Persistent Polarizing Effects of Persuasion: Experimental Evidence from Turkey

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Abstract

Exposing voters to non-state-provisioned information is presumed to counter incumbents' efforts to keep voters uninformed in order to remain in power. In this study, I estimate the effect of randomized information campaigns on voter behavior and ideology in Turkey. My design allows me to estimate heterogeneous effects of information campaigns. I find that voter response to the same campaigns increased political polarization and the effect persisted at least two years. I conclude that reducing censorship can be polarizing and, 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 counter electoral support for the incumbent, the information campaigns increased polarization. Moreover, I find that this transitory exposure to the same information campaigns had persistent polarizing effects according to administrative election data gathered approximately one and two years later.

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, but did increase ideological polarization. Moreover, existing ideological divisions resulted in different interpretations of the same information or beliefs about the quality of the information source. While a zero average effect is a common outcome of information interventions designed to affect consumer

[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).

or voter behavior, I use a stratified randomization design to show that this null result masks both intended and backfiring effects of the information campaigns on vote share. I cannot directly rule out a mobilization effect, but do not observe any treatment effect on voter turnout until two years after the referendum. Overall, these results suggest that 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.

This study circumvented a number of research challenges both in terms of implementation and experimental design. First, the study had to be done in a context where democratic institutions were 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, they can be challenging and unpredictable environments in which to implement experimental methods. 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 and an unprecedented number of terrorist attacks are viewed as precipitating the April 2017 constitutional referendum. The incumbent announced that parliament would vote on holding a referendum with just one month's notice and, after the motion passed, the date for the referendum was set by the incumbent for three months 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. It was expected that party affiliation would be predictive of how voters react to the information campaigns. This expectation required a plan to conduct subgroup analysis along partisan lines. The intention to estimate heterogeneous treatment effects was directly incorporated into the experimental design and specified in a registered pre-analysis plan.[4] This is important for inference because otherwise one can search over many possible partitions of the

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

data (Athey and Imbens, 2016).[5] Specifically, randomization was conducted within strata of quartiles of the difference in past average vote share between Recep Tayyip Erdoğan's party (incumbent) and the main opposition party in two 2015 general elections. Vote share for all elections was observed at the ballot box level and the information campaigns were delivered in neighborhoods that I randomly selected to be in the treatment group. 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. Following the pre-analysis plan, I estimate the average treatment effect and heterogeneous treatment effects by quartile of past vote share. 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 effect was in incumbent majority quartiles and the positive effect was in opposition majority quartiles, the information campaigns resulted in ideological polarization on average.

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. Both of the 2018 elections were related to the referendum. Erdoğan received the majority vote share in the presidential election and so the constitutional amendments that were voted on in 2017 went into effect. The general election was to elect Members of Parliament (MPs). 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. In

[5]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.

contrast, I do not find that the information campaigns had a meaningful effect on voter turnout on average or by quartile, except in the second quartile of the 2019 local election. A null effect on voter turnout was discussed as a likely outcome in the PAP because voting is compulsory in Turkey and is high. In the referendum, voter turnout among the experimental sample, and nationally, was approximately 85%. However, it is still possible that there were unobserved countervailing effects on voter turnout in the 2017 and 2018 elections, driven by the mobilization and demobilization of voters supporting opposing parties.

Finally, I use recent developments in the literature to discuss how voters can become polarized in response to exposure to the same information campaigns.[6] One campaign included objective information on policy outcomes related to the deteriorating economy and increased terrorism under the incumbent. The second campaign included information on the longer-term implications of the proposed constitutional change and did not mention the incumbent.[7] Such information is not easily accessible in the media and in-person conversations between people with opposing views are limited and risky in this environment. 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 exposure to the same information if they hold different views on why policy outcomes have deteriorated, which determines whether they prefer to place more or less constraints on the executive branch as the best policy option. Under this interpretation, there is multidimensional uncertainty and heterogeneity in voters' views on what factors explain changes in policy outcomes (Benoit and Dubra, 2016; Andreoni and Mylovanov, 2012).[8] It is possible that the persuasion effect is instead driven by voter response to the bias of the information source rather than the information itself. However, if the voters are rational, we would expect them to filter out the bias and not be persuaded by it or not engage with the canvassers at all. Instead, while I estimated significant heterogeneous treatment effects on vote share, I find that the share of voters in a neighborhood

[6]There were context-specific constraints that prevented me from collecting individual-level data on beliefs to pinpoint the mechanism. Kendall et al. (2015) is an example of a study that collects both administrative vote share data and self-reported individual-level data before a municipal election in Italy. The data they collect allow them to observe whether individuals update their beliefs in response to the information contained in the campaign messages.

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

[8]There are a number of theoretical papers that provide both Bayesian and non-Bayesian mechanisms to explain how exposure to the same information can increase polarization (Piketty, 1995; Dixit and Weibull, 2007; Acemoglu et al., 2016; Loh and Phelan, 2019; Benoit and Dubra, 2016). There are also lab experiments in the psychology literature, e.g., the seminal study by Lord et al. (1979), and the economics literature, e.g., Andreoni and Mylovanov (2012). Fryer et al. (2019) provide evidence using an online experiment and test for non-Bayesian mechanisms.

that completed conversations with canvassers is not correlated with neighborhood-level vote share. Moreover, the number of neighborhoods in which canvassers faced threats (20% of all treatment neighborhoods) within each quartile is similar. Finally, if the voters are uncertain about the bias of the information source, then we would expect the effects of the information compaigns on vote share to be temporary, but they are in fact persistent (Bray and Kreps, 1987; DellaVigna and Kaplan, 2007).

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. While a single study, Pons (2018) does estimate an effect of a precinct being randomly assigned to door-to-door canvassing in France. Using administrative data, he finds that a nation-wide doo-to-door campaign increased the vote share of the campaigning party by .5 percentage points and by .37 percentage points two years later. While a direct comparison is difficult, the implied persuasion rates that are calculated using comparable estimated effects are higher in the present study.

The majority of studies also find that exposure to the same information campaign or media outlet has a uniform effect, rather than a polarizing effect, on vote share. Two exceptions are studies that evaluate this relationship in countries with weak democratic institutions (Adena et al., 2015; Peisakhin and Rozenas, 2018).[9] During the period of German democracy in the

[9]In the literature on the relationship between media and vote share, exposure to certain media outlets is modelled as consumption of ideological slant (e.g.DellaVigna and Kaplan (2007)); however, the existing empirical analysis in this literature cannot rule out that the effect of media on vote share is driven by exposure to new information. Most of these studies find media consumption to have a uniform effect on voter share; therefore, self-selection of voters with different ideological views into separate media outlets is what drives increased polarization. Under this interpretation, consumers have preferences for media that are slanted toward their own ideology (Martin and Yurukoglu, 2017). Again, exceptions are the studies by Adena et al. (2015) and Peisakhin and Rozenas (2018), where exposure to the same media outlet increases

1920s, the Weimar government introduced pro-government political news using the radio, which uniformly reduced increasing Nazi popularity (Adena et al., 2015). The authors find that this effect on vote share was reversed after Hitler was appointed chancellor and pro-Nazi radio began to be broadcast. However, once the dictatorship was consolidated, exposure to the same radio outlet had polarizing effects on anti-Semitic behavior. In areas where anti-Semitic sentiment was historically low, the effect of the radio on anti-Semitic behavior was negative and in areas where anti-Semitic sentiment was higher, the effect of the radio was positive. Similarly, Peisakhin and Rozenas (2018) use quasi-experimental evidence to show that in areas with strong 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 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 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. Therefore, 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.

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

polarization.

[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).

of whether increased competition in the provision of oppositional information reduces voter support for the consolidation of power in an illiberal democracy 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 et al., 2018).

Most of the other empirical literature on ideological polarization, more generally, analyze self-selection into different media sources (Gentzkow and Shapiro, 2011; Martin and Yurukoglu, 2017). However, Gentzkow et al. (2018) argue that the observed polarization among voters in certain studies cannot be explained by self-selection into different media sources and build a theoretical model to show that polarization can persist even when voters are exposed to the same information. While the present study cannot isolate whether the effects are driven by the information itself or its source, it provides experimental field evidence that exogenous exposure to the same information campaigns have persistent effects of increased polarization and the estimates are significant.[14]

3 Background

3.1 Democratization, Identity, and Polarization in Turkey

Turkey was ruled as a one-party regime under the Republican People's Party (CHP), the current main opposition party, until transitioning to a multi-party system in 1946. Since then, the level of democracy has plummeted three times, including during the time of this study, as shown in Figure A2. The strong military, the weak state institutions, and society's ideological cleavage have been associated with episodes of economic and political instability. For example, there have been a number of military interventions, including three coups (1960, 1971, 1980) and one "postmodern coup" (1997). In 2001, there was an economic recession and the country was under the unstable leadership of a multi-party coalition.

The current ruling party, the Justice and Development Party (AK Party), was founded by

[12]There is also an extensive related literature on the impact of non-partisan information campaigns on voter behavior in middle-income and lower-income countries.[13] 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.

[14]The majority of empirical studies on increased polarization in response to exposure to the same information have been mostly confined to lab and online experiments or descriptive studies. This hypothesis has been tested widely in lab experiments discussed in the psychology literature, such as the study by Lord et al. (1979). For a discussion of relevant lab and online experiments in the economics and psychology literature, see Benoit and Dubra (2016) and Fryer et al. (2019), among others. For an example of a descriptive study, see Flaxman et al. (2016), among others.

Erdoğan in 2001 and first entered a general election in 2002. The AK Party won the majority of seats in parliament and the 2002 general election marked the first time Turkey was ruled by a single-party government since 1987, bringing stability to the country for the first time since the recession. Only one other party, the CHP, also gained seats in parliament. The AK Party has maintained a majority in parliament ever since coming to power. While different measures of the degree of democracy and democratic norms rose or were stable under the first decade that they ruled, Figure A2 shows that these measures sharply fell starting around 2014.

The AK Party's religiosity caused the military to threaten its overthrow. However, with the help of an alliance with an Islamic cleric, Fethullah Gülen, who spearheaded grassroots mobilization efforts, the party continued its rule uninterrupted and the politicization of the divide between secularists and religious conservatives remained, albeit under a different leadership. Eventually, the alliance between the AK Party and Gülen dissolved and the July 2016 coup attempt is considered partly a manifestation of this disbandment.

The coup attempt itself was poorly organized and failed rapidly, but was considered a catalyst in the call for a referendum. It was labeled as a terrorist attack in the media and by the state and deepened ideological polarization in Turkish society. Following the coup attempt, a state of emergency was declared and more than 150,000 civil servants, academics, and journalists were detained. Despite the arrests being internationally condemned, a survey that I conducted provides evidence that the majority of sampled voters self-reported as supporting these mass arrests, presumably for national security. According to additional survey evidence, voters in Turkey have mixed views on whether the coup attempt can be partly blamed on Erdoğan's past relationship with Gülen. Others do not place any blame on Erdoğan and view the coup attempt as an incident that was completely beyond his control. Figure A3 shows that this disagreement is along partisan lines. This split in the attribution of responsibility for the coup attempt, as well as other disruptive events like the record devaluation of the Turkish lira (Figure A4), reflects the polarized views among voters.

3.2 Party Platforms Before the April 2017 Referendum

The salience of national security concerns was not only triggered by the coup attempt. The number of violent attacks had already reached an unprecedented level, civil conflict had restarted, and hundreds of thousands of civilians from Kurdish-majority areas had been displaced. Figure A5 shows that the number of violent attacks had risen significantly in 2015 and reached a historical peak in 2016 (and decreased sharply after the 2017 referendum).

The AK Party argued that switching to a presidential system and that enacting the proposed amendments would bring more stability and improve national security (Esen and Gumuscu, 2017). The opposition argued that the current leadership, who had already taken steps over the past few years to consolidate power, was to blame for the deteriorating conditions and increasing their power would only exacerbate the problems.

3.3 Elections from 2015 to 2019

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. 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 no 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.

Data from the two 2015 elections are also used to provide descriptive statistics on baseline polarization in the experimental sample. The events threatening national security that took place between the two elections presumably increased ideological polarization. First, there was a severe escalation in civil conflict in Turkey between the state and the Kurdistan Workers' Party (PKK) and an official military operation was initiated in July 2015. The snap November election was announced in August. Then the deadliest terrorist attack in Turkey's history occurred in October in Ankara, the capital. It is believed that the attack was carried out by two individuals with links to the Islamic State of Iraq and the Levant (Daesh). On aggregate, there was very little change in the vote share distribution for the CHP and the AK Party between the two 2015 elections in Izmir, where the field experiment was conducted. However, Table A1 shows that this zero aggregate change again masks increased polarization. When discussing the results, I will show that the increase in polarization in response to exposure to the information campaigns occurs in the same parts of the distribution as this prior plausible increase in polarization.

The main outcome data used in this study includes voters' support for measures weakening democratic institutions in the April 2017 referendum. The referendum was on switching from a parliamentary system to a presidential system and on eighteen amendments to the constitution.[15] Prior to these changes, the highest position of leadership was that of the prime minister and the role of president was largely viewed as ceremonial. Erdoğan served as prime minister from 2002 to 2014, stepping down just before his term limit. In 2007, Erdoğan 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. A number of the proposed amendments to the constitution in the referendum would consolidate power under the president. For example, a new power granted to the president is the ability to bypass the parliament completely and introduce legislation by issuing decrees with the force of law (Jenkins, 2016). Therefore, this was a high-stakes election on institutional reform.

The outcome data to measure the longer term effects of the information campagins are from the 2018 general and presidential elections, which were originally scheduled for November 2019. Instead, in April 2018 it was announced that there would be snap elections and the two elections took place on June 24, 2018. Prior to the referendum in 2017, the MHP declared that the party's position was "Yes" on consolidating power under the incumbent, in alliance with the AK Party. By the time of the 2018 elections, some members officially split from the MHP and formed a separate party, the Iyi Party. The MHP and the AK Party also formed an official electoral alliance in February 2018 ahead of the general and presidential elections. Voting for each election occurred at the same time. Since the amendments to the constitution passed in the 2017 referendum, the elected president in the 2018 presidential election would be both the head of state and the head of government of Turkey. Oppositional candidates declared before the election that they would nullify the referendum if elected. There were four presidential candidates and the election was held in only one round since Erdoğan received 53% of the vote share and the CHP's candidate, Muharrem İnce, got 31%. The remaining two candidates together received less than ten percent of the votes.[16]

The final outcome data are from the local elections that were held in March 2019 in which metropolitan mayors were elected.

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

[16]In the general election, 600 members of parliament were selected by party-list proportional representation in 87 electoral districts and by the D'Hondt method. The vote share distribution for the general election was similar to the presidential election for the AK Party and the CHP.

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 information campaign, voters were exposed to messages on policy outcomes (PO campaign). This included statistics on economic loss and information on 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 (see Figure A6 for a timeline of the whole study, including important events that preceded the 2017 referendum). The second information campaign was 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 the campaign at all. 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 3 separately shows the neighborhood-level distribution of the

[17]There are 81 provinces in Turkey.

"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, 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. This 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. For this reason, this measure is equivalent to the difference in past average vote share between the two parties. In addition, vote share is highly persistent in Izmir and many parts of Turkey. 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 A10. 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 A11. 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).

The statistical power for estimating the heterogeneous treatment effects on vote share varies across the quartiles of past average vote share. This is driven by the heteroskedasticity of the error term in the "No" vote share conditional on past vote share. Figure A7 shows residuals plotted against fitted values from regressing the "No" vote share on the November 2015 vote share for the opposition party and on November 2015 voter turnout. We see that the variance in the "No" vote share decreases with past vote share for the opposition party, but not with past voter turnout. Table A1 shows a similar pattern for the percentage change in the opposition party's vote share between the two elections in 2015 (June and November). In the lower quantiles of the 2015 vote share distribution, where the opposition party's support is weaker, variance in the percentage change in vote share is highest. In addition, Table A1 shows that the overall percentage change in vote share was nearly zero on average, but this

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

masks an underlying increase in polarization between the two elections.[19] The percentage change is negative in the quantiles where the opposition party's vote share is lower and positive where it is higher. In contrast, the average and variance in the percentage change of voter turnout between the two elections is stable across quartiles (Table A1). These baseline descriptive statistics of vote share are reflected in the precision in the estimated heteregoeneous treatment effects on vote share within each quartile and the sign of the effects. When designing the experiment, it was clear that treatment effect would have had to be very large in quartile 1 in order to observe a statistically significant effect. However, targeting these neighborhoods was considered priority for the campaign party and so they were included in the experimental sample.

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.[20] In-person conversation is considered to be one of the most effective methods to affect voter behavior (Pons, 2018; Green and Gerber, 2015).[21] Reaching more neighborhoods, but only partially covering them, would have reduced the statistical power of the experiment.

The second issue, the budget and capacity constraint on volunteers, were also important factors for 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. According to 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

[19]The increase in polarization may be due to the terrorist attack and resurgence of civil conflict that took place between the two elections. See Section 3.3 for background information.

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

[21]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.

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.[22]

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 A8 and A9 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.

B. Implementation

To observe whether the implementation of the information campaigns varied with vote share and whether there was selective canvassing, I monitored the rate at which conversations were completed and I geocoded 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 (reach rate). 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

[22]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.

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 A2 shows the average number of registered voters per neighborhood that canvassers could reach. These descriptive statistics show that the average reach rate did not vary with the quartiles of past vote share. This result also suggests that the estimated heterogeneous treatment effects of the information campaigns on vote share are unlikely to be driven by the source of the information (opposition party volunteer canvassers) rather than the information itself. If the bias of the information source was important, the voters should not have been persuaded by the campaign or engaged with the voters at all, leading to a lower reach rate in the quartiles with negative treatment effects. Instead, the reach rate is similar across quartiles.

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. Finally, the reach rate allows me to scale the estimated treatment effects of the campaigns in order to calculate persuasion rates under different assumptions.

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.[23] 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

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

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.[24] T_{nq} is an indicator for whether the neighborhood was assigned to one of the two treatment groups and δ_q are strata (quartile) fixed effects. Randomization into the control group or treatment group of each of the two information campaigns was stratified by quartiles of the CHP's average vote share for the two elections in 2015. Within each quartile there are 25 treatment neighborhoods and 112 or 113 control neighborhoods; in total, 48 neighborhoods were covered in the PO campaign and 52 were covered in the CB campaign.

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

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.[25] In the pre-analysis plan, I also specified a vector of control variables measuring past voter data from each election in 2015 (X'_{nq}). The control variables include the number of registered voters, the number of valid votes, the number of votes for the CHP, vote share for the CHP, and voter turnout. These control variables are included at the neighborhood level and the dependent variables are at the ballot box level. Standard errors are clustered at the neighborhood level. I show that the pre-specified control variables are balanced between the treatment and control groups in Table A3. 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. 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). I also pre-specified a test for the difference in the effect between the PO and the CB campaigns.

An alternative approach is to run the regression with the dependent variable at the neighborhood level. The results using this specification, combined with weighting observations by the number of registered voters in a neighborhood, is almost equivalent to running equation 1, where the dependent variable is at the ballot box level. Both of these approaches, relative to running the regression at the neighborhood-level without weights, can increase efficiency

[24]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.

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

if the variance of the outcome variable is decreasing in the number of registered voters and the number of ballot boxes in a neighborhood is correlated with the number of voters in a neighborhood. The two approaches can also affect the estimates if there are heterogeneous treatment effects by neighborhood size Solon et al. (2015). I find, and show, that the increase in the magnitude of the estimated effect and the efficiency gains are largest for quartiles 1 and 2, but the reverse is true in quartiles 3 and 4. As discussed in Section 4.2A, this is because neighborhood size is decreasing in vote share for the incumbent party. Overall, the results using all approaches point to persist polarization. In the pre-analysis plan, I do not explicitly discuss ballot box level analysis or weighting. As discussed by Solon et al. (2015), it is illustrative to estimate the results with and without weighting for population size.

4.4 Voter Campaign Experiment: Results

A. Average Treatment Effect versus Heterogeneous Effects on Vote Share

Table 1 shows that the combined average treatment effect of the two campaigns on the "No" vote share in the April 2017 referendum is zero. Table 2 and Figure 4 show that the zero average effect is masking substantial heterogeneous effects. Table 2 shows the results from estimating the ITT using equation 1. 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. The results from estimating equation 1, but using the neighborhood level measure of the dependent variable is shown in Table A4. 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.

I conduct randomization inference exercizes within quartiles 2 and 3 for 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 .004 or .006 depending on the program that is used. For quartile 3, I find that the p-value is approximately .063 or .050 depending on the program that is used.[26] The calculated p-values are described as approximate because they are sensitive to the seed used

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

for the randomization, as the authors of both programs attest to.

Table A5 shows the estimated average treatment effect across quartiles with and without the pre-specified covariates. Given that 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 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 3 shows the persistent heterogeneous treatment effects by campaign using data from the 2018 and 2019 elections. Across all elections, and within each quartile, I cannot reject the null hypothesis that the estimated treatment effect for each campaign is the same. Panels A and B show that the overall estimated treatment effects in quartiles 2 and 3 remain significant and there is no change in their magnitudes in the 2018 presidential and general elections. These elections took place on the same day fourteen months after the referendum. Panel C shows the estimated treatment effects for a local election that took place in March 2019, approximately 23 months after the referendum. This election was irrelevant to the referendum, unlike the 2018 general and presidential elections. The polarizing effects of the campaign on the vote share still persist except that the increase in the "No" vote share in the third quartile becomes less precise. The estimated effects for the local election may be underestimates because in constructing the dependent variable I assume that all smaller parties, which were not represented in the 2018 elections, are opposition parties.

Table A9 shows the results for the same set of specifications, but I combine the treatment arms and vote share is calculated using the number of registered voters instead of the number of valid votes as the denominator. This makes the estimates across elections more comparable. The table shows that the magnitudes of the estimated coefficients remain the same across elections except that it diminishes in the 2019 local election and the estimated effect in quartiles 2 and 3 are significant only at the 10% level in the 2019 local election.

C. Voter Turnout

Table 2 shows that the campaigns had no effect on voter turnout in the April 2017 referendum across all quartiles. Table 4 shows the estimated heterogeneous treatment effects by quartile and for each election. The mean voter turnout in the control group ranges from 85%–87% across quartiles in the 2017 and 2018 elections. Across these three elections, the magnitudes of the estimated treatment effects are small. 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. However, the mean voter turnout in the control group in the 2019 local election in smaller than the other elections. We also see that the estimated treatment effect on voter turnout is sizable in the second quartile. More precisely, in Table A10, 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.

Figure A12 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 A13.

D. Persuasion Rates

I calculate the persuasion rates under two different assumptions: that everyone in a neighborhood assigned to the treatment group was exposed to the information campaigns and that only those who completed a conversation with a canvasser (and received a pamphlet) were exposed to the information campaign. The assumption that everyone in a treated neighborhood is exposed to the campaign is conservative. First, I know that in 20% of neighborhoods assigned to the treatment group, canvassers faced threats and could not complete the campaigns. However, I do not have information on how much of the canvassing they were able to complete in these neighborhoods. Moreover, only 12% of households visited had at least one voter who completed a conversation with a canvasser and this reach rate is comparable across quartiles (Table A2). However, pamphlets were reportedly left at all households in neighborhoods assigned to the treatment group and in which the canvassers did not face threats. Therefore, scaling the persuasion rates by the reach rates overestimates the persuasion rates. Under either assumption, the calculated persuasion rates will be underestimates since the estimated effects within each quartile are also be obscuring opposing effects on vote share.

The persuasion rate as defined by DellaVigna and Gentzkow (2010) involves dividing the scaled estimated treatment effects by the share of voters who would have voted "Yes" ("No") if there were no campaigns in quartiles 3 and 4 (quartiles 1 and 2). I use the control group to construct these shares. Under the first assumption, in which everyone is assumed to have been exposed to the information campaigns in the treatment neighborhoods, the persuasion rate is 5.3% in quartile 2 and 4.3% in quartile 3. Instead, if we consider only households who completed conversations with canvassers as treated, and we assume there are 2.2 registered voters on average in each household, then the persuasion rates are 25 and 22% in quartiles 2 and 3, respectively.[27]

The persuasion rates found in this study are higher than the persuasion rates in studies that 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). Using the estimated effect of a precinct being randomly allocated for canvassing, Pons (2018) finds that the persuasion rate of an information campaign in France is around 2.6%.

[27]The average household size, including children, between 2017 and 2019 was 2.98 in Izmir. These statistics are from the Turkish Statistical Institute.

5 Interpretation of Results

The empirical findings of this study provide strong evidence that the same information campaigns increased "population polarization" through a persuasion effect (Benoit and Dubra, 2016). In this section, I discuss the possible mechanisms that could explain this effect.

Information:[28] Since 2013, when Turkey started experiencing significant instability, the value of the local currency in Turkey, the lira, has been falling. The rate of depreciation rapidly escalated toward the end of 2016, after the coup attempt. In January 2017, after a record drop in the value of the lira, a nationally representative survey was conducted in Turkey covering issues such as the referendum and the economy.[29] In the survey, voters were asked the degree to which the drop in the value of the Turkish lira had an impact on their personal life. The top panel of Figure A4 shows that voters who self-report as either incumbent or opposition supporters both agree that the depreciation of the lira had a negative impact on their life. However, we see in the bottom panel of Figure A4 that voters have different views of why the value of the lira dropped, depending on their party affiliation. Opposition voters predominantly blamed the current leadership (president and parliament). 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]

The CB and PO information campaigns took place after the survey and right before the April 2017 referendum, when voters chose to weaken constraints on the executive branch. The survey results on the lira suggest that the PO campaign, which included information on economic conditions or terrorist activity, could have an ambiguous effect on voting outcomes. Take, for example, voters who underestimated how poor the economy was and blamed any poor conditions on external factors. They may have chosen "Yes" in the referendum because they believed that the incumbent party's capacity to unilaterally increase national security, such as making mass arrests, would reduce external sources of instability and subsequently improve the economy. More generally, they may have supported removing constraints on the executive branch so that incumbent policies could be more effective, and implemented rapidly, when external threats were high. On the other hand, voters who also underestimated

[28] A theoretical framework describing how the information contained in the campaigns could increase polarization can be found in Section A.1 of the Appendix.

[29] The survey was done by an American polling company, which cannot be identified in this study. While I was not involved with the survey, I had access to the results.

[30] Similarly, voters in Turkey have mixed views on whether the coup attempt can be blamed partly on Erdoğan's past relationship with Gülen. Some do not place any blame on Erdoğan and view the coup attempt as an incident that was completely outside of his control. This can be seen in the results reported in Figure A3.

how poor the economy was, but blamed the incumbent for their policies, or for being the cause of threats to national security in the first place, voted against increasing authoritarianism. To use the language in the literature on disagreement or polarization in response to a common signal, the information on policy outcomes provided in the campaign is an "equivocal signal" (Benoit and Dubra, 2016). Voters are provided with a unidimensional signal to a multidimensional problem (Loh and Phelan, 2019).[31] The effect that giving voters more information on incumbent policy outcomes, i.e., the economy and terrorist activity, has on vote choice depends on their views, and levels of uncertainty, regarding whether the incumbent is to blame for the policy outcomes or not (Benoit and Dubra, 2016).

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. As in the case of the PO campaign, the different types of voters had different views on the optimal policy. Upon learning more about the long-run implications of the referendum and, in particular, that government institutions could change, voters who preferred weak constraints on the executive branch were more likely to vote "Yes" and others who disagreed with the proposed insitutional reform were more likely to vote "No." Thus, learning the stakes of the referendum can have a similar effect as the provision of information on policy outcomes. A benefit of studying the impact of the CB campaign separately, which was neutral in content, is that it rules out that the negative language on policy outcomes in the ET campaign alone increased polarization.

Information Source: Voters may have reacted to the fact that the information was coming from a biased source. However, if voters are rational, they will filter out the bias and not be persuaded by it. If they are uncertain whether the source is biased then the effects of the information on vote share would be temporary (Bray and Kreps, 1987; DellaVigna and Kaplan, 2007). While this study rules out these explanations, 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). However, the campaigns were carried out in a province that has been an opposition stronghold for many years. Most likely, voters already had a precise view of the quality of the opposition party. Moreover, if the source of the information generated a response from the voters, we would expect the aggression of the voters and the inability of canvassers to engage with the voters to be correlated with vote share. Table A2 suggests that this is not the case: it shows that the reach rate or the number of threatening neighborhoods does not vary across

[31] This issue of uncertainty over an additional dimension is discussed in Andreoni and Mylovanov (2012). Fryer et al. (2019) provide a similar framework, but there is only one dimension and agents are non-Bayesian.

quartiles of vote share.

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 prespecify 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 voters are reacting to the source of the information or the information itself and whether it's possible to separate the two. Under the latter interpretation, voters face 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.[32]

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

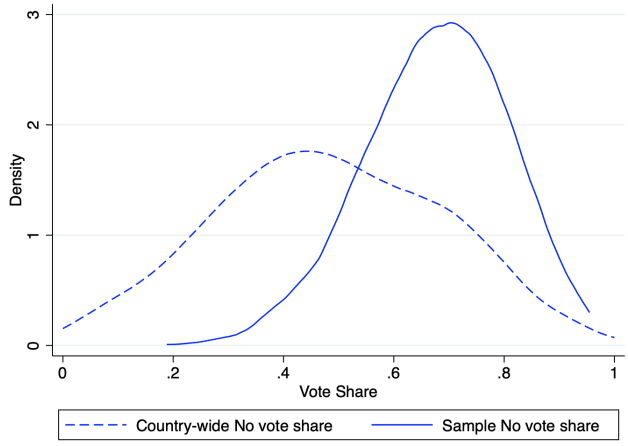
Figure 2: Pamphlet on Checks and Balances



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:

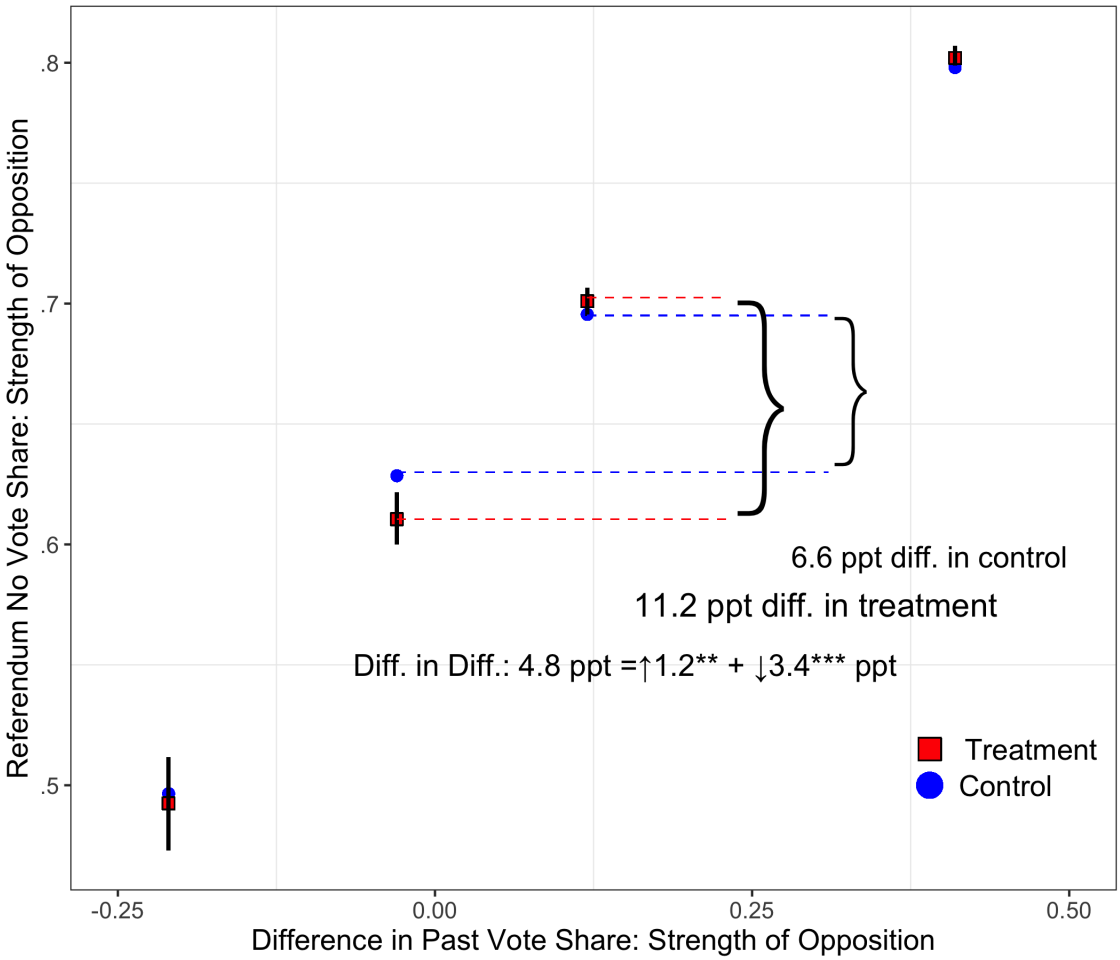
- If the constitution changes the elected president will have unlimited power
- The elected person will do what they want for 5 years
- Because they will not be held accountable by the elected parliament
- They will not be held accountable by the judiciary
- If the constitution changes the elected president 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: "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 4:
Heterogeneous Effect of Both Door-to-Door Campaigns By Quartile



This figure displays the ITT estimated effects that are also shown in Table 2. The x-axis includes the variable used for stratification (the differential in average vote share between the opposition and incumbent parties from the 2015 elections) and the y-axis includes the "No" vote share from the April 2017 referendum.

Table 1:
Average Treatment Effect on Vote Share and Voter Turnout: 2017 Referendum

	(1) No Vote Share	(2) Voter Turnout
Treatment	0.0002	0.0017
	(0.0062)	(0.0011)
Mean	.675	.872
N Ballot	3992	3992
N Nbhd	550	550
R squared	.785	.401

The dependent variables are at the ballot box level. The dependent variable in column 1 is the "No" vote share. The dependent variable in column 2 is voter turnout. Pre-specified controls are included at the neighborhood level, which is the level of randomization. Strata fixed effects are also included in all specifications. 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 Effect on Vote Share by Quartile: 2017 Referendum

	Referendum 2017			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	-0.006	-0.034***	0.012**	0.008**
	(0.019)	(0.010)	(0.006)	(0.004)
Mean	.523	.635	.713	.819
N Ballot	919	983	1058	1032
N Nbhd	138	137	138	137
R squared	.279	.416	.409	.664

The dependent variable in each column is the "No" vote share 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 3:
Persistent Effects on Vote Share by Quartile and Campaign

Panel A	Presidential 2018			
	(1)	(2)	(3)	(4)
	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
N Nbhd	138	137	138	137
R squared	.286	.441	.43	.626
Panel B	General 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Policy Outcomes	0.006	-0.037***	0.019*	0.004
	(0.028)	(0.012)	(0.010)	(0.004)
Checks & Balances	-0.038	-0.037**	0.013*	0.011
	(0.025)	(0.017)	(0.007)	(0.006)
Mean	.512	.604	.682	.803
N Ballot	1015	1093	1160	1138
N Nbhd	138	137	138	137
R squared	.267	.423	.42	.637
Panel C	Local 2019			
	(1)	(2)	(3)	(4)
	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
N Ballot	1096	1191	1274	1232
N Nbhd	138	137	138	137
R squared	.199	.329	.321	.667

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.

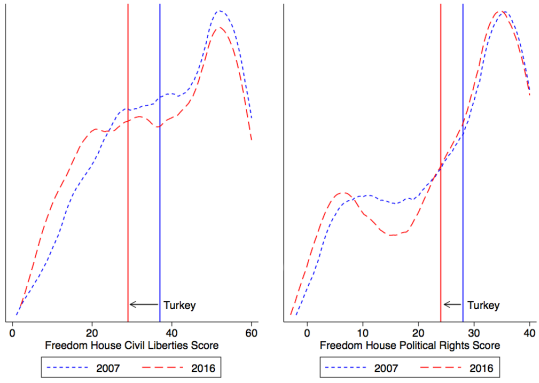
Table 4:
Treatment Effects on Voter Turnout by Quartile and Election

Panel A	Referendum 2017			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	-0.000	0.006***	0.001	0.002
	(0.003)	(0.002)	(0.002)	(0.002)
Mean	.857	.87	.879	.882
N Ballot	919	983	1058	1032
R squared	.403	.385	.301	.361
Panel B	Presidential 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	0.003	0.007*	-0.004	0.001
	(0.004)	(0.004)	(0.003)	(0.002)
Mean	.853	.865	.879	.882
N	1015	1093	1160	1138
R squared	.334	.305	.199	.243
Panel C	General 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	0.004	0.007**	-0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.002)
Mean	.853	.865	.878	.881
N	1015	1093	1160	1138
R squared	.341	.31	.209	.248
Panel D	Local 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	0.001	0.016**	0.004	-0.008**
	(0.006)	(0.007)	(0.005)	(0.003)
Mean	.81	.816	.823	.811
N	1096	1191	1274	1232
R squared	.383	.384	.343	.338
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.

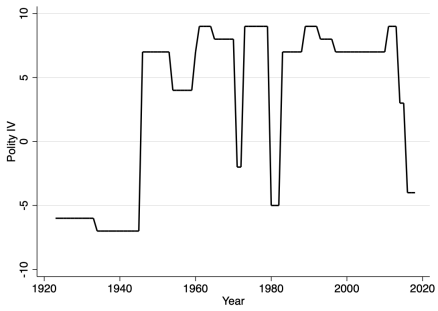
A For Online Publication: Appendix

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



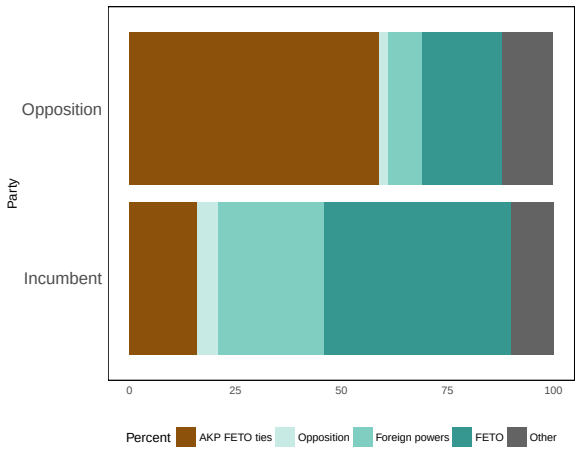
This figure shows the distribution of aggregate scores for civil liberties and political rights in 2007 and 2016 across countries. 193 countries are included in 2007 and 198 in 2016. The figure also shows Turkey's position within the distribution. There was a decline in both measures across all countries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

Figure A2: Polity IV Score for Turkey



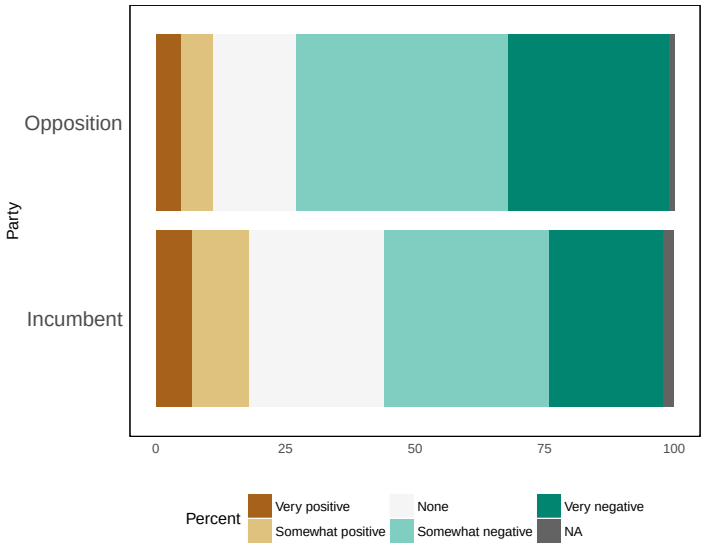
The data used to create this figure are from the Polity IV project. The specific variable used is Polity 2, which is a measure of overall degree of democracy in a country.
Source: Polivy IV Project (Monty et al., 2019)

Figure A3: Who is responsible for the coup attempt?

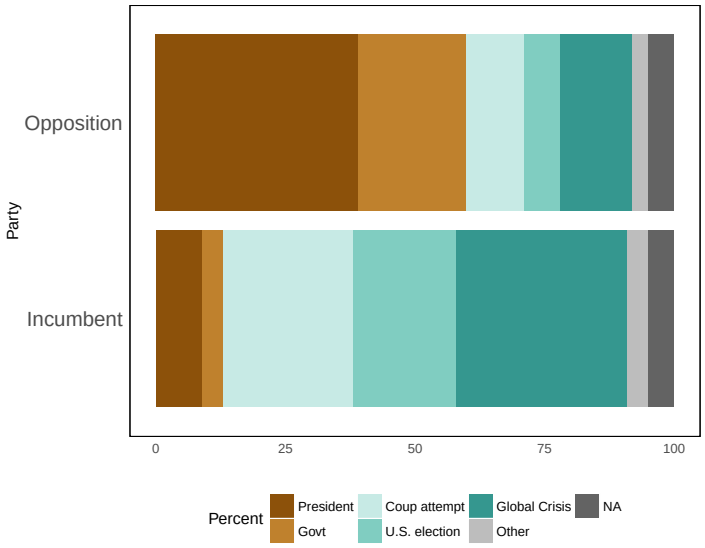


The data used to create this figure are from a survey conducted by a U.S. based firm. The name of the firm cannot be disclosed. The sample is nationally representative and includes 1,215 voters. The survey was conducted between December 25th, 2016 through January 11, 2017, which is after the coup attempt and before the April 2017 Referendum.

Figure A4: Does the drop in the value of Turkish Lira have any impact on your personal life?

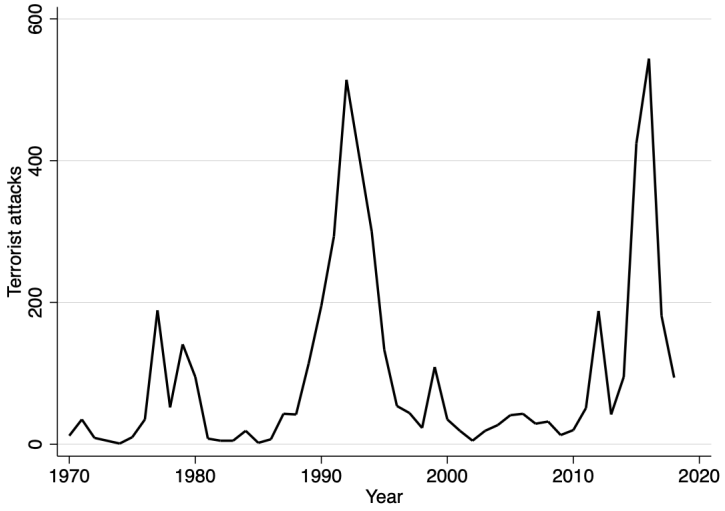


Who is most responsible for the latest devaluation of Turkish Lira?



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.

Figure A5: Violent Attacks in Turkey



This figures shows the number of violent attacks in Turkey from 1970-2016.
Source: Global Terrorism Database.

Figure A6: Timeline

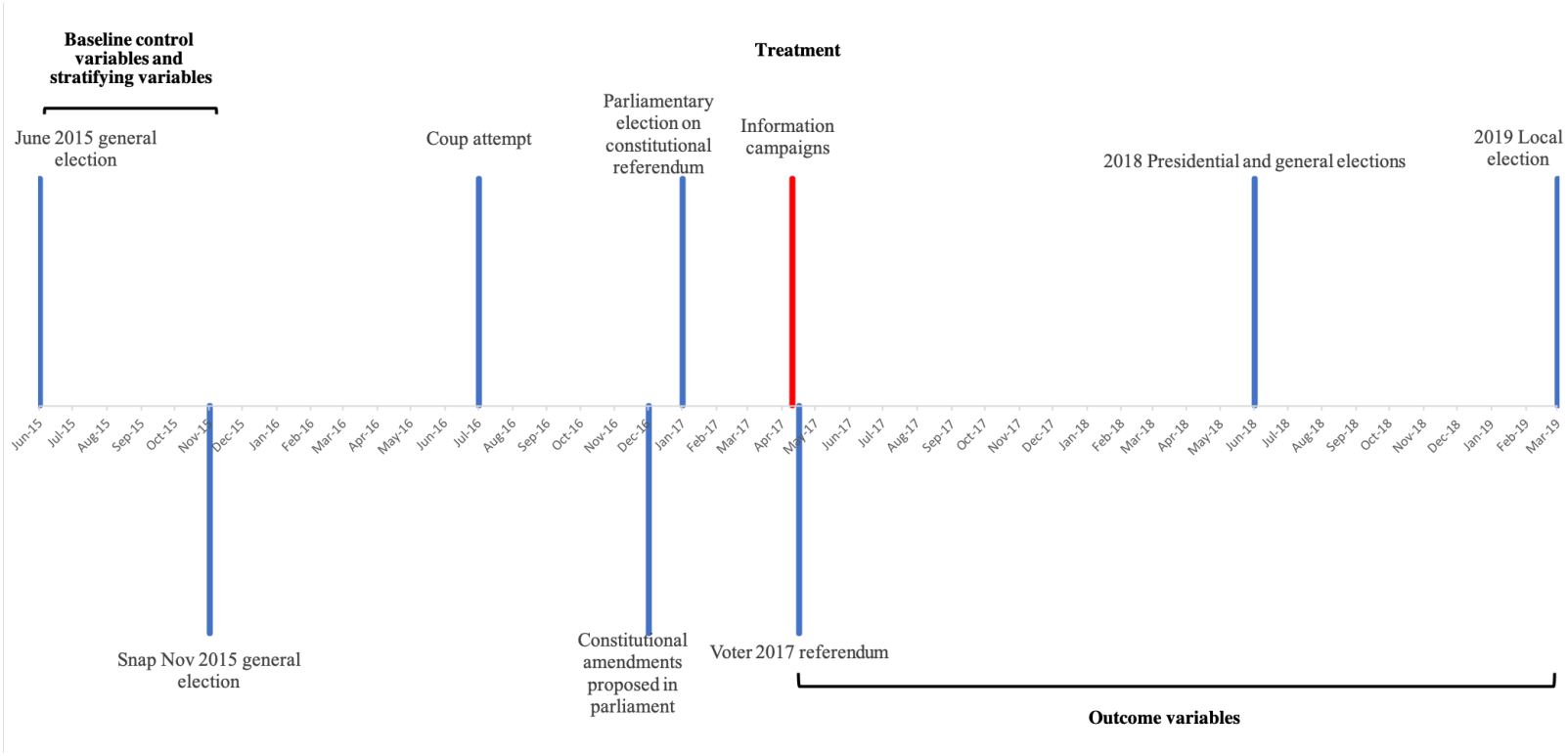
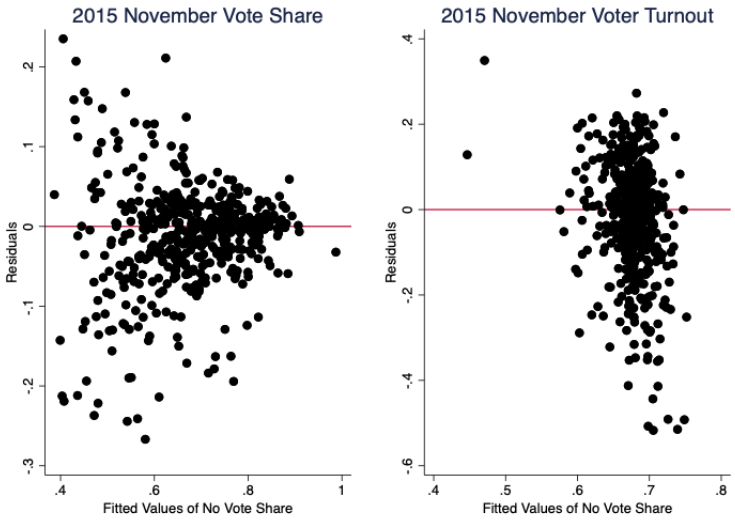


Figure A7: Residuals vs Fitted Values for Vote Share and Voter Turnout



In the left panel, the residuals from regressing the "No" vote share on the November 2015 vote share for the opposition party is plotted against the fitted values. In the right panel, the residuals from regressing the "No" vote share on November 2015 voter turnout is plotted against the fitted values. In both regressions, observations are weighted by the number of registered voters in each neighborhood and only neighborhoods assigned to the control group are included.

Figure A8: Map of Sample Within Turkey and with Province Borders



Figure A9: Map of Sample Within Izmir and with District Borders

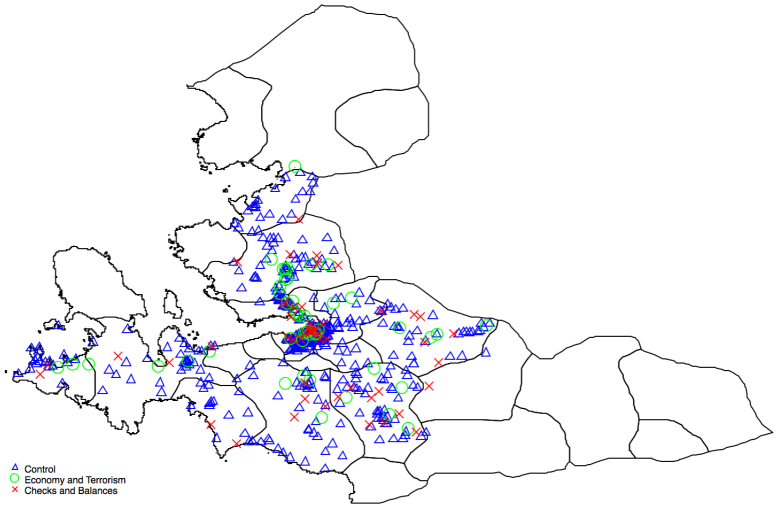
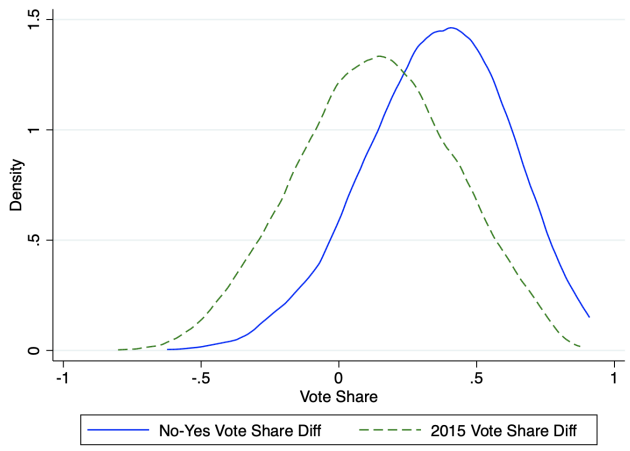


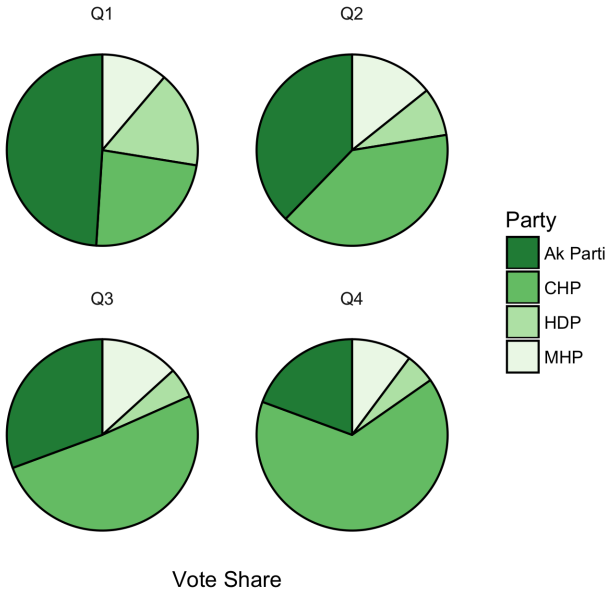
Figure A8 shows the location of the neighborhoods in the experimental sample within Turkey. Figure A9 shows the location of neighborhoods in each treatment group within the province of Izmir.

Figure A10: 2017 "No/Yes" Vote Share Differential and 2015 Opposition and Incumbent Vote Share Differential



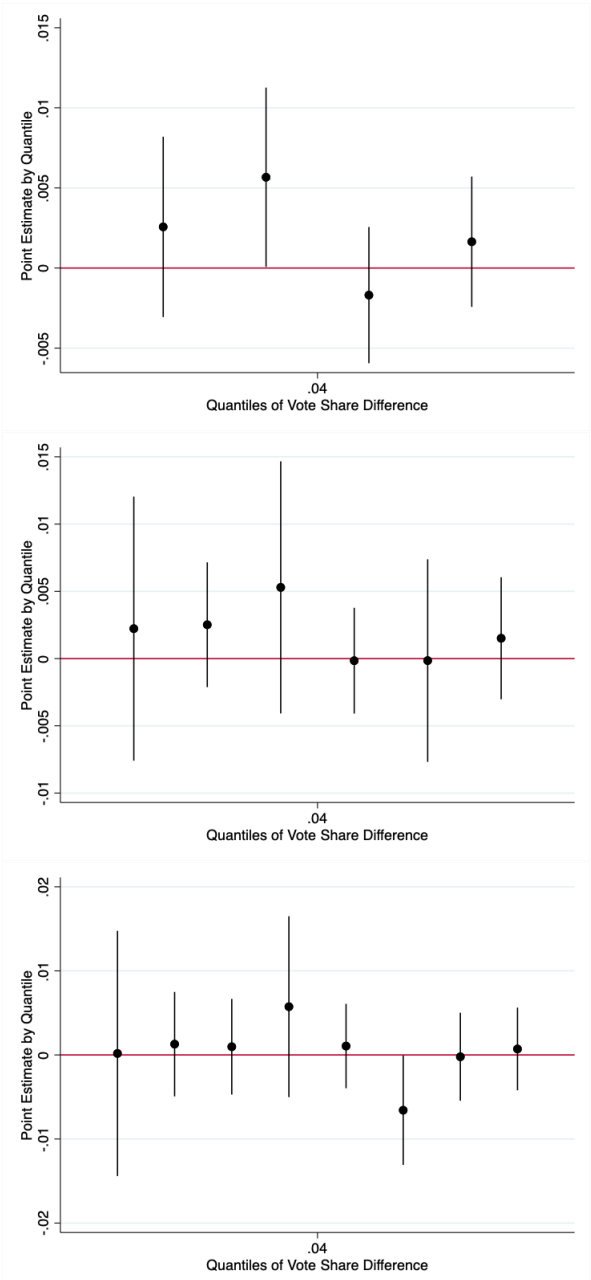
This figure shows the sample distribution of the neighborhood-level "No/Yes" vote share differential and the differential in average vote share between the opposition and incumbent parties from the 2015 elections. Observations are weighted by the number of registered voters in a neighborhood.

Figure A11: Vote Share Distribution of the November 2015 Election Results in Izmir



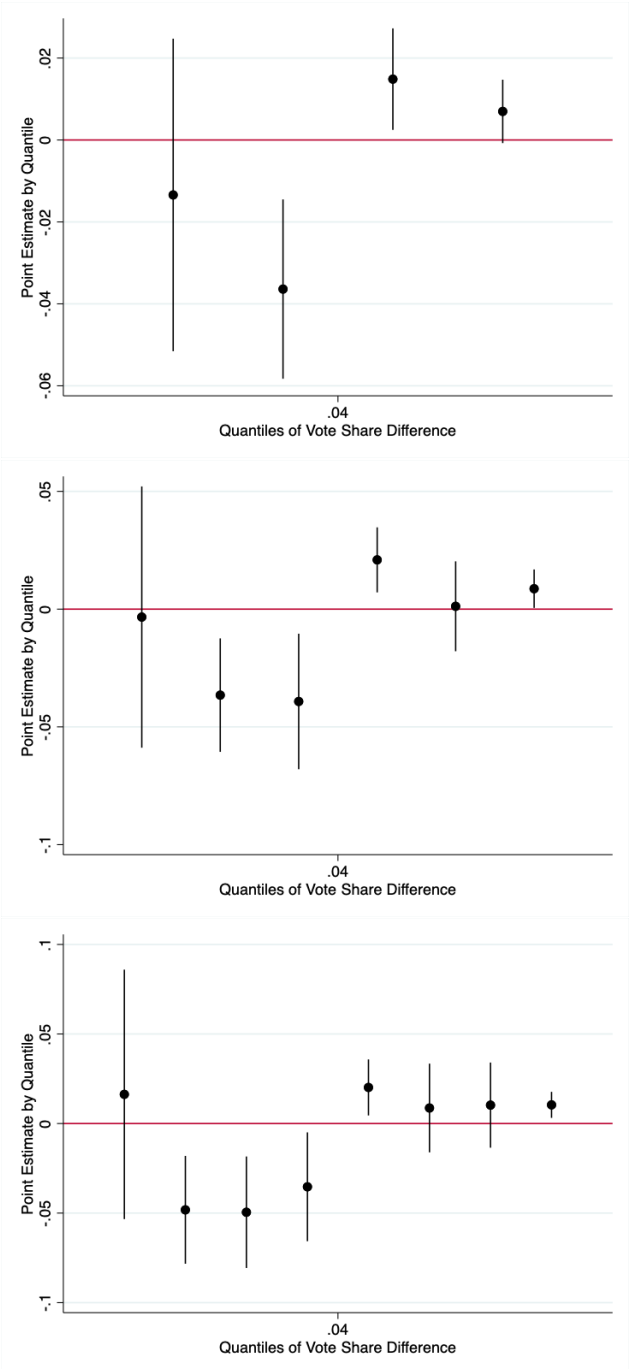
This figure shows the neighborhood-level vote share distribution for all parties in the 2015 November election in the experimental sample.

Figure A12:
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 (differential in average vote share between the opposition and incumbent parties from 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 A13: 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 (differential in average vote share between the opposition and incumbent parties from 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: Percentage Change Between the June and November 2015 Elections

	Vote Share		Turnout	
	(1)	(2)	(3)	(4)
Octiles	**Mean**	**sd**	**Mean**	**sd**
1	-0.00	0.15	0.01	0.02
2	-0.02	0.12	0.02	0.02
3	-0.01	0.08	0.02	0.02
4	0.01	0.08	0.01	0.03
5	0.01	0.07	0.01	0.02
6	0.02	0.09	0.02	0.02
7	0.03	0.06	0.01	0.03
8	0.04	0.04	0.01	0.02
N	550	550	550	550

Octiles refer to eight quantiles of the variable used for stratification (the differential in average vote share between the opposition and incumbent parties from the 2015 elections). The first column shows the mean percentage change in the vote share for the opposition party between the two general elections that took place in June and November of 2015. The second column shows the standard deviation of this change. The third and fourth columns show the same statistics, but for voter turnout.

Table A2: Number of Neighborhoods Reached and Share of Voters Spoken to by Canvassers

	All		Reached	
	(1)	(2)	(3)	(4)
Quartiles	**Mean**	**N**	**Mean**	**N**
1	0.10	25	0.12	21
2	0.14	25	0.18	20
3	0.12	25	0.15	20
4	0.13	25	0.17	19
Total		100		80

Quartiles refers to the four quantiles of the variable used for stratification (the differential in average vote share between the opposition and incumbent parties from the 2015 elections). Column 1 shows the average share of registered voters who opened their doors and completed a conversation with the canvassers (reach 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 reach rate, but excludes neighborhoods that the party volunteers could not reach because they faced threat and aggression. Column 4 shows the number of neighborhoods that could be reached.

Table A3: 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 A4:
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. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

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 differential in average vote share between the opposition and incumbent parties from 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 differential in average vote share between the opposition and incumbent parties from 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:
Effects on Vote Share by Quartile Using Registered Voters

Panel A	Referendum 2017			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	-0.005	-0.026***	0.012**	0.009**
	(0.016)	(0.009)	(0.006)	(0.004)
Mean	.448	.553	.627	.722
N Ballot	919	983	1058	1032
R squared	.267	.442	.431	.653
Panel B	Presidential 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	-0.011	-0.026**	0.012*	0.007
	(0.015)	(0.010)	(0.006)	(0.004)
Mean	.435	.529	.609	.714
N Ballot	1015	1093	1160	1138
R squared	.259	.46	.411	.601
Panel C	General 2018			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	-0.012	-0.028***	0.012*	0.008*
	(0.015)	(0.010)	(0.006)	(0.004)
Mean	.437	.523	.599	.708
N Ballot	1015	1093	1160	1138
R squared	.228	.44	.408	.611
Panel D	Local 2019			
	(1)	(2)	(3)	(4)
	Q1	Q2	Q3	Q4
Treatment	-0.008	-0.014*	0.014*	-0.002
	(0.014)	(0.008)	(0.007)	(0.006)
Mean	.371	.452	.515	.616
N Ballot	1096	1191	1274	1232
N Nbhd	138	137	138	137
R squared	.187	.346	.359	.64

This table shows the average treatment effect by quartile, but using a different measure fpr vote share. The denominator for vote share is now the number of registered voters instead of the number of valid votes. 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 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	.758	.0434	.0274	.0395
F-stat	.0954	4.16	4.97	4.32
Mean	.652	.711	.753	.802
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	.00673	.063
F-stat	.262	2.61	7.57	3.51
Mean	.644	.697	.744	.798
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	.0171	.0939
F-stat	.239	3.8	5.83	2.85
Mean	.645	.694	.739	.795
N Ballot	1015	1093	1160	1138
	Local 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
Mean	.59	.634	.669	.714
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.