Persistent Polarizing Effects of Persuasion: Experimental Evidence from Turkey

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Abstract

I experimentally test the hypothesis that exposing voters to non-state-provisioned information can counter electorally motivated state censorship. I examine the electoral effects of voter information campaigns implemented in the run-up to a referendum intended to weaken constraints on the executive in Turkey. The experiment was implemented at the neighborhood level and designed to estimate heterogeneous effects of information campaigns. Using administrative data on electoral results, I show that the treatment increased policy polarization over the referendum and was also associated with greater partisan polarization in presidential, general, and local elections over the next two years. The evidence supports the thesis that in a setting where voters disagree on whether strengthening the state is a good policy, reducing censorship can backfire and cause some voters to switch their party affiliation and support the incumbent. I conclude that because average measures mask both positive and negative treatment effects, 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, civil liberties and political rights around the globe are deteriorating.[1] Nearly half of democracies today are characterized by low levels of political accountability and civil liberties, including media censorship and the politicization of ideological divisions.[2] It has been well documented that states' disproportionate control over their citizens' information environment is an important channel to affect electoral support and consolidate 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 information from a non-state-owned source on voter behavior and ideology in Turkey. The information was delivered via two randomized door-to-door campaigns to approximately a quarter million voters before a constitutional referendum. The referendum was initiated by the incumbent leader and was high stakes: it was on an institutional reform that would weaken constraints on the executive branch. The information campaigns were organized by members of the largest party opposing the referendum and carried out by party volunteers.[3] I find that rather than uniformly countering electoral support for the incumbent, exposure to the same information campaigns increased policy polarization over the referendum. 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. Contrary to the campaign team's intentions, these longer-term results provide supporting evidence that the information campaigns caused some voters to switch their party affiliation and support the incumbent because they prefer a policy to strengthen the state.

Relative to liberal democracies where voters already have access to diverse information sources, information campaigns are usually presumed to have a large average effect on vote share in countries where media censorship is high. I show to the contrary that the information campaigns in Turkey had a zero average effect on vote share across the ideological distribution. 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. This outcome is instead consistent with theoretical studies showing that common exposure to the same signals can

[1] The backsliding of civil liberties and political rights across countries and in Turkey is shown in Figure A1.

[2] Using Polity data, Bidner et al. (2015) classify the majority of electoral democracies as minimalist and Mukand and Rodrik (2020) define close to half of democracies as illiberal.

[3] The opportunity to conduct this evaluation was an outcome of a non-partisan 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).

result in persistent ideological polarization (Acemoglu et al., 2016; Gentzkow et al., 2021). For example, baseline ideological divisions can result in different interpretations of the same information or views about the quality of the opposition party (information source). In an illiberal democracy, where there is high censorship and targeted state repression, this mechanism may be exacerbated. Overall, the results in this study suggest that i) the estimated effects of information campaigns, or media, on vote share in existing studies may be underestimates because potential backfiring effects were not accounted for in the empirical designs and ii) information transmission is an important mechanism in explaining persistent political polarization.

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 changes in democratic institutions are irregular and initiated by incumbent leaders during tumultuous periods. Therefore, it is rare to have 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 largest 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). The peak of 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. Since it was not possible to collect individual-level data, the vote share data served as a proxy for aggregate differences in priors or preferences that are systemic along partisan lines.[4] The intention to estimate heterogeneous treatment effects by stratum was specified in a registered pre-analysis plan.[5] This is important for inference because otherwise one can search over many possible partitions of the data

[4]There were context-specific constraints that prevented me from collecting individual-level data on beliefs. Kendall et al. (2015) is an example of a study evaluation a political information campaign that collects both administrative vote share data and self-reported individual-level data before and after a municipal election in Italy.

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

(Athey and Imbens, 2016).[6]

The same information campaigns were delivered 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.[7] 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 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 relatively stronger. The campaigns significantly decreased the "No" vote share by 3.4 percentage points (5.3 percent, p-value .001) in quartile 2 and, insignificantly, by .6 percentage points (1.1%, p-value .764) in quartile 1. Since the negative effects were in quartiles where the "No" vote share was relatively lower and the positive effects were in quartiles where the "No" vote share was relatively higher, the information campaigns resulted in policy polarization. The difference in average vote share between the second and third quartile increased by more than 5 percentage points (50%). While I cannot directly rule out a mobilization effect because of the possibility of 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 was important for uncovering the duration of the underlying heterogeneous effects. There were three elections that took place in the two years after the referendum, allowing me to estimate whether the polarizing effects of the information campaigns persisted. It is rare to have a series of elections following a single campaign experiment and to have administrative election data that are publicly available at the level of treatment. This circumvents the issue of having to conduct follow-up surveys and gather self-reported voting data, which can suffer from selective attrition. General and presidential elections were held on June 24, 2018, and a local election was held on March 31, 2019. I find that there was no weakening in the magnitude or significance of the treatment effects in either 2018 election. Using data from the local election, I find evidence that the polarizing effects of the information campaigns continued to persist in 2019. The magnitude of both the estimated negative and positive effects are similar to that of the previous elections and the negative effect remains statistically significant. Altogether, these findings provide suggestive evidence that policy views regarding a stronger state remained salient in regular elections and so the information campaigns also increased polarization along partisan lines.

Finally, I use recent developments in the literature to propose an explanation for the results.

[6]Duflo et al. (2020) also discuss how pre-specification can significantly strengthen results when heterogeneity in outcomes across subgroups is considered a key focus of a study.

[7]Both campaigns were also explicit in telling voters to choose "No" in the referendum to weaken the constraints on the executive branch.

The general design of the experiment rules out 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. see Gentzkow and Shapiro 2011; Martin and Yurukoglu 2017). In this study, the results suggest that voters either interpreted the same information differently or learned about the policy position of the opposition party in relation to their policy views. Accordingly, this caused a switch in their planned vote in the referendum and party affiliation in subsequent elections. Survey evidence shows that voters in Turkey have different views on why policy outcomes have deteriorated, ranging from external threats and terrorist attacks to poor incumbent performance. Voters can become polarized in response to the same information if they hold different views on why policy outcomes have deteriorated, which determines their ideological position on whether to place more or less constraints on the executive branch as the best policy option. Under this interpretation, voters face an identification problem (e.g. see 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). In turn, the opposition party unintentionally lost the support of moderate "secular" authoritarians, whose previous support had not been based on a shared ideological position, but possibly shared identity. 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 future elections.[8]

The remainder of this paper is organized as follows. I start by relating my study to the literature in Section 2. In Section 3, I provide background information on the ideological divide in Turkey and the administrative data I use on elections prior, during, and after the referendum. Section 4 presents the experimental design and the results. In Section 5, I discuss various explanations of the results. Section 6 concludes.

2 Literature Review

Partisan information campaigns are predicted to have larger effects on vote share in countries with weaker democratic institutions (Enikolopov et al., 2011). This is because information availability is limited and so voters are more likely to be exposed to new information in the campaigns. In the present study, I find that the persuasion rates of the information campaigns are substantial. In contrast, in the U.S., a relatively liberal democracy, a meta-analysis of 49 information campaign experiments found that the campaigns had a zero average effect on vote share (Kalla and Broockman, 2018). Presumably, Kalla and Broockman (2018) find no effect because voters are already sufficiently informed about the content of the campaign messages. Other possible explanations are that the studies were not designed to test for polarization, despite growing ideological polarization in the U.S. over the past several decades, or, due to a lack of administrative data, suffered from self-reporting bias, which can be different across treatment and control groups. In France, Pons (2018) finds

[8]In the political science literature, this type of voter has been described as "authoritarian." (Stenner, 2005)

that the effect of a precinct being randomly assigned to door-to-door canvassing increases changes vote share, but the estimated effects, which may be underestimates if there were unobserved polarizing effects, are smaller than in the present study.

A lesser explored hypothesis is whether information transmission is more likely to increase political polarization in illiberal democracies. In some models of persistent disagreement, agents have a different understanding about the distribution of signals that map to each state of the world and interpret the same information differently (e.g. see Acemoglu et al. 2016; Gentzkow et al. 2021). In countries with weak democratic institutions this source of polarization may be exacerbated because of the salience of policies that are difficult to learn about. For example, Piketty (1995) applies this idea of "costly experimentation" (Rothschild, 1974) in the context of persistent disagreement over restributive policies. Alesina et al. (2018) reference this literature in explaining why only left-wing voters, and not right-wing voters, self-report more support for redistribution after learning that they have underestimated social mobility in society. In countries like Turkey, voters do not have an opportunity to self-experiment with policies and learn about policy outcomes under different regimes (e.g. changes in tax rates or health care). For example, in the referendum and subsequent elections, the evidence suggest that voters are determining whether a stronger state is a good policy, which is difficult to learn about. Moreover, in these models, factors like selective learning from direct experience (e.g. arrest of someone innocent or guilty), the degree of bias in the information source, and endogenous trust in the information source also affect polarization. Therefore, different interpretations of policy outcomes may be starker in a historically censored information environment with longstanding and politically salient identity based divisions. For example, in contrast to Alesina et al. (2018) who study policy preferences in liberal democracies, I find significant effects among both groups of voters along an ideological spectrum.

The majority of empirical studies find that exposure to the same political information campaign or media outlet also has a uniform effect, rather than a polarizing effect, on vote share or voter behavior.[9] Consistent with the hypothesis outlined above, two exceptions are studies that evaluate this relationship in countries with weak democratic institutions (Adena et al., 2015; Peisakhin and Rozenas, 2018). For example, Peisakhin and Rozenas (2018) use quasi-experimental evidence to show that in areas with strong average pro-Russian priors the higher availability of Russian analog television increased vote share for parties with a "pro-Russian" agenda. The reverse was true in areas with weaker average pro-Russian priors. In contrast, in the U.S., DellaVigna and Kaplan (2007) find that Fox News uniformly increased vote share for the Republic Party and that this effect was stronger in areas that had higher Democratic support. They do not find any backfiring effect. In starker contrast to

[9]In the literature on media and persuasion, exposure to certain media outlets is modelled as consumption of ideological slant (e.g. see DellaVigna and Kaplan 2007) and consumers have preferences for media that is 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; although, the degree of selective exposure is found to be small (Gentzkow and Shapiro, 2011; Flaxman et al., 2016). While the existing empirical analysis in this literature finds 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.

the present study, Levy (2020) shows that exposure to counter-attitudinal news can decrease negative attitudes toward the opposing political party in the U.S. Though, the study does not find any effects on political opinion and the effects are only observed two weeks after the intervention. In contribution to this literature, I provide experimental field evidence that exposure to the same information outlet increases ideological polarization in a country with weak democratic institutions, which corroborates the results found by Adena et al. (2015) and Peisakhin and Rozenas (2018) using quasi-experimental methods. Moreover, using administrative data on vote share, rather than self-reported attitudes or opinions, I show that the effects persist despite the fact that voters were exposed to the information campaigns only once and the information content was more neutral.

The results of the present study have important implications for political accountability. Generally, increasing competition in a market of partisan information sources is considered fundamental to political accountability. Voters can form more accurate beliefs about the relative performance of politicians and change their support accordingly (Becker, 1958; Downs, 1957).[10] In the context of illiberal democracies, this research stream has mostly been restricted to quasi-experimental studies that analyze whether exposure to state media increases electoral support for the incumbent (Adena et al., 2015; Enikolopov et al., 2011; Knight and Tribin, 2019).[11] The present study directly contributes to our understanding of whether increased competition in the provision of oppositional information uniformly counters voter support for the incumbent or further polarizes voters.[12] The findings are consistent with a relevant research stream arguing that under certain assumptions a more competitive information market can also increase ideological polarization (Mullainathan and Shleifer, 2005; Gentzkow and Shapiro, 2006; Gentzkow et al., 2021).

3 Background

3.1 Democratization, Identity, and Polarization in Turkey

The institutional reforms after the founding of Turkey fomented the primary current ideological division in society. The founder, and military general, Mustafa Kemal Atatürk, enacted top-down and sweeping reforms that reduced religious expression, excluded Islamic politics, and imposed secularism among a largely pious and rural population. Despite the recognized gains from Atatürk's modernizing initiatives, these reforms created a political and cultural divide between liberal secular and religious conservative populations, or rather, elites versus

[10] Coase (1974), Strömberg (2001), Besley and Burgess (2002), Djankov et al. (2003) and Besley and Prat (2006) argue that voters will have more accurate beliefs, leading to greater political accountability, when the media market is competitive.

[11] An exception is a study by Chen and Yang (2019).

[12] There is also an extensive related literature on the impact of non-partisan information campaigns or 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) and a series of non-partisan experimental information campaigns conducted jointly in six countries by researchers from Evidence in Governance and Politics (Dunning et al., 2019). The effects of these information campaigns on political accountability have been mixed; the studies have either found that the information campaigns decrease support for the poor performance of incumbents or they find a null result.

non-elites.[13]

The exclusion of Islamic politics was successful for decades until the deliberate and slow emergence of the Justic and Development Party (AK Party) under the leadership of Erdoğan (Tuğal, 2019). As discussed by Tuğal (2021), the AKP 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 took the opportunity to position themselves as opposing the "secular elites", who had contempt for the uneducated and rural masses. Throughout the years, Erdoğan would often insert his shared identity with "Black Turks" as opposed to "White Turks" in his speeches (Acemoglu et al., 2013) and refer to his followers as the "real people" (Temelkuran, 2019, p.20) against the "establishment" or "elite" (Aydin-Düzgit, 2019).

Beyond the use of divisive language, the AK Party also increased repression of the opposition over the years and media censorship. This trend was associated with a period of increased political polarization as shown in Figures A2 and A3.[14] Consistent with episodes of democratic backsliding in other countries, the Turkish state has 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). These threats were salient to voters. There was an unprecedented level of terrorist attacks (Figure A4), the restart of civil conflict and state repression in Kurdish-majority areas, and the coup attempt in July 2016 during the period in which political polarization increased. 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.

The timing of the referendum was related: the AK Party proposed the constitutional amendments and transition to a presidential system just five months after the coup attempt. The AK Party's main platform before the referendum was that the proposed insitutional changes would bring more stability and improve national security (Esen and Gumuscu, 2017). This platform relied on voters' views as to whether worsened national security, and concurrent deteriorating economic conditions, was out of the control of the state or not. Political polarization on this issue before the referendum is reflected in Figure A5. Along ideological lines, voters were split on whether deteriorating economic conditions were attributable to the state.

[13]The religious reforms ranged from replacing sharia courts with a secular civil code to placing religious institutions under state control. Daily cultural life was also affected; for example, language reforms required the call to prayer to be in Turkish rather than Arabic. For brief summaries about the roots and evolution of this ideological cleavage see Aydin-Düzgit (2019) and Acemoglu and Robinson (2013), among others.

[14]This measure of political polarization is defined by V-Dem as differences of opinions on major political issues in society (Coppedge et al., 2021).

3.2 Elections from 2015 to 2019

A. 2015 General Elections

The stratification variables used for randomization are from two 2015 general elections. The first general election was held in June and it marked the first time in which the AK Party did not win a majority in parliament since the party was founded. The AK Party failed to form a coalition and so a snap general election was held in November 2015. In this snap election, the AK Party was able to secure a majority in parliament. The stratification variable is the average neighborhood-level vote share from these two elections. At the time of the 2015 general elections, there were four parties with representation in parliament: the AK Party, the CHP, the Nationalist Movement Party (MHP), and the Peoples' Democratic Party (HDP). The AK Party and the CHP comprised approximately 80% of the vote share in the 2015 elections in Izmir, where this study took place. For this reason, the average vote share for the CHP from these two elections, which is equivalent to the vote share differential between the two main parties, was used to stratify the randomization. There is a low correlation between the vote shares for the MHP or the HDP and the AK Party in Izmir, whereas there is a strong negative correlation between the vote shares for the CHP and the AK Party.

B. April 2017 Referendum

The main outcome data used analyzed in this study are voter support and turnout for the constitutional referendum on switching from a parliamentary system to a presidential system and on eighteen amendments to the constitution.[15] In March 2017, the European Commission for Democracy Through Law provided a detailed account on the implications of the proposed constitutional amendments for the system of checks and balances, separation of powers, and independence of the judiciary in Turkey (Commission, 2017). The Commission warned the 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." Figure A6 illustrates that these views are also reflected in indices that describe features of democracy (Coppedge et al., 2021). 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). In addition, it provided ambiguous procedures by which a president could serve up to three five-year terms. With respect to Erdogan, the successful adoption of the constitutional amendments permit him to potentially extend his position of power to 2033. Erdoğan served as prime minister from 2003 to 2014, stepping down just before his term limit in order to be elected as president in 2014.[16] 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.

[15]Further detail on the amendments can be found in Commission (2017) and Esen and Gumuscu (2017).

[16]In 2007, Erdoğan had called for a referendum that would change a law and allow the president to be nationally elected in 2014. This referendum passed and so Erdoğan became the first nationally elected president of Turkey in 2014 and was able to retain a leadership position before his term as prime minister ended.

C. 2019 Local Elections

The outcome data to measure the longer term effects of the information campagins are from the general and presidential elections that both took place on June 24, 2018 and the local elections that were held on March 31, 2019. In these elections, I calculate the incumbent vote share as including the vote share for both the AK Party and the MHP because they formed an official electoral alliance in February 2018. After the announcement of this alliance, some members officially split from the MHP and formed a separate party, the Iyi Party, which I classify as an opposition party in the data. In the local elections, metropolitan mayors, district municipal mayors, and municipal councilors were elected. I estimate the persistent effect of the information campaigns on vote share and voter turnout for each of the local elections separately.

See Figure A7 for a timeline of the elections and important events before and after the information campaigns were implemented.

4 Voter Campaign Experiment

4.1 Campaign Content

The intention of the door-to-door information campaigns was to increase the "No" vote share in the April 2017 referendum, on whether to weaken constraints on the executive branch. In one treatment arm, voters were exposed to information on past policy outcomes (PO campaign). The campaign included information on deteriorating economic conditions and increased terrorist activity under the current leadership since the presidential election in 2014. In that election, Erdoğan extended the duration of his political power by transitioning from his position as prime minister to president and starting the process of consolidating power under the presidency. 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. The campaign did not include any party-specific content; rather, the message was that the referendum would affect everyone regardless of political alignment and would involve institutional change. Overall, both information campaigns included content that was not easily accessible in the media.

In treated neighborhoods, messages were conveyed to voters both verbally, if they opened their door, and in a pamphlet. The control neighborhoods were not exposed to either campaign and treatment neighborhoods were randomly assigned to one of the two information campaigns. The original text of the pamphlets for each campaign can be seen in Figures 1 and 2. The canvassers also received training from the campaign team on how to deliver the same 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. The funding and details of the content of the campaigns were determined by a campaign manager and staff from the opposition party. The overall strategy was selected by

individuals from the opposition party and was not a part of a centralized party campaign.

4.2 Sampling, Empirical Design, and Implementation

A. Sampling and Empirical Design

The door-to-door information campaigns were implemented in Izmir, the third most populous province in Turkey.[17] Figure A8 separately shows the neighborhood-level distribution of the "No" vote share for the whole country and for the control group sample. It illustrates that the experiment was conducted in an opposition stronghold, but, in terms of external validity within Turkey, there is substantial overlap with the distribution across the country. In general, the opposition party has had longstanding high electoral support in Izmir. For the purpose of implementation, this 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 difficult for the campaign team because of a general environment of suspicion and potentially high repercussions from being reported to the authorities.

Randomization was at the neighborhood level and was stratified by quartiles of past average vote share for the main opposition party. The distribution and quartiles of the stratifying variable are shown in Figure 3. The average vote share was calculated using two general elections that were held in 2015.[18] I use 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. As a result, the 2015 vote share for the main opposition party is strongly correlated with the "No" vote share in the referendum, as shown in Figure A9. The 2015 vote share for the main opposition party explains approximately 78% of the variation in the "No" vote share across quartiles. The other two opposition parties comprise a small proportion of the overall vote share in the sample and the vote share for either party has a low correlation with the incumbent party's vote share in Izmir, as shown in Figure A10. For statistical power and balance it is important to stratify on a variable that is strongly correlated with the outcome variable of interest (Bruhn and McKenzie, 2009).

In terms of statistical inference, a number of challenges specific to an illiberal democracy affected the planned implementation of the campaigns and the sampling selection of neighborhoods prior to the randomization procedure. 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 opened their doors and engaged with the canvassers at the neighborhood level.[19] In-person conversation is considered to be one of the most effective

[17] There are 81 provinces in Turkey.

[18] The first election was in June 2015 and a snap election was held in November 2015.

[19] The experimental design did not affect the total number of households the campaign team planned to target. This was determined by the number of volunteers available to them. However, there was an explicit discussion between myself and the campaign team that there would be more statistical power in

methods to affect voter behavior (Pons, 2018; Green and Gerber, 2015).[20] Reaching more neighborhoods, but only partially covering them, would have reduced the statistical power of the experiment.

The second issue regarding the capacity constraint was also an important factor in considering the statistical power of the experiment. Not only did the compliance rate within each neighborhood need to be sufficiently high, but a minimum number of neighborhoods needed to be reached. 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 the most recent general election 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.[21]

In the end, the sampling selection, prior to randomization, included 14 of 30 districts and 550 out of 1294 neighborhoods in Izmir. Of the 550 neighborhoods, 100 were assigned to the treatment group and 450 to the control group. 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 100 neighborhoods. 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 A14 and A15 show the geographic information for the experimental sample and the spatial correlation in the dropped districts.

Finally, there was uncertainty about how many neighborhoods the canvassers could reach and it was not possible to do a pilot in this environment. To address this uncertainty, I randomly assigned a treatment order. Following Nickerson et al. (2006), any neighborhood not covered by the campaign would be rolled into the control group. However, the randomized ordering does not enter the empirical analysis because the canvassers attempted all 100 neighborhoods assigned to the treatment group.

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the experiment if they targeted all households in a neighborhood instead of maximizing the number of neighborhoods and limiting the number of households visited.

[20] Pons (2018) shows that door-to-door canvassing is effective for vote choice and Green and Gerber (2015) summarize evidence from a number of studies suggesting that it is effective for turnout.

[21] The campaign team also asked that neighborhoods in which neither of the two main parties had at least 30% in the prior general election were dropped prior to randomization. This resulted in dropping only 15 neighborhoods.

B. Implementation

I also observed whether the implementation of the information campaigns varied by quartile or whether there was selective canvassing within neighborhoods. After geocoding every street in each neighborhood, the canvassers were provided with an optimal route for each neighborhood and they recorded the number of people they completed a conversation with per street. This procedure allowed me to ensure that 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 faced threats (aggressive behavior, warnings to call the police, etc.). In the 80 neighborhoods where the volunteers did not face 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.

4.3 Data, Empirical Strategy, and Pre-analysis Plan

A. Data

Voter data for the 2019 local election, 2018 general election, 2018 presidential election, 2017 referendum, and the two 2015 general elections were scraped from the Supreme Election Council's website.[22] 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

Using the OLS specification below (Equation 1), 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. 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 2018 presidential election was to elect a president. The analogous outcome variable for vote share in that 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 is the vote share for the opposition parties.[23] T_{nq} is an indicator

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

[23] 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.

for whether the neighborhood was assigned to one of the two treatment groups and δ_q are strata (quartile) fixed effects. Randomization into the control group or treatment group for each of the two information campaigns was stratified by quartiles of the CHP's average vote share for the two elections in 2015 (see Figure 3). Within each quartile there are 25 treatment neighborhoods and 112 or 113 control neighborhoods; in total, 48 neighborhoods were covered in the PO campaign (12 in each quartile) and 52 were covered in the CB campaign (13 in each quartile).

$$Y_{bnq} = \alpha_1 + \beta_1 T_{nq} + X'_{nq}\lambda_1 + \delta_q + \epsilon_{bnq} \tag{1}$$

C. Pre-analysis Plan

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.[24] I also pre-specified a test for the difference in the effect between the PO and the CB campaigns. The vector of control variables measuring past voter data from each election in 2015 in equation 1 (X'_{nq}) were also pre-specified. 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 in Table A2. I chose these control variables because their equivalent from past elections explain a significant amount of variation in voter turnout and vote share of future elections. Including them in the regression is important for statistical power. In contrast, control variables that are uncorrelated with the outcome variable can reduce power in small samples and within each quartile there are only 25 neighborhoods in the treatment group (Bruhn and McKenzie, 2009).

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).[25] There are efficiency gains from including population weights in the neighborhood level analysis if there is population-size-related heteroskedasticity. This happens when the variance of a group average outcome variable is correlated with group size. The point estimates can also be different if there are heterogeneous treatment effects by population size (Solon et al., 2015). I find that both are true when comparing the neighborhood-level analysis with population weights to that without population weights. When comparing the ballot box level to 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

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

[25] 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.

with the number of voters in a neighborhood, although it is a coarser measure of population size. I present the results for each empirical strategy because it is illustrative to understand the implications of incorporating population size in the analysis (Solon et al., 2015). Most importantly, however, the results using all approaches show that the information campaigns increased policy polarization.

4.4 Voter Campaign Experiment: Results

A. Average Treatment Effect versus Heterogeneous Effects on Vote Share

Figure 4 and Panel A of Table 1 shows the results from estimating the ITT using equation 1. Figure 4 illustrates the positive correlation between the quartiles of the 2015 average vote share for the opposition and the 2017 "No" vote share. Across quartiles (labeled "Overall"), I find that the combined average treatment effect of the two campaigns on the "No" vote share across quartiles 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. In 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 the 2015 vote share for the opposition was relatively higher and decreased the "No" vote share in quartiles where the 2015 vote share for the opposition was relatively weaker, the information campaigns increased policy polarization on average. 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 exercizes 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 exercizes, 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 for quartile 1 is high regardless of the program that is used (above .7). These results are summarized in Table A4.[26] 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

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

the pre-specified covariates. Since I specified estimating heterogeneous treatment effects by quartile and by campaign (CB and PO) in the pre-analysis plan, I 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 also show the estimated effects by campaign and within each quartile with and without covariates in Table A8. It should be noted that when designing the experiment, and as indicated by the R squared value in Table A8, it was understood that the statistical power to estimate a treatment effect within each quartile depended on including the pre-specified covariates. So, the main results are all based on regressions that include the pre-specified covariates.

B. Persistence

Table 1 also shows the persistent effects of the information campaigns by quartile in 2018 and 2019. I show the longer term results for the 2018 presidential election and the 2019 metropolitan mayor election. The equivalent results for the 2018 general election and the other two 2019 local elections are shown the in the appendix. Table 1 shows that the magnitude and statistical significance of the estimated effects by quartile in 2018 is nearly equivalent in comparison to 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 results of testing 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 mayor and municipal councillors. 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.

C. Voter Turnout

Table 3 shows the esimated effect of the information campaigns on voter turnout overall and by quartile in 2017, 2018, and 2019. The average voter turnout for the control group is 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 are 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. In these regressions, I divide the number of "No" votes by the number of registered voters instead of the number of valid votes to calculate the vote share. 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 the information campaigns on the numerators: 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. The confidence intervals of the estimated effect in quartile 1 are too large to make inference. 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 in the 2019 local election is lower than the other elections. So, it is possible that the information campaigns deterred lower turnout in the second quartile.

Figure A16 shows 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 turnout and the coefficients are close to zero. This is in contrast to the estimated treatment effects on vote share at different quantiles, as shown in Figure A17.

Overall, the results provide evidence that the effects of the information campaigns on vote share cannot entirely be explained by turnout. The average treatment effects overall and by quartile are precisely estimated and they are small. It is possible that there are differential effects within quartile. Unlike in other studies, this would require that voters supporting the opposition are mobilized and voter supporting the incumbent are demobilized in the third quartile and the reverse is true in the second quartile. I discuss this possibility in further detail in Section 5.

D. 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 ii) the treatment response is monotonic within quartile. The first assumption is conservative because in 20% of neighborhoods assigned to the treatment group, canvassers faced 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 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. Using the language of Jun and Lee (2021), the first assumption implies that the persuasion rate of the information campaigns is calculated under a "sharp persuasion design," as opposed to a "fuzzy persuasion design," under which data on treatment is jointly observed with assignment to treatment. Under the sharp persuasion design there is no difference between the actual treatment and the ITT. In general, a major issue in studies on persuasion is the accuracy in which treatment is observed, if at all. 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, this

study illustrates that it is a strong and unrealistic assumption for a variety of contexts.

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 without having been exposed to the persuasive message. Therefore, the point estimates are divided by the average "No" vote share among the control group in quartiles 1 and 2 and by the average "Yes" vote share among 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 in this study are higher 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. For example, the equivalent of θ_L for the door-to-door information campaign experiment analysed by Pons (2018) is approximately 1.2%.[27] More generally, the main contribution of this study is not to provide a precise estimate of a persuasion rate in a new context relative to other experiments on persuasion, but to show that the information campaigns have sizable effects on vote share. This is in contrast to the majority of campaign experiments in liberal democracies. More significantly, it cautions against using the common assumption of a monotonic treatment response in studies on persuasion by showing that the same information campaigns simultaneously, and persistently, dissuade some voters and persuade others.

5 Interpretation of Results

Using neighborhood level vote share data, the results show that the information campaigns increased policy polarization. Observing polarization at the neighborhood level implies that the marginal voter affected by the information campaigns, whether through persuasion or mobilization, is not randomly distributed across the sampled vote share distribution. Instead, the relative share of incumbent versus opposition voters is consequential and individual level beliefs or behavior are locally correlated. The source of this polarization can be explained by an array of existing theories that relate ideological polarization to individual-level Bayesian or non-Bayesian updating in response to common signals. Below, while non-exhaustive and limited to theories based on Bayesian updating, I outline a few potential mechanisms that match the results. I primarily focus on "attitude polarization," e.g. see Lord et al. 1979; Benoit and Dubra 2016 in which either the bias of the information source or the information itself increased disagreement over whether to support weakening constraints on the executive.

[27] This persuasion rate is derived from dividing the ITT point estimate reported in Table 3, panel B, column 3 in (Pons, 2018) of a precinct being allocated to canvassers by the fraction of voters who would have supported candidates other than Francois Hollande. I choose this persuasion rate because it is the comparable statistic. Note that the persuasion rate reported in the study is different and ranges from 9.5 to 12.7%.

Information from campaigns: 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. secularists versus Islamists) can arise from selective exposure to signals, for example.[28] 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, leading the first group to blame outside forces and to desire more power for the incumbent; and the second group to blame the incumbent and increase support for retaining constraints on the incumbent's power.[29] In both groups, voters switch from their intended vote and cross identity lines. Figure A5 shows these "switchers": voters who self-report with the incumbent (opposition) party, but whose views are aligned with the average voter supporting the opposition (incumbent) party. With this new information, attitude polarization increases and voters switch their vote in the referendum accordingly. In subsequent elections, voters also switch their affiliation to the party closest to their policy position. The results that I observe using neighborhood-level vote share can be mapped to this individual-level behavior only if voter priors are correlated within a neighborhood. For example, this implies that voters 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 (i.e. "the secular authoritarians" in Figure A5 are located in neighborhoods in the first and second quartiles indicated in Figure 3.)[30]

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 put more weight on the policy dimension of their utility function relative to the consumption value they get from 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 per-

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

[29]Figure A5 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 of why the value of the lira dropped. Opposition voters predominantly blamed the current leadership. Incumbent voters blamed external factors outside the control of the leadership, such as the coup attempt, the global economic crisis, and the U.S. presidential election.

[30]Correlation in priors is a natural assumption because voters are exposed to the same private signals, e.g. dominant views discussed in parks, or get direct feedback from one another e.g. innocent friend or guilty neighbor arrested.

suaded by the information (Bray and Kreps, 1987; DellaVigna and Kaplan, 2007). While this study rules out this explanation, it's possible that voters reacted to the biased source 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, consider 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 mobilized in quartiles 3 and 4 and supporters of the incumbent party mobilized in quartiles 1 and 2. However, I find that the information campaigns have a zero average effect on turnout by quartile (or octile, as shown in Figure A16). In addition, Izmir is an opposition stronghold (both in terms of the referendum and regular elections.) So, if the effects on vote share are explained by turnout then there must be differential mobilization within quartiles. This would require opposition voters to mobilize in quartiles 3 and 4, incumbent voters to demobilize in the same quartiles, and for the reverse to occur in quartiles 1 and 2. This would result in the observed polarization based on where there are relatively more incumbent or opposition voters on average. A potential explanation for this mechanism is peer effects. For example, voters in quartiles 3 and 4 who will vote for the opposition party regardless of the information campaign may still be affected in their motivation to influence their neighbors. As a result, we might expect different turnout effects between quartile 3 and 4, for example, which 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-owned information is considered to be an important channel to counter the consolidation of power in an illiberal democracy. This study uses experimental field evidence to show that exposure to information from a non-state-owned source can instead be polarizing. I use a stratified randomization design to directly test for a polarized electorate in a high-stakes context where voters were choosing whether to undermine democratic institutions. The average effect is zero, but this outcome masks both a significant increase and decrease in voter support for the referendum. The results are striking because the polarizing effects are driven by differences in reaction to the same information campaigns. Moreover, the polarizing effects persist for more than a year and the negative effect on the "No" vote share persists approximately two years later in a local election. Many empirical studies argue that trends of increasing ideological polarization are driven by voters self-selecting into different information sources. These results point to a more insidious explanation in this context: exposure to the same information delivered in-person increases polarization. To

the degree that truth is consensus, the results of this study suggest that the voters do not have a shared sense of reality.

Partisan campaign experiments in the U.S. have been effective in changing voter turnout, but not vote share (Kalla and Broockman, 2018). Most of these experiments do not pre-specify testing for opposing reactions to the campaigns along partisan lines, despite the phenomenon of increasing polarization in the U.S. The majority of existing studies also rely on self-reported voting outcomes. In addition, most studies on political persuasion through campaigns take place in a competitive information environment; therefore, voters may already have been sufficiently informed regarding the content of the campaign messages. Any of these factors could contribute to a null result. By contrast, Turkey is a context where there is high media censorship and identity-based divisions are presumably more salient. The results that I find instead build on findings reported in a study by Adena et al. (2015). The authors use quasi-experimental variation to show that exposure to the same state-owned radio source, a novel media outlet at the time, had uniform effects on voter behavior under a democracy, but polarizing effects once a dictatorship was consolidated in Germany.

Further research is needed on whether the results are driven by voters facing an identification problem: due to the salience of external factors, voters are not certain why conditions are bad. The same information campaign on poor policy outcomes can therefore increase ideological polarization based on heterogeneous voter views on whether external factors or the incumbent is to blame. Those who blame external threats choose to weaken constraints on the executive branch so that the incumbent is better able to protect them and the economy (Stenner, 2005).

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Figure 1: Pamphlet on Policy Outcomes

This is the pamphlet used in the PO campaign. It was created by members of the campaign team. The content of the pamphlet translates to the following:

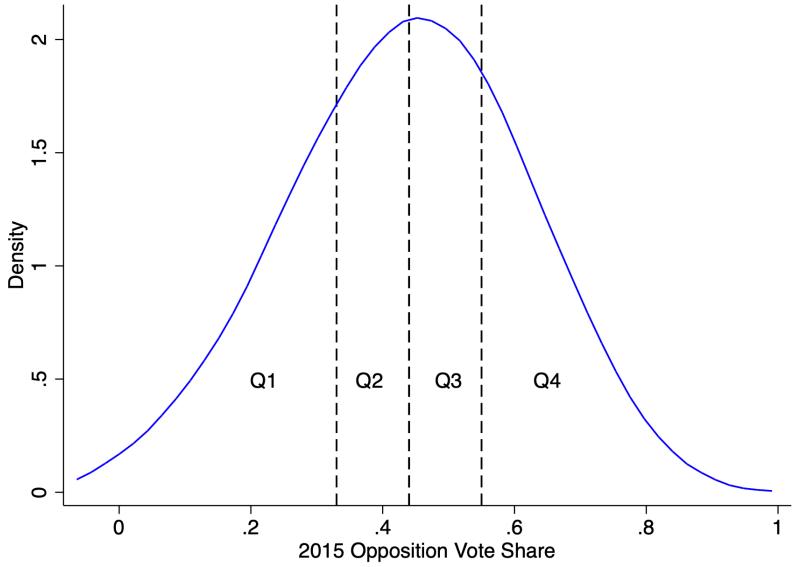
- Since 2014, there has been poor leadership
- Under which 1 million citizens have lost their job
- The per capita income has dropped by $1,000 in the past one year
- The economy has contracted and inflation has increased
- Turkey is losing under one leader
- Since 2014, unprecedented level of terrorist activity
- Terrorist organizations are more easily able to conduct attacks
- Security is weak
- More power should not be given if terrorism could not be reduced
- The headlines state: "#NO Turkey will win," "NO to poor economic policy," "NO to terrorism."

Figure 2: Pamphlet on Checks and Balances

This is the pamphlet used in the CB campaign. It was created by members of the campaign team. The content of the pamphlet translates to the following:

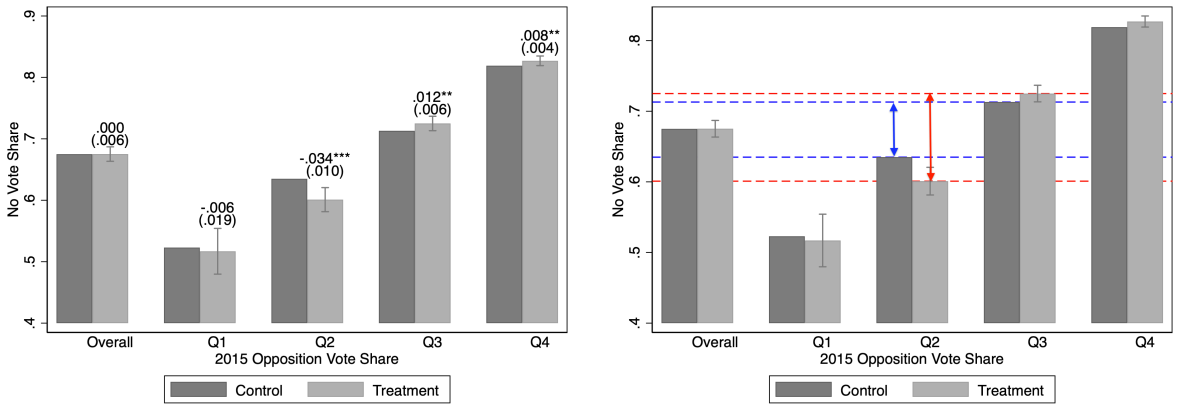
- If the constitution changes the elected president will have unlimited power
- The elected person will do what they want for 5 years
- Because they will not be held accountable by the elected parliament
- They will not be held accountable by the judiciary
- If the constitution changes the elected president will have complete power over the state
- The person sitting in the palace will replace the legislative, judiciary, and executive branches
- No one person deserves this much power
- This power can also be passed to someone that is not wanted
- That person can use the power for bad
- The headlines state: "#NO Turkey will win," "NO to a one person regime," "NO to palace order"

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.

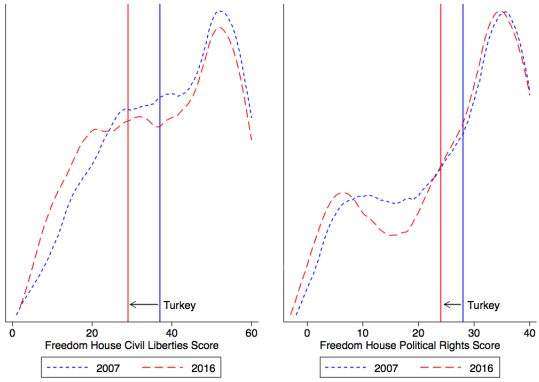
Table 3:
Treatment Effects on Voter Turnout by Quartile and Election

	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.

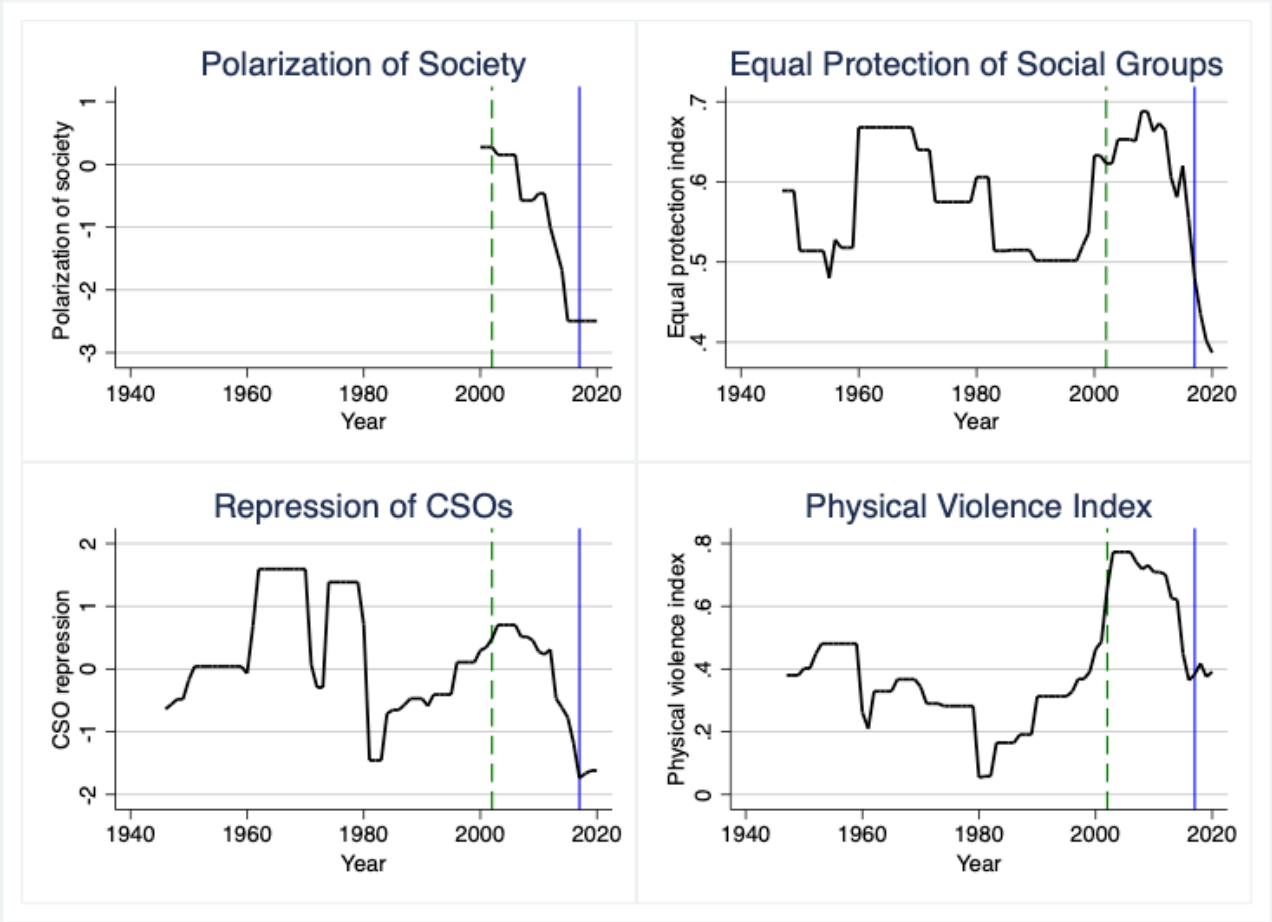
A For Online Publication: Appendix

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



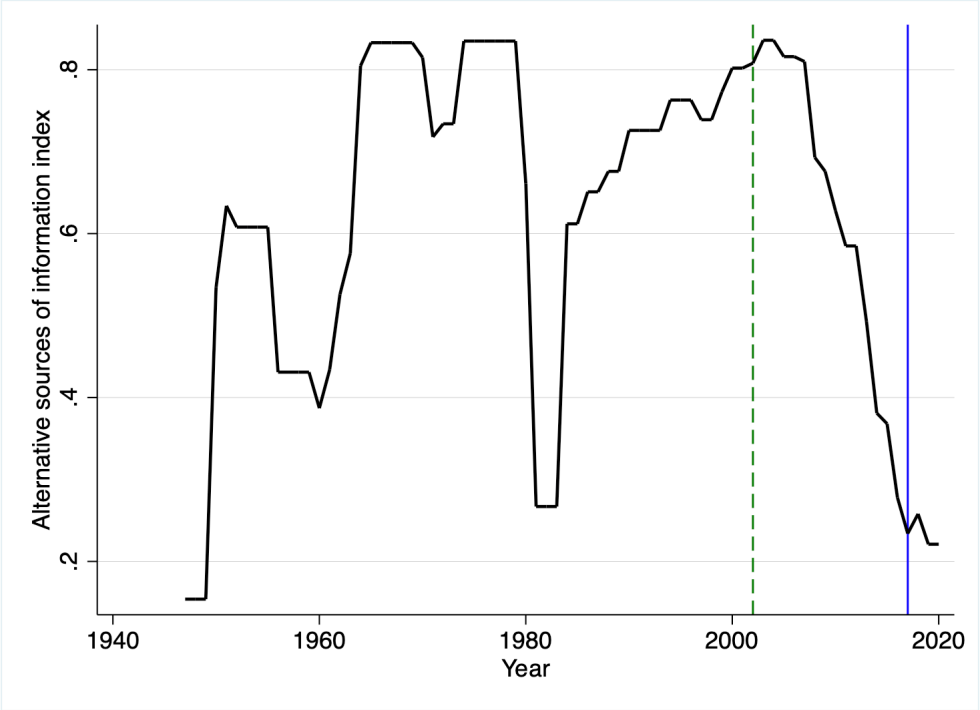
This figure shows the distribution of aggregate scores for civil liberties and political rights in 2007 and 2016 across countries. 193 countries are included in 2007 and 198 in 2016. The figure also shows Turkey's position within the distribution. There was a decline in both measures across all countries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

Figure A2: Polarization and State Repression



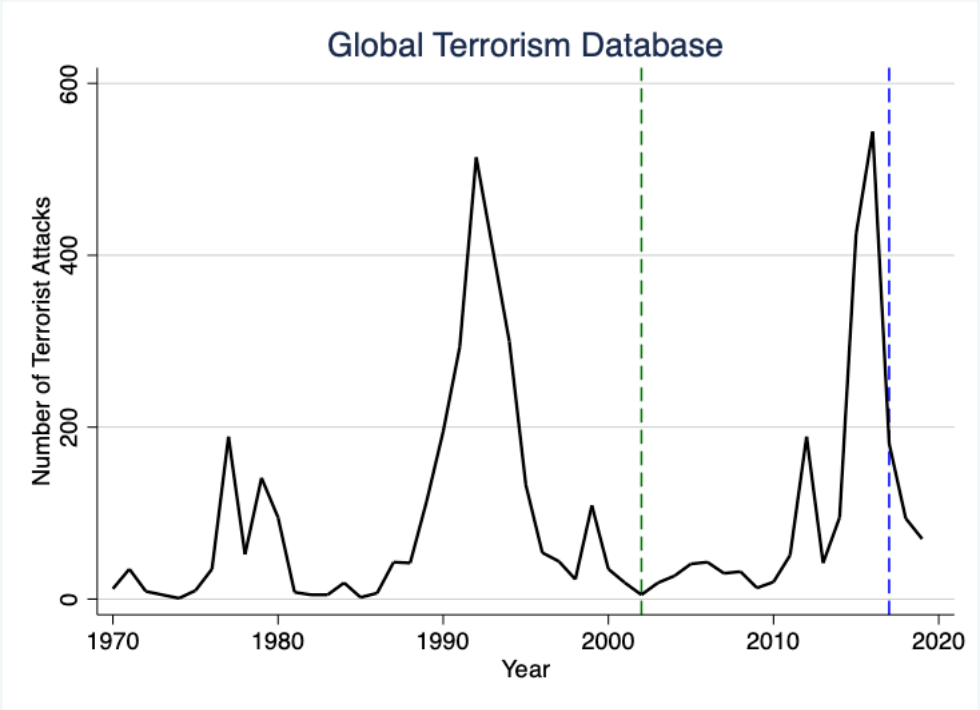
These figures show time series plots of indicators relating to polarization and state repression from the V-Dem database. Polarization is defined as differences of opinions on major political issues in society. Equal protection of social groups is defined as the protection of rights and freedoms across 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.
Source: V-Dem (Coppedge et al., 2021)

Figure A3: Access to Alternative Information



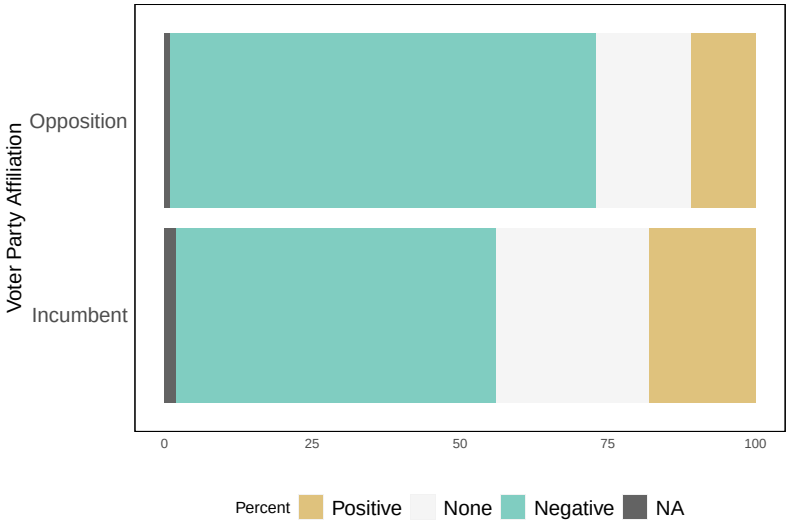
This figure shows a time series plot of an access to alternative information index from the V-Dem database. This index is defined as the extent to which the media is (a) un-biased in their coverage (or lack of coverage) of the opposition, (b) allowed to be critical of the regime, and (c) representative of a wide array of political perspectives. To provide an 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). 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 A4: Terrorist Attacks in Turkey

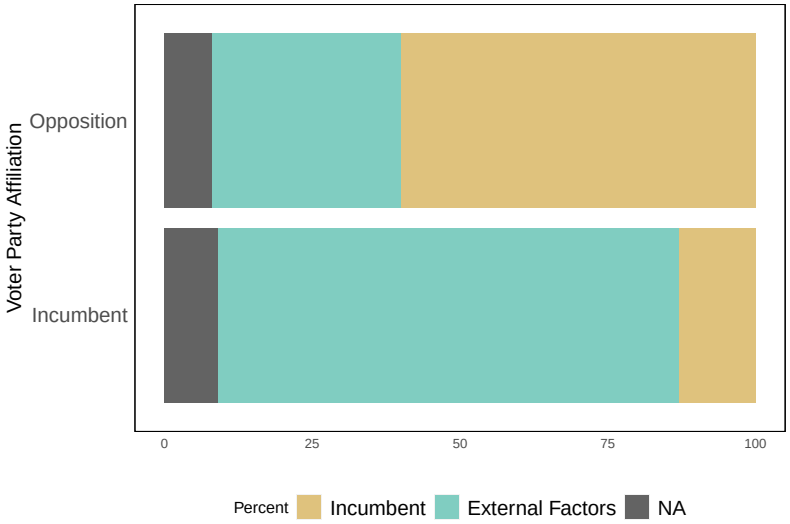


This figure shows the number of terrorist attacks in Turkey from 1970-2019. According to the Global Terrorism Database, a terrorist attack is defined as the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation. Observations are available starting in 1970.
Source: Global Terrorism Database (START, 2021).

Figure A5: Does the drop in the value of Turkish Lira have any impact on your personal life?

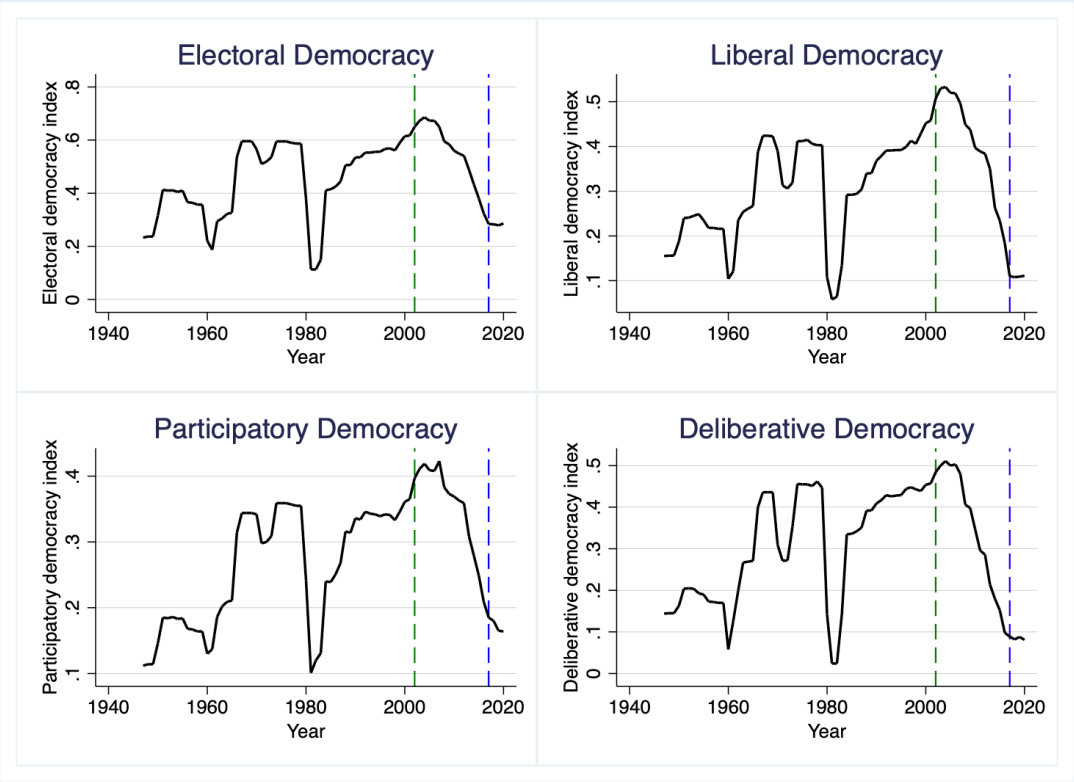


Who is most responsible for the latest devaluation of Turkish Lira?



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 low drop in the value 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.

Figure A6: 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 A7: Timeline

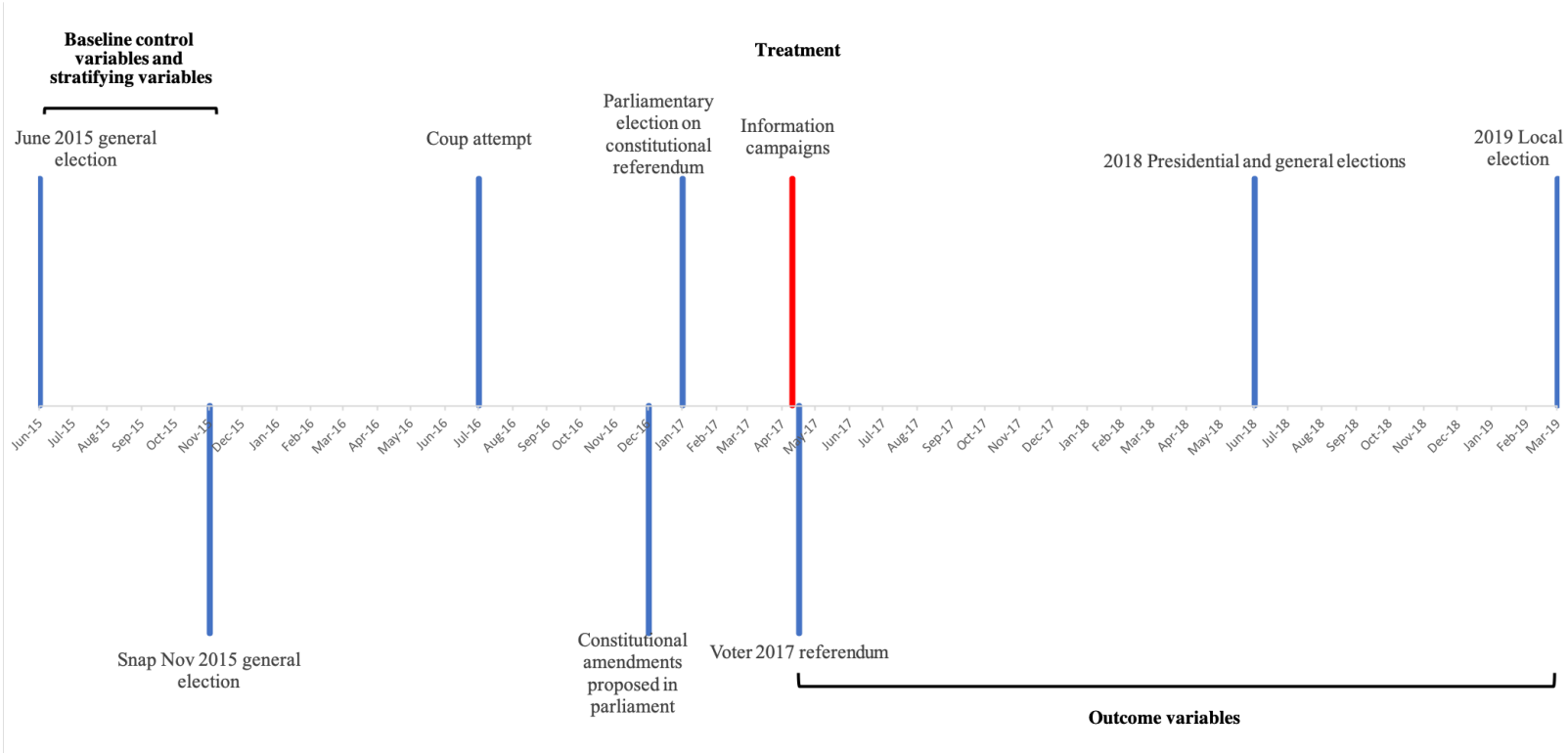
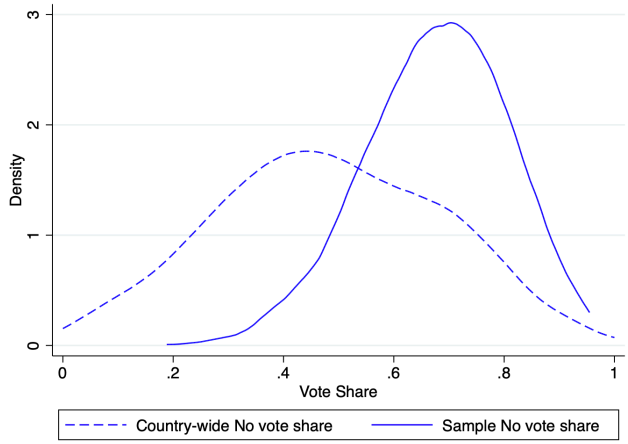
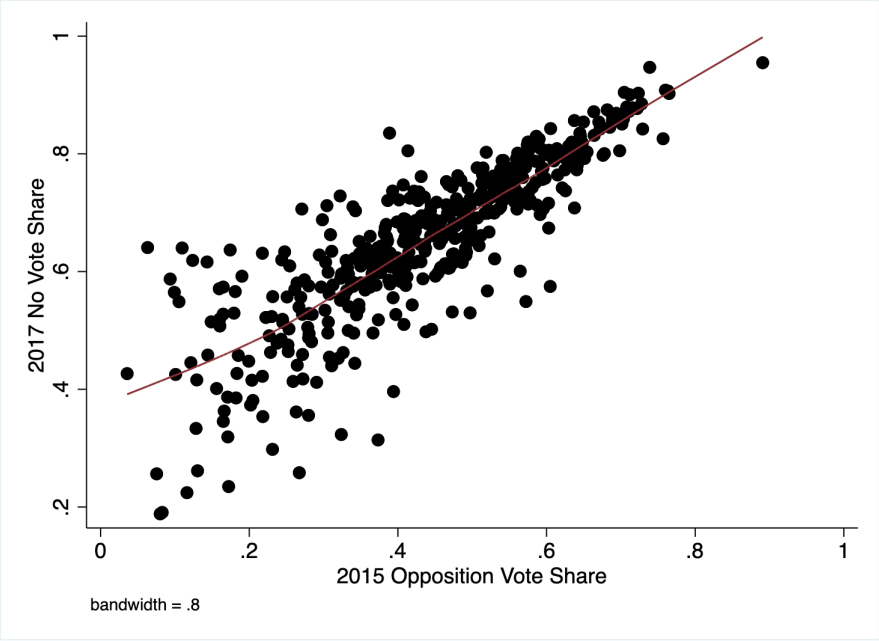


Figure A8: "No" Vote Share Distribution Across Country and Sample



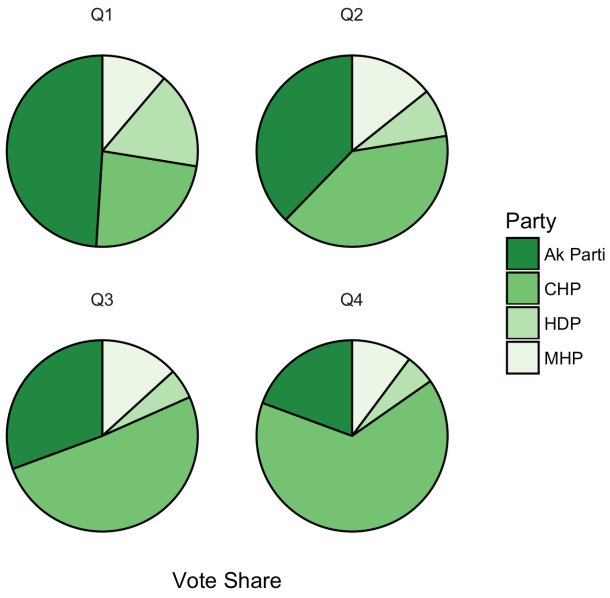
This figure shows the distribution of the neighborhood-level "No" vote share for Turkey in blue and for the experimental sample among the control group. The distributions are weighted by the number of registered voters in a neighborhood.

Figure A9: Correlation Between 2015 and 2017 Vote Shares



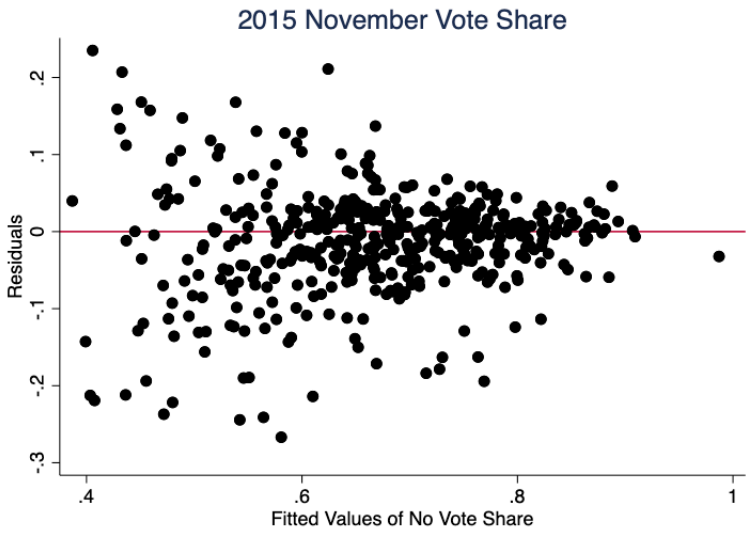
This figure shows the correlation between the vote share for the opposition party from the 2015 general elections and the "No" vote share in the control group of the experimental sample.

Figure A10: Vote Share Distribution of the November 2015 Election Results in Izmir



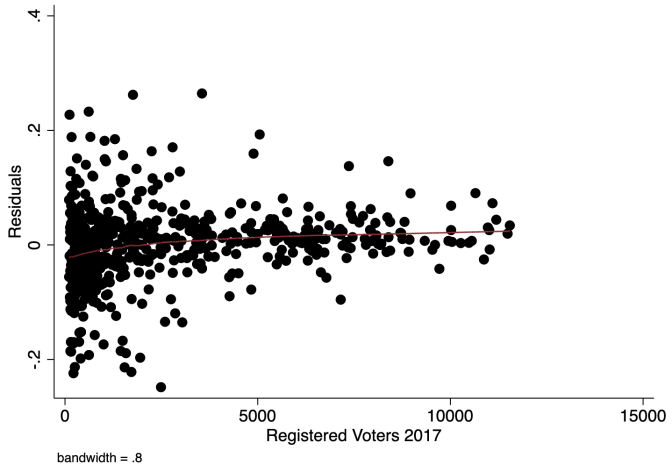
This figure shows the neighborhood-level vote share distribution for all parties in the 2015 November election in the experimental sample.

Figure A11: Residuals vs Fitted Values for Vote Share



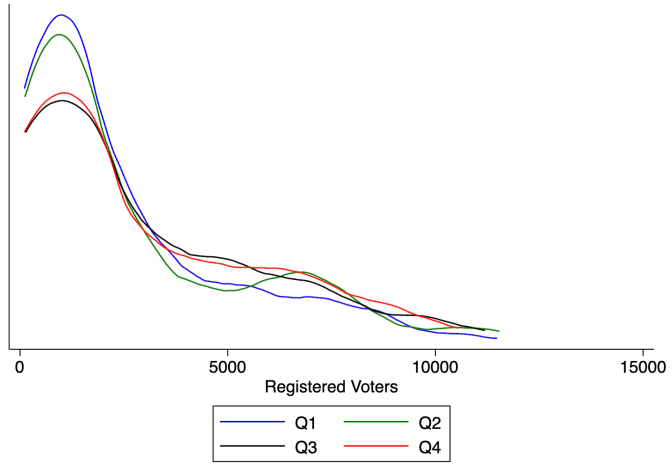
The residuals from regressing the "No" vote share on the November 2015 vote share for the opposition party is plotted against the fitted values. Observations are weighted by the number of registered voters in each neighborhood and only neighborhoods assigned to the control group are included.

Figure A12: 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 A13: Distribution of Neighborhood Size



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

Figure A14: Map of Sample Within Turkey and with Province Borders

Figure A15: Map of Sample Within Izmir and with District Borders

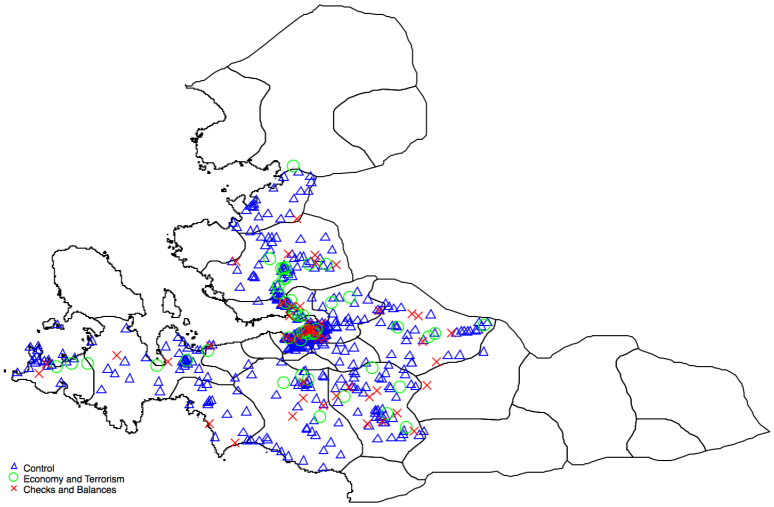
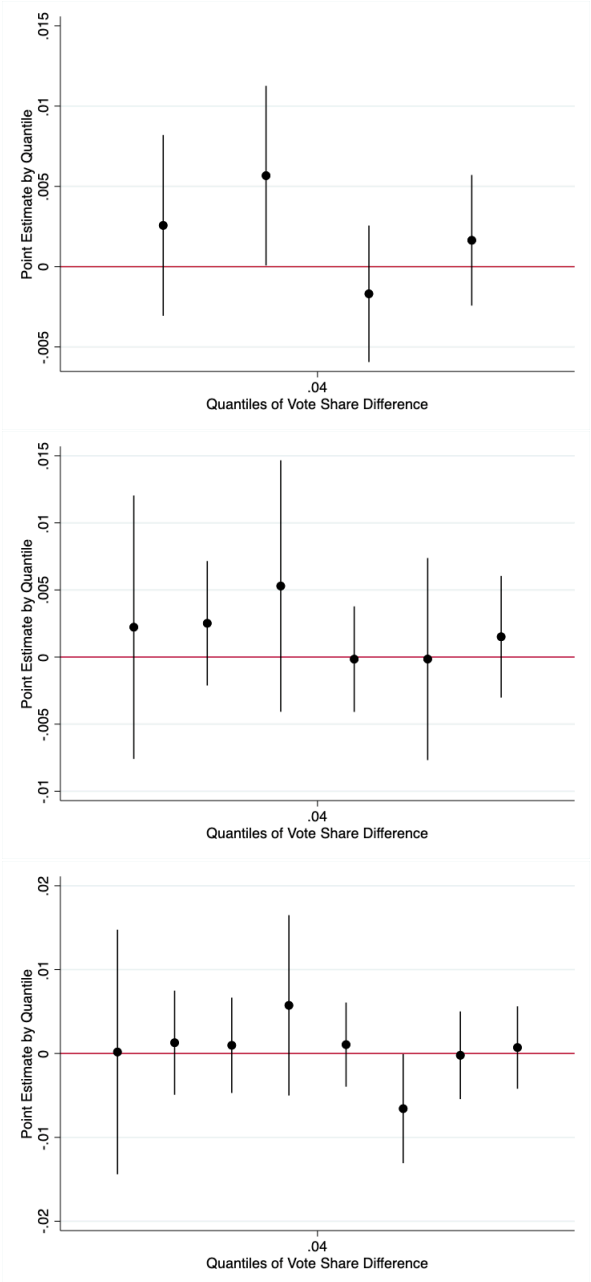


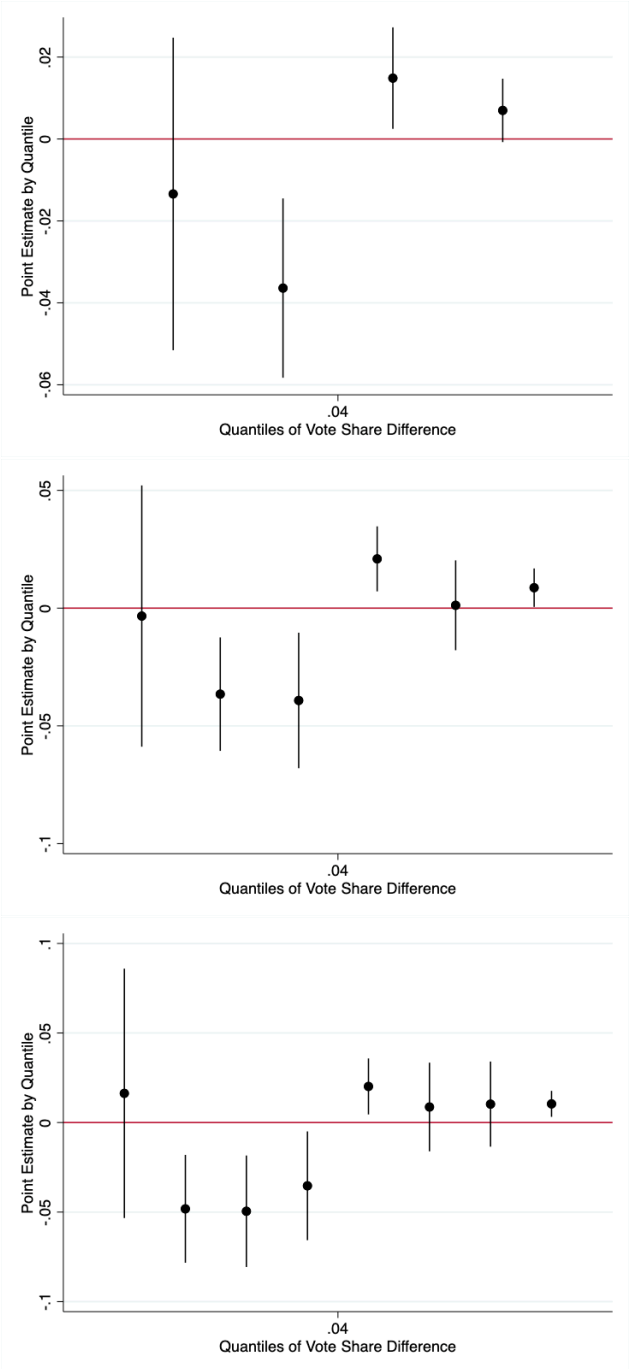
Figure A14 shows the location of the neighborhoods in the experimental sample within Turkey. Figure A15 shows the location of neighborhoods in each treatment group within the province of Izmir.

Figure A16:
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 A17: 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.