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How do cash transfers affect participation in elections?

Victor Araújo*

Abstract

It is commonly accepted that income deprivation suppresses civic engagement. Yet, it is still unclear how policies that seek to tackle deprivation, such as anti-poverty schemes, affect political participation in targeted constituencies: Do they mobilize new citizens (mobilization) or keep engaged those with the habit of voting (mitigation)? I theoretically distinguish between these two mechanisms by focusing on cash transfers, the most widely adopted anti-poverty scheme worldwide. Empirically, I evaluate the *Renda Básica de Cidadania*, the largest unconditional cash transfer in Latin America, which allows for isolating the effect of cash payments on voting behavior. Estimates from a difference-in-differences design reveal a three percentage points increase (a net growth of 4%) in voter turnout in posttreatment elections. Leveraging municipal-level data in a synthetic control method approach, I show that a mitigation mechanism induces this effect, i.e., the payment of monthly cash transfers reduced the incentives to abstain in elections.

Keywords: Anti-poverty schemes; poverty alleviation; unconditional cash transfers; political safety net; voter turnout.

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1 Introduction

Poverty remains one of the most pressing problems facing humanity across societies: roughly 25% of the world lives on less than $3.65 per day, and 648 million people in the world, about 8% percent of the global population, live in extreme poverty i.e., subsisting on less than US$2.15 per day (Hasell et al., 2022). The well-documented effects of poverty on individuals' well-being (e.g., Duflo and Banerjee, 2011), brain development conditions (e.g., Lipina and Posner, 2012), and health trajectories (e.g., Rowley et al., 2021) contrast with much less solid evidence of whether and how economic hardship shapes predispositions to engage in political activities (e.g., Carr et al., 2014; Mood and Jonsson, 2016)

A growing literature on civic participation provides some insights. For example, Schaub (2021) reports compelling evidence that acute financial hardship reduces voter turnout. Corroborating this argument that income causes voting, Markovich and White (2022) show that seasonal increases in the minimum wage have the potential to prompt voter turnout among the poor. Similarly, Akee et al. (2020) found that positive income shocks can increase voting propensity among children raised in initially poorer families. Despite these recent and promising developments in the scholarship, we know surprisingly little about the engaging effects of the most popular and widely adopted anti-poverty scheme: cash transfers (Barrientos, 2013; Bastagli et al., 2019). Around one billion individuals are beneficiaries of cash transfers globally, meaning that one out of six people in the world received at least one monthly payment in 2021 (Gentilini, 2022).

There have been attempts in the literature to assess the political effects of cash transfers (e.g., Manacorda, Miguel and Vigorito, 2011; De La O, 2013; Labonne, 2013; Linos, 2013;

Zucco Jr, 2013; Frey, 2019; Conover et al., 2020; Araújo, 2021). However, much of what we know so far is based on assessments of the impact of *conditional* cash transfer programs.[1] One challenge these studies face is that, by design, conditional transfers consist of more than a cash transfer: conditionalities often include higher degrees of citizen engagement with other state services, most commonly in education and health. As such, "both the program transfers and the conditionalities play an important role in shaping political activity (Schober, 2019, p.592)."

The big crash of the US economy in the late 2000s, followed by an ensuing global recession in 2009, prompted new voices in defense of unconditional cash transfers (Gentilini et al., 2019). For those advocating such programs, the absence of conditionalities offers the possibility of reducing costs with targeting and eligibility while allowing immediate responses to economic shocks, natural disasters, and other humanitarian crises (Nettle et al., 2021). Several ongoing initiatives seek to assess the impact of unconditional cash transfers on various socioeconomic outcomes (Banerjee, Niehaus and Suri, 2019). Preliminary evidence from Kenya (Banerjee, Duflo and Sharma, 2021) and Finland (Hirvonen, Schafer and Tukiainen, 2022) point to mixed effects of unconditional cash transfers on the propensity to vote. Two shortcomings apply in this case. First, the estimated effect of cash transfers on political participation relies on self-reported measures of vote intention instead of actual voting. Second, these pilots were not designed to capture changes in voter turnout over time. Therefore, it is still unclear whether cash transfers affect political participation in targeted constituencies by mobilizing new citizens (mobilization) or incentivizing the continued participation of citizens with the habit of voting (mitigation).

In this paper, I theoretically distinguish between these two mechanisms by assessing the

[1] Conditionalities may include enrolling children into public schools, getting regular check-ups at the doctor's office, receiving vaccinations, or the like.

impact of an unconditional cash transfer scheme that allows for isolating the effect of monthly cash payments on voter turnout over time. Specifically, I evaluate the *Renda Básica de Cidadania* (Citizens' Basic Income; henceforth RBC)—implemented in Maricá, a municipality in Rio de Janeiro, Brazil, and currently the largest unconditional cash transfer in Latin America. Financed by revenues from oil and gas exploitation, the RBC has been running since 2014, and 42,000 of the 165,000 (25%) inhabitants of Maricá receive a monthly unconditional transfer of R$170 (≈ US$35). All individuals with residence in Maricá for at least three years and who earn less than R$3,300—three times Brazil's national monthly wage of R$1,100 (≈ US$200) are eligible to receive the RBC. Until the end of 2021,[2] the RBC was the only unconditional cash transfer program in the Brazilian state of Rio de Janeiro. Leveraging this opportunity, I use municipalities unaffected by this intervention as a counterfactual and estimate the impact of the RBC on voter turnout. Because the RBC is paid out in *Mumbucas*, a local currency that can only be used in Maricá, this counterfactual is not affected by potential spillovers created from the program, e.g., beneficiaries spending their benefits in neighboring municipalities.

Using data from all polling stations in Rio de Janeiro for a twelve election-year period, i.e., more than two decades of observation (N = 363,602), I provide evidence that voter turnout in Maricá and the other municipalities in the same state followed parallel trends before the intervention. This detected pattern suggests that, in the absence of the RBC, voter turnout rates would not have changed in Maricá. Estimates from a difference-in-differences design show that the RBC increased voter turnout by three percentage points in Maricá, a net growth of 4% in the posttreatment period. Crucially, this result holds across several models using a restricted sample with oil-producers-only municipalities, a placebo treatment unit (in-space

[2]In November 2021, Niterói, another municipality located at the cost of Rio de Janeiro state, started running a pilot to implement its unconditional cash transfer program.

placebo), and a time-event placebo (in-time placebo).

In light of the decreasing rates in voter turnout in the wider Rio de Janeiro state, results of my difference-in-differences design suggest that the RBC has been keeping engaged individuals who already had the habit of voting (mitigation) rather than mobilizing new citizens to vote (mobilization). Leveraging municipal-level data in a synthetic control method approach, I provide complementary evidence that corroborates this interpretation: while the share of voters participating in elections in Maricá remained constant during the posttreatment period, fewer citizens voted in the municipalities without an anti-poverty program, i.e., the payment of monthly cash transfers reduced the incentives to abstain in elections in Maricá.

From a theoretical point of view, the results reported in this paper illuminate previous findings in the literature. The evidence that poor voters reward incumbents responsible for implementing cash transfers does not imply an intrinsic mobilization effect of such policies. Cash transfers can stimulate retrospective voting without bringing new citizens to participate in politics. Regarding its empirical contribution, to my knowledge, the current study is the first to comprehensively investigate the effect of an unconditional cash transfer program on voter turnout. Most studies evaluating unconditional cash transfer programs focus on their impact on economic dimensions such as poverty, inequality, and labor market (e.g., Berman, 2018; Hoynes and Rothstein, 2019; Kangas et al., 2019; Hamilton and Mulvale, 2019; Banerjee, Niehaus and Suri, 2019). The current study adds to this growing literature by showing how cash transfers affect political participation.

2 How can poverty suppress political participation, and what can cash transfers do about it?

The cumulative scientific evidence in the past few decades has firmly confirmed a long-standing theory in social sciences: poverty is stressful (e.g., Haushofer and Fehr, 2014). As poverty exacerbates concerns with personal economic well-being, the poor are more likely to withdraw from such external matters as politics (Pacheco and Fletcher, 2015; Burden et al., 2017). "When a person experiences economic adversity, his scarce resources are spent on holding body and soul together–surviving–not on remote concerns like politics" (Rosenstone, 1982, p.26).

Indeed, several studies have confirmed that poverty alleviation induced by income shocks (e.g., Akee et al., 2020), inequality reduction (e.g., Shafer et al., 2021), increases in the minimum wage (e.g., Markovich and White, 2022), and means-tested welfare programs (e.g., Kogan, 2021) stimulate participation in elections. The evidence suggesting that cash transfers sway voters in favor of the incumbent (e.g., Araújo, 2021) is often interpreted as evidence of the intrinsic mobilization effect of this type of anti-poverty policy.

However, where cash transfers have been implemented, there is no robust evidence of increases in voter turnout. The case of Brazil[3] is illustrative in this regard. The Bolsa Família program, one of the largest cash transfer schemes in the world, was created by Workers' Party (PT) in 2004. From 2004 to 2014, the federal government invested yearly 0.5% of Brazil's gross domestic product to lift from poverty as many people as possible. As documented in the literature, the higher concentration of beneficiaries in the northeast region enhanced the political support of the PT among the poor (Hunter and Power, 2007; Zucco, 2008) without

[3]Recent evidence from the Mexican case suggests a similar picture. Despite more than two decades of investments in a pioneer cash transfer pilot, *Progresa*, there is only weak evidence of higher turnout rates in targeted constituencies (Imai, King and Velasco Rivera, 2020).

increasing voter turnout rates (Cepaluni and Hidalgo, 2016). Despite adopting a compulsory voting system, around 20% of Brazilians registered to vote refrain from casting a ballot in local and general elections (Speck and Peixoto, 2022). Crucially, this documented abstention is largely driven by the non-participation of the poor (Katz, Levin et al., 2018).

Perhaps surprisingly, the influential "resource model" proposed by Brady, Verba and Schlozman (1995) and further developments in the literature (e.g., Lawless and Fox, 2001; De La O, 2015; Diaz-Cayeros, Estévez and Magaloni, 2016; Emmenegger, Marx and Schraff, 2017; Amat and Beramendi, 2020; Schaub, 2021) leave open the question of how poverty alleviation translates into political participation. Even if one accepts that a minimum level of income is a necessary condition to participate in politics (Lipset et al., 1960; Brody and Sniderman, 1977), individuals facing social deprivation may react differently once lifted from poverty.

As suggested by Hassell and Settle (2017) in their insightful study, life stress affects individuals' predisposition to vote, but this effect is conditional on their past political involvement: individuals who are not routinely involved in the electoral process are more likely to disengage from politics. Indeed, the literature has shown that voting is habit forming (Gerber, Green and Shachar, 2003). Casting a ballot in one election increases one's propensity to go to the polls in the future (Dinas, 2012; Green, McGrath and Aronow, 2013; Holbein et al., 2021). This implies that one should expect the marginal return of poverty alleviation to be higher for individuals with the habit of participating in politics.

Therefore, while cash transfers may prompt new voters to participate in elections, the hypothesis that they might have a limited effect on non-active voters should not be discarded at first glance. In other words, whether cash transfers stimulate participation in elections through

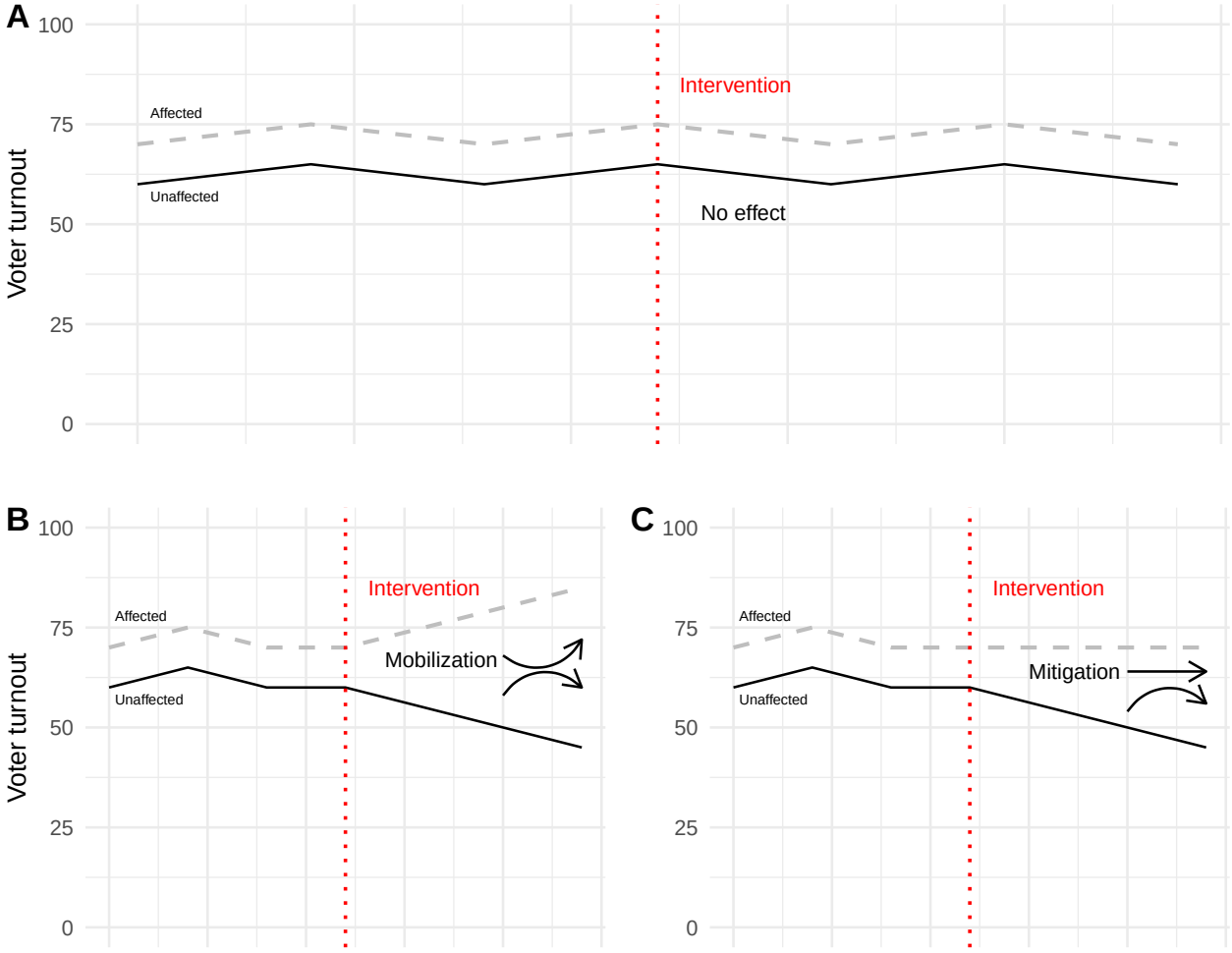
a mobilization or a mitigation mechanism is an empirical question. In both cases, however, constituencies targeted by cash transfers (compared to non-targeted constituencies) will depict higher levels of voter turnout. In the first case (mobilization), cash transfers enfranchise new citizens by eliminating the barriers imposed by food, housing, and informational restrictions.[4] In the second case (mitigation), poverty alleviation facilitates the participation of already mobilized voters.[5]

Figure 1 illustrates the different channels through which cash transfers might affect participation in elections. When a mobilization effect is in place (see panel B), voter turnout is expected to increase in targeted constituencies while decreasing in non-targeted ones due to acute income deprivation. By contrast, if the mitigation effect operates (see panel C), voter turnout is expected to stay constant in targeted constituencies while decreasing in the ones unaffected by the cash transfer program, i.e., where income deprivation persists. In both situations, there should be a mechanical increase in voter turnout, but with different normative implications. Under the occurrence of a mobilization effect, social spending with cash transfers has the potential to enlarge the pool of citizens participating in elections. If a mitigation effect applies, one should expect investments in cash payments to generate stability in the levels of voter participation. At best, cash transfers can stimulate enfranchisement and reduce the inequality of representation in democracies (mobilization). At the least, cash transfers can prevent *de facto* disenfranchisement among the poor, thus securing minimum levels of electoral

[4]Cash transfer might cut some of the indirect costs of voting, e.g., the time gathering information about the candidates running for office (Bidadanure, 2019). Often, beneficiaries of anti-poverty schemes use the monthly transfer to buy out hours in the job market. Although most people do not stop working once they start receiving cash transfers (Baird, McKenzie and Özler, 2018), the time earned from the benefit can be invested in the consumption of political information (Van Parijs and Vanderborght, 2017).

[5]Whether or not the direct costs of voting are prohibitive, recipients of cash transfers are likely to face fewer constraints to vote. The reasoning is straightforward: additional money can pay for the time spent to register for elections and reduce the material costs of showing up to cast a ballot on election day (Morales, 2018).

Figure 1: Cash transfers and participation in elections: Mobilization versus mitigation



Note: Elaborated by the author.

participation (mitigation).

In this paper, I distinguish between these two mechanisms by evaluating the impact of a large-scale unconditional cash transfer scheme implemented in Brazil. As described in the next section, the *Renda Básica de Cidadania* has been running uninterruptedly since 2014. Maricá was the only municipality in Rio de Janeiro state to adopt a policy of this type. Thereby, under the assumption of pretreatment parallel trends, one can causally identify the effect of this program on political participation.

This setting offers three other key advantages over the previous empirical explorations in the literature. First, it allows for an explicit comparison of overtime levels of voter turnout in constituencies affected and unaffected by a cash transfer scheme. Therefore, one can visually distinguish between a mobilization or a mitigation effect of monthly cash transfers. Second, unlike studies identifying the political effects of unconditional cash transfers through randomized controlled trials (e.g., Banerjee, Duflo and Sharma, 2021; Hirvonen, Schafer and Tukiainen, 2022), I rely on data on actual voting instead the self-declarations of intention to vote. Third, as the RBC does not impose conditionalities, I can isolate the impact of income on voter turnout. In effect, any modality of cash transfer–regardless of whether embedded in conditionalities–may trigger mobilization or mitigation effects. Yet, only unconditional cash transfers allow for isolating the effect of receiving an income transfer on political behavior (Schober, 2019).[6]

Of course, one should expect a cash transfer to impact several dimensions of political

[6]When focusing on the effects of *conditional* cash transfers on voting behavior, one cannot disentangle the effect caused by the transfer itself from the one created by the conditionalities. As voting correlates with schooling (e.g., Sondheimer and Green, 2010), one should expect children attending schools to vote once they reach adulthood. Furthermore, as a consequence of existing multiplier effects (Angelucci and De Giorgi, 2009), the benefit typically affects the household and not only individuals enforced by conditionalities to attend school.

participation. For the sake of simplicity and empirical tractability, this paper focuses on voter turnout, one of the key dimensions used to judge the quality of representation in democracies (Altman and Pérez-Liñán, 2002).[7]

3 The *Renda Básica de Cidadania*: A large-scale unconditional cash transfer program in Brazil

In 2004, the Brazilian Congress approved legislation[8] establishing a national unconditional cash transfer that should be paid to all Brazilians and foreigners living in Brazil for more than five years regardless of gender, income, or social condition. Eduardo Suplicy (Workers' Party, PT), an economist by training who, at that time, held a seat in the Brazilian upper chamber (*Senado*), was the main enthusiast behind this policy. Several of his ideas, originally published as a book (Suplicy, 1998), guided the discussion and the consequent approval of the legislation that established, on paper, an unconditional cash transfer in Brazil. While the federal government never carried out this program, it served as an inspiration for Maricá to formulate its unconditional cash transfer some years later.

In December 2013,[9] under the government of the elected mayor Washington Quaquá (PT), Maricá created its unconditional cash transfer program: The *Renda Básica de Cidadania* (RBC). Currently the largest unconditional cash transfer program in Latin America, the RBC is financed by royalties from oil and gas exploitation. In this section, I discuss the RBC eligibility criteria, as well as its fiscal and political backgrounds.

[7] As pointed out by Altman and Pérez-Liñán (2002), greater participation–whether it is voluntary or encouraged by compulsory vote–makes democratic governments responsive to a larger share of the population.

[8] Federal Law 10.835/2004.

[9] Municipal-level Decree number 213/13.

3.1 Eligibility criteria

The RBC pays a monthly income for those who have lived in Maricá for at least three years and earn less than R$3,300–three times Brazil's minimum monthly wage of R$1,100 (US$200). While the RBC does not yet cover Maricá's entire population, the program approximates a typical basic income scheme[10] due to the absence of conditionalities to keep receiving the monthly transfer, the fact that cash payments are transferred to individuals instead of households, and the plan[11] for its further expansion (Silva, Morais and Santos, 2020).

Once officially in the program, each beneficiary is issued a card where they receive the cash transfer.[12] Issued by Maricá's community bank, *Banco Mumbuca*, the transfer is made in a local digital currency called Mumbuca.[13] Beneficiaries can then use this card to buy food and several other items from local stores and merchants that accept Mumbuca in Maricá. Mumbucas are restricted to this municipality and cannot be used in other localities, but virtually all shops and services in Maricá have been integrated into the program since 2015 (Dektar et al., 2020).

Since its creation, the RBC has featured several different designs. From 2014 to 2016, it paid 85 Mumbucas (1 Mumbuca is equivalent to R$1) per month to roughly 14,000 households. In 2017, the RBC rose to 130 Mumbucas per household per month. In June 2019, the RBC shifted from a monthly payment of 130 Mumbucas per household to 130 Mumbucas per individual,[14] bringing the total number of beneficiaries to 42,000 (Dektar et al., 2020). In response to the

[10] In fact, the RBC was conceived as a universal basic income in its original plan formulated in the municipal-level Decree-law number 125/2015.

[11] The RBC has already reached 42,000 (25%) of the 165,000 inhabitants of Maricá, and the local authorities are currently working to extend the program for all Maricá citizens by the end of 2024, which would place the RBC as one of the largest universal basic income programs in the world.

[12] An example of this card is displayed in Appendix B.

[13] This name was chosen in honor of the Mumbuca River located in Maricá. Mumbuca is a word in Tupi, one of the more than 250 languages of Brazilian native populations. In the original Tupi, Mumbuca means "Little black woman".

[14] Municipal-level law number 2.869/2019.

Covid-19 outbreak, the RBC was increased to 300 Mumbucas in March 2021. In December 2021, this value was re-adjusted, and since then, each beneficiary has received a monthly transfer of 170 Mumbucas.

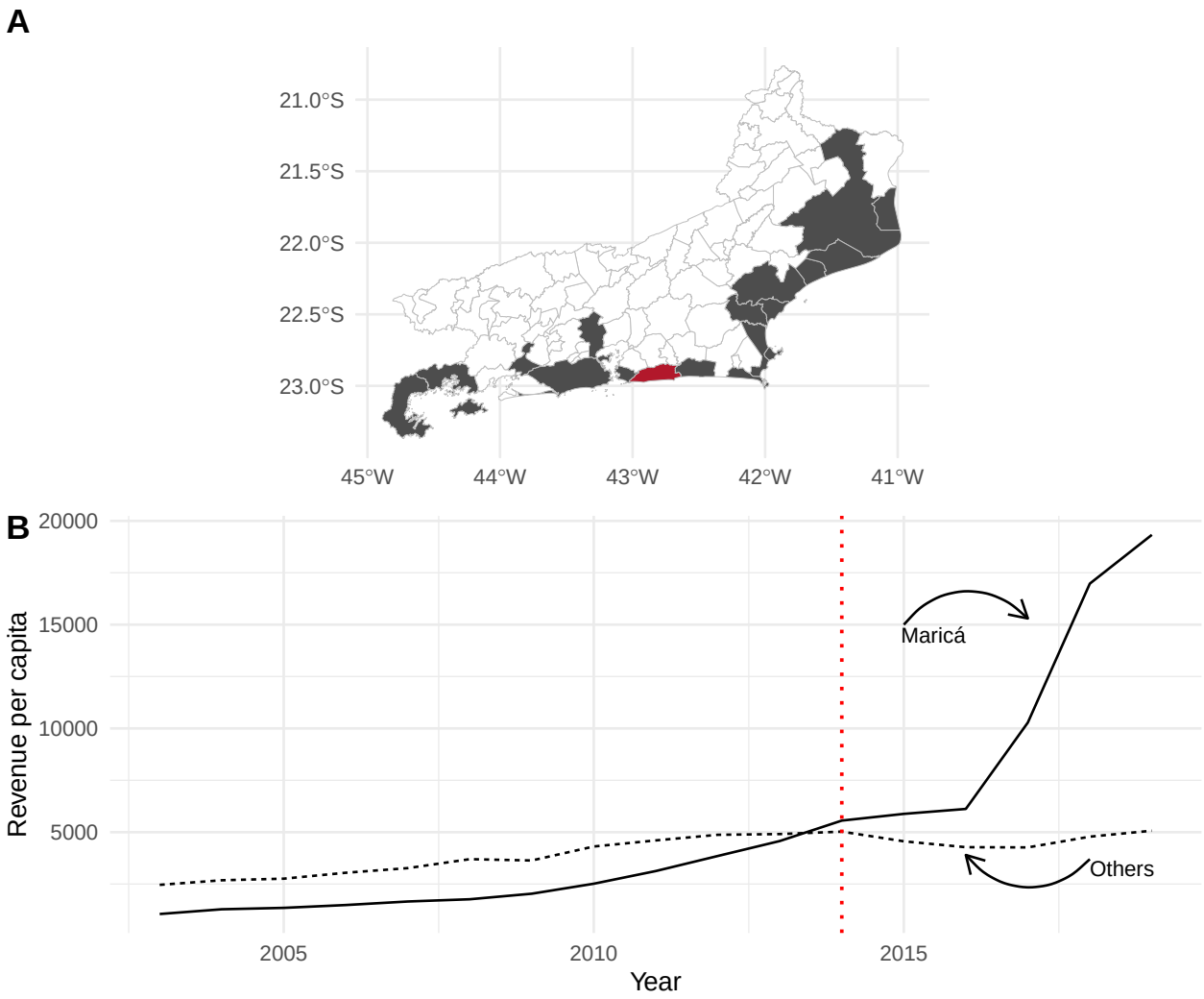
3.2 Fiscal background

From a fiscal perspective, this program was made possible after the Brazilian federal government discovered a new area for the exploitation of oil and gas: The Santos Basin Pre-salt Zone (SBPZ) – see Figure 2 (panel A). The exploitation of natural resources in the SBPZ placed Brazil among the countries with the most significant oil potential in the world and made it a net oil exporter (da Silva and de Matos, 2016; Sauer and Rodrigues, 2016). In 2017, the SBPZ accounted for 50.7% of Brazil's national oil and natural gas production (Alves, Schmitz and Polette, 2020).

Despite fluctuations in oil prices, Maricá has experienced a substantive increase in its revenues in the last decade. Figure 2 (panel B) shows that Maricá had a total revenue per capita of R$1,056 in 2003. Ten years later, it was R$4,573, more than four times larger. Since then, Maricá has been experiencing exponential growth in its revenue per capita due to the rise of the price of oil per barrel on the world market. This is the case because the proximity to the oil and gas fields is the main criterion for defining the royalties shares distribution among municipalities in the SBPZ.[15]

[15] According to Law nº12.351/2010, the closer to the oil and gas fields, the more royalties a given municipality should receive, which made Maricá the main net beneficiary of the royalties in the SBPZ (Alves, Schmitz and Polette, 2020). In the course to pass this legislation in the Brazilian Congress, the representatives of the states producing oil and gas (i.e., Alagoas, Amazonas, Bahia, Ceará, Espírito Santo, Maranhão, Rio de Janeiro, Rio Grande do Norte, Sergipe, and São Paulo) managed to build a coalition in the Brazilian Congress to prevent the royalties from being equally distributed among producers and non-producers states in Brazil. Therefore, the rule of royalties allocation as it was defined was not meant to specifically benefit Maricá (Sauer and Rodrigues, 2016).

Figure 2: Map of Rio de Janeiro state locating the Santos Basin Pre-salt Zone (A) - Revenue per capita in Maricá versus other municipalities (2003-2019) in Rio de Janeiro state (B)



Note: Panel (A) shows all the 92 municipalities in Rio de Janeiro state. The red one is Maricá; The black ones are the other 17 municipalities (Saquarema, Niterói, Rio de Janeiro, Duque de Caxias, Cabo Frio, Macaé, Casimiro de Abreu, Armação dos Búzios, Carapebus, Rio das Ostras, Quissamã, Itaguaí, Paraty, Angra dos Reis, Campos dos Goytacazes, São João da Barra, Arraial do Cabo) in the Santos Basin Pre-salt Zone; The white ones are the municipalities outside of the Santos Basin Pre-salt Zone. Panel (B) informs the total revenue per capita in Maricá and the other municipalities (others) in Rio de Janeiro state from 2003 to 2019. The unit of analysis is the municipality. Compiled by the author with data from the Institute for Applied Economic Research (IPEA).

3.3 Political background

It is worth noting that the adoption of the RBC in Maricá is mainly explained by its increasing fiscal capacity. From a political point of view, there was nothing particular about Maricá that could justify the implementation of an unconditional cash transfer. As reported in Appendix C (Panel A), before winning the mayoral elections in 2008, the PT was defeated twice (2000 and 2004) in Maricá. Also, the PT's candidates in presidential disputes have been rejected by the majority of Maricá's voters in all elections held since 1998, see Appendix C (Panel B). In combination, this indicates that the electorate in Maricá was not particularly inclined to vote for parties with a left-wing orientation. Reinforcing this interpretation, evidence from two decades (2000-2020) of local elections shows that the representatives elected to the local council in Maricá had similar characteristics to those elected in other municipalities in Rio de Janeiro state, as reported in Appendix D.

4 Empirical strategies and data

In this paper, I compare overtime levels of voter turnout in Maricá to other municipalities in Rio de Janeiro without an unconditional cash transfer program to address whether and how the RBC has affected electoral participation. To this purpose, I use two different yet complementary empirical strategies, namely, a difference-in-differences design and a synthetic control method approach. In what follows in this section, I describe in detail these identification strategies and the data sources employed in each one of them.

4.1 Difference-in-differences design

I use an ordinary least-squares (OLS) regression model to estimate the causal effect of the RBC on voter turnout in elections held in Brazil from 1998 to 2020.

$$Y_{i,s,t} = \alpha + \beta Treat_s + \gamma PostElec_t + \delta_r DiD(Treat_s X PostElec_t) + e_{i,s,t} \quad (1)$$

The unit of analysis, i, is the polling station,[16] s denotes the municipality, the level where the treatment occurred, while t refers to time. *Treat* is a dummy that takes a value of 1 if the polling station is located in Maricá. *PostElec* is a dummy variable to identify units in the posttreatment period. *PostElec* controls for the fact that conditions change over time for all units, whether treated or not. DiD is an interaction term created by multiplying *Treat* and *PostElec* that indicates treated polling stations in the post-treatment period. In difference-in-differences models, conventional standard errors often understate the standard deviation of the estimators,[17] meaning that standard errors are biased downward (Bertrand, Duflo and Mullainathan, 2004; Cunningham, 2018). To account for this, I run models with the standard errors clustered at the municipal level, the level of the intervention (Abadie et al., 2017).

4.1.1 Data

In Brazil, voting is compulsory for citizens between the ages of 18 and 70.[18] However, the penalty for not voting is a small fine of R$ 3.51 (roughly US$ 0.90), so abstention remains

[16] Running models at this level of analysis allows for granularity, which translates into more statistical power and, thus, more precise estimates.

[17] In this set-up, for example, it is possible that voter turnout in each polling station is not only correlated within the municipalities but also serially correlated. Therefore, one should cluster the standard errors.

[18] Voting is optional for those between the ages of 16 and 18 or over 70.

an option (Zucco Jr and Nicolau, 2016). Despite the compulsory voting rule, around 20% of registered voters do not show up to vote on election day (Speck and Peixoto, 2022).

I use data[19] from Brazil's Electoral Court (Tribunal Superior Eleitoral, TSE) to create the outcome (dependent) variable used in this study: the share of registered voters who did show up to vote on election day (i.e., voter turnout) in polling stations located in Rio de Janeiro state. I gathered this information for all races, whether local[20] or general[21] elections, held in Brazil from 1998 to 2020. My panel dataset has 363,602 data points encompassing twelve election-year periods.

Furthermore, I use data from the TSE to compute measures of characteristics of the electorate in each polling station in Rio de Janeiro state, namely: 1) average schooling, 2) average age; and 3) the incidence of women registered to vote. Data on voters' characteristics are only available from 2008 onward. For this reason, my difference-in-differences estimates conditioning on these covariates (discussed later in this paper) have fewer observations.

4.2 Synthetic control method

I employ a synthetic control method as a complementary identification strategy. This method was first intuitively employed by Card (1990) and later formalized and proposed by Abadie, Diamond and Hainmueller (2010, 2015). Similar to a difference-in-difference design, a synthetic

[19] Appendix A reports descriptive statistics for all variables used in this paper.

[20] Every four years, Brazilians of 5,568 municipalities vote to elect a mayor (*Prefeito*) and local council representatives (*Vereadores*). Given the evidence that data from the 1996 local elections are incomplete and likely to be affected by inaccurate data processing (De Magalhaes, 2015), my panel dataset includes all disputes held since 2000.

[21] Every four years, Brazilians of 5,568 municipalities vote to elect a president (*Presidente da República*), state governors (*Governadores*), legislators for the subnational councils (*Deputados Estaduais*), legislators for the national-level lower chamber (*Deputados Federais*), and legislators for the national-level upper chamber (*Senadores*). My panel dataset includes all disputes held since 1998, the first year this data is complete and publicly available at the polling station level.

control estimation exploits the differences in treated and untreated units across the outcome of interest. The main contrast is that, in a synthetic control estimation, untreated units do not receive the same weight in the analysis. Instead, a weighted average of the units unaffected by the intervention is used to simulate how the outcome of interest would vary in the posttreatment in the absence of the intervention. This counterfactual (i.e., the synthetic control) is created from the weights identified from the comparison employing a vector of pretreatment covariates.

The fundamental idea is that a combination of units in the donor pool– i.e., a list of untreated units–may approximate the characteristics of the treated unit much better than any unaffected unit alone. In my setting, there is a sample of $J + 1$ municipalities indexed by j, among whom j = 1 is Maricá (i.e., the treated unit), and $j = 2$ to $J + 1$ are municipalities in Rio de Janeiro state that constitutes the donor pool. The estimated synthetic control of Maricá[22] is

$$\hat{\alpha}_{it} = Y_{it} - \sum_{j \geq 2} w_j Y_{jt} \tag{2}$$

where the unit of analysis, j, is the municipality, t refers to the number of pretreatment periods of the T total periods, and w is an observation-weight matrix constructed from a set of k pretreatment characteristics (i.e., predictors). After controlling for observables, the estimation method proposed by Abadie and Gardeazabal (2003) relies on a standard fixed-effect model that allows for unobserved heterogeneity that is only time-invariant.

[22]Online Appendix E reports the means of predictors before the intervention. As shown, the synthetic Maricá approximates the actual Maricá better than the simple average for all unaffected municipalities in Rio de Janeiro state.

4.2.1 Data

To obtain my synthetic control estimates, I use data from the TSE to calculate voter turnout rates at the municipal level. This panel dataset (N = 1,196) spans all municipalities in Rio de Janeiro state (N = 92) and pools together elections held between 1998 and 2020. It also includes a set of municipal-level covariates (predictors), namely: the estimated population, the gross domestic product per capita, tax revenue per capita, and the homicide rates per 100.000 inhabitants. All these indicators were downloaded from the data repository maintained by the Institute of Applied Economic Research (IPEA).[23]

5 Results

5.1 Difference-in-differences estimates

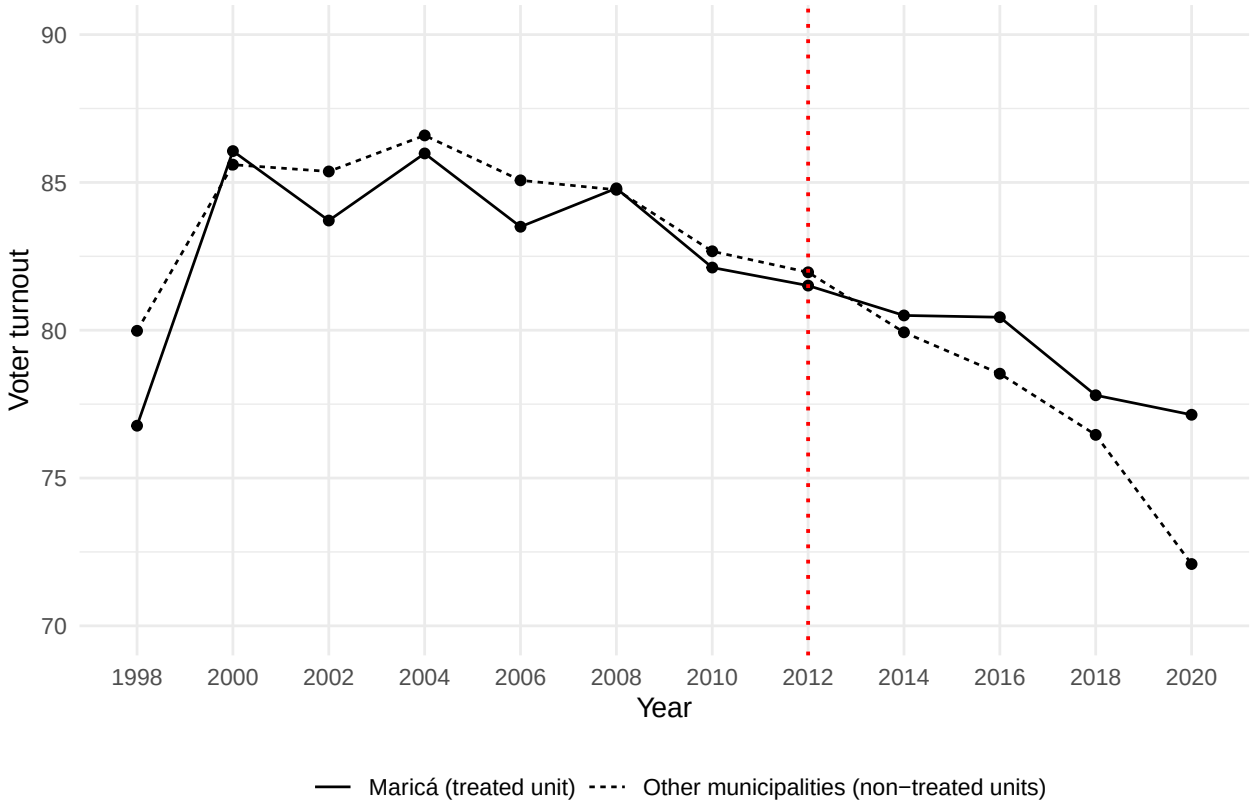
The validity of difference-in-differences estimates relies on the assumption that a control group approximates the traveling path of the treated units (i.e., parallel trends assumption). Because we cannot observe this counterfactual conditional expectation, this assumption is untestable by definition (Angrist and Pischke, 2008). Yet, one can empirically address whether or not there is evidence of its violation. In a typical setting, when this assumption holds, the difference between affected and unaffected units should be constant (i.e., variate in parallel) before the intervention (Cunningham, 2018).

Figure 3 plots voter turnout for two groups in elections held every two years from 1998 to 2020. The first one, represented by the solid line, depicts voter turnout in Maricá (treated unit), while the second accounts for the other municipalities in Rio de Janeiro state (non-

[23] It is available online at http://www.ipeadata.gov.br/Default.aspx.

treated units). The trend of voter turnout in the two groups was arguably parallel before the implementation of the RBC in Maricá. From the peak observed in 2000, passing through four cycles of up and downs between 2002 and 2008, until the declining trend initiated in 2010, both groups have been traveling in parallel.

Figure 3: Voter turnout in Maricá versus other municipalities in Rio de Janeiro state (1998-2020)



Note: The unit of analysis used to compute the mean is the polling station (N = 363,602). The solid line (treated unit) refers to voter turnout (%) in the polling stations in Maricá. The dashed line corresponds to polling stations in the other municipalities in Rio de Janeiro state. Time points in 1998, 2002, 2006, 2010, 2014, and 2018 account for general elections. Time points in 2000, 2004, 2008, 2012, 2016, and 2020 account for local elections.

Figure 3 reveals at least two other important patterns. First, there is a temporal coincidence between the adoption of the RBC in Maricá and a progressive gap between the two lines. Second, the gap between both lines is, to a great extent, driven by a decline in voter turnout in non-treated municipalities. While both groups of municipalities have been experiencing lower levels of electoral participation since 2008, this decline has been more accentuated in

municipalities unaffected by an unconditional cash transfer. This visual inspection suggests that voter abstention in Maricá would be higher without a monthly cash transfer program in place.

Table 1 confirms this graphical description. The DiD is the coefficient that, under the assumption of parallel trends in the pretreatment period, identifies the causal effect of the RBC on voter turnout, i.e., the average treatment effect on treated units (ATT). Panel A (models 1-3) reports the estimates using the full sample. Panel B (models 4-6) reports the estimates using a restricted sample with the oil-producers-only municipalities. In this case, I compare Maricá with the other 17 net beneficiaries of oil and gas in the SBPZ. As municipalities in this area also benefit from royalties and had an equivalent fiscal capacity when the RBC started running in 2014 (as shown in Figure 2, panel B), this is presumably a more conservative estimate of the impact of the RBC on outcomes of interest.

Table 1 shows an increase of three percentage points in voter turnout in Maricá. This estimated effect means an average net increase[24] of 3.5% in turnout in elections held in the post-treatment period. Substantively, this is equivalent to an average addition of 11 voters casting ballots in each polling station in Maricá.[25] In other words, 2.821 citizens in Maricá, roughly 3% of all citizens registered to vote, would not have voted in post-treatment elections in the absence of the RBC. The DiD estimates using the restricted sample are reassuring: On average, voter turnout increased by three percentage points after the RBC started running, a net increase of 4%. Importantly, this effect stands in the models with clustered standard

[24]I use the standard formula of percentage growth to estimate these effects. For example, before the intervention, the average voter turnout was 83.10 in Maricá (starting value). As per model 1 (the baseline model) in Table 1, the estimated effect of the RBC on voter turnout is 3.002 percentage points. I use these values in the standard formula of percentage growth by dividing the estimated effect by the starting value: 3.002/83.10 = .03612 x 100 = 3.612, or roughly 3.5%.

[25]On average, 344 voters showed up to vote in each polling station in Maricá before the intervention (1998-2012).

errors, municipal-level fixed effects, and those that include socio-demographic controls.

Table 1: The effect of RBC on voter turnout

	Full sample			Oil-only-municipalities		
	(1)	(2)	(3)	(4)	(5)	(6)
DiD (Treat X PostElec)	3.002***	2.979***	1.886***	3.359***	3.326***	2.088***
	(.4620)	(.4827)	(.2659)	(.6306)	(.6690)	(.3617)
R^2	0.235	0.3185	0.582	0.242	0.307	0.581
Obs.	363,598	363,598	226,646	199,916	199,916	124,661
N.Clusters	92	92	92	18	18	18
Clustered SE	✓	✓	✓	✓	✓	✓
Municipal-level FE		✓	✓		✓	✓
PS-level Controls			✓			✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of analysis is the polling station (PS). The dependent variable is (%) voter turnout. The following variables, measured at the polling station level, are included in the models with controls: The average level of voters' schooling; the average age of voters; and the incidence of women registered to vote. The full sample comprises all the municipalities in Rio de Janeiro state (N = 92), while the restricted one (Oil-only-municipalities) accounts for municipalities in the Santos Basin Pre-salt Zone (N = 18). This restricted sample is composed of the following municipalities: Saquarema, Niterói, Rio de Janeiro, Duque de Caxias, Cabo Frio, Macaé, Casimiro de Abreu, Armação dos Búzios, Carapebus, Rio das Ostras, Quissamã, Itaguaí, Paraty, Angra dos Reis, Campos dos Goytacazes, São João da Barra, Arraial do Cabo.

Even if the assumption of parallel trends holds, it could be argued that unobservable factors, and not solely the RBC, account for the increase in voter turnout in Maricá. While this hypothesis can never be completely ruled out, I provide reassuring evidence that this should be less of a concern in my setting.

I begin discussing the results of alternative models leveraging a placebo treatment unit. In this case, I use the other 17 municipalities in the SBPZ to create an in-space placebo treatment which takes a value 1 if the polling station is located in the SBPZ (excluding Maricá) and 0 otherwise. If the adoption of the RBC explains the change in voter turnout observed in the posttreatment period, the comparison between municipalities without an unconditional cash transfer should not reveal any effect on this outcome of interest. As reported in Table 2, this is

indeed what I find: none of the reported estimates (models 1 to 3) are statistically significant at the conventional levels.

Table 2: In-space and in-time placebo estimates

	In-space			In-time		
	(1)	(2)	(3)	(4)	(5)	(6)
DiD (Treat X PostElec)	-.8000	-.7667	-.5049	.5565***	.6059***	-.5002
	(.6896)	(.7229)	(.4577)	(.1780)	(.1998)	(.3365)
R^2	0.258	0.319	0.583	0.034	0.144	0.535
Obs.	361,108	361,108	224,897	231,468	231,468	94,533
N.Clusters	91	91	91	92	92	92
Clustered SE	✓	✓	✓	✓	✓	✓
Municipal-level FE		✓	✓		✓	✓
PS-level Controls			✓			✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of analysis is the polling station (PS). The dependent variable is (%) voter turnout. The following variables, measured at the polling station level, are included in the models with controls: The average level of voters' schooling; the average age of voters; and the incidence of women registered to vote. The full sample comprises all the municipalities in Rio de Janeiro state (N = 92), while the restricted one (Oil-only-municipalities) accounts for municipalities in the Santos Basin Pre-salt Zone (N = 18). This restricted sample is composed of the following municipalities: Saquarema, Niterói, Rio de Janeiro, Duque de Caxias, Cabo Frio, Macaé, Casimiro de Abreu, Armação dos Búzios, Carapebus, Rio das Ostras, Quissamã, Itaguaí, Paraty, Angra dos Reis, Campos dos Goytacazes, São João da Barra, Arraial do Cabo.

I also run in-time placebo estimates by choosing an alternative timing for the occurrence of the intervention. Instead of the actual threshold (> 2012), I use the period immediately before,[26] the one ranging from 2010 to 2012, as the posttreatment. Table 2 (models 4 to 6) shows the results. The baseline model without controls (model 4) is positive and statistically significant, even though the estimated coefficient is substantively small. Furthermore, and crucially, the most conservative model with socio-demographic controls (model 6) is negative and statistically non-significant at conventional levels. All in all, results reported in Table 2 are reassuring and suggest no consistent or robust evidence that a change in voter turnout

[26] In this case, I follow the procedure recommended by Angrist and Pischke (2014).

would be observed without the decision to implement an unconditional cash transfer in Maricá.

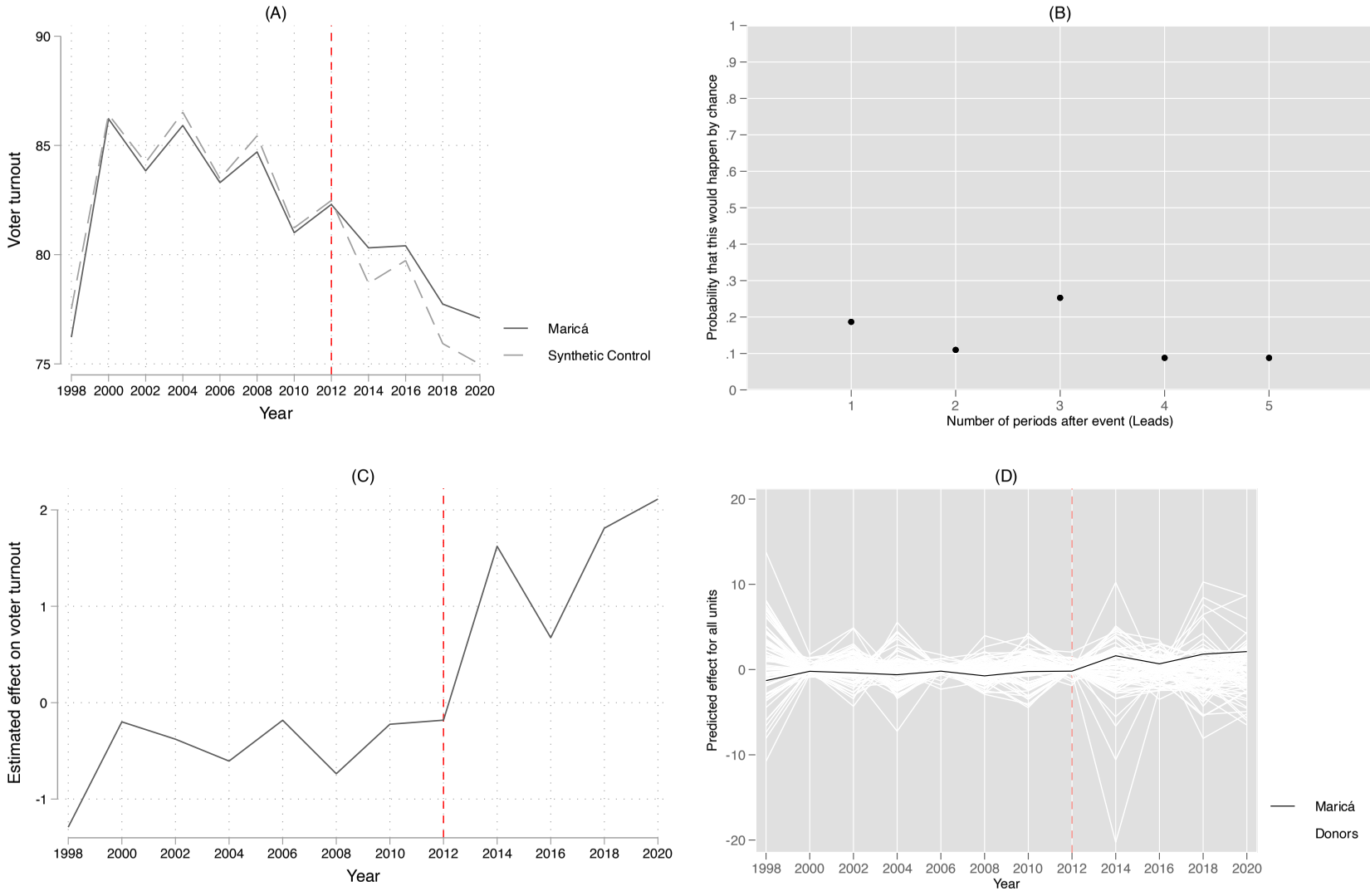
5.2 Synthetic control method estimates

What would have happened to voter turnout in Maricá had the adoption of an unconditional cash transfer never occurred? Results from my difference-in-differences estimates suggest that voter abstention in Maricá would be higher without a monthly cash transfer program, which corroborates the interpretation that the RBC prompted a mitigation effect. In this section, I report synthetic control method estimates that further corroborate this assessment.

Figure 4 (panels A to D) displays the results. Panel A plots the projection of voter turnout for two groups: the actual municipality of Maricá and its synthetic control.[27] These two lines traveled in parallel until 2012, the last pretreatment election. After this point, there is a clear gap between them driven by an acute decrease in voter turnout in the synthetic Maricá. Following the procedure proposed by Abadie, Diamond and Hainmueller (2015), I test whether this estimated effect holds when leaving out of the analysis (one by one) municipalities used to generate the synthetic Maricá. Results for this sensitivity analysis reported in Online Appendix G are reassuring: the pattern depicted in Figure 4 (Panel A) is not driven by a bad choice of the algorithm used to compute the synthetic control. I also employ the test developed by Galiani and Quistorff (2017) to detect if the trend observed in the posttreatment period would happen by chance: as indicated by the probabilities in Figure 4 (Panel B), this is rather unlikely.

[27] Online Appendix F reports the list of units in the donor pool selected by the algorithm to create the synthetic control, namely: São Pedro da Aldeia (weight: 0.391), Itaboraí (weight: 0.183), Porciúncula (weight: 0.118), Itaguaí (weight: 0.061), Miracema (weight: 0.033), Cabo Frio (weight: 0.032), Nilópolis (weight: 0.029), and Macaé (weight: 0.010).

Figure 4: The effect of the RBC on voter turnout - Synthetic control method estimates



Note: The unit of analysis is the municipality. I used the package synth (Abadie, Diamond and Hainmueller, 2011) to generate the figure in panel A and the package synthrunner (Galiani and Quistorff, 2017) to produce panels B to D. (A) plots the voter turnout for Maricá and its synthetic control; (B) plots the p-values (standardized effects) for posttreatment periods; (C) plots the difference between Maricá and its synthetic control (the estimated effect); (D) plots the predicted voter turnout in the posttreatment period for all units (Maricá + municipalities in the donor pool).

Panel C plots the difference between the two lines reported in panel A, i.e., the estimated effect. By default, the difference between Maricá and its synthetic control creates an overall positive effect of the RBC on voter turnout. However, this result has been mechanically induced by a drop in abstention in municipalities unaffected by the program, as indicated in Panel D. Compared to the municipalities in the donor pool, the levels of participation in Maricá remained more or less constant after the RBC started running in 2014.

One can read these findings using different lenses. A more pessimistic interpretation would emphasize the absence of mobilization effects and the implication that follows: cash transfers might not work to enlarge the pool of citizens willing to participate in elections. A more positive and perhaps realistic view would take into consideration the decreasing levels of participation in elections in Brazil (Speck and Peixoto, 2022) and elsewhere (e.g., Gallego, 2015). Given this context of widespread and growing rates of absenteeism, the evidence that monthly cash transfers can at least slow down consolidated trends of voter abstention is good news.

Conclusion

Do cash transfers mobilize new citizens (mobilization) or keep engaged the ones with the habit of voting (mitigation)? This paper sought to answer this question in the context of Maricá, Brazil, home to what is currently the largest unconditional cash transfer in Latin America: the RBC. Estimates from a difference-in-differences design and a synthetic control method indicate a temporal coincidence between the adoption of this cash transfer program and the stability of voter participation levels in Maricá. In parallel, in municipalities unaffected by the intervention, a process of continuous erosion in voter turnout is taking place. Together, the available evidence points to the effect of the RBC in mitigating voter abstention, rather than

in mobilizing new voters.

As elections shape democratic representation and impact how democracies function, efforts to enfranchise the poor matter (e.g., Lijphart, 1997; Beramendi and Anderson, 2008; Norris, 2012). Results reported in this paper call for caution in assuming that anti-poverty schemes have intrinsic mobilization effects. In contexts of increasing absenteeism, they may rather work as political safety nets to preserve the participation of citizens with the habit of voting. While this effect should not the neglected, it is certainly less comprehensive than political theorists (e.g., Morales, 2018) and enthusiasts of such programs usually claim.

But there are also reasons for hope. Recent evidence indicates that vulnerability reduction can depress political engagement as the incentives to respond to clientelistic electoral strategies shrink (Bobonis et al., 2022). Evidence from Maricá suggests that poverty alleviation is not intrinsically linked to reduced participation, thus prompting the question: Under what conditions does vulnerability reduction preclude the willingness to vote? While this paper does not provide a conclusive answer to this question, it suggests that vulnerability reduction induced by cash transfers does not seem to alienate the poor – quite the contrary.

Of course, usual concerns regarding external validity apply in this case. On the one hand, the evidence that unconditional cash transfers facilitate participation in elections does not seem to be Maricá-specific, as suggested by preliminary evidence from the *Finnish Basic Income Experiment* (Hirvonen, Schafer and Tukiainen, 2022). On the other hand, addressing whether or not cash transfers affect participation through mobilization or a mitigation mechanism will require further research. One could argue that the mitigation mechanism documented in this paper results from the increasing trend of absenteeism in the Brazilian elections. It should be noted, however, that declining levels of participation are not a "privilege" of Brazil and

have been plaguing democracies worldwide (e.g., Kostelka and Blais, 2021). Therefore, the mechanism operating in Maricá will likely travel to other settings with constituencies affected by anti-poverty schemes and constrained by similar institutional designs.

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Mobilization versus Mitigation

How do cash transfers affect participation in elections?

Online appendix

Contents

A Descriptive statistics

Table 1: Descriptive statistics - Polling station-level data

Variable	Obs	Mean	Std.Dev.	Min	Max
Voter turnout (%)	363,598	81.40	7.159	1.265	100
Treatment	363,602	.0068	.08246	0	1
Average age*	226,650	46.99	6.472	17	72.16
Average schooling*	226,650	4.414	.6849	1.813	7.184
Incidence of women†	226,650	3.103	.1087	2	4
Share of PT's votes‡	271,662	28.67	19.16	.1270	96.32

Note: Compiled by the author with data from the *Tribunal Superior Eleitoral* (Superior Electoral Court, TSE). The unit of analysis is the polling station (PS). †This indicator varies between 2 and 4. Values closer to 4 indicate a higher incidence of women voters in a given polling station. ‡This indicator refers to the share of votes for the PT in elections. This variable has fewer observations because PT did not present candidates in all municipalities in local elections (2000-2020) covered in the dataset. *Socio-demographic variables measured at the level of polling stations are available only from 2008 onward. That explains the fewer number of observations for these variables.

Table 2: Descriptive statistics - Municipal-level data

Variable	Obs	Mean	Std.Dev.	Min	Max
Voter turnout (%)	1,104	83.21	5.433	60.39	97.78
Treatment	1,104	.01086	.10373	0	1
Estimated population	1,011	173138	665318	4405	6718903
GDP per capita	1,011	2562	3534	585	33904
Revenue per capita	973	801221	2054733	0	2.43e+07
Homicides per 100.000	928	29.59	19.16	2.449	120.3

Note: Compiled by the author with data from the Institute of Applied Economic Research (IPEA). The unit of analysis is the municipality.

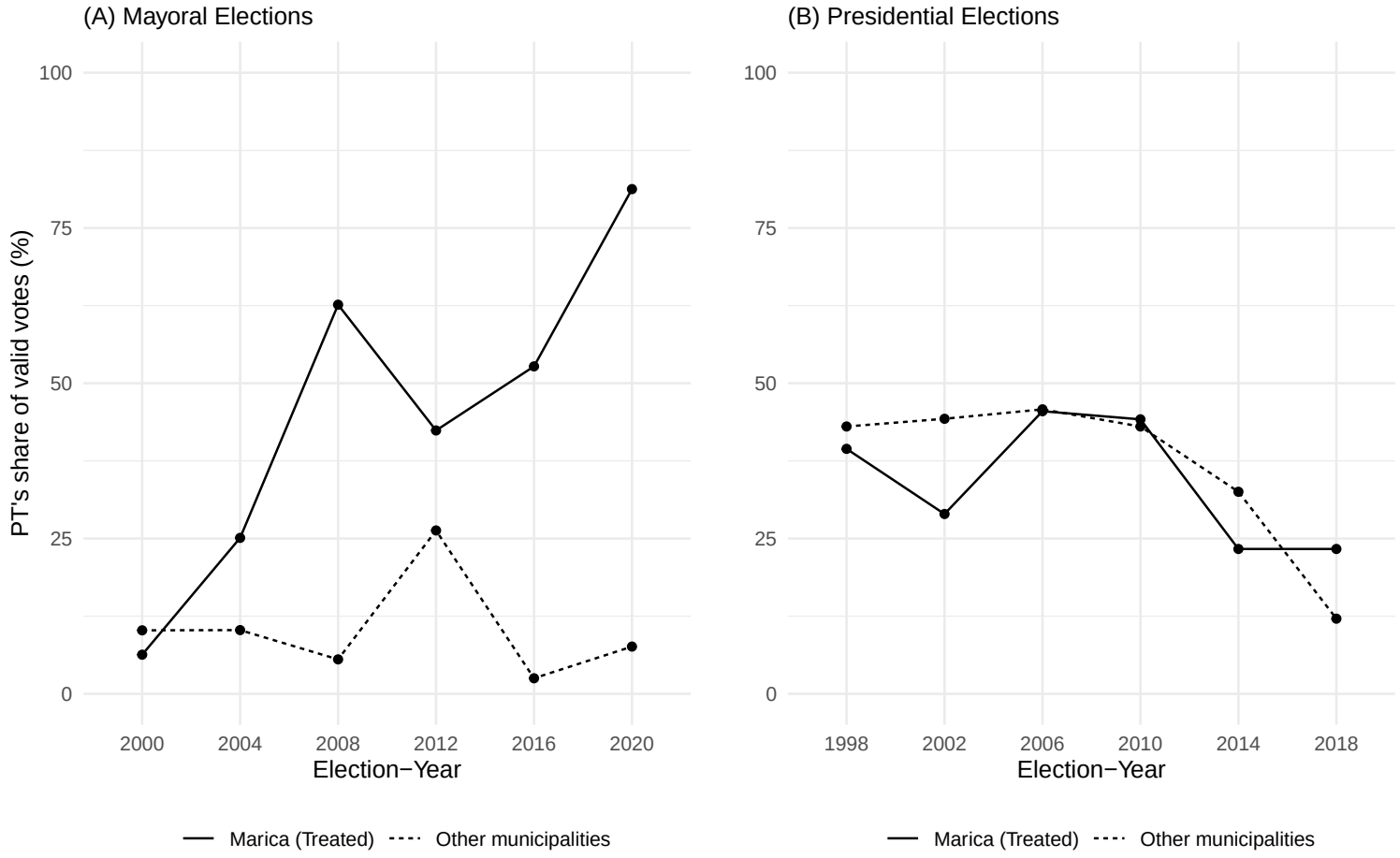
B Facsimile: RBC's card

Figure 1: RBC's card used to shop with Mumbucas

Figure 2: Note: The image reproduces an RBC beneficiary shopping with her card in Maricá.

C Electoral support for the PT in Rio de Janeiro state - Mayoral and presidential elections (1998-2020)

Figure 3: Vote support for the PT in Maricá versus municipalities in the control group – Local (2000-2020) and general (1998-2018) elections



Note: The unit of analysis used to compute the mean is the polling station. The solid line (Treated) refers to the share of valid votes (%) for the PT in the polling stations located in Maricá. The dashed line corresponds to polling stations in the other municipalities in Rio de Janeiro state (N = 91).

D Characteristics of the elected representatives in Rio de Janeiro state

Table 3: A one-way ANOVA test of means: profile of winners in the elections for the local council (2000-2020)

	Summary of means						
Variable	Year	Maricá	SBPZ	Others	F	Prob >F	Obs.
(%) Female winners							
	2000	11.76	4.859	7.842	0.94	0.3958	92
	2004	10.01	8.812	8.503	0.02	0.9791	92
	2008	8.974	8.802	9.507	0.48	0.6196	92
	2012	9.141	9.639	9.150	0.51	0.6041	92
	2016	9.227	9.573	9.272	0.51	0.5998	92
(%) Winners belonging to a left-wing party							
	2000	29.411	33.18	29.93	0.36	0.7021	92
	2004	10.00	19.48	15.56	0.62	0.5411	92
	2008	63.63	22.12	17.39	5.94	0.0038**	92
	2012	36.36	23.00	21.98	0.51	0.6045	92
	2016	23.52	15.01	14.17	0.39	0.6762	92
(%) Winners with a bachelor degree							
	2000	23.52	26.21	22.48	0.39	0.6784	92
	2004	40.00	26.22	26.86	0.33	0.7224	92
	2008	18.18	30.74	27.30	0.40	0.6687	92
	2012	36.36	33.65	26.29	1.73	0.1840	92
	2016	29.41	32.10	25.33	1.30	0.2788	92
(%) Winners younger than 40 years old							
	2000	23.52	34.25	34.63	0.28	0.7551	92
	2004	30.00	34.87	30.16	0.65	0.5261	92
	2008	45.45	32.35	33.33	0.25	0.7799	92
	2012	18.18	36.33	33.81	0.71	0.4933	92
	2016	47.05	29.51	35.66	1.58	0.2112	92
(%) Winners with a law degree							
	2000	3.379	4.705	3.120	0.93	0.3991	92
	2004	10.00	2.575	5.020	1.28	0.2832	92
	2008	3.573	4.840	3.330	0.64	0.5276	92
	2012	3.417	7.121	2.612	4.86	0.0099***	92
	2016	5.882	5.087	3.270	0.69	0.5021	92

Note: Compiled by the author with data from the *Tribunal Superior Eleitoral* (Superior Electoral Court, TSE). The unit of analysis is the municipality. A one-way ANOVA is used to determine whether or not there is a statistically significant difference between the means of these three independent groups. SBPZ refers to all municipalities in the Santos Basin Pre-salt zone excluding Marica (N = 17). The category others refers to all municipalities in Rio de Janeiro state (N = 91).

E Synthetic control method adjustment: predictors balancing

The main assumption when running a synthetic control method is that a combination of units in the donor pool - i.e., a list of untreated units - better approximates the characteristics of the treated unit. While this assumption cannot be tested, one can assess its validity by comparing the balance of predictors with (Synthetic control) and without (Avg. unaffected units) the weighting process used by the algorithm developed by Abadie et al. (2010). As Table 4 informs, the synthetic control fairly approximates the treated unit for all the predictors included in the analysis.

Table 4: Predictors balancing: Synthetic control versus the average of unaffected municipalities in Rio de Janeiro state

Variable	Treated	Synthetic control	Avg. unaffected units
Estimated population	103566.9	103362.8	165679.9
GDP per capita	1595.099	1585.064	2663.718
Revenue per capita	19717.75	19741.76	16696.78
Homicides per 100.000	42.49953	42.51435	30.19021
Turnout (2000)	86.21663	86.41523	87.84939
Turnout (2006)	83.29622	83.47822	85.23631
Turnout (2012)	82.29884	82.47988	85.19959

Note: Compiled by the author with data from Brazil's Electoral Court (Tribunal Superior Eleitoral, TSE) and the Institute of Applied Economic Research (IPEA).

F Calculated weights for municipalities used in the synthetic Maricá

To generate the original synthetic Maricá (reported in the main text; Figure 4), I used the algorithm developed by Abadie et al. (2010). This procedure allows for a data-driven selection of municipalities similar to Maricá in the pretreatment period. Table 5 lists these selected municipalities used to project the synthetic control in the posttreatment period.

Table 5: Units (i.e., municipalities) used in the synthetic Maricá and their weights

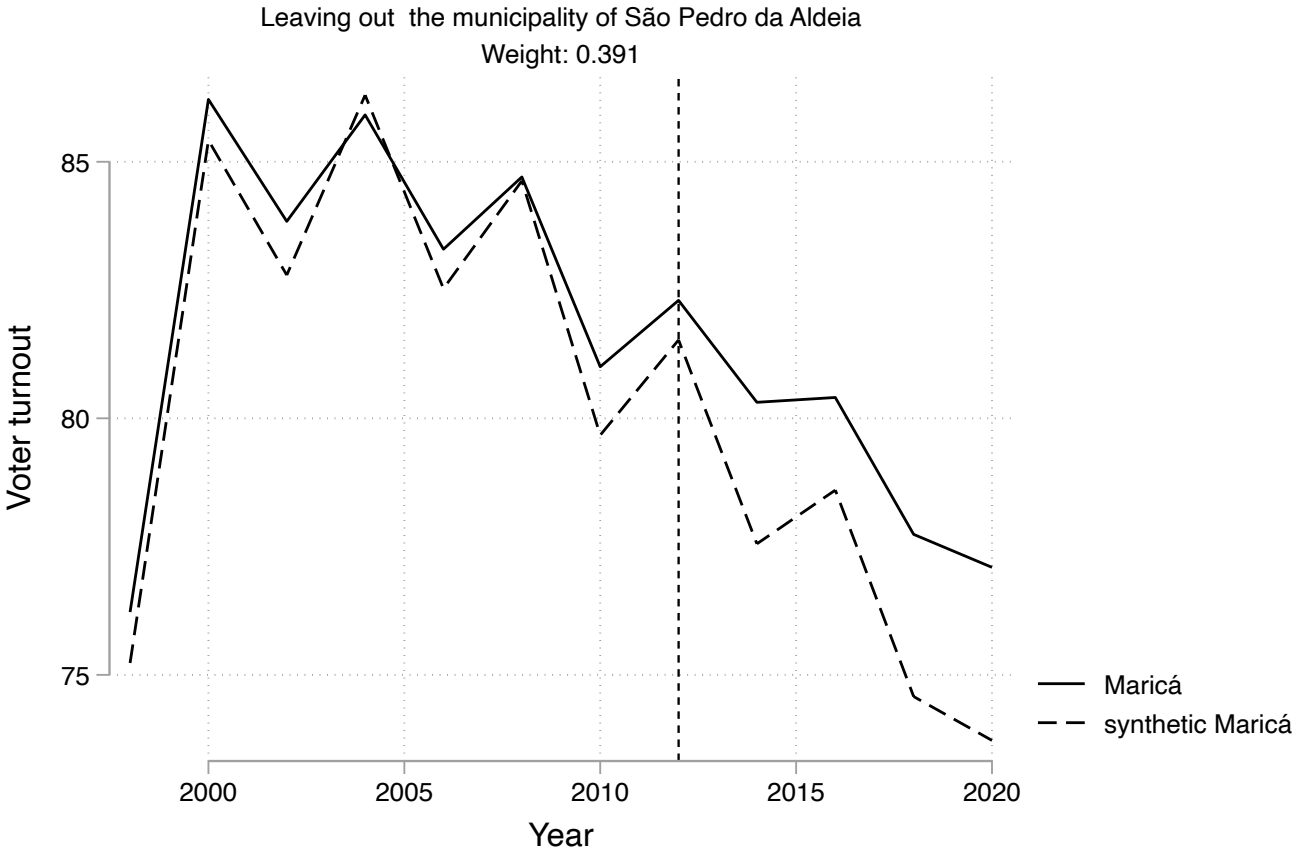
Unit	Weight
São Pedro da Aldeia	0.391
Itaboraí	0.183
Porciúncula	0.118
Itaguaí	0.061
Miracema	0.033
Cabo Frio	0.032
Nilópolis	0.029
Macaé	0.010

Note: Municipalities not reported in the table received a weight equal to zero in the analysis. Compiled by the author with data from Brazil's Electoral Court (Tribunal Superior Eleitoral, TSE) and the Institute of Applied Economic Research (IPEA).

G Leaving-one-out sensitivity analysis

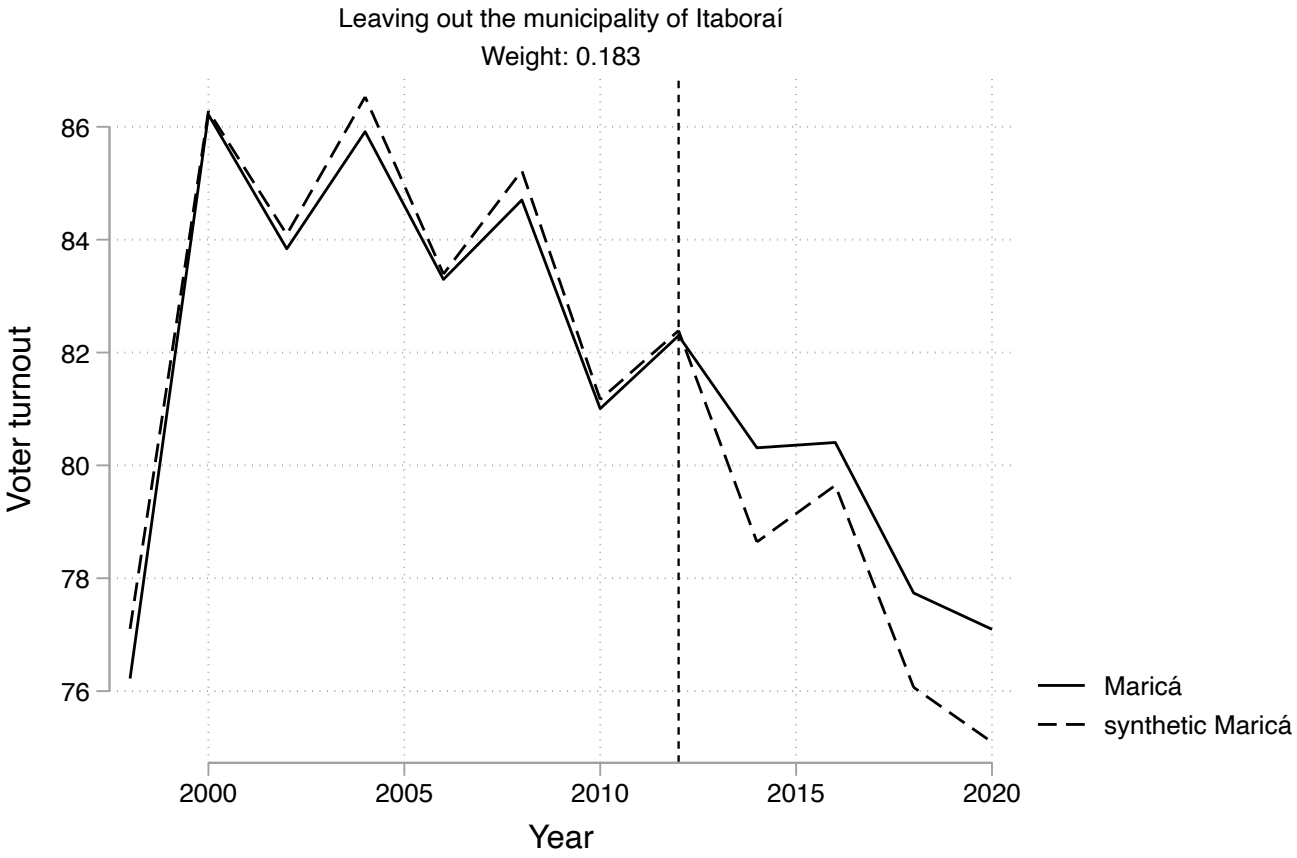
As recommended by Abadie et al. (2015), one should always account for the sensitivity of the synthetic control method. Figures 5 to 12 report the results of this sensitivity analysis leaving out each of the eight municipalities used in the original synthetic Maricá. As shown, I found consistent estimates in all cases: starting in 2014, there is a clear gap between the two lines explained by lower levels of voter turnout in the synthetic Maricá. Crucially, this result is not driven by share-picking municipalities from the donor pool.

Figure 4: Estimating the synthetic Maricá without São Pedro da Aldeia, Rio de Janeiro state, Brazil



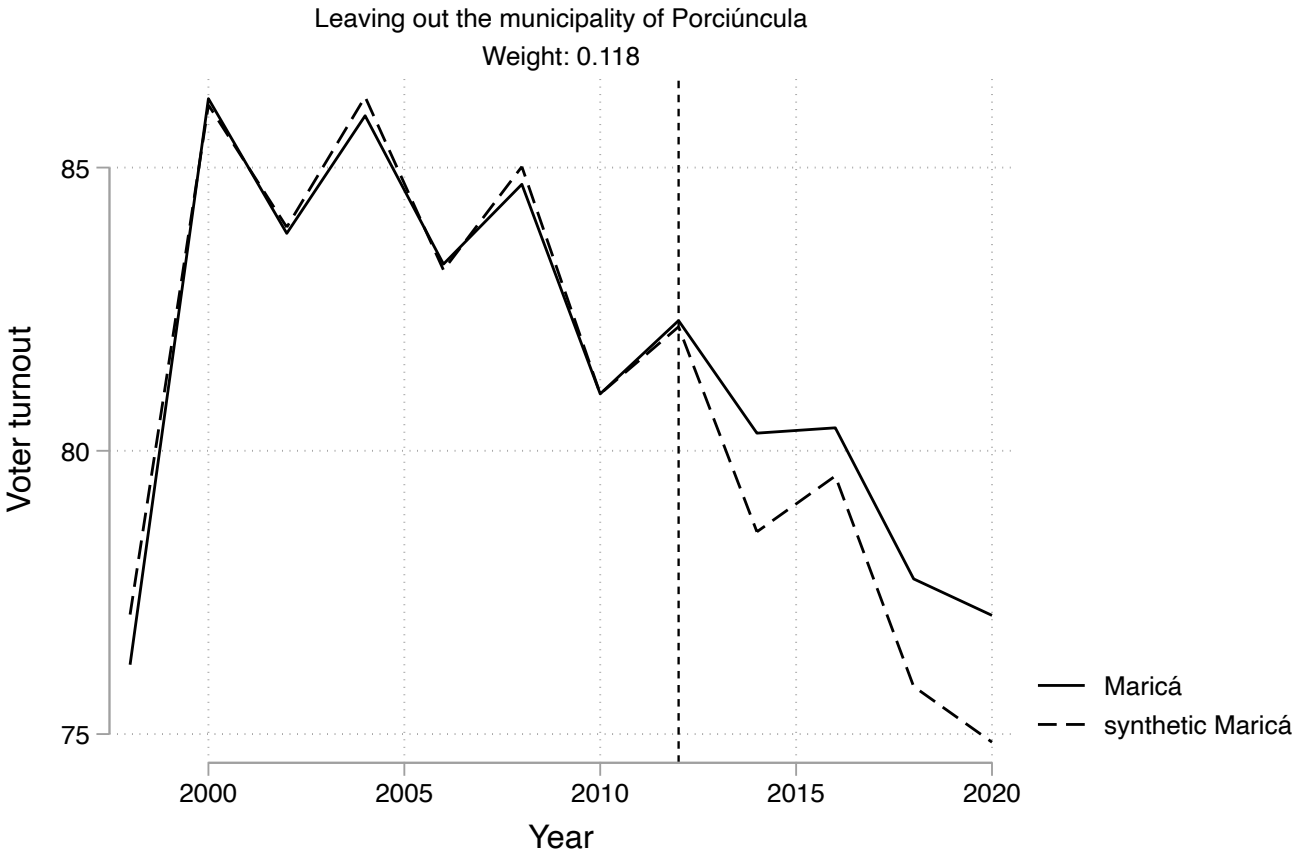
Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: Itaboraí (weight: 0.183), Porciúncula (weight: 0.118), Itaguaí (weight: 0.061), Miracema (weight: 0.033), Cabo Frio (weight: 0.032), Nilópolis (weight: 0.029), and Macaé (weight: 0.010).

Figure 5: Estimating the synthetic Maricá without Itaboraí, Rio de Janeiro state, Brazil



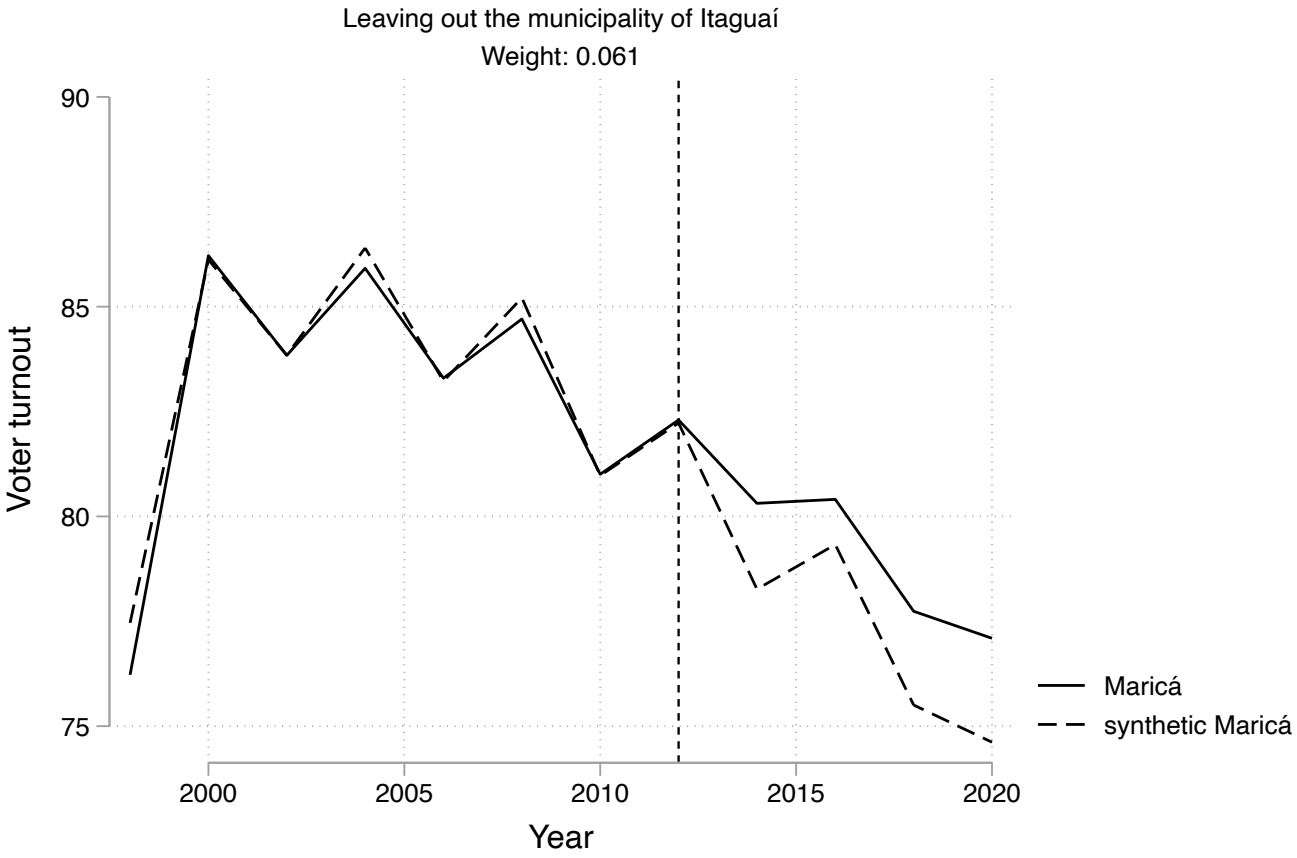
Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: São Pedro da Aldeia (weight: 0.391), Porciúncula (weight: 0.118), Itaguaí (weight: 0.061), Miracema (weight: 0.033), Cabo Frio (weight: 0.032), Nilópolis (weight: 0.029), and Macaé (weight: 0.010).

Figure 6: Estimating the synthetic Maricá without Porciúncula, Rio de Janeiro state, Brazil



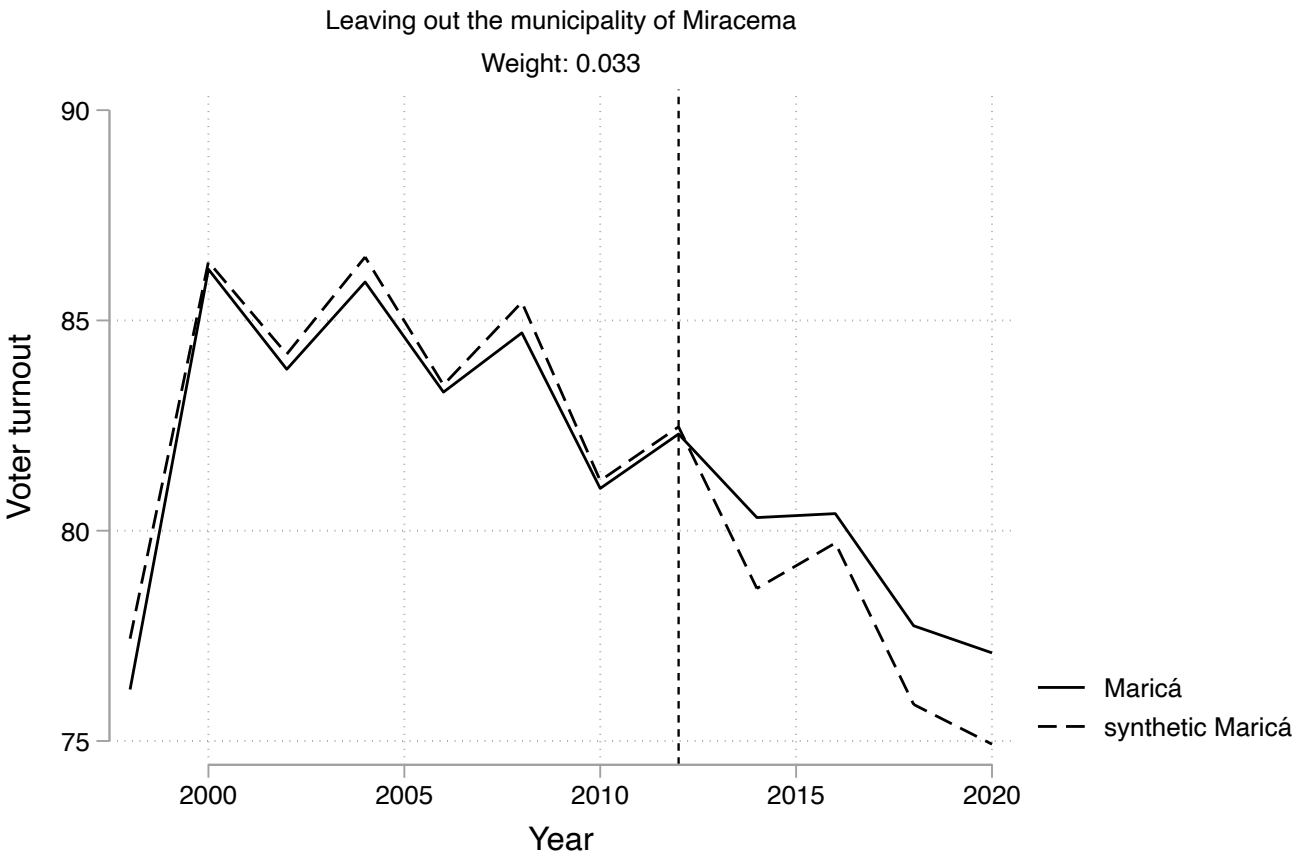
Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: São Pedro da Aldeia (weight: 0.391), Itaboraí (weight: 0.183), Itaguaí (weight: 0.061), Miracema (weight: 0.033), Cabo Frio (weight: 0.032), Nilópolis (weight: 0.029), and Macaé (weight: 0.010).

Figure 7: Estimating the synthetic Maricá without Itaguaí, Rio de Janeiro state, Brazil



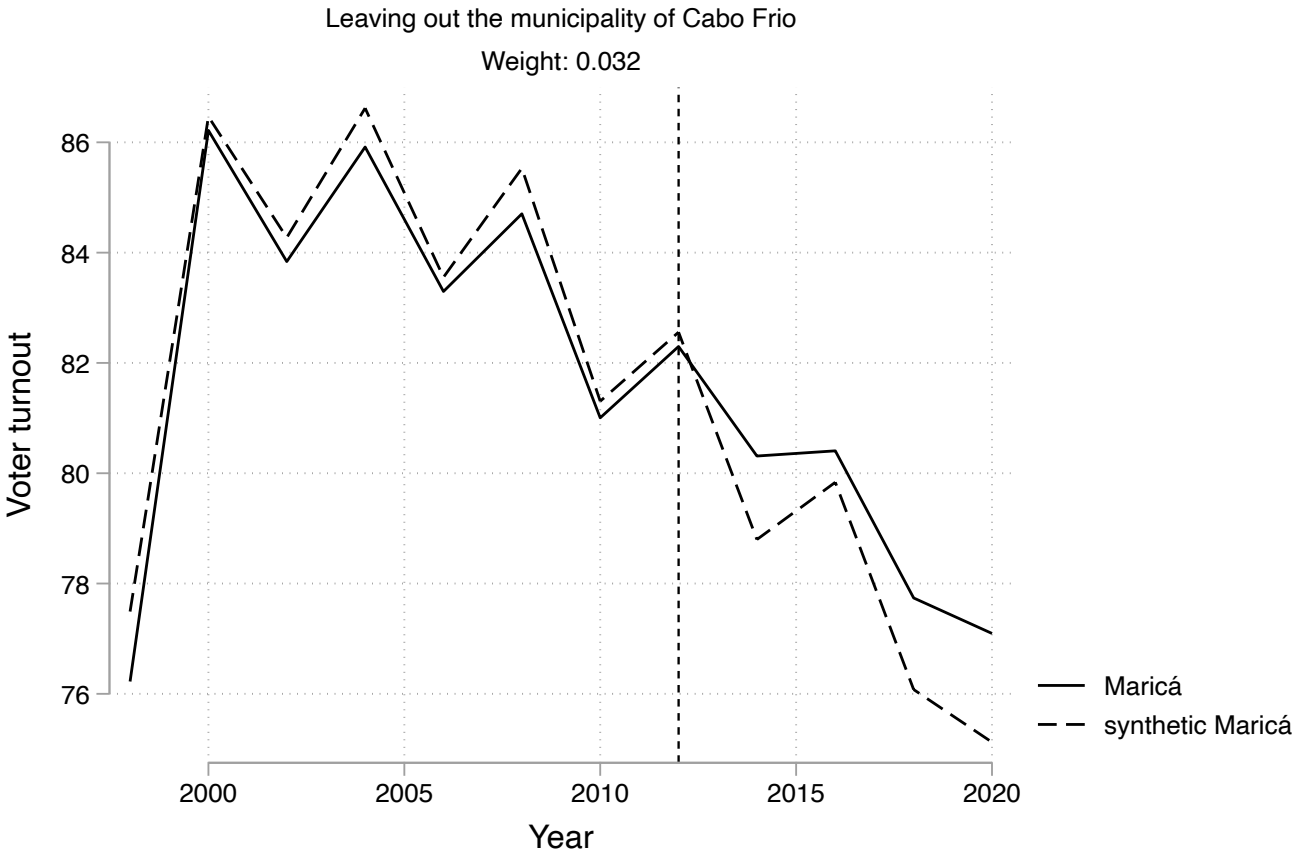
Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: São Pedro da Aldeia (weight: 0.391), Itaboraí (weight: 0.183), Porciúncula (weight: 0.118), Miracema (weight: 0.033), Cabo Frio (weight: 0.032), Nilópolis (weight: 0.029), and Macaé (weight: 0.010).

Figure 8: Estimating the synthetic Maricá without Miracema, Rio de Janeiro state, Brazil



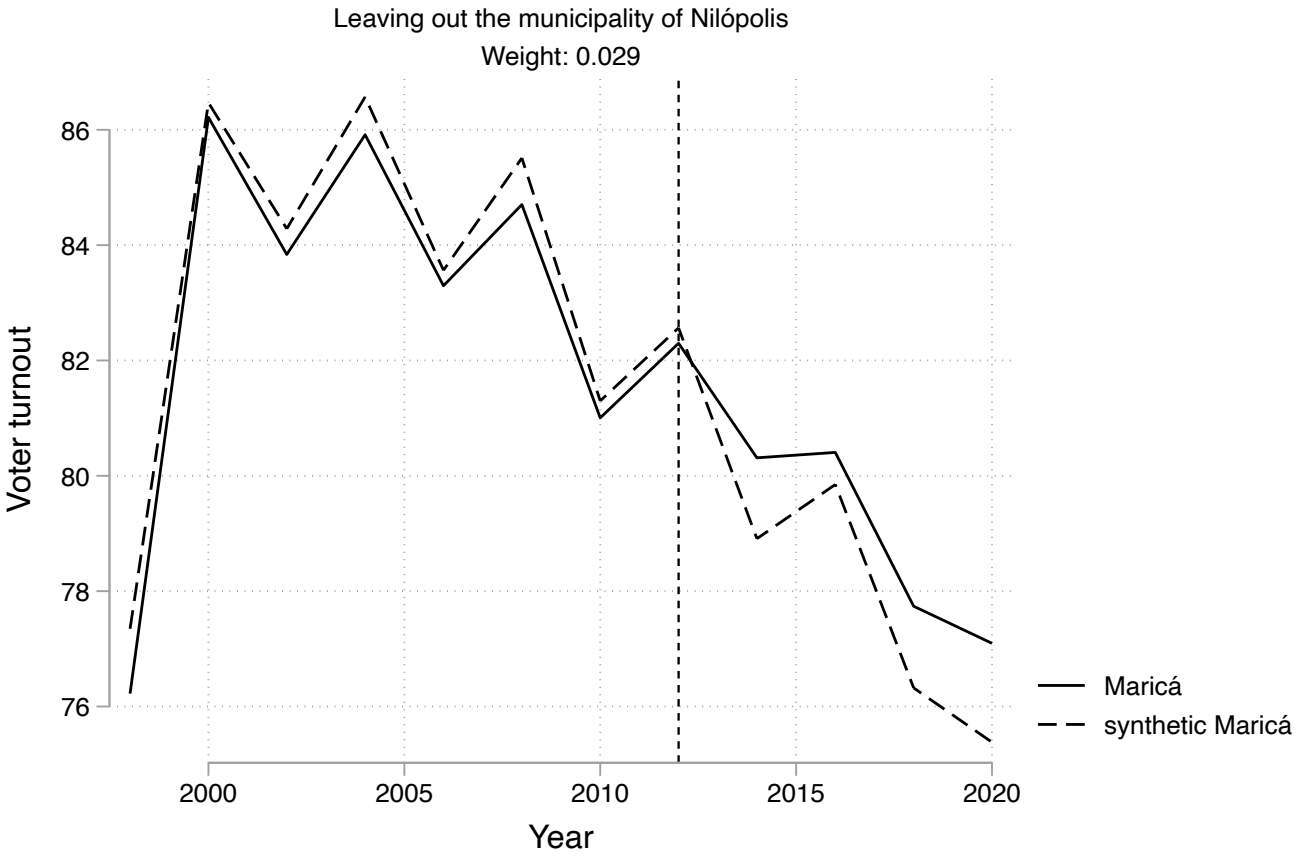
Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: São Pedro da Aldeia (weight: 0.391), Itaboraí (weight: 0.183), Porciúncula (weight: 0.118), Itaguaí (weight: 0.061), Cabo Frio (weight: 0.032), Nilópolis (weight: 0.029), and Macaé (weight: 0.010).

Figure 9: Estimating the synthetic Maricá without Cabo Frio, Rio de Janeiro state, Brazil



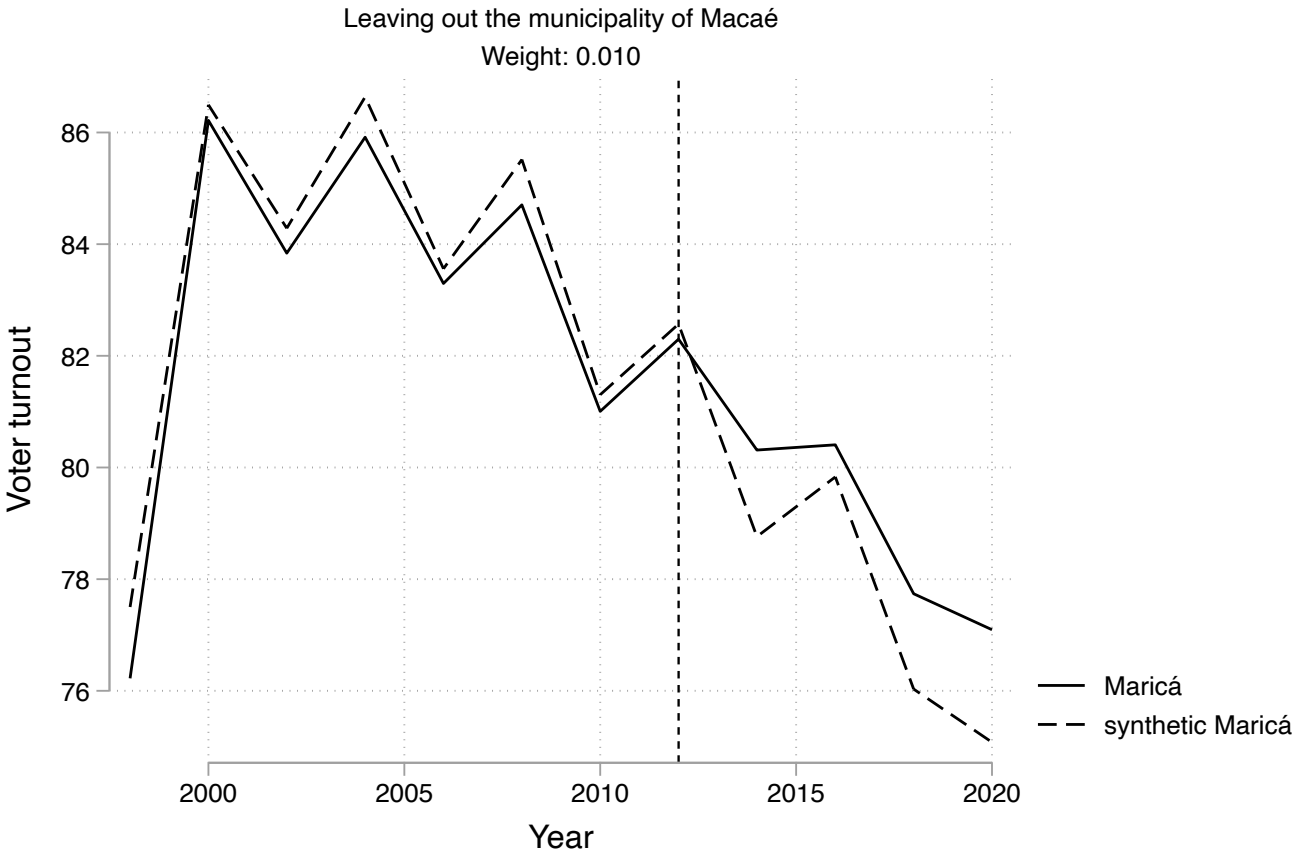
Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: São Pedro da Aldeia (weight: 0.391), Itaboraí (weight: 0.183), Porciúncula (weight: 0.118), Itaguaí (weight: 0.061), Miracema (weight: 0.033), Nilópolis (weight: 0.029), and Macaé (weight: 0.010).

Figure 10: Estimating the synthetic Maricá without Nilópolis, Rio de Janeiro state, Brazil



Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: São Pedro da Aldeia (weight: 0.391), Itaboraí (weight: 0.183), Porciúncula (weight: 0.118), Itaguaí (weight: 0.061), Miracema (weight: 0.033), Cabo Frio (weight: 0.032), and Macaé (weight: 0.010).

Figure 11: Estimating the synthetic Maricá without Macaé, Rio de Janeiro state, Brazil



Note: The unit of analysis is the municipality. The synthetic control method is composed of the following municipalities from the donor pool: São Pedro da Aldeia (weight: 0.391), Itaboraí (weight: 0.183), Porciúncula (weight: 0.118), Itaguaí (weight: 0.061), Miracema (weight: 0.033), Cabo Frio (weight: 0.032), and Nilópolis (weight: 0.029).

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