0.219
F-stat	0.00	0.02	2.73	1.53
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	0.538	1.216	0.373	0.051
p-value	0.947	0.822	0.086	0.042
F-stat	0.00	0.05	2.99	4.22
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 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 all 2019 local elections 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. Each column shows the estimation result by stratum. Strata are quartiles of the average vote share for the main opposition party in the 2015 elections. Standard errors are clustered at the neighborhood level.