Who Buys Vote-buying? How, How Much, and at What Cost?

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September 5, 2021

Abstract

In this paper, I estimate the causal effects of a local food-subsidy program on electoral outcomes. I exploit the variation in voters' walking distances from the program stores to identify their accessibility to the program. I find that a distributive spending in the amount of $\sim 5\%$ of GDP per capita buys an additional vote for the incumbent. I then investigate *who* –based on partisanship– responds to the subsidy, and *how much* and *how* they respond. The findings indicate that all types of voters respond to the distributive spending in line with the reciprocity rule; however, they respond through different channels and in different magnitude. Importantly, the salient channel for opposition voters is *abstention-buying*, whereas incumbent supporters respond by an increased turnout.

Keywords: vote-buying, subsidy, causal inference, spatial data, electoral response
JEL Codes: D72, H24, H40

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1 Introduction

Vote-buying campaigns by politicians are a worldwide phenomenon. Politicians often seek electoral gains by making distributive transfers and expect reciprocal voters to respond to this spending (Finan and Schechter, 2012). They try to do so using different kinds of transfers[1] and targeting strategies (Finan and Mazzocco, 2016). Previous scholarship has identified several groups that are likely to be targeted by politicians.[2] Although of crucial importance in vote-buying, targeting also makes it very difficult for researchers to isolate the causal effects of such programs, especially when incumbents target the transfers based on political ideology.[3] The lack of causal estimates, in turn, hinders the evaluation of the cost-effectiveness of such programs. Accordingly, it also makes it challenging to investigate the reciprocity of different political groups in the context of vote-buying. As a consequence, we still have scant evidence on matters such as *who* –based on partisanship– is responding to distributive transfers, *how much* and *how* they respond, and *how much spending* is required to buy an additional vote.

In this paper, I use a local food-subsidy program that took place in Turkey in 2019, involving the opening of state-run grocery stores at several locations in Istanbul two months prior to municipal elections. This food-subsidy program provided temporary economic relief in the form of low-cost groceries to its beneficiaries during a period of historically high food prices. Although it is clearly not possible –neither conventional– to fight the high inflation rates in food prices with only 52 grocery stores in a city with 15.5 million inhabitants, the subsidy program was part of an election campaign to gain some extra votes for the incumbent in the upcoming elections. This program was not targeted to any specific group such that anyone who made a trip to one of these stores could benefit from it. The locations of the stores, prices, and sales quantities were publicly disclosed. Therefore, not only does this program provide an ideal setting for estimating the effects of pocketbook considerations on voting behavior, but it also enables a calculation of the net cost of an additional vote generated by the program.

Using this program, I first document evidence of pocketbook considerations in voting. This is a difficult question to causally answer, because it first requires one to identify those voters who benefit from the program; and second, one has to understand how those voters would have behaved in the absence of the program. I overcome this issue by using actual election outcomes at the polling-station level with an empirical strategy that exploits the variation in voters' geographical accessibility to the grocery stores of the program. I support the credibility of this empirical strategy by means of a placebo-in-place test on the districts where the program was not implemented. The

[1] Several studies provide evidence for the electoral effects of distributive spending, such as conditional cash transfers, means-tested programs, public good provisions, vote-buying campaigns, etc. See Manacorda et al. (2011), Caprettini et al. (2019), Kogan (2018), Vannutelli (2019), Akbulut-Yuksel et al. (2020), De la Calle and Orriols (2010), Adiguzel et al. (2020), Bechtel and Hainmueller (2011), and Cantú (2019), among others.

[2] Core supporters (Banerjee and Somanathan, 2007; Cox and McCubbins, 1986), relatively moderately (Dixit and Londregan, 1996; Lindbeck and Weibull, 1987), reciprocally (Finan and Schechter, 2012), and well-informed voters (Grossman and Helpman, 1996), constituencies with higher shares of minorities (Hill and Jones, 2017), the voters for whom politically relevant information is available (Duarte et al., 2019), and regions with more and poorer voters (Finan and Mazzocco, 2016).

[3] Targeting is a typical source of the endogeneity problem in vote-buying studies (Golden and Min, 2013). If incumbents target a specific group of people, we then are not sure whether the increase in incumbent support is due to the transfers made or whether the targeted group already tended to favor the incumbent for other reasons rather than the transfers.

clean causal estimates and the polling-station level data also give me the possibility of estimating the spending required to buy an extra vote.

I then investigate *how* and *how much* different political groups respond to the subsidy program, focusing here on core incumbent, swing, and core opposition voters. Specifically, for each group of voters, I separately estimate the direction and strength of vote-switching (Stokes, 2005), turnout-buying (Nichter, 2008), and *abstention-buying* channels.

The results of this study indicate a robust positive effect of the food-subsidy program on the incumbent support. Though small, the effect is comparable to the margin of victory in the election. This result is robust to alternative specifications of the econometric model and present in different sub-samples of the data. Based on this causal estimate and the prices and sales quantities at the grocery stores, I calculate that $\sim$ %5 of GDP per capita is required to generate an additional vote for the incumbent. Interestingly, but perhaps not surprisingly, this is much smaller than the spending required –%32 of the GDP– to generate an additional vote in the U.S. (Chen, 2013; Levitt and Snyder Jr, 1997).

Furthermore, the separate analyses of core supporters and swing voters reveal which channels –vote-switching, turnout-, or abstention-buying– are more salient for each group in vote-buying. The findings indicate that the swing voters are the most responsive group, and they respond mostly by vote-switching, as one would expect. On the other hand, while turnout-buying is the most salient channel for core incumbent supporters, it is the abstention-buying channel that describes the response of core opposition supporters. Failing to account for partisanship, therefore, is likely to obscure this important distinction between the responses of these groups to vote-buying campaigns, since countervailing effects –abstention and turnout– cancel each other out when partisanship is not controlled for.

Importantly, the abstention-buying from core opposition voters suggests a reciprocity on their side to the incumbent that is ideologically opposed, but made a distributive transfer to them. Such evidence is noteworthy because it implies that incumbents may still electorally benefit without engaging in clientelistic campaigns when making distributive transfers.

This paper has two main contributions to the literature on vote-buying. The first one pertains mainly to the empirical strategy that is based on the actual walking distances between voters and the program stores. Given that quota of a maximum three kg of subsidized groceries per visit was in practice, the potential beneficiaries of the program needed to commute frequently to the program stores to benefit from it. The walking distances to the stores therefore truly reflect the voters' accessibility to the program in this particular setting, as well as introducing a variation in the likelihood of voters to benefit from it.

Since I also work with polling-station level election outcomes and precise geographical location data of polling stations and grocery stores, I am able to quantify the accessibility of voters to these stores at a very granular level, which has been more often than not a rare combination in the literature to date (see Bobonis et al. (2017) for an exception).[4] The availability of granular geographical data facilitates the delineation of the catchment areas of the program stores, which in turn allows an analysis of spatial political heterogeneity that has not been studied within the vote-buying context

[4]Data at the polling-station level is the most disaggregate level possible in this context while also being stable in terms of voter assignment and geographical location.

before.

Moreover, this empirical strategy and the data on prices and sales quantities at the program stores enables the calculation of the cost effectiveness of the vote-buying campaign in this particular setting. Finally, it also allows me to mitigate the ecological inference problem, a common source of concern in the previous literature.[5]

The second contribution of the paper concerns the specific channels through which different political groups respond to the program. The previous studies have mostly focused on the vote-switching model while the turnout-buying model has been relatively overlooked (Weschle, 2014; Tillman, 2008; Blais, 2006). The studies that did study the turnout-buying channel, on the other hand, investigated the responses to vote-buying campaigns by focusing on the mobilization of core incumbent supporters (turnout-buying) rather than the abstention of opposition voters (abstention-buying).[6] In this paper, I document that the abstention-buying channel is at least as strong as the other two channels in vote-buying.

Importantly, the abstention-buying channel in voting-buying reveals a distinction between the role of political ideology in pocketbook and sociotropic considerations in voting.[7] Specifically, while partisanship aggravates the polarization in opinions on the national economic performance of incumbents (Healy et al., 2017; Yagci and Oyvat, 2020), in the case of pocketbook considerations, different political groups respond to the transfer in line with the reciprocity rule. This hints us that amidst extreme partisanship and political polarization, the pocketbook considerations are still an important predictor of vote choice, which is also consistent with the theoretical political economy literature (Ansolabehere et al., 2014).

The most related two papers to this study are by Akbulut-Yuksel et al. (2020) and Adiguzel et al. (2020). The former investigates the electoral returns to expansive expressway construction by the incumbent party in Turkey between 2002 and 2011 by using province-by-year variation in the construction of expressways. They find that that a 10% increase in the length of expressways leads to a 0.3pp increase in the incumbent vote share. Importantly, they show that the electoral returns are not due to economic benefits generated by the expansion of expressways but rather due to the perceived competence of the incumbent signaled by the visibility of the newly constructed expressways. The present study differs from this paper primarily by documenting the economic benefits as the driving force behind the electoral gains rather than the visibility or competence signaled by the subsidy program. Also, the present paper utilizes a more granular data set to quantify the electoral effects, albeit with a narrower focus than Akbulut-Yuksel et al. (2020) –Istanbul vs. Turkey.

The paper by Adiguzel et al. (2020), on the other hand, study the electoral ef-

[5]The ecological inference problem refers to the intricacy of inferring conclusions about individual-level behavior from more aggregate data. The essence of the problem is that several different individual-level relationships may exist and generate an observationally equivalent result at a more aggregate level. Using data as close as possible to the individual level is therefore one way of mitigating this problem.

[6]Bierbrauer et al. (2017) provide evidence of demobilization of opposition voters with a theoretical model. Nevertheless, the only study that provides empirical evidence, to the best of my knowledge, is within the context of a disaster-relief policy by the U.S. government in 2004 by Chen (2013).

[7]Sociotropic considerations refer to the mechanism in which voters evaluate national economic performance under the incumbent's term rather than their own personal economic situation when making voting decisions (Meya et al., 2020; Kinder and Kiewiet, 1979; Kiewiet and Lewis-Beck, 2011; Aytaç, 2018; Yagci and Oyvat, 2020). Başlevent and Kirmanoğlu (2016) and Çarkoğlu (2012) document evidence for sociotropic considerations particularly for the Turkish electorate.

fects of a change in the accessibility to free healthcare in Istanbul. They exploit the exogenous changes in the walking times to the nearest free health clinic after a reform called *the Family Medicine Reform* that has been implemented by the incumbent party. Similarly, they document that the decreases in walking times to the nearest health clinics lead to an increase of 0.7pp in incumbent support. They moreover show that this effect is more pronounced in low-income and healthcare service dependent communities. The study by Adiguzel et al. (2020) is the closest one to the present paper in the sense that both studies utilizes a granular data set at the polling station level and leverage the distance between polling stations and the service provided.

Nonetheless, the present study mainly differs from these two studies by i) quantifying the vote- and turnout-buying channels, and showing how and through which channels voters of different political views respond to the subsidy program, ii) estimating how much spending is required to buy an additional vote, and finally, iii) focusing on a temporary, non-programmatic, pre-election food subsidy program rather than a more programmatic and systematic public good provision, such as free health clinics or expressway construction. The last point implies that even a short-lived subsidy program, such as the one studied in this paper as opposed to a more programmatic public good provision, bring about electoral effects comparable to those of the latter.

The rest of the paper is organized as follows: Section 2 provides the empirical setting where the program was implemented and the details of its organization. Section 3 describes the data and the empirical strategy. Section 4 presents the results. Sections 5 discusses how distinct political groups respond to the program. In Section 6, I provide the concluding remarks.

2 Empirical Setting

A food-subsidy program that was set up in Istanbul, Turkey, in 2019 provides an ideal setting to study vote-buying and the responses to it. This program subsidized some essential grocery products and delivered these subsidized groceries via state-run grocery stores that were installed in the district centers of Istanbul. Several reasons render this program a suitable setting for the purpose of this study.

First, the program was instituted in March 2019 –two months before the municipal elections– when the food-price inflation was at its historical peak of a 30% annual rate (inflation in overall prices was 20% in the same period). In an economy where food-related spending constitutes a quarter of the consumer basket (TurkStat 2019), a 30% inflation in food prices stands for a severe adverse shock to people's pocketbooks. For the very same reason, the high food prices were the most salient topic, especially in urban areas, as the election approached.[8]

Second, the program did not involve a targeting mechanism by the implementer, such as addressing swing or core supporters. A vast majority of Istanbul's districts had the program implemented regardless of their political orientation. This implies that, at least at the district level, the program allocation was not clientelistically distorted by political favoritism or that it was not strategically targeted to swing voters.

[8] A survey study by Aydin et al. (2019) provides supporting figures. The percentage of people who reported the cost of living as the most important problem in Turkey was 17.8 at the beginning of January 2019. It was the second-most-reported problem after unemployment. The previous version of the same study reports that unemployment and cost of living had been the third- and fourth-most-reported problems in the previous year.

Third, the program was implemented between the two elections in 2018 and 2019. The relatively short time between these elections strengthens the comparability of their outcomes and aids us in refuting alternative arguments. Finally, the political context in which these two elections took place was one of a highly polarized electorate and high partisanship. Hence, votes were frozen within the opposing blocks of the two main parties with little possibility of shifting in between (IstanPol Report, 2019).[9]

This particular context has two implications for the findings of this paper. First, if the program is effective in buying votes, we expect to find evidence for the turnout-buying channel because vote shifts are expected to be rare due to high levels of polarization. Second, any evidence for the vote-switching channel in this setting should be deemed as particularly strong evidence for its presence in general, because the electoral context in this study makes vote-switching relatively unlikely compared to an electoral context with low levels of polarization.

2.1 Institutional Background

In this paper, I focus on two consecutive elections in Turkey: the presidential elections of 2018 and the municipal elections of 2019 in Istanbul.[10] Following the constitutional change in April 2018, the presidential election in June 2018 was both the first presidential election and the first election to allow parties to form alliances before the competition. These alliances were formed for gaining the half of the votes to win the presidency in the first round or to exceed the 10% threshold to enter the parliament. The then incumbent president, Recep Tayyip Erdoğan, of the *Justice and Development Party* (hereafter the AKP), was re-running for the presidency under the new constitution after 16 years of rule. To secure 50% of the votes in the first round, the AKP formed the so-called *Cumhur* Alliance with the Nationalist Movement Party (the MHP) for the presidential elections of 2018. The presidency was won by the *Cumhur* Alliance and its candidate Recep Tayyip Erdoğan. His vote share in Istanbul was just above 50%.

The *Cumhur* Alliance also participated in the municipal elections of March 2019 in Istanbul. Their candidate was the former prime minister Binali Yıldırım of the AKP.[11] The electoral campaign by the *Cumhur* Alliance for the municipal election in Istanbul, however, was exclusively run by the president, Recep Tayyip Erdoğan. He campaigned himself, holding large meetings in Istanbul squares, and by appearing on television. In short, the president used his own popularity among the electorate to ask for votes in the municipal election.

The main reason behind all these efforts was to keep controlling the Istanbul Metropolitan Municipality, its huge municipal budget, and economic potential. Despite all the efforts exerted by the popular president Erdoğan, the *Cumhur* Alliance lost the Istanbul Metropolitan Municipality to the candidate of the major opposition party in a very tight competition where the margin of victory was 0.25%.[12]

[9] The Turkish version of this report is available online here.

[10] The implications of comparing a presidential election to a municipal election are discussed in detail in Section 4.3.

[11] The incumbent party in the Istanbul Metropolitan Municipality was the AKP before the municipal election in March 2019.

[12] Although it is not relevant for the purpose of this study, I feel obliged to point out that the municipal elections of Istanbul in 2019 were canceled –to be re-run in June 2019– due to alleged vote stealing by the opposition party members. The official results of the March 2019 municipal elections,

2.2 The Food-Subsidy Program: State-run Grocery Stores

The incumbent party's response to high inflation in food prices was to launch state-run grocery stores in big cities of Turkey, including Istanbul in the beginning of February 2019 –approximately two months before the municipal elections (Bakış and Acar, 2019). Figure 1 shows the timeline of these events. The newly launched state-run grocery stores supplied subsidized food –mainly vegetables, but also legumes– under a campaign called "Fighting Inflation Together".

Although the program had some limited potential to provide a temporary relief to people who live close enough to the program stores, launching 52 grocery stores was by no means an effective tool to curb inflation in food prices in a city with more than 15 million inhabitants. Having noted that, the state-run grocery stores were quite popular in terms of people shopping at these stores, as covered by several Turkish and foreign media outlets.[13] Therefore, we know that it actually provided a financial relief to some people in the form of subsidized groceries.

This food subsidy program differs from the typical programmatic public good provisions in several ways. First, it was not a systematic policy implemented by the central government as in the cases of expressway construction and free health clinics in Akbulut-Yuksel et al. (2020) and Adiguzel et al. (2020), but instead the municipalities of a few cities including Istanbul implemented it idiosyncratically. Second, although public good provisions by the government typically include durable services, such as universal healthcare/health clinics, roads, bridges, free education/schools, water wells, dams, etc., this food subsidy program was announced as a temporary policy from the very beginning. Third, the program was launched supposedly as a mean to fight the inflation in food prices while it was also obvious that it was not an effective tool to curb the inflation in food prices with such a policy.[14] Therefore, there was a clear discrepancy between the goal and the capabilities of the program, which is not the typical case in public good provisioning.

It was instead largely perceived as a populist move by the incumbent to increase its support in the approaching elections.[15] This view of the subsidy program has been reinforced by president Erdogan's comments on the issue before the elections, who blames "ugly games" played on the Turkish society by external forces and condemns the increasing food prices as an attempt to terrorize the society. The facts that the program spanned only two and a half months prior to the elections, and that it was largely abolished immediately after the elections, strongly suggest that the program was in fact a move by incumbent to increase its votes in the upcoming elections.

On the other hand, it is important to note that this program did not specifically target any group of voters (core supporters, swing voters, etc.) as is the typical situation in vote-buying strategies. However, the fact that the program was announced shortly before the elections as a temporary policy and immediately canceled after the elections render it more of a vote-buying strategy by the incumbent. In this sense, the salient mechanism was the feelings of obligation/reciprocity on the side of voters, as is demonstrated by Finan and Schechter (2012), though without identification and

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however, were announced by the Higher Election Board before the cancellation.

[13] See the articles from BBC Turkish, The Atlantic, and, Reuters.

[14] This has also been voiced by Turkish economists in the media. See the articles from the Atlantic and Financial Times.

[15] The populist and opportunistic character of this program is also recognized by a European Social Policy Network report commissioned by the European Commission (Adaman and Erus, 2019).

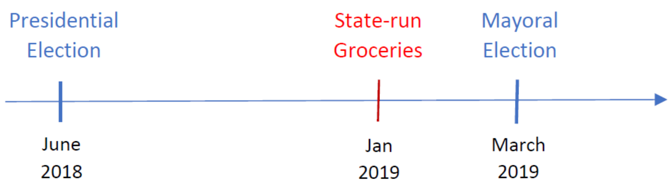

Figure 1: Timeline of events

targeting of reciprocal voters by the incumbent in this particular case.

The most comprehensive implementation of this food-subsidy program came about in Istanbul with 52 grocery stores. The plan was implemented by the Istanbul Metropolitan Municipality, which was held by the incumbent AKP at the time. While mobile food trucks were also used in other cities, only fixed grocery trucks and tents were installed at the district centers in Istanbul. The grocery stores initially sold eight different vegetables that are very common and standard in Turkish cuisine and that also exhibited high inflation: cucumbers, eggplants, onions, two kinds of paprika, potatoes, spinach, and tomatoes.[16] At a later stage, chickpeas, lentils, and rice were also included among the groceries. Each individual was entitled to buy a maximum of 3 kg of vegetables and legumes in total in a single visit. This meant that, to benefit from the subsidies, one needed to make frequent visits to one of the program stores.

Table 1 presents the prices of these groceries at the program stores and those at the Istanbul wholesale food market. The prices at the latter are the averages of minimum daily prices for every product during the first week of February 2019. According to these prices, the average discount rate at the state-run stores is around 30%. However, it should be noted that the prices at the wholesale food market are not the prices a typical consumer finds. Final consumers face higher prices due to intermediaries such as transporters, supermarkets, etc. In addition to this, I use the minimum daily prices at the wholesale food markets. Considering these two points together suggests that 30% is a conservative estimate of the discount rate. However, even this 30% discount is substantial when applied to already high food prices and when one considers the 30% inflation in food prices.

Regarding the allocation of state-run grocery stores, 34 out of 39 districts of Istanbul had at least one implemented prior to the municipal elections. The remaining five districts were excluded because they were mostly rural and drew on active agricultural production. Within the districts, the grocery stores were located at central places such as main squares, or next to the municipality and other official buildings. All the store locations were easily accessible on foot, in areas that are crowded during daytime, and with areas available for queuing and storing the food products. Figure 2 shows the locations of state-run stores and the population of Istanbul at the neighborhood level reported by the Istanbul Metropolitan Municipality.

Although the choice of store locations is clearly not random, given that central locations are preferred for logistic reasons such as accessibility, population size, and storage and queuing areas, I assume that the choice of locations is *as-if random* conditional on the fact that central places are chosen within districts. I discuss the validity of this assumption and the potential threats to it in detail in Section 3.2 and the

[16] Gadenne (2020) studies a highly similar policy in India and documents that such a policy brings economic relief to its beneficiaries. However, she does not analyze the responses to this policy.

Table 1: Food prices at the state-run grocery stores and Istanbul wholesale food market

	Prices (in Turkish Lira)	
	State-run Stores	Wholesale (min. prices)
Cucumber	4	4
Eggplants	4.5	6.8
Onion	2	3.16
Paprika type-1	6	8.6
Paprika type-2	6	10
Potato	2	3.06
Spinach	4	3.8
Tomato	3	8

Note: All reported prices are per kg of product. The prices reported for the wholesale food market are the averages of the minimum daily prices per kg of each product during the first week of February 2019.

alternative mechanisms that this assumption may entail in Section 4.3.

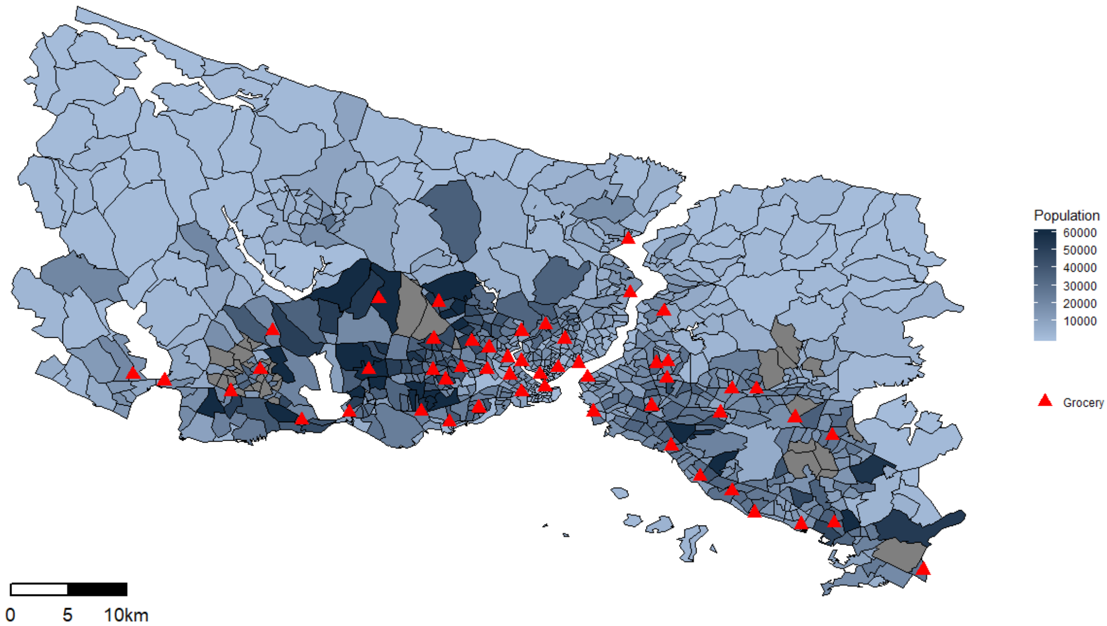

Figure 2: Population and the locations of state-run grocery stores

Note: The map shows Istanbul's population at the neighborhood level and the locations of the state-run grocery stores. The red triangles represent the state-run grocery stores. The population size increases from light to dark blue. A few observations with a larger population than 60,000 are truncated to 60,000. The map is trimmed from both the east and the west for a fine-grained look. There were no state-run grocery stores in the truncated regions.

3 Empirical Framework

The following two subsections describe first the data sets used, and second, the empirical strategy that allows a causal interpretation of the estimated effects of the food-subsidy program.

3.1 Data

To study the effects of the food-subsidy program on voting behavior, I build a data set by combining data from several sources. These include election outcomes at the polling-station level, precise geographical coordinates of the polling stations and the program stores. I supplement these data with administrative data on the demographic and socioeconomic characteristics of Istanbul's neighborhoods.[17]

The first data set provides the election outcomes at the polling-station level for the two elections in 2018 and 2019. I obtain these data from the major opposition party (CHP–*Cumhuriyet Halk Partisi*) in Turkey. For both elections, this party published the election results at the ballot box level on their website. They also reported the name of the polling stations to which the ballot boxes belong. I aggregate the ballot box level results to the polling-station level since the polling station is the most disaggregate level that is also geographically meaningful and stable in terms of voter assignment.[18] This aggregation yields 1,589 polling stations located in the districts of Istanbul where the program was implemented. The average number of registered voters at these polling stations is ~ 6000 for both elections.

The main dependent variables of the analysis are the incumbent vote share and the turnout rates at the polling-station level. I operationalize these variables, respectively, as the number of votes for the incumbent over the number of total votes and the number of total votes over the number of registered voters.

A second data set includes the geographical coordinates of the polling stations and state-run stores. The locations of the state-run stores were determined and announced by the Istanbul Metropolitan Municipality. I geo-code the polling stations and state-run stores through *Google Maps* based on the names of the polling stations and the addresses of the stores as given by the municipality.

The main variable of interest in this paper is the *Distance* between the polling stations and the nearest program stores. I compute this distance variable using the *Google Maps Distance Matrix API*. The computed distances in km represent the walking distances on a weekday at noon from a polling station to the nearest state-run store.[19] The treatment variables that I adopt in the following analyses are the distance variable, its square root, and its discretized version.

The third data set provides administrative data on the demographic and socioeconomic characteristics of the neighborhoods from MahallemIstanbul project.[20] This project gathers data from different administrative records for the neighborhoods of Istanbul.[21] However, it covers only up to the year 2017. In the analyses in the subse-

[17] Neighborhoods are the smallest administrative units in Turkey and are followed by districts and provinces. The province of Istanbul has 782 neighborhoods and 39 districts.

[18] The assignment of voters to polling stations depends on their proximity to the polling stations. Therefore, it is safe to assume that voters who live near to each other vote in the same polling station.

[19] See Figure D.1 for the distribution of the distance variable.

[20] Administrative data at the level of polling stations, unfortunately, does not exist.

[21] This is a joint data-gathering project by the Istanbul Metropolitan Municipality, its companies,

quent sections, I include population size, the female share of the population, average age, and the share of population with low education from 2016. I include the level of house prices and rents from 2017 to proxy the economic development level of the neighborhoods. Table C.1 shows the descriptive statistics of these variables.

3.2 Empirical Strategy

The identification strategy that I leverage in this paper is based on the variation in the accessibility of voters to the state-run stores operating under the food-subsidy program. The accessibility of voters to these stores depends on the distance they have to walk to the nearest store. As such, I build the *Treatment* variable based on the *Distance* variable and operationalize it in three versions.

First, I use the *Distance* variable itself as the treatment since this is the most straightforward and assumption-free metric. The treatment effect, however, is unlikely to be linear. Going an extra km further away from a state-run store is not likely to have much effect on voting behavior if one is already too far away from it. Therefore, the second version of the treatment variable is the square root of the *Distance* variable, which accounts for this non-linear functional form.

Third, I use a binary treatment variable that is also based on the *Distance* variable. This binary treatment variable allows me both to translate the estimates of the treatment effects into actual number of votes and identify the geographical range of the catchment areas of the program. The delineation of geographical catchment areas, in turn, enables the analysis of inter-group interactions. I formally define the binary treatment variable like the following:

$$Treatment_i = \begin{cases} 1, & if \quad Distance_i \leq k\ km \\ 0, & if \quad Distance_i > k\ km \end{cases}$$

where $Distance_i$ is the distance of polling station i to the nearest state-run store. $Treatment_i$ is 1 when the polling station i falls within k km of any state-run store (*treatment* group), or 0 otherwise (*control* group).

The catchment areas refer to the areas within which the individuals could benefit from the state-run grocery stores and are defined as a circle of radius k around each state-run store. This is based on the idea that, beyond a certain distance, the costs of commuting to the program stores will offset the gains from the program (Ichino and Nathan, 2013). After I document that the program has an effect on voting behavior through the first two versions of the treatment variable (*Distance* and its square root), I then experiment with different treatment cut-off values (k) to identify the geographical range of the food-subsidy program. This experimentation suggests 2 km as the geographical range of the catchment areas.[22] In other words, the polling sta-

the governorship of Istanbul, several ministries, and some other government institutions. It is available online at https://www.mahallemistanbul.com/.

[22]In Appendix A, I explain in detail the analysis that suggests 2 km as the treatment cut-off value. Independently of this analysis, however, Google Maps estimates that 2 km takes 25 minutes to walk on average. This is a reasonable upper limit for the potential beneficiaries of the program, considering that a round trip requires a 50-minute walk. I do not consider the public transport option since it is costly and would significantly offset the gains from the program, especially when one considers that each person was entitled to buy only 3 kg of subsidized groceries in a single visit.

tions that fall within the catchment areas of the state-run stores are the treated ones. These catchment areas contain, on average, 15 polling stations, whose assigned voters benefited from the food-subsidy program.

The main goal of this empirical analysis is to compare polling stations that benefited from the food-subsidy program (*treatment* group) with those that did not (*control* group). Such a comparison would yield causal estimates if the choice of grocery locations were at random. However, we know that the choice of sites is instead influenced by logistic and geographic factors such as the centrality of the location, easy accessibility, population size, and availability of space for storage and queuing.

The second-best method for establishing causality is to ensure that the choice of store locations is *as-if random*. If we could safely assume that the choice of store locations is exogenous to the factors that can also affect voting behavior, we then would be confident about the causality of the estimated effects. To show –albeit indirectly– to what extent this assumption holds, I check the balance between the treatment and control groups on observable variables that are likely to affect both election outcomes and the choice of store locations.

Table 2 reports the means of observable variables for treatment and control groups. It suggests that the sample is well-balanced. The t-test comparisons show that the only statistically significant difference is that of the average age variable. The magnitude of this difference, however, is not substantial enough to have a meaningful impact on the outcome.[23]

Yet, the t-test approach does not account for the district-level variation. Therefore, alternatively, in the last column of Table 2, I report the coefficients of the binary treatment variable (with a 2 km cut-off) from regressions of observable variables –at the polling station level– on the binary treatment variable and the district fixed effects with standard errors clustered at the district level. The specification concerning, for example, the previous incumbent vote share is as follows:

$$Prev\_IncVote_{ijk} = \beta \cdot BinaryTreatment_i + D_k + \mu_{ijk},$$

where $Prev\_IncVote_{ijk}$ corresponds to the previous vote share of the incumbent at the polling station i in neighborhood j of district k, and D_k corresponds to the dummy variable for district k. The last column of Table 2 also confirms that none of the observable variables at the polling station level significantly differs between treatment and control groups. I shall discuss the alternative mechanisms that the as-if random allocation assumption may entail in Section 4.3.

[23] When comparing the neighborhood-level characteristics between the treatment and control groups through t-tests, I assume that each polling station reflects the same characteristic as the neighborhood it belongs to. Since neighborhoods are very small administrative units in the current setting and each neighborhood includes on average ~ 2 polling stations, the measurement error stemming from this assumption of homogeneity within neighborhoods is expected to be minimal.

Table 2: Balance on observables

Variable	Treatment	Control	Difference	Treatment Coef.
Polling station level:				
Previous Inc. Vote *(%)*	44.34	42.38	1.96	0.541
Previous Turnout *(%)*	75.93	76.99	-1.06	-2.996
No. of Votes Cast	5446.39	5472.15	-25.76	-190.82
No. of Registered Voters	6221.56	6191.26	30.31	-187.25
Distance *(in km)*	1.06	3.22	-2.16***	
Neighborhood-level:				
Population *(in thousands)*	25.93	27.23	-1.30	
Share of Females in the Population *(%)*	0.44	0.44	0.00	
Average Age	29.83	28.51	1.32*	
Share of Low-educated People *(%)*	0.49	0.49	0.00	
House Prices *(0-10)*	3.56	3.55	0.01	
House Rents *(0-10)*	3.54	3.38	0.16	
No of Observations	785	804		1589

Note: The first two columns report the means of treatment and control groups. The third column reports the differences in group means and their statistical significance from corresponding t-tests. The last column shows the coefficients of binary treatment variable in regressions of each observable variable on binary treatment (with a 2 km cut-off) and district fixed effects with standard errors clustered at the district level. Previous Inc. Vote corresponds to the vote share of the incumbent in the previous elections (2018). No. of Votes Cast and No. of Registered Voters are also from the 2018 elections. Share of Low-educated people indicates the share of people with no education, primary education, or elementary education in the total population. House Prices and House Rents are index variables that can take discrete values from 0 to 10. The higher values indicate higher prices and rents. Distance indicates the distance between polling stations and nearest program groceries. The observations are weighted by the number of registered voters at each polling station in the 2018 presidential elections when comparing treatment and control groups in all observable variables except No. of Votes Cast, No. of Registered Voters, Distance, and Population. *p<0.1; **p<0.05; ***p<0.01.

To reduce the concerns about omitted variable biases to the minimum, I include all observables as control variables in the subsequent analyses. I hope that doing so aids us in accounting for any pre-treatment difference in observables between the treatment and control units (Duflo et al., 2007).

In this regard, an important related factor that further strengthens the causal interpretation of the estimated effects is the very short time –nine months– between the two elections of interest. These nine months were characterized by high partisanship, votes locked in the two opposing blocks, and little room for vote shifts between the vote blocks.[24] Moreover, the most salient topic before the last election was the high inflation of food prices, along with no changes in other main policy areas.

Taken together, these characteristics of the context suggest that the first election provides a useful control variable (or baseline measurement of the outcome) for the second election. A very high correlation (0.99) of incumbent vote shares between these two elections supports this argument. Accordingly, in the subsequent analyses, the previous incumbent vote share explains almost all the variation in the incumbent vote share in the second election with a coefficient very close to one (and with an R-squared of 0.99). Therefore, the inclusion of the previous incumbent vote share as a control variable is fundamental in helping us account for a great deal of pre-existing differences and aids in refuting alternative mechanisms that may cause changes in voting behavior.

[24]IstanPol Report, 2019. See Footnote 9.

An underlying assumption in this empirical strategy is that individuals prefer to shop from the nearest state-funded grocery store. It is of course possible that some individuals may prefer another state state-funded store rather than the closest one, or switch between different state-funded stores. Nonetheless, I would expect that, even in the case of shopping from more than one state-funded store, individuals would still take the travel time into account. In other words, I would not expect individuals to completely ignore distances to alternative state-funded groceries, and travel much larger distances even when they had a closer option. This in turn reassures that the treatment level of individuals would not be seriously affected from such possibilities.

Finally, to estimate the effect of the food-subsidy program on the incumbent vote share and turnout rates, I use the following econometric specifications:

$$\begin{aligned} IncVote_{ijk} &= \beta_0 + \beta_1 \cdot Treatment_i + \beta_2 \cdot \boldsymbol{X_j} + \beta_3 \cdot Prev\_IncVote_{ijk} && (1)\\ &+ \beta_4 \cdot Prev\_Turnout_{ijk} + D_k + \epsilon_{ijk}, \\ Turnout_{ijk} &= \alpha_0 + \alpha_1 \cdot Treatment_i + \alpha_2 \cdot \boldsymbol{X_j} + \alpha_3 \cdot Prev\_IncVote_{ijk} && (2)\\ &+ \alpha_4 \cdot Prev\_Turnout_{ijk} + D_k + u_{ijk}, \end{aligned}$$

where $IncVote_{ijk}$ and $Prev\_IncVote_{ijk}$ correspond to the incumbent vote shares in the 2019 and 2018 elections, whereas $Turnout_{ijk}$ and $Prev\_Turnout_{ijk}$ correspond to the turnout rates in the 2019 and 2018 elections at the polling station i in neighborhood j of district k. $\boldsymbol{X_j}$ is a vector of control variables at the neighborhood level, including population size, the female share of the population, the share of population with low education, average age, and the level of house prices and rents. D_k corresponds to the dummy variable for district k.

4 Electoral Returns

Below I first present evidence for the effect of the food-subsidy program on voting behavior. I then proceed to show the robustness of the results, discuss the alternative mechanisms, and present a placebo test.

4.1 Baseline Results

In this subsection, I estimate the causal effect of the food-subsidy program on the incumbent vote share and turnout rate for the entire sample, using the three different versions of the *Treatment* variable. In all the subsequent analyses, I include the district fixed effects and cluster the standard errors at the district level. Moreover, to provide more accurate estimates of the effects, I weight the observations by the number of registered voters at each polling station in the 2018 presidential election.

Table 3 shows the results of the baseline analysis. The first three models show the effect of the food-subsidy program on the incumbent vote share using the treatment variables, respectively, the *Distance*, the square root of the *Distance*, and the binary treatment variable with a 2 km cut-off. The negative coefficients of the treatment variable in Models (1) and (2) indicate that the incumbent vote share increases when the distance between polling stations and the nearest state-run grocery store decreases.

These coefficients are small, yet they are comparable to the margin of the second election. To translate these estimates into numbers of votes, I turn to the models with the binary treatment variable.

Models (3) and (6) report the coefficients of the binary treatment variable with a 2 km cut-off for the incumbent vote share and turnout, respectively. The positive and statistically significant coefficient of the treatment variable in Model (3) indicates a positive effect of state-run stores on the incumbent vote share. This coefficient implies that being within 2 km of a state-run store increases the incumbent vote share by 0.4pp. Although small, this effect is still larger than the 0.25pp margin of victory observed in the second election.

To compare the size of this effect with the margin of the election, I convert both percentages to the actual number of votes. The 0.4pp treatment effect on the treated group amounts to ∼16000 votes, whereas 0.25pp margin amounts to ∼21,000 votes. In short, the effect of the food-subsidy program turns out to be still comparable to the margin of the election.[25] Given that this subsidy program was of temporary nature and rather small-scale compared to the size of the city, and that the Turkish electorate was highly polarized between the incumbent and opposition blocks with little possibility of vote shifts in between, the small treatment effects should not be very surprising.[26]

On the other hand, Models (4), (5), and (6) suggest that the program has no statistically significant impact on turnout rates. As the next section shows, however, these null effects are due to the heterogeneous effects of the food-subsidy program on turnout conditional on partisanship.

An important, but little-known aspect of vote-buying campaigns is the efficiency with which they generate electoral gains. In this regard, although not studying vote-buying campaigns specifically, Chen (2013) and Levitt and Snyder Jr (1997) both estimate that, in the U.S., buying an additional vote requires a distributive spending of \$14000. Chen (2013)'s calculation is for a disaster-relief program that took place in the U.S. in 2004. His estimate of \$14000 translates into 32% of the GDP per capita of the U.S. in 2004.

My back-of-the-envelope calculations for the case of the food-subsidy program in Turkey in 2019 indicates that the spending required to buy an extra vote is 663.75 TL.[27] This corresponds to 5.3% of GDP per capita of Turkey in 2019. Therefore, although the types of transfers are quite different, these figures suggest that it is much cheaper to engage in vote-buying through distributive transfers in Turkey than it is in the U.S.

4.2 Robustness Checks

In the previous section, we have already shown that the results remain robust to alternative operationalizations of the treatment variable. In this section, I first show that the baseline results are robust to the removal of neighborhood-level control variables. I then test the robustness of results to an alternative coding of the dependent variable.

[25]The incumbent party lost the second election. The size of the effect in terms of actual votes provides an insight into how much additional spending on the program would have secured the electoral victory. According to the estimates, increasing the number of state-run stores by half would reverse the outcome of the second election in favor of the incumbent.

[26]IstanPol Report (2019) points out the high polarization levels in the Turkish electorate between the two elections of interest to this paper. The Turkish version of this report is available online here.

[27]I discuss the calculation method and its assumptions in detail in Appendix B.

Table 3: Baseline Results

	Dependent variable:					
	Incumbent Vote			Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance	-0.071^{**}			-0.015		
	(0.035)			(0.022)		
$\sqrt{Distance}$		-0.303^{**}			-0.100	
		(0.134)			(0.086)	
Treatment-2km			0.406^{***}			0.087
			(0.130)			(0.090)
Previous Inc. Vote	0.934^{***}	0.934^{***}	0.933^{***}	0.009^{*}	0.009^{*}	0.009^{*}
	(0.007)	(0.007)	(0.008)	(0.005)	(0.005)	(0.005)
Previous Turnout	0.008	0.010	0.010	1.059^{***}	1.061^{***}	1.060^{***}
	(0.035)	(0.035)	(0.035)	(0.034)	(0.034)	(0.034)
Neigh.-level controls	Yes	Yes	Yes	Yes	Yes	Yes
District F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	45.31	45.31	45.31	80.96	80.96	80.96
Observations	1,575	1,575	1,575	1,575	1,575	1,575
R^2	0.987	0.987	0.988	0.797	0.797	0.797

Note: The reported results are from OLS estimations. The Distance variable indicates the distance in km between polling stations and nearest state-run stores. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. Treatment-2km indicates the binary treatment variable with a 2 km cut-off. All regressions include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01.

In Table 4, Models (1) and (3) report the results of our baseline specifications, whereas Models (2) and (4) report the results of the same specifications, but without the neighborhood-level control variables. These estimations indicate that the baseline results remain robust to the removal of neighborhood-level control variables.

Second, I check the robustness of baseline results to an alternative coding of the dependent variable. In the baseline analysis, the dependent variable is coded as the ratio of the number of incumbent votes to the number of total votes. Alternatively, the dependent variable can be coded as the ratio of the number of incumbent votes to the number of registered voters. Model (5) and (6) report the estimations of baseline specification with the alternative coding of the dependent variable.[28] The results, once again, remain the same.

4.3 Alternative Mechanisms

The primary candidate for alternative mechanisms concerns the comparison of presidential with municipal elections. One can plausibly argue that voters' perceptions,

[28]Whenever I use the alternative coding for the incumbent vote share, I also use the alternative coding for the previous incumbent vote share.

Table 4: Robustness checks

	Dependent variable:					
	Inc. Vote		Turnout		Inc. Vote	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment-2km	0.406***	0.431***	0.087	0.093	0.392***	0.086
	(0.130)	(0.144)	(0.090)	(0.092)	(0.130)	(0.090)
Previous Inc. Vote	0.933***	0.921***	0.009*	0.014***	0.898***	0.010**
	(0.008)	(0.006)	(0.005)	(0.005)	(0.008)	(0.005)
Previous Turnout	0.010	0.040	1.060***	1.041***	0.044	1.056***
	(0.035)	(0.037)	(0.034)	(0.034)	(0.033)	(0.035)
Neigh.-level controls	Yes	No	Yes	No	Yes	Yes
District F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	45.31	45.31	80.96	80.96	38.14	80.96
Observations	1,575	1,589	1,575	1,589	1,575	1,575
R^2	0.988	0.987	0.797	0.784	0.986	0.797

Note: The reported results are from OLS estimations. Inc. Vote indicates the vote share of the incumbent. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. Treatment-2km indicates the binary treatment variable with a 2 km cut-off. The control variables at the neighborhood level are population, share of females in population, average age, share of low-educated people, house prices, and house rents. Models (5) and (6) use an alternative coding of the dependent variable. The standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01.

expectations, and incentives differ substantially over these two types of elections. Nevertheless, for these differences to constitute an alternative mechanism, they must also be correlated with the distance to the nearest state-run grocery store. Only under these circumstances would an alternative mechanism originating from this distinction provide a valid alternative explanation for our results.

We know, however, that the municipal elections of major cities such as Istanbul are perceived no differently than general elections in Turkey. The very high correlation (0.98) of the incumbent vote share in 2014 municipal and 2015 presidential elections in Istanbul supports this argument.[29] The same argument has also been documented by Adiguzel et al. (2020) for the consecutive mayoral and general elections held, respectively, in 2009 and 2011, and in 2014 and 2015 for Istanbul. Therefore, I see no reason why the comparison of these two different types of elections should pose any threat to the credibility of the causal estimates.

On the other hand, this comparison brings important advantages to the research design of this study. In particular, there are only nine months between these two elections, which is a much shorter period compared to five years between the same type of two elections. This ensures that there are fewer new voter registers, and less inflow and outflow of voters to and from polling stations. Moreover, the incumbent party entered both elections within the same alliance. Finally, there were no significant policy changes, such as those concerning Kurdish or refugee policies that could affect voting behavior.

[29] Kalaycıoğlu (2014) documents this exclusively for the electorate of Turkey.

A second candidate for an alternative mechanism relates to the center-outskirts distinction –or urban-rural distinction– across polling stations. Since the state-run stores are not allocated randomly but to central places within districts, the treatment variable is likely to be correlated with being near a central polling station as opposed to being in the outskirts. Therefore, the center-outskirts gradient would be an effective alternative mechanism if voting behavior differed across central and outer polling stations for reasons other than the state-run stores. However, even if this is the case, controlling for the previous incumbent vote share and turnout –which were measured only nine months previously– should eliminate the effects of such differences.

Nevertheless, a third candidate as an alternative mechanism may be a factor that affects the central and outer polling stations differently in the two consecutive elections studied in this paper. This is precisely what the food-subsidy program does. The program affects only the polling stations that are close enough in the latter election. It has no effect on the polling stations in the former election simply because it did not exist at the time. Other than the food-subsidy program, the presence of such a factor would be a major threat to the causal identification of the estimated effects. Nevertheless, I find it difficult to come up with such a factor, given the very short time between these two elections.

Although it does not eliminate this concern completely, I subset my entire sample gradually to sub-samples of polling stations that are closer to central areas to show that the effect persists within each sub-sample. More specifically, I subset my sample to polling stations within 10, 9, 8, 7, 6, and 5 km of state-run stores. I then estimate Equation 1 separately on these sub-samples.

Figure 3 shows the treatment effects estimated on these sub-samples. The results indicate that the treatment effect does not change across sub-samples. This finding, in turn, implies that a gradual shut-down of the center-periphery channel does not affect the estimated treatment effects. If the effective mechanism were a factor related to the center-periphery distinction –other than the food-subsidy program–, we then would have expected that shutting down that channel would have affected the estimated effects. Figure 3 suggests that this is not the case.[30] The balance tests on observables reported in Table 2 also suggest that none of the observable characteristics is correlated to the treatment. These findings reassure that the center-outskirt gradient is not likely to be a serious threat for the identification.

Fourth, one may raise the concern that the food-subsidy program might also affect voters' sociotropic evaluations. Two arguments, however, render this concern innocuous. First, the food-subsidy program has received broad media coverage by both pro- and anti-government media outlets as well as by foreign press (Erkoyun, 2019; Hamsici, 2019; Samson and Yackley, 2019; Yackley, 2019). This broad media coverage then implies that voters were well aware of the program, regardless of their distances to the grocery stores of the program. I, therefore, see no reason for our distance-based treatment variable to capture any change in the sociotropic evaluations of voters.

Second, even if voters change their sociotropic evaluations in response to this program, we would expect them to update their assessment of the national economy negatively. The negative updating would then imply that pocketbook and sociotropic considerations work in opposite directions, and that the effects we estimated for pocketbook considerations are conservative ones.

The fifth alternative mechanism relates to the other types of transfers, such as direct

[30]The regression tables underlying Figure 3 are reported in Table C.4.

food transfers or coal subsidies, that are typically targeted to incumbent supporters who are in need by the incumbent municipalities (Marschall et al., 2016; Bulut, 2020). If the share of voters who receive these benefits is correlated with the distance to the nearest state-run grocery, then it means that these other types of transfers confound the treatment. To address this issue, I investigate how the share of low-educated and -income voters –the typical benefiters of these clientelestic transfers– differ between treatment and control groups.

Table 2 shows how the treatment and control groups differ in observables by means of a t-test (fourth column). Since data on the share of low-income people is not available, I use the house prices and rents to proxy it. The fourth column shows that the treatment and control groups do not differ from each other in a statistically meaningful way in terms of the share of low-educated people, house prices and rents. I believe that these findings suggest that the other types of transfers do not pose a serious threat for the identification of the causal effect of the state-run grocery stores.

Lastly, another alternative mechanism may be related to a rally-around-the-flag effect. Since the incumbent has blamed the high food-price inflation on the large producers or "external" forces who try to undermine the Turkish economy, one can argue that this would trigger a rally-around-the-flag sentiment among the Turkish electorate. Though plausible as a general mechanism, once again, I do not see any reason to think that the distance-based treatment variable would pick up any variation generated by such mechanism.

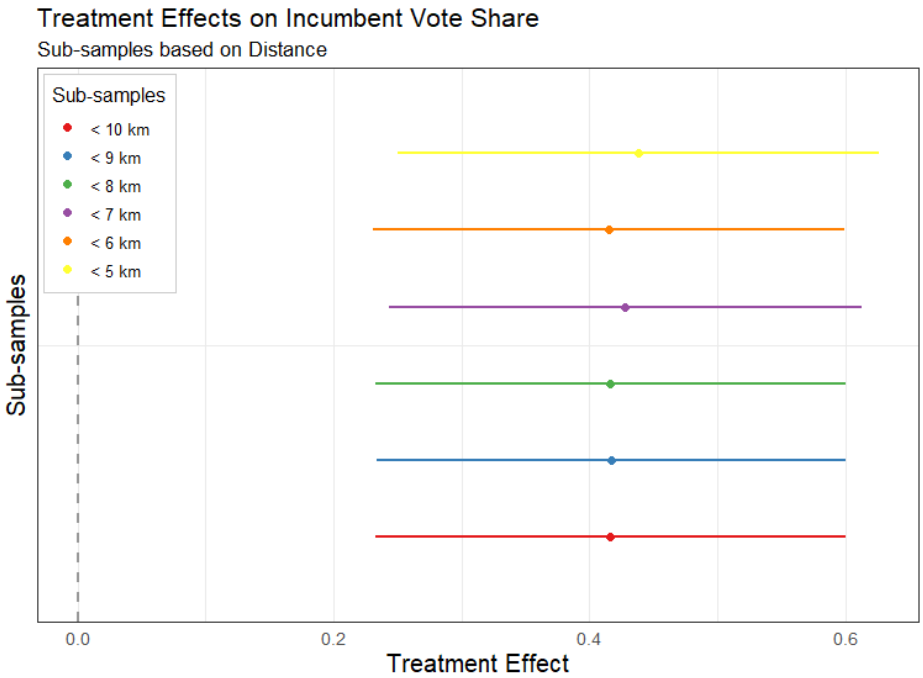

Figure 3: Alternative mechanism: center vs. outskirts

Note: The figure shows the estimated treatment effects and their 95% confidence intervals in different sub-samples of the data set based on the distance to nearest state-run stores. The dependent variable is the incumbent vote share. The cut-off for the binary treatment variable is chosen as 2 km. The results are from OLS estimations that include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The vertical dashed line corresponds to a treatment effect of zero. The point estimates and confidence intervals in different colors represent the estimates in different sub-samples of the data set. For example, <5 km denotes the sub-sample of polling stations within 5 km of the state-run stores. The confidence intervals are built based on the standard errors clustered at the district level.

4.4 Placebo Test

In this section, I provide supporting evidence for the causality of the estimated effects of the food-subsidy program on voting behavior by means of a placebo-in-place analysis. To do so, I repeat the baseline analysis in the districts where the program was not implemented. I treat these excluded districts as if there were program stores in their central squares, although actually there were none. If it was only the food-subsidy program that caused the change in voting behavior, we then should not see any significant effect of the placebo treatment on voting behavior in the excluded districts.

Since the food-subsidy program was implemented in 34 out of 39 districts of Istanbul, the remaining five districts provide a suitable sample for a placebo-in-place test. These five districts –*Adalar*, *Arnavutkoy*, *Catalca*, *Silivri*, and *Sile*– are the outer districts of Istanbul, and they were excluded from the food-subsidy program due to their active agricultural production.

Figure 4 shows the areas where the program was implemented, and where not, at the neighborhood level. Among the excluded districts, *Adalar* is a district that consists of several islands. I exclude this district from the analysis due to different uses of transport and also difficulties with public transport. The remaining four excluded districts constitute a placebo sample of 172 observations.

These four districts are located in the outskirts of Istanbul, composed of mostly agricultural areas, and commonly characterized by larger mean area (710 km^2 vs. 57 km^2) and less mean population (136,000 vs. 357,000) compared to the treated districts. They are also typically composed of a small district center and sparse settlements –villages– around the district center and within the administrative borders of the districts.

They were clearly excluded from the subsidy program from the very beginning due to their relatively stronger agricultural economy, and hence, no state-run grocery store has ever been installed in any of these districts. I, however, treat these districts as if they had one state-run grocery installed.[31] As for the locations of these placebo stores, I first zoom in the small district centers, and second, within these district centers I choose a central square/park as these are the typical state-run store locations in the treated districts.[32]

I run the placebo-in-place test by estimating Equation 1 and 2 on the placebo sample with three different versions of the treatment variable as in the baseline analysis. In all models, I weight the polling station-level observations by the number of registered voters in 2018 and include district fixed effects.

An important drawback of this placebo analysis is that there are too few clusters. Since the placebo sample consists of polling stations in four districts where the program was not implemented, the estimated cluster-robust standard errors are very much likely to be downwards biased. To address this issue, I use the wild clustered bootstrap method by Cameron et al. (2008) to compute 95% confidence intervals around the coefficient estimates.

Table 5 presents the results of this placebo analysis. The three models reported in this table are identical to the first three models reported in Table 3. They show the effects of the placebo treatment on the incumbent vote share when the treatment variable is, respectively, the distance to the nearest placebo store, the square root of this distance, and the binary treatment variable with a 2 km cut-off.

The treatment effects in Model 1 and 2 in Table 5 are extremely close to zero with confidence intervals including zero. These treatment effects (0.002 and -0.001) are much smaller than the ones in Model 1 and 2 (-0.071 and -0.303) in Table 3. The treatment effect in Model 3 in Table 3, on the other hand, is estimated as 0.161 and equivalent to 40% of the treatment effect in Model 3 of Table 3. The confidence interval of this estimate also includes zero. In overall, I believe that these findings do not pose a threat for the causality of the estimated effects in the baseline analyses. The placebo results for turnout are similar and provided in the appendix.

5 Responses Conditional on Partisanship

In this section, I focus on the heterogeneity in the responses of different political groups to the vote-buying campaign. The baseline specification abstracted the analysis from the possibility of heterogeneous treatment effects. Yet, if the treatment affected the

[31] Most of the districts, which were actually part of the program, had one state-run grocery store with a few exceptions with denser population having more than one store.

[32] Note that changing the location of placebo stores within district centers are not likely to affect the treatment status of polling stations in these districts because the districts centers are small settlements themselves, the surrounding villages are further away than 2 km, and walking is not an option since it is only motor vehicle ways between the villages and the district center.

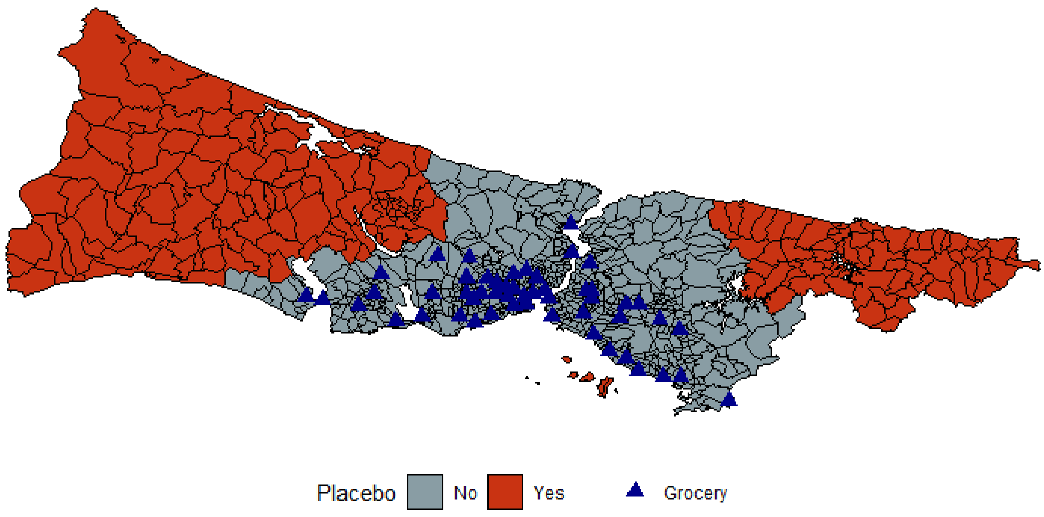

Figure 4: Placebo areas vs. grocery-receiving areas

Note: The map is at the neighborhood level. The grey neighborhoods represent the neighborhoods of the districts where the program was implemented. The red neighborhoods represent the neighborhoods of the districts that are excluded from the program. The blue triangles represent the state-run stores in the districts where the program was implemented.

incumbent vote share and turnout rates differently in different political groups, failing to account for this conditioning then may obscure important dynamics, especially if the mechanisms work in opposite directions for different groups.

Accordingly, to investigate the heterogeneity in responses, I classify polling stations into core incumbent, swing, and core opposition constituencies based on a margin of victory variable. I define the *Margin* of victory and *Partisanship* variables, for each polling station i based on the previous election results as follows:

$$Margin_i = Prev\_IncVote_i - Prev\_OppVote_i$$

$$Partisanship_i = \begin{cases} \textit{Core Incumbent}, & if \quad Margin_i \geq 0.25 \\ \textit{Swing}, & if \quad -0.25 < Margin_i < 0.25 \\ \textit{Core Opposition}, & if \quad Margin_i \leq -0.25 \end{cases}$$

where $Prev\_OppVote_i$ corresponds to the vote share of the main opposition party in the previous election.

To investigate how different political groups respond to the program, I first subsample my data set based on the *Partisanship* variable. This yields three data sets consisting of just core incumbent, swing, and core opposition polling stations. I then

Table 5: The regression results for placebo-in-place analysis: Incumbent vote share

	Dependent variable:		
	Incumbent Vote		
	(1)	(2)	(3)
Distance	0.002		
	(−0.031, 0.035)		
$\sqrt{Distance}$		−0.001	
		(−0.309, 0.307)	
Treatment-2km			0.161
			(−0.378, 0.699)
Previous Inc. Vote	0.845***	0.845***	0.846***
	(0.525, 1.165)	(0.546, 1.145)	(0.698, 0.995)
Previous Turnout	−0.024	−0.024	−0.025
	(−0.161, 0.113)	(−0.160, 0.112)	(−0.159, 0.109)
Neigh.-level controls	Yes	Yes	Yes
District F.E.	Yes	Yes	Yes
Mean of Dep. Var.	52.09	52.09	52.09
Observations	172	172	172
Log Likelihood	−391.525	−391.531	−391.420
Akaike Inf. Crit.	809.049	809.061	808.841

Note: The reported results are from OLS estimations. The numbers in parentheses are the 95% confidence intervals built according to the wild clustered bootstrap method by Cameron et al. (2008). The Distance variable indicates the distance in km between polling stations and nearest state-run stores. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. Treatment-2km indicates the binary treatment variable with a 2 km cut-off. All regressions include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. *p<0.1; **p<0.05; ***p<0.01.

estimate Equations (1) and (2) separately on these three sub-samples, using the binary treatment variable with a 2 km cut-off.

The results indicate that the treatment effects on the incumbent vote share are heterogeneous over different types of constituencies, but are not countervailing. On the other hand, the treatment effect on turnout is positive in core incumbent constituencies, while it is negative in core opposition constituencies. These effects cancel each other out and yield a null aggregate effect on turnout when partisanship is not taken into account (as it is the case in the baseline specifications, see Table 3). Figure 5 reports the estimated coefficients of treatment on both the incumbent vote share and turnout in three sub-samples of the data set.[33]

In line with the baseline results, Part (a) in Figure 5 shows that different political groups responded positively to the food-subsidy program consistent with the reciprocity rule. More specifically, the program affected the incumbent vote share

[33] The regression results underlying Figure 5 and 6 are reported in Table C.3.

positively in core incumbent and swing constituencies. The treatment effect on the incumbent vote share in core opposition constituencies, although close to that in core incumbent constituencies in magnitude, has wider confidence intervals.[34] In sum, although all political groups respond to the vote-buying program positively, their responses differ in magnitude, with swing voters being the most responsive group.

On the other hand, Part (b) in Figure 5 paints a quite different picture of the treatment effects on turnout in different sub-samples. First, in core opposition constituencies, the treatment effect on turnout is negative. This indicates that, in these constituencies, there is a significant amount of *abstention-buying*. In other words, the core opposition voters respond to the vote-buying by abstaining more. This finding suggests that the material benefits provided by the incumbent make them hold more favorable views for the incumbent, which is in line with reciprocity rule.

Second, Part (b) also shows that the program did not affect turnout in swing constituencies, yet it has a slightly positive effect on the turnout in the core incumbent sub-sample. Taking all these findings together, allowing for heterogeneous effects of the treatment conditional on partisanship yields a richer set of results that the baseline model cannot provide. In fact, the baseline model obscures the treatment effects on turnout by averaging out the effects over different political groups. This, in turn, obscures the response by core opposition voters to the vote-buying campaign of the incumbent.

Having reported the heterogeneous treatment effects conditional on partisanship, I next consider the relative strengths of the vote-switching and turnout-buying channels within core incumbent and core opposition sub-samples. Separate analyses of different sub-samples facilitates identifying how much of the change in the incumbent vote share can be attributed to the turnout- or abstention-buying channel.[35]

In the swing constituencies, however, it is more difficult to recover the true turnout or abstention effect because both the turnout- and abstention-buying may be happening at the same time and may cancel each other out. If this should be the case, it would imply that the estimated turnout effect for the swing sub-sample is a conservative one. On the other hand, we can expect to find more vote-switching in the swing sub-sample, particularly a stronger vote-switching channel than the ones in core incumbent and core opposition constituencies.

Figure 6 reports the same treatment effects as in 5, but in a way that facilitates comparing the strength of vote-switching and turnout-buying channels more easily within each sub-sample. Part (a) in Figure 6 shows that the magnitude of the vote-switching and turnout-buying channels are closer to each other in the core incumbent sub-sample than they are in the other two sub-samples. This result, in turn, suggests that the increase in the incumbent vote share may be mostly driven by the increased turnout of core incumbent supporters. In other words, given that the core incumbent constituencies are composed of incumbent voters by construction, the increase in the

[34] The most likely reason for the wider confidence intervals is the relatively small number of polling stations in the core opposition sub-sample (296 polling stations) compared to the numbers of polling stations in other two sub-samples (588 polling stations in core opposition sub-sample, 691 polling stations in swing sub-sample).

[35] Note that the separate analyses of core constituencies do not exclude the possibility of the incumbent supporters (in core incumbent constituencies) abstaining or the opposition supporters (in core opposition constituencies) mobilizing due to the food-subsidy program. Nevertheless, these incidents are very unlikely, and even if present, they would imply conservative estimates of the treatment effects on turnout.

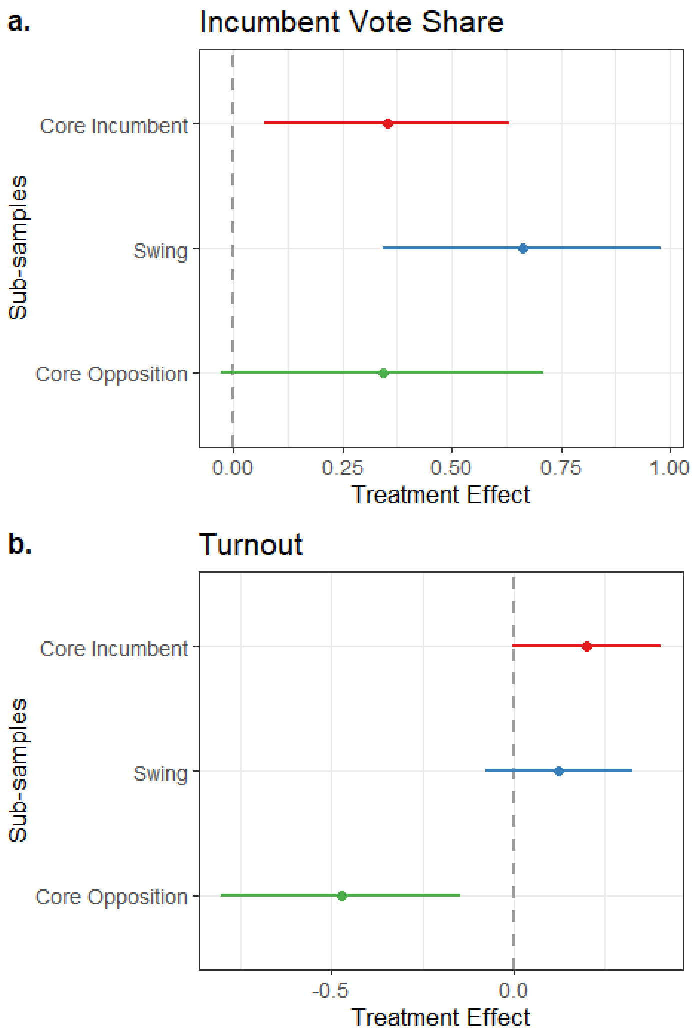

Figure 5: Vote-switching and turnout-buying in different sub-samples

Note: The figure shows the estimated treatment effects and their 95% confidence intervals in three sub-samples of the data set. The dependent variable is the incumbent vote share in Part (a) and turnout rate in Part (b). The cut-off for the binary treatment variable is chosen as 2 km. The results are from OLS estimations that include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The vertical dashed line corresponds to a treatment effect of zero. The confidence intervals are built based on the standard errors clustered at the district level. The red, blue, and green point estimates and confidence intervals represent the treatment effects respectively in the core incumbent, swing, and core opposition sub-samples.

incumbent vote share indicates a mobilization of core supporters.

On the other hand, for the swing sub-sample, Part (b) in Figure 6 shows that the statistically significant channel is the vote-switching rather than the turnout channel. The reason for the null effect on turnout may be found in the countervailing effects of the treatment on turnout within the swing sub-sample. Yet, the greater discrepancy between the strength of the vote-switching and turnout channels in the swing sub-sample –compared to the discrepancy in the core incumbent and opposition sub-samples– can also be explained by the greater amount of vote-switching in the swing sub-sample.

Finally, Part (c) in Figure 6 shows that, in the core opposition sub-sample, the treatment effects on the incumbent vote share and turnout manifest themselves in opposite directions. This suggests that, in core opposition constituencies, the increase in the incumbent vote share is at least partly driven by the decreased turnout of opposition supporters. This is, once again, what I call *abstention-buying*.

In sum, although all political groups respond to the vote-buying campaign in line with the reciprocity rule, they do so through different channels and in different magnitude. The swing voters are the most responsive group to the vote-buying by mostly switching their votes from one side to the other thanks to the food-subsidy program. Although in slightly smaller magnitude, the core incumbent and opposition voters also respond to the vote-buying campaign consistent with the reciprocity rule. They do so, respectively, by increasing and decreasing their turnout rates.

As a robustness test, I repeat the analysis in this section with interaction models. More specifically, I estimate regressions on the entire sample by bringing the treatment variable to interact with the *Partisanhip* variable. The results of these estimations, reported in Table C.5, are largely in line with those reported above.

Moreover, I also repeat the analysis with *Distance* variable and with its square root, instead of the binary treatment variable. Figure D.2 and D.3 present the results of these analyses. Although the estimated effects for both the incumbent vote share and turnout keep the same ordinal ranking as in the previous analysis (see Figure 5), an important difference compared to the previous analysis is that the estimates become less precise, in other words, they have wider confidence intervals. Nevertheless, this is most likely due to the uninformative variation –for the purpose of this study– in the continuous *Distance* variable. An example of an uninformative variation in this setting is as follows: once a voter is already too far away from the state-run grocery stores, going an extra kilometer further away does not make much difference for his/her access to the program. Yet, the continuous version of the treatment variable represents this variation anyway.

Taking the square root of the *Distance* variable accounts for this non-linearity to some extent and produces estimates that are closer to the ones we obtain with the binary treatment variable (see Figure D.3). However, overall, the distance variable and its square root are not good measures of the accessibility of voters to the stores, especially when the program is expected to be effective within a very limited area.[36]

6 Conclusion

In this paper, I have provided a detailed analysis of a vote-buying program in the form of subsidized groceries in the wake of historically high food-price inflation. Using the variation in voters' accessibility to the grocery stores adhering to the program, I first estimated the causal effect of this program on voting behavior. I then examined how different political groups respond to this vote-buying campaign, how much they respond, the conditioning role of interactions between different political groups, and the spending required to produce an additional vote for the incumbent.

This study makes two main contributions the literature on vote-buying. Although pocketbook considerations in economic voting are more apt for the theoretical politi-

[36]The levels of statistical significance of the first three models in Table 3 also support this argument. Specifically, in terms of statistical significance, the binary treatment variable reaches the highest level, whereas the square root of distance comes second, and the distance variable itself comes in third place.

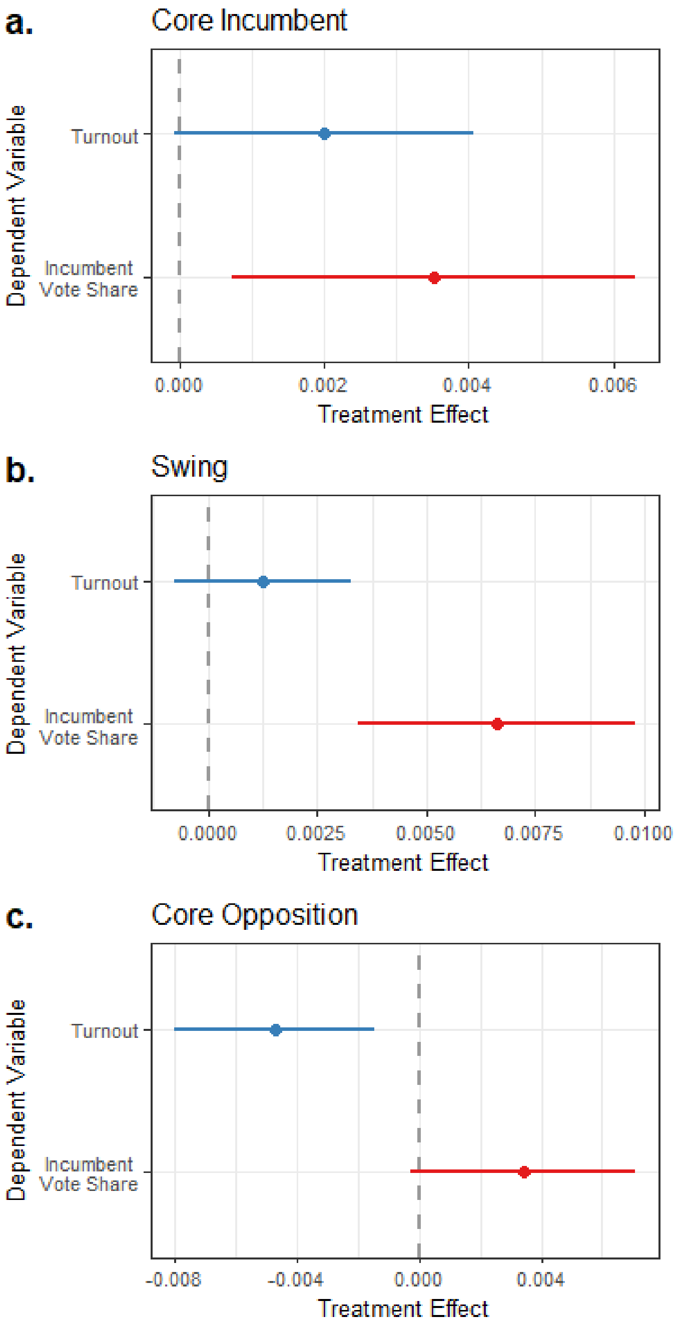

Figure 6: Partisan sub-samples: vote- and turnout-buying effects

Note: The figure shows the estimated treatment effects and their 95% confidence intervals in three sub-samples of the data set. The dependent variables are the incumbent vote share and turnout. The cut-off for the binary treatment variable is chosen as 2 km. Part (a) shows the treatment effects in the core incumbent sub-sample, Part (b) in the swing sub-sample, and Part (c) in the core opposition sub-sample. The results are from OLS estimations that include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The vertical dashed line corresponds to a treatment effect of zero. The confidence intervals are built based on the standard errors clustered at the district level. The red and blue point estimates and confidence intervals represent the treatment effects on incumbent vote share and turnout.

cal economy literature than sociotropic considerations are, the survey-based empirical literature has largely concluded that the predominant type of economic voting is sociotropic. The first contribution of the paper, therefore, is the documentation of causal evidence for pocketbook considerations by using a food-subsidy program and a distinct empirical strategy that allows me to quantify the accessibility of voters to the program stores in a precise manner. This empirical strategy enables me to obtain causal estimates of the effects of this program on voting behavior in terms of the actual number of votes, and hence, to calculate the net cost of an additional vote generated by the program.

Second, I examine the ways in which the different political groups respond to vote-buying. Previous literature has identified two such channels –the vote-switching and turnout-buying channels. However, it focused mostly on the former channel and overlooked the latter. The second contribution of the paper is thus related to the turnout-buying channel. I document that the vote-switching and turnout-buying channels co-exist, and that the turnout channel is at least as important as the vote-switching channel.

As a concluding remark, the vote-buying campaign studied in this paper failed to grant an electoral victory to the incumbent party. It could, however, easily have changed the outcome of the election, given that the estimated effects are very close to the margin of victory. The findings of this study therefore bring about some critical implications for the electoral effects of distributive spending. First of all, even the short-lived subsidy programs that do not provide meaningful remedies to persistent economic hardships can reduce voters' willingness to hold incumbents accountable for these economic hardships and can be decisive in electoral competitions (Leight et al., 2020).

On a more positive note, however, the findings also imply that incumbents may still benefit electorally without engaging in clientelistic campaigns that target only some specific groups of voters when making distributive transfers –even when the movitivation is purely vote-buying. This finding should be encouraging for governments to respect democratic norms and provide effective public service without favoritism.

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Appendix A The Choice of Treatment Cut-off

We expect the state-run grocery stores to be effective within a certain geographical range, mainly for two reasons. First, voters' accessibility to the program stores depends on their walking distances to these stores and is decreasing in *distance to the nearest state-run store*. Second, voters needed to make frequent visits to the program stores, given that quota of a maximum three kg of subsidized groceries at one visit was in practice. Therefore, since commuting to the stores is costly, we expect that voters beyond a certain distance will not benefit from the program. This, in turn, suggests a geographical range within which the local food-subsidy program can cater to voters.

I operationalize this geographical catchment area as catchment circles of radius k around the state-run grocery stores. The radius k corresponds to the treatment cut-off value. Notice that this cut-off value is identical for all program stores. Considering that the program stores were located at places with similar characteristics (such as being central and/or nearby other official government buildings), the same treatment cut-off for all program stores is a reasonable and practical choice.

Note also that before setting out to identify the geographical reach of state-run stores, I have already shown that the program has a statistically significant effect on the incumbent vote shore by using the continuous *Distance* variable and its square root (Section 3). To decide the geographical range of the state-run stores, I estimate Equation 1 with a categorical treatment variable, which is defined as the following:

$$Treatment_i = \begin{cases} \textit{0-1km}, & if \quad \quad Distance_i \leq 1\ km \\ \textit{1-2km}, & if \quad 1 < Distance_i \leq 2\ km \\ \textit{2-3km}, & if \quad 2 < Distance_i \leq 3\ km \\ \textit{3-4km}, & if \quad 3 < Distance_i \leq 4\ km \\ \textit{>4km}, & if \quad 4 < Distance_i. \end{cases}$$

Table A.1 reports the results of this regression. I specify the reference level for the categorical treatment variable as the *0-1km*. Therefore, the coefficients of other levels correspond to the contrast of each level with the reference level *0-1km*. We start with the assumption that the polling stations, which have the reference level *0-1km*, are in the treatment group since they are the closest. The results in Table A.1 show that level *1-2km* does not have a significant effect on the dependent variable compared to the reference level *0-1km*. However, the levels *2-3km*, *3-4km*, and *>4km* differ significantly and negatively from the reference level, in their effects on the dependent variable. This suggests that the treatment cut-off value is 2 km.

The reason that I decide the treatment cut-off value based on the incumbent vote share –but not turnout rate– is the countervailing treatment effects on turnout over different political groups. The treatment effects on the incumbent vote share, on the other hand, are not countervailing over these groups.

Alternatively, I experiment with different treatment cut-off values. Figure A.1 shows the estimated coefficients of treatment when the cut-off value is chosen as 1, 2, 3, or 4 km. The model with treatment cut-off 1 km, for example, defines the treatment group as the polling stations that fall within 1 km of any state-run store, and the remaining polling stations as the control group.

Figure A.1 mainly reveals two results. First, the geographical range of the effect is greater than 1 km. The red estimate at the bottom, which is statistically not different from zero, implies that either there is no treatment effect or it is washed away due to the composition of treatment and control groups –i.e., due to the treated units in the control group. Since we already know that the treatment has a statistically significant positive effect from our baseline results (and also from some other models in Figure A.1), the red coefficient shows us that the geographical range of the treatment is greater than 1 km.

Second, the comparison of the two coefficient estimates at the bottom (the red and blue estimates) indicates that the geographical range of the effect extends to within 2 km of state-run stores. The increases in the magnitude of the treatment effect and its precision show the presence of treated units within 1 to 2 km of state-run stores. On the other hand, extending the treatment group to within 3 and 4 km of state-run stores decreases both the magnitude and precision of the estimated treatment effect.

In Table 2, I show that the treatment and control groups are well-balanced under the treatment with 2 km cut-off.

Table A.1: The choice of treatment cut-off value

	Dependent variable:
	Incumbent Vote
Treatment (Reference level: *0-1km*)	
1-2km	0.028
	(0.151)
2-3km	-0.303^{*}
	(0.157)
3-4km	-0.419^{***}
	(0.163)
>4km	-0.569^{**}
	(0.287)
Previous Inc. Vote	0.932^{***}
	(0.008)
Previous Turnout	0.009
	(0.035)
Neigh.-level controls	Yes
District F.E.	Yes
Mean of Dep. Var.	45.31
Observations	1,575
R^2	0.988

Note: The reported results are from OLS estimations. The dependent variable is the incumbent vote share. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. Treatment is a categorical variable with five levels: *0-1km, 1-2km, 2-3km, 3-4km,* and *>4km*. The reference level is chosen as *0-1km*. All regressions include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01.

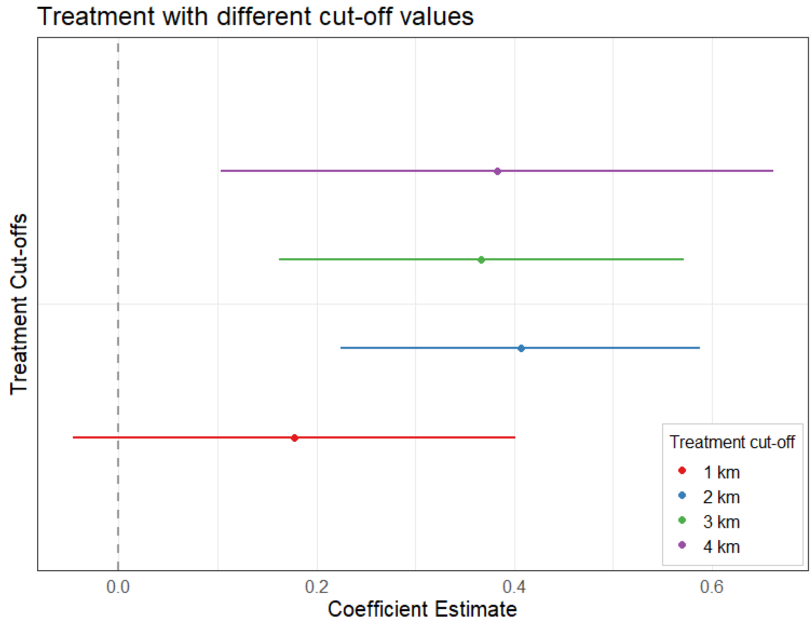

Figure A.1: Treatment effects with different treatment cut-offs

Note: The figure shows the estimated treatment effects and their 95% confidence intervals with different treatment cut-off values. The dependent variable is the incumbent vote share. The results are from OLS estimations that include control variables at the neighborhood level:population, share of females, average age, share of low-educated people, house prices, and house rents. The vertical dashed line corresponds to a treatment effect of zero. The confidence intervals are built based on the standard errors clustered at the district level. The red, blue, green, and purple point estimates and confidence intervals represent the estimates of treatment effects with different treatment cut-off values.

Appendix B Cost of the Food-Subsidy Program

In calculating the total net cost of the food-subsidy program –from the day of onset to the election day– I use the prices at the state-run grocery stores, daily prices at the Istanbul wholesale food market, and the quantity of sales in the first day of the food-subsidy program, as reported by the Istanbul Metropolitan Municipality.

The first step to calculate the total net cost of the program is to find how much loss (in Turkish Lira, TL) the Metropolitan Municipality makes for each kg of product sold. I calculate the loss per kg for every product that is covered in state-run stores with the following steps: First, for every product, I take the difference between the price at state-run stores and the price at the Istanbul wholesale food market for the first week of February 2019 –the week before state-run stores started. To obtain the latter, I take the average of minimum daily prices at the wholesale food market in the first week of February 2019. Note that using minimum prices at the wholesale food market gives a conservative estimate for the loss per kg.

The second step is to multiply the loss per kg with the quantity sold in one day for each product and report their sum as the total daily loss. The third step is to multiply the total daily loss with the number of days from the day of onset of state-run stores to the election day. The resulting number is a conservative estimate of the total net cost of the program.

The calculation method described above requires several assumptions:

1. Loss for each product is constant over time, and is the difference between the price (per kg) at the state-run stores and price (per kg) at the Istanbul wholesale food market.

2. To obtain a conservative estimate of the cost of the program, I take the average of minimum daily price for each product at the wholesale food market in the first week of February 2019. However, I give estimates of overall cost when the loss is halved and when increased by 50% as well.

3. The quantity sold for each product is constant over time and the same as the first-day quantity, reported by the metropolitan municipality.

4. Since I do not have prices for legumes at the wholesale food market, I cannot calculate the loss per kg for these products in the same way that I calculate it for other products. Alternatively, for legumes, I assume that the loss per kg is the average loss per kg of other products. This implies that the loss per kg for legumes is 2 TL.

Under these assumptions, let us define:

$$p_g = \begin{bmatrix} 6 \\ 6 \\ 3 \\ 4 \\ 2 \\ 4.5 \\ 4 \\ 2 \end{bmatrix}, \; p_w = \begin{bmatrix} 8.6 \\ 10 \\ 8 \\ 3.8 \\ 3.06 \\ 6.8 \\ 4 \\ 3.16 \end{bmatrix}, \; q = \begin{bmatrix} 2000 \\ 1000 \\ 118000 \\ 16000 \\ 70000 \\ 2700 \\ 15000 \\ 73000 \end{bmatrix}, \; q^l = \begin{bmatrix} 17880 \\ 9800 \\ 5900 \end{bmatrix},$$

where p_g is the vector of prices at state-run stores of the products that were covered from the very beginning (in other words, products except legumes), p_w is the vector of prices of the same products at the Istanbul wholesale food market, q is the vector of daily sale quantity for each product except legumes (in kg), and q_l is the vector of daily sale quantity for each legume (in kg). I separate legumes from other products because legumes were added to the state-run stores at a later stage. The number of days that legumes were on sales is 34, whereas it is 49 for the other products.

The total cost of products other than legumes (let us denote this by TC_1) to the municipality is given by the following:

$$TC_1 = (p_g - p_w)^T q * 49 = -37,293,410 \ TL,$$

whereas the total cost of legumes (let us denote by TC_2) to the municipality is given by the following:

$$TC_2 = \begin{bmatrix} -2 & -2 & -2 \end{bmatrix} q^l * 34 = -2,283,440 \ TL.$$

Therefore, the total cost of the program is given by:

$$TC = TC_1 + TC_2 = -39,576,850 \ TL.$$

To calculate how much spending is required to gain an additional vote, we divide the absolute value of the total cost by the number of actual votes gained through the food-subsidy program. The latter is calculated in Section 4.1. Consequently, the cost of an additional vote (c) is given by:

$$c = \frac{|TC|}{16521} = \frac{39{,}576{,}850}{16521} = 2395.548 \ TL.$$

Finally, we can calculate the percentage of the GDP per capita that the cost of an additional vote (c) corresponds. The GDP per capita of Turkey in 2019 is $45242,96 \ TL$. Dividing c by the GDP per capita of Turkey yields this percentage:

$$\frac{c}{GDPpc\_Turkey} = \frac{2395.548}{45242{,}96} = 5.29\%.$$

Halving the loss (per kg of product) and increasing it by 50% yields, respectively, 2.65% and 7.94%. These percentages are still much smaller than those of the U.S., where Chen (2013)'s calculation yields 32% as the percentage of GDP per capita required to buy an extra vote.

Appendix C Tables

Table C.1: The descriptive statistics of the variables

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Polling station level:							
Incumbent Vote *(%)*	1,589	45.314	14.984	8.558	35.915	56.715	93.814
Turnout *(%)*	1,589	80.961	3.767	34.934	79.361	82.874	100.000
Previous Inc. Vote *(%)*	1,589	47.412	16.374	8.597	36.475	59.854	94.075
Previous Turnout *(%)*	1,589	86.538	3.215	44.516	85.363	88.110	100.000
No. of Total Votes	1,589	5,459	2,904	16	3,435	7,303	16,638
No. of Registered Voters	1,589	6,206	3,302	10	3,864	8,328	19,084
Distance *(in km)*	1,589	2.760	2.892	0.002	1.213	3.244	24.919
Neighborhood-level:							
Population *(in thousands)*	667	20.070	15.627	0.097	8.434	27.330	88.956
Share of Females in the Pop. *(%)*	667	0.497	0.035	0.161	0.488	0.510	0.604
Average Age	664	33.546	4.118	25.080	30.595	36.032	46.260
Share of Low-educated People *(%)*	668	0.545	0.148	0.178	0.446	0.656	0.824
House Prices *(1-10)*	668	4.117	2.687	0	2	6	10
House Rents *(1-10)*	668	3.991	2.679	0	2	6	10

Note: Previous Inc. Vote corresponds to the vote share of the incumbent in the previous election (2018). No. of Total Votes and No. of Registered Voters indicate, respectively, number of votes cast and number of registered voters in the 2018 elections. Distance variable indicates the distance between the polling stations and the nearest state-run grocery store. No. of Polling Stations Under AKP Mayor reports the number of polling stations that belong to district municipalities with AKP mayors based on the 2014 local elections. Share of Low-educated people indicates the share of people with no education, primary education, or elementary education in the total population. House Prices and House Rents are index variables that can take discrete values from 0 to 10. The higher values indicate higher prices and rents. Distance indicates the distance between polling stations and nearest program groceries.

Table C.2: The regression results for placebo-in-place analysis: Turnout

	Dependent variable:		
	Turnout		
	(1)	(2)	(3)
Distance	−0.012		
	(−0.090, 0.066)		
$\sqrt{District}$		−0.069	
		(−0.373, 0.235)	
Treatment-2km			−0.055
			(−2.167, 2.057)
Previous Inc. Vote	−0.022	−0.022	−0.023
	(−0.270, 0.225)	(−0.250, 0.204)	(−0.250, 0.204)
Previous Turnout	0.473***	0.474***	0.475***
	(−0.626, 1.572)	(−0.650, 1.600)	(−0.650, 1.600)
Neigh.-level controls	Yes	Yes	Yes
District F.E.	Yes	Yes	Yes
Mean of Dep. Var.	85.78	85.78	85.78
Observations	172	172	172
Log Likelihood	−448.226	−448.249	−448.348
Akaike Inf. Crit.	922.453	922.498	922.696

Note: The reported results are from OLS estimations. The numbers in parentheses are the 95% confidence intervals built according to the wild clustered bootstrap method by Cameron et al. (2008). The Distance variable indicates the distance in km between polling stations and nearest state-run stores. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. Treatment-2km indicates the binary treatment variable with a 2 km cut-off. All regressions include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. *p<0.1; **p<0.05; ***p<0.01.

Table C.3: The regression results for vote- and turnout-buying channels in different partisan sub-samples

	Dependent variable:					
	Incumbent Vote			*Turnout*		
Sub-sample:	Core Inc.	Swing	Core Opp.	Core Inc.	Swing	Core Opp.
Treatment-2km	0.352**	0.661***	0.342*	0.200*	0.124	−0.474***
	(0.143)	(0.163)	(0.189)	(0.105)	(0.103)	(0.168)
Previous Inc. Vote	0.944***	0.918***	0.905***	0.035***	0.012	0.009
	(0.012)	(0.012)	(0.019)	(0.009)	(0.008)	(0.017)
Previous Turnout	0.054	0.033	−0.116***	0.930***	1.014***	1.063***
	(0.057)	(0.046)	(0.045)	(0.042)	(0.029)	(0.040)
Neigh.-level controls	Yes	Yes	Yes	Yes	Yes	Yes
District F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	59.51	43.29	21.55	81.33	80.45	81.43
Observations	588	691	296	588	691	296
R^2	0.953	0.926	0.954	0.756	0.832	0.863

Note: The reported results are from OLS estimations. The dependent variables are the incumbent vote share and turnout. Treatment-2km indicates the binary treatment variable with a 2 km cut-off. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. *Core Inc.*, *Swing,* and *Core Opp.* correspond to respectively Core Incumbent, Swing, and Core Opposition sub-samples. All regressions include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01.

Table C.4: The treatment effects under sample restrictions based on distance

	Dependent variable:					
	Incumbent Vote					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment-2km	0.417***	0.417***	0.417***	0.428***	0.415***	0.438***
	(0.132)	(0.133)	(0.133)	(0.136)	(0.133)	(0.134)
Previous Inc. Vote	0.932***	0.933***	0.933***	0.932***	0.933***	0.933***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Previous Turnout	0.009	0.013	0.014	0.014	0.005	−0.001
	(0.035)	(0.035)	(0.035)	(0.036)	(0.034)	(0.032)
Neigh.-level controls	Yes	Yes	Yes	Yes	Yes	Yes
District F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	45.11	45.06	45.04	44.95	44.99	44.93
Observations	1,533	1,526	1,518	1,498	1,469	1,422
R^2	0.988	0.988	0.988	0.988	0.988	0.988

Note: The reported results are from OLS estimations. The dependent variable is the incumbent vote share. Treatment-2km indicates the binary treatment variable with a 2 km cut-off. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. Models (1), (2), (3), (4), (5), and (6) are estimated on the restricted samples of polling stations within 10, 9, 8, 7, 6, and 5 km of state-run stores, respectively. All regressions include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01.

Table C.5: Interaction of binary treatment variable with partisanship

	Dependent variable:	
	Incumbent Vote	Turnout
	(1)	(2)
Treatment-2km	0.369**	0.164
	(0.180)	(0.139)
Treatment-2km × Partisanship		
(Reference level: *Core Incumbent*)		
× *Swing*	0.153	0.014
	(0.265)	(0.158)
× *Core Opposition*	−0.026	−0.516***
	(0.304)	(0.196)
Previous Inc. Vote	0.934***	0.018***
	(0.010)	(0.006)
Previous Turnout	0.023	1.045***
	(0.038)	(0.032)
Neigh.-level controls	Yes	Yes
District F.E.	Yes	Yes
Mean of Dep. Var.	45.31	80.96
Observations	1,575	1,575
R^2	0.988	0.799

Note: The reported results are from OLS estimations. The dependent variables are the incumbent vote share and turnout. Treatment-2km indicates the binary treatment variable with a 2 km cut-off. Partisanship is a categorical variable with three levels: *Core Incumbent*, *Swing*, and *Core Opposition*. The reference level is chosen as *Core Incumbent*. Previous Inc. Vote indicates the vote share of the incumbent in the previous election. The regressions control for polling-station level previous incumbent vote share and previous turnout, and also include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01.

Appendix D Figures

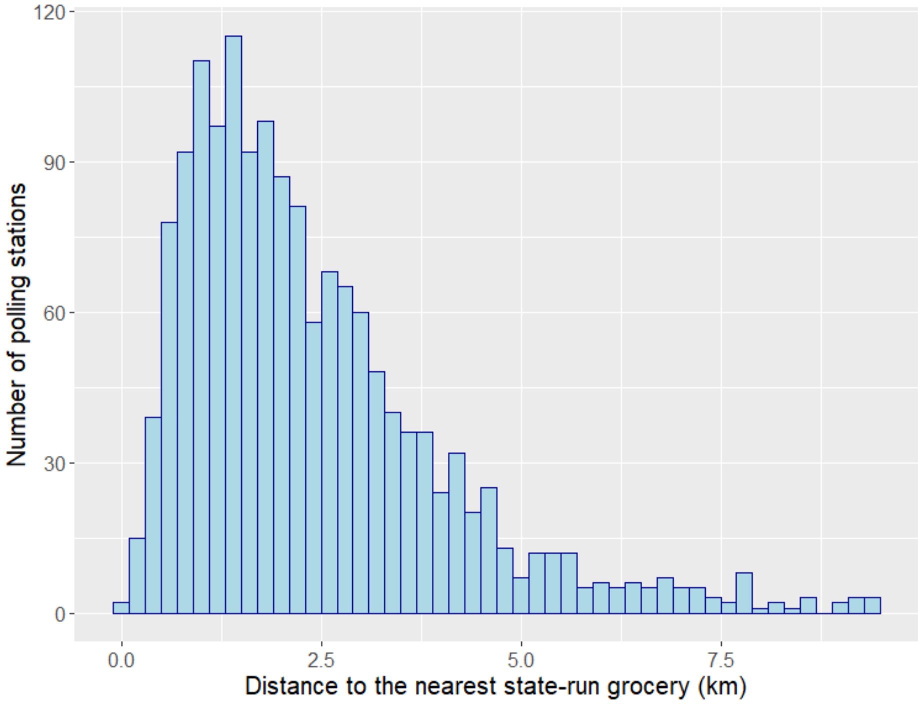

Figure D.1: Distribution of the distance variable

Note: The figure shows the histogram of the distance from polling stations to the nearest program groceries. The distribution is truncated at 10km. ∼97% of the polling stations fall within 10km of program groceries in the districts where the program was implemented.

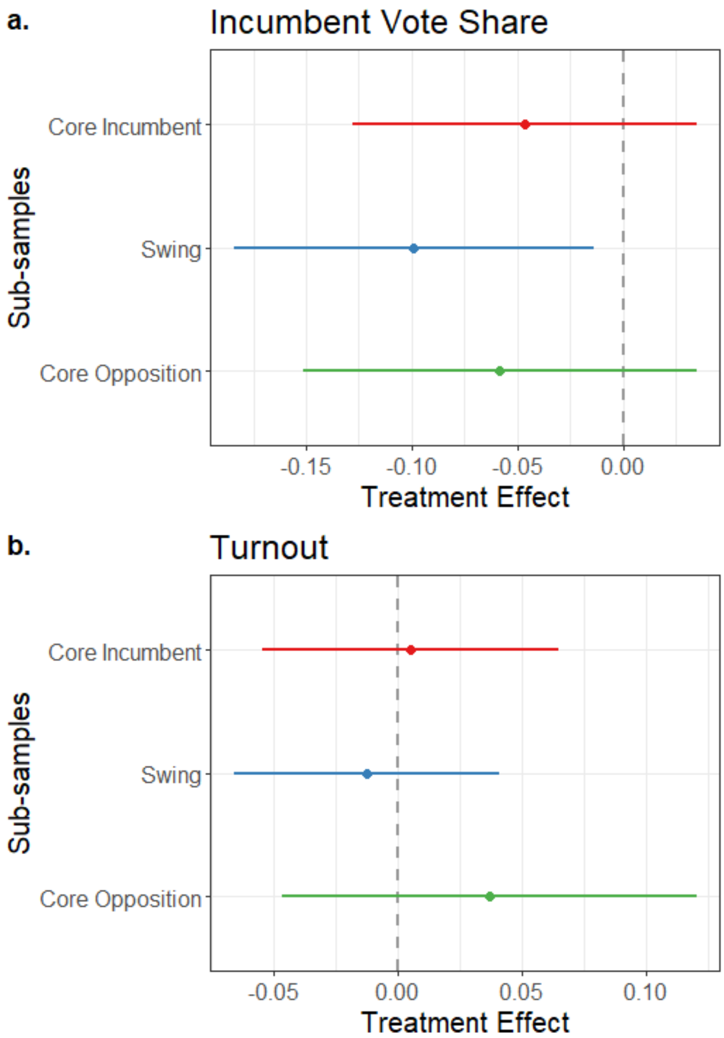

Figure D.2: Robustness test: partisan conditioning with continuous Distance variable

Note: The figure shows the estimated treatment effects and their 95% confidence intervals in three sub-samples of the data set. The dependent variable is the incumbent vote share in Part (a) and turnout rate in Part (b). The treatment variable is the continuous Distance variable. The results are from OLS estimations that include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The vertical dashed line corresponds to a treatment effect of zero. The confidence intervals are built based on the standard errors clustered at the district level. The red, blue, and green point estimates and confidence intervals represent the treatment effects respectively in the core incumbent, swing, and core opposition sub-samples.

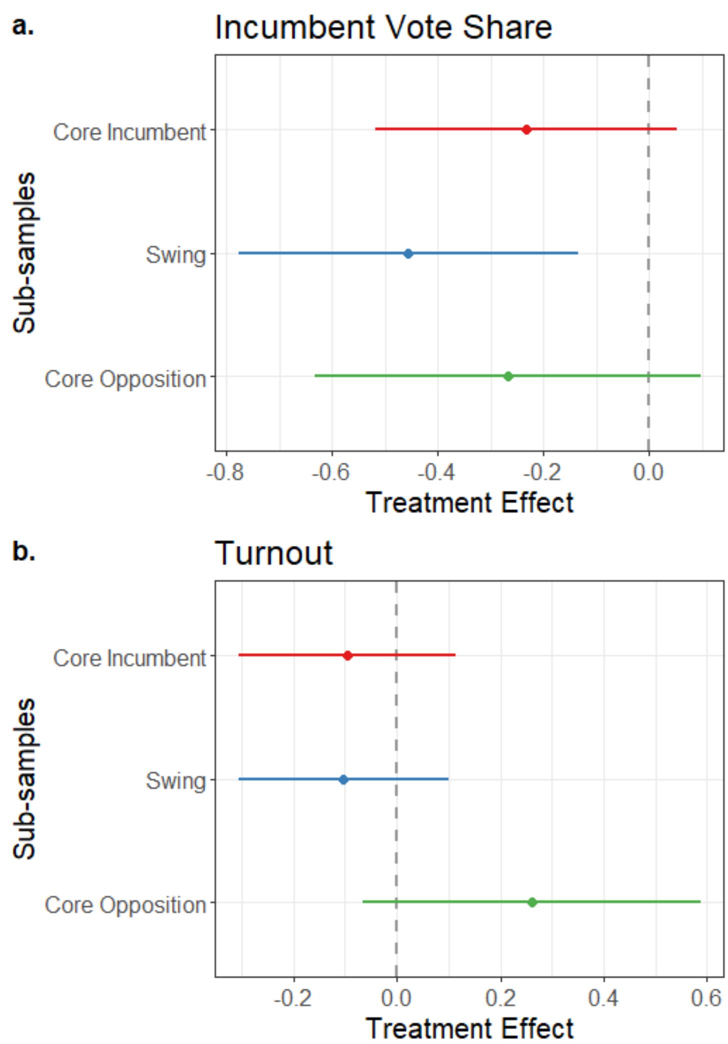

Figure D.3: Robustness test: partisan conditioning with the square root of Distance variable

Note: The figure shows the estimated treatment effects and their 95% confidence intervals in three sub-samples of the data set. The dependent variable is the incumbent vote share in Part (a) and turnout rate in Part (b). The treatment variable is the continuous Distance variable. The results are from OLS estimations that include control variables at the neighborhood level: population, share of females, average age, share of low-educated people, house prices, and house rents. The vertical dashed line corresponds to a treatment effect of zero. The confidence intervals are built based on the standard errors clustered at the district level. The red, blue, and green point estimates and confidence intervals represent the treatment effects respectively in the core incumbent, swing, and core opposition sub-samples.