The electoral effects of large-scale infrastructure policies: evidence from a rural electrification scheme in Brazil

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Abstract

This paper analyzes the conditions under which major infrastructural investments generate electoral returns. It addresses when and how the constraints imposed by myopic voters under democracy can be overcome. We argue that sustained policy spillovers are critical to broadening the pool of beneficiaries and yielding significant returns to the incumbent in the medium to long run. We make this case by analyzing Luz para Todos (LPT) – a large-scale rural electrification scheme implemented in Brazil by the Workers' Party (PT). Leveraging the LPT's quasi-experimental allocation, we document its positive and persistent impact on the PT's vote support several years after the program started running. We then illustrate the mechanism of policy spillovers by showing the impact of the LPT on the provision of education in targeted areas. Our findings suggest that infrastructure policies are more likely to generate electoral returns when the policy provision entails spillover effects through other policies.

Keywords: Electricity; education supply; elections; policy spillovers; intention to treat; fuzzy regression discontinuity design.

[*]**Replication files are available in the JOP Dataverse(https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the JOP replication analyst.**

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Introduction

The expansion of infrastructural investments is a central aspect of politics in the developing world. By 2030, estimates predict a state investment of US$ 86 billion per year in electrification infrastructure (Pachauri et al., 2013). A significant share of these investments reflects the arrival and consolidation of democracy. For example, Trotter (2016) and Kroth et al. (2016) show that democracy and enfranchisement are associated with a higher electrification supply in Sub-Saharan African countries (e.g., Ghana, Swaziland, Uganda, Senegal, Rwanda, and South Africa). Likewise, granular and compelling evidence from the Indian case indicates that expansions in electricity coverage follow election cycles (Min, 2015; Baskaran et al., 2015).

Much like other efforts to expand the capacity and territorial presence of the state, infrastructural investments pose a dilemma for incumbents. These policy initiatives are seen as generating both short-term costs and uncertain political returns in the medium to long run. Pursuing them may be necessary, but it is also politically risky. This paper claims that large-scale infrastructure projects are likely to generate electoral returns when the policy provision entails spillover effects through other policies. In other words, the trap set by myopic voters under democracy can, under certain conditions, be overcome. We analyze this process by studying investments in electrification – policies with massive effects on individuals' and communities' well-being. The lack of electricity is a reality for over 1 billion people living in low and middle-income countries (Lee et al., 2020). Although electrification initiatives have been flourishing in recent years (Javadi et al., 2013; Lee et al., 2020), our knowledge of their electoral effects remains limited.

We argue that infrastructural policies are more likely to generate electoral returns when costs are not concentrated and visible, benefits are broad and produce positive spillovers, and an efficient credit claim strategy is deployed. Building on previous studies, we delimit the scope of conditions for incumbents to draw electoral rewards from large-scale infrastructures. Regarding policy design, we argue that the low concentration and visibility of costs reduce the opposition to the program while broad benefits and positive spillover enhance the electoral support of beneficiaries. On the credit claim dimension, an efficient strategy of attribution of responsibilities can gear electoral returns. Hence, policy design and credit claim strategies are key conditions for incumbents to derive sustained electoral rewards in the medium to long run.

Importantly, our paper introduces an important distinction between short and medium to long-run benefits. In the short run, it may occur that electrification does not reach its potential to generate electoral returns. But it does happen after the indirect benefits derived from policy spillovers materialize. We pay particular attention to one crucial though non-exclusive policy channel bearing on human capital formation: expanding access to education for people previously excluded due to lack of electricity.

We assess this argument through an in-depth study of the electoral consequences of Luz para Todos (henceforth LPT), a large-scale rural electrification scheme implemented by the Workers' Party (PT) in Brazil. LPT was a successful case in terms of timing and target, and the provision of electricity was accompanied by subsidies to poor households to help them pay their bills (Pereira et al., 2011). From 2004 to 2015, more than 3.2 million households, health facilities, and public schools were connected to the electrical grid through the LPT program.

Methodologically, we take advantage of the quasi-experimental conditions under which the LPT was implemented: As the main rule, a municipality was eligible for the program if fewer than 85% of the households had access to electricity, according to the 2000 Brazilian census. We exploit this rule of allocation through intention to treat (ITT) estimates and a fuzzy regression design (FRD) using both municipal-level and individual-level data.

We document three main findings. First, LPT has a positive electoral impact on the vote share of PT, the political party responsible for implementing the LPT. Second, electoral benefits accrue only several years after the program's introduction. Our results indicate no effect of the LPT on respondents' propensity to vote for the PT in the 2006 presidential elections, the first after the program implementation. Conversely, we find a positive impact of the LPT on the PT's vote support in the presidential elections held in 2010, 2014, and 2018. Even in elections held in 2022, roughly two decades after the program started running, we documented a positive and statistically significant impact of the LPT on the PT's vote share in targeted municipalities. Third, the reason for such lag (and persistence of the observed effect once it appears) lies, to a great extent, in the importance of intermediate policy spillovers. As we show in this paper, the arrival of electricity increased school attendance rate and, thus, the satisfaction with the state provision of education in targeted municipalities. Crucially, our findings indicate that LPT impacted education in rural areas of Brazil by increasing the offer of evening classes for low-income voters previously excluded from the educational system.

Our analysis makes several contributions. By establishing how medium to long-run effects work through spillovers generated by policies such as human capital formation, we contribute to the identification of an important mechanism governing the electoral consequences of major infrastructural initiatives. Informed by previous contributions (e.g., Stokes, 2016), we purposely chose a case study where costs and benefits are not concentrated, the implementation was effective, and parties engaged in effective credit-claiming over time. Given these conditions, spillovers generate significant electoral gains. We also show that policies need not have short-term returns to generate significant electoral payoffs. By implication, our analysis shows that, provided that politicians enjoy sufficiently long time horizons, it is perfectly rational for them to launch such efforts. Establishing this fact sheds new light on the calculus of elites and the political economy of infrastructural development. Under specific conditions, the range of politically effective policies in developing contexts broadens beyond targeted efforts (e.g., De La O, 2015; Diaz-Cayeros et al., 2016; Calvo and Murillo, 2019; Amat and Beramendi, 2020).[1] This is a relevant insight to understand the working of democracy in low and middle-income countries.

Furthermore, our findings contribute to ongoing discussions on the effects of electrification on development. Previous studies have highlighted the impact of electricity provision on economic dimensions (Dinkelman, 2011; Lipscomb et al., 2013; Fetter and Usmani, 2020; Lee et al., 2020), while our research underscores the political consequences of electrification in contexts of vulnerability. In line with some recent studies, our work adds to a growing literature on the economic (Bernard and Torero, 2015; Lenz et al., 2017; Arvate et al., 2018; Lee et al., 2020) and electoral (Ansolabehere and Konisky, 2009; De Bem Lignani et al., 2011; Harding, 2015; Min, 2015; Angulo Amaya et al., 2020; Acemoglu et al., 2021; Boas et al., 2021) implications of implementing large-scale electrification schemes.

Wiring Votes: Electricity and Elections

Under what conditions do infrastructure projects, such as large-scale expansions of electricity produce electoral rewards for incumbents? Extant contributions have paid attention to three important dimensions: the concentration of costs and benefits and the effectiveness in implementation. Once these conditions apply, we argue that an important driver of the electoral implications of infrastructures lies in the policy spillovers they generate. By that, we mean expanding public goods to sub-populations previously excluded due to the lack of infrastructure. This, in turn, has an interesting implication for the time lag to observing electoral returns on infrastructural investments: to the extent that policy spillovers are a significant driver of these returns, we may observe them primarily in the medium to long run. In the rest of this section, we present this logic in detail.

We start from a standard framework of representative democracy. To win elections, elites must

[1] An explicit comparison of the effectiveness of targeted clientelistic policies relative to programmatic infrastructural investments falls outside the scope of this paper.

satisfy voters utility. Assuming a single, over-arching dimension of politics, the utility of a representative voter j for incumbent candidate i is determined by v_i, the perceived competence of incumbent candidate i, as well as the distance between the voter's ideal point, x_j, and the perceived platform of the incumbent candidate, x_i:

$$u_j(x_i, v_i) = v_i – a(x_j – x_i)^2$$

where $a > 0$ scales the relative importance of ideology versus competence for the voter. [2]

We are interested in understanding the role the large scale provision of infrastructures play in voters' decision-making. Voters face a well-known problem in collecting unbiased information about incumbent performance. However, to the extent that voters react to performance indicators on macroeconomic performance, public health, real-estate values, or educational outcomes, this basic framework applies. When policies approximate citizens' preferences, incumbents are rewarded at the margin. This implies that, when keeping constant the relative level of competence of competing candidates constant ($|v_i - v_c|$), there should be electoral returns to improvements in the provision of services voters find of value.

This premise underpins a growing stream of research on the electoral value of public investments in infrastructure and service provision, particularly in low and middle-income countries (Baskaran et al., 2015; Min, 2015). In the absence of encompassing welfare states as tools for coalition building (Esping-Andersen, 2017), incumbents see these investments as a tool to forge sustained exchanges (votes for services) with citizens. Harding (2015), for instance, provides micro-level evidence that Ghana voters hold their governments accountable for the quality of public roads, especially when the attribution of responsibility is clear. Infrastructure politics, as he puts it, makes elections in young

[2]Voter utility for candidate c, the challenger, is determined in the same way:

$$u_j(x_c, v_c) = v_c – a(x_j – x_c)^2$$

democracies something "more than contests in corruption and ethnic loyalties" (p.685).

Analyzing the electoral effects of infrastructures from the perspective of the accountability-responsiveness link requires paying attention to two major dimensions of the policy: the concentration of costs and benefits, and the process of implementation. On the first dimension, if costs are spatially concentrated, the electoral calculus of infrastructures becomes less obvious. Stokes (2016)'s analysis of climate policy illustrates how the excessive concentration of costs undermines political support for interventions with broad public benefits. Ontario voters of localities who saw large wind turbines set-up near, and disrupting, their communities reduced their support for their incumbent in subsequent provincial elections by 10%.[3]

Interestingly, excessive concentration carries similar consequences in the case of benefits as well. If the pool of beneficiaries is relatively narrow, those outside the immediate scope of the intervention perceive themselves as a low priority for the incumbent and become more likely to punish her. In a recent contribution focusing on the electoral implications of education quality initiatives in Brazil, Boas et al. (2021) show voters perceive trade-offs across issue areas. Those who benefit from the policy, see the incumbent as a "good type"; those who do not, value the incumbent negatively and vote accordingly. An important implication follows: the broader the pool of beneficiaries associated with the investment, the higher the expected electoral return.

In addition to the spatial concentration of costs and benefits, delivery matters. A central premise for service provision and infrastructural investments to generate a positive exchange between incumbents and voters is that they actually reach people directly and without much leakage. If the policy is delegated to private partners who capture the implementation process, voters are unlikely to reward the incumbent. Indeed, as illustrated by recent analyses in South Africa (De Bem Lignani et al., 2011), India (Zimmermann, 2020), and Colombia (Angulo Amaya et al., 2020), the relationship between provision and performance may actually reverse in such cases.

[3] Ansolabehere and Konisky (2009) provide evidence of a similar dynamics in the case of power plants.

The extant research shows how each of these dimensions moderates the intuitive expectation that improvements in service provisions lead to better electoral performance. Assuming that costs are manageable and the implementation minimally effective, infrastructure and service provision have the potential to generate substantial electoral returns. Whether that is the case and how, we argue, depends on the policy spillovers on public goods provision, and an efficient credit claim strategy. From this perspective, not all infrastructural investments are created equal.

Executives often invest in infrastructure as a short-term boost in local demand. The world is rich in bridges to nowhere, roads to industrial parks that are unused once the short-term boost on local employment ahead of the election has been served. Absent increasing returns through the expansion of opportunities and services for the community, the electoral returns of such infrastructural expenses are bound to be short-lived. By contrast, insofar as infrastructural investments facilitate the expansion of public goods, three channels are activated:

1. Infrastructures alter economic and social relations at the local level, changing the local production possibility frontier. Voters' aspirations broaden as investment options expand.

2. In parallel, as public goods expand, vulnerability (Bobonis et al., 2022; Frey, 2022) declines among lower income strata.

3. As a result of these two processes, the pool of actual beneficiaries broadens (i.e. concentration declines) with significant welfare effects progressively being attributed to the incumbent. Provided that the spillovers across public goods are large enough, infrastructures carry on a multiplier effect on political rewards.

Consistent with this logic, there is evidence that the expansion of public education played a fundamental role in solidifying social-democratic allegiances in Norway (Acemoglu et al., 2021). The expansion of infrastructures, such as electricity, that facilitate access to education to previously excluded groups can trigger similar dynamics in developing contexts as well.

Importantly, to the extent that it rests on the effective implementation of public services and policy spillovers, the realization of electoral gains is neither smooth nor automatic. In the short term, infrastructure deployment is ripe with frictions, both economic and political. Prior economic relationships linger, vulnerabilities lag, and the process of credit attribution follows only from the effective implementation of the expansion of public goods such as educational opportunities. Crucially, parties need time to materialize the expansion of provision into electoral rewards. Potential political returns reach a meaningful scale only after this occurs. As a result, electoral rewards become more likely in the medium run than in the short run, a window in which transaction costs, unrealized gains, and unclaimed credit coexist.

Our empirical focus will zoom into this particular process in detail. To that end, we select a case where all the initial conditions identified by the extant literature (low concentration of costs and (eventual) benefits on the one hand and effective implementation on the other) apply. Furthermore, our setting allows tracing the specific mechanism of policy spillovers and evaluating the short and medium to long run electoral effects of the newly deployed infrastructures. The case in question is one of the largest and most important efforts to expand electricity in underdeveloped, poorer areas in the global south: the Luz para Todos (LPT) initiative by the Workers' Party (PT) government in Brazil.

The politics of electrification in Brazil

Infrastructure provision: from military governments to democracy

In 1970, only 73 out of 3,952 Brazilian municipalities provided access to electric power for at least 80% of the population (Arretche, 2018). The developmental state adopted by the authoritarian regime

(1964-1985) mainly increased provision. However, such expansion benefited wealthier municipalities. By the end of the military government, there was a clear divide between more prosperous regions (in the South and Southeast) and poorer ones (North and Northeast). While the former regions moved faster toward universal access, the latter presented low coverage rates. Indeed, household infrastructure provision also occurred at an unequal cross-region pace regarding other critical areas such as water supply, sanitation, and sewage collection. Developmental state policies partly drove this path. Access was provided by state-owned companies whose investment rates were primarily driven by returns obtained through household tariffs.

In this context, improving access to infrastructure services could be an optimal strategy for parties to reap electoral rewards under democracy. The Brazilian Social Democratic Party (Partido da Social Democracia Brasileira, PSDB) and the Workers' Party (Partido dos Trabalhadores, PT) were the two major competitors in Brazilian presidential elections after democratization in 1985 (Samuels and Zucco, 2018). Both PT and PSDB shared a common concern regarding the need to provide massive infrastructure services, although employing divergent *modus operandi* (Arretche et al., 2020).

Between 1995 and 2002, the PSDB's government stimulated the infrastructure provision through private investments without the public sector's direct participation. The expansion of electrification towards universal coverage occurred mainly in the more affluent South, Southeast, and Central-west regions. The coverage expansion in the poorer North and the Northeast regions happened at a much slower pace. By contrast, the left-leaning PT (2003-2016) has adopted a Keynesian strategy that accelerates economic growth, occupation for low-skilled workers, and social inclusion. Since the more impoverished population is spatially concentrated in the Northeast region and this region shelters nearly 30% of Brazilians, prioritizing the poor in the provision of infrastructure would necessarily mean increasing the state's presence in previously neglected areas. Direct state provision, along with subsidies for the poor, was the PT's chosen strategy to accomplish that goal. Therefore, social fees for infrastructure services became central components of its plan to reach the vulnerable population.

The Luz para Todos (LPT): A program to electrify Brazil's rural areas

The Luz para Todos (LPT) program was officially launched in 2003, the first year of Luiz Inácio Lula da Silva's (PT) presidential administration.[4] Its goal was to end the exclusion from electricity in the most impoverished areas of Brazil. The program was coordinated by the Ministry of Mines and Energy and executed by state-level electric power companies and rural electrification cooperatives in 24 of the 26 Brazilian states. The federal government invested more than 15 billion reais (approximately 3 billion dollars). From 2004 to 2015,[5] more than 3.2 million households, health facilities, and public schools were connected to the electrical grid through the LPT program. The LPT program was funded by two energy funds – The Energetic Development Account (CDE) and the Reversion Global Reserve (RGR). The LPT beneficiaries were entitled to the Electric Power Social Tariff, a discount on electric power granted to residential clients registered in the Federal Government's Social Programs database (Cadastro Único). In some cases, the deduction could reach 100%, provided that household consumption was limited to 50 kWh/month (Slough et al., 2015).

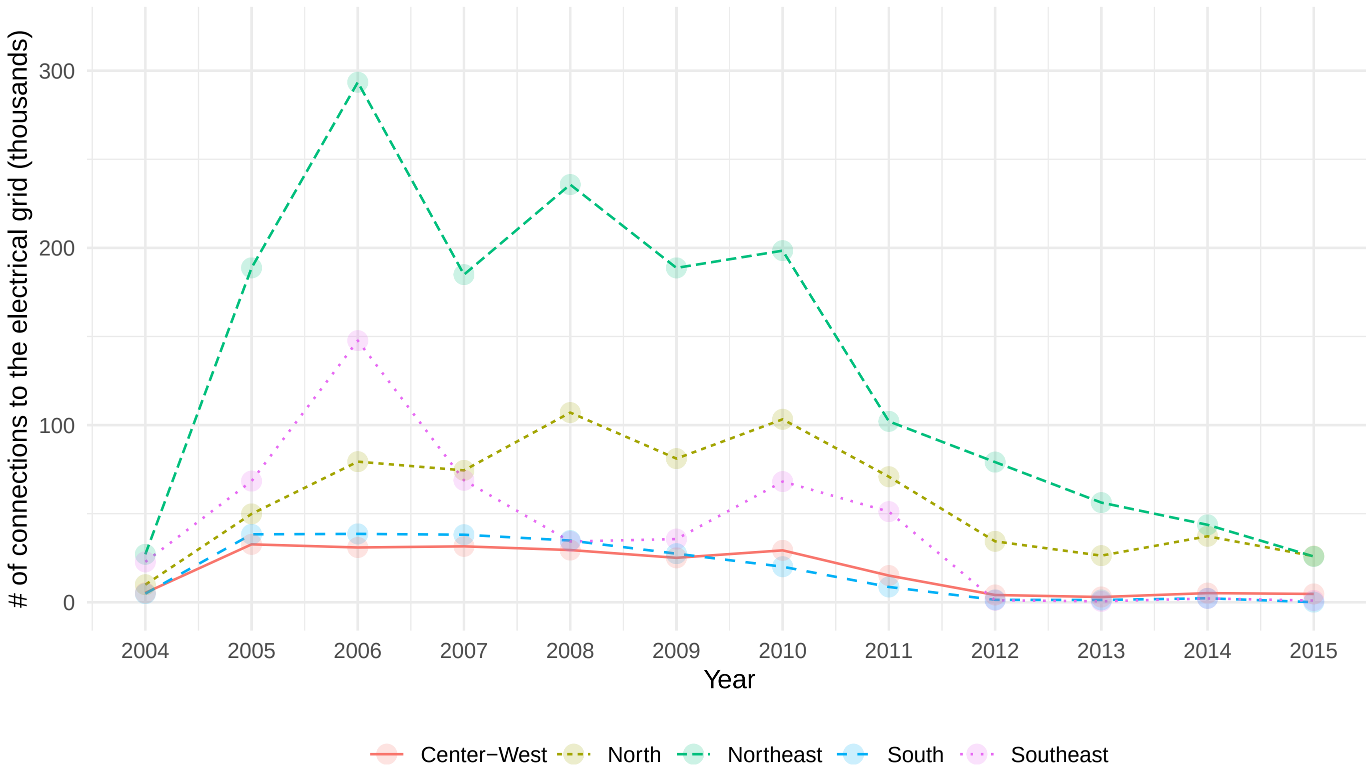
As Figure 1 shows, peaks of new connections occurred in election years (2006 and 2010 general elections and 2008 local ones). This evidence points toward an electoral cycle inducement mechanism similar to that found by Baskaran et al. (2015) for Indian state-level elections. Yet, unlike the Indian case, we found no evidence that municipalities benefited were selected by the electoral preferences of their population. Neither being a contested constituency or a safe district appear to have been a criteria for the provision of electricity in Brazil's rural areas. Instead, LPT seems to have been an

[4]The electrification program was officially launched in November 2003 trough the Programa Nacional de Universalização do Acesso e Uso da Energia Elétrica (Decree-Law no 4.873, de 11 de Novembro de 2003).

[5]Formally, the program is still running, but operating within a much lower capacity since 2015, the last year for which there is publicly available information on its implementation.

effective programmatic policy.

Figure 1: Number of new households, health facilities, and schools connected to the electrical grid through the LPT program (2004-2015)



Note: Compiled by the authors with data from the Brazilian Ministry of Mines and Energy (MME, 2018). The y-axis is represented in thousands. The information depicted in this figure is not available for the period after 2015.

Online appendix A shows electrification coverage in Brazilian municipalities before implementing LPT. In 2000, access to electricity was practically universal in the Southeast. In many poor municipalities, however, not even half of the population was covered. In the Northeast, the Brazilian region with the lowest electricity coverage levels in 2000, even the most deprived rural areas such as Sertão Baiano (in the state of Bahia) and Cariri (in Ceará) had a higher level of electricity coverage in 2010. On average, 86.5 percent of Brazilians were living in a household with electricity in 2000. This percentage increased to 97.2 percent in 2010, after the implementation of LPT.

LPT did not require the construction of new electric power plants and dams that could have raised the opposition of nearby voters, as documented by Ansolabehere and Konisky (2009) for USA voters. Instead, the main LPT achievement has been to reach the poorest in rural areas. It was less a matter of power generation and more of electricity distribution. Yet, LPT did entail redistribution. The costs of the extension of electric networks and the Social Tariff were subsidized by the tariffs paid by non-beneficiaries. These costs then were diffuse and less visible. Hence, although infrastructure projects may be politically costly, the design of LPT by extending electric power already available and diffusing the costs of the subsidies benefiting the rural poor could enhance the net political benefits that incumbents could obtain.

Evidence shows that PT worked efficiently to remind voters that LPT could be attributed to the federal government and to the Worker's Party. In the 2010 presidential campaign the incumbent President Lula not only linked the "end of darkness age" and its benefits to his own government but also credited candidate Dilma Rousseff, his former Ministry of Energy, as the author of the LPT. In fact, a proposal to make access to electricity universal was in the Worker's Party platform since the 1989 elections, the first presidential elections PT had a candidate. Yet, once implemented LPT turned out to be a showcase of inclusive policies by PT. An explicit strategy to get credit from the program and link its authorship to PT candidates was intensely highlighted in electoral campaigns.

In sum, the concentration of benefits and the diffusion of costs, the programmatic implementation of the program, and policy design effective in reaching the targeted poor allowed PT to claim credit on LPT.

Empirical strategy

A municipality was eligible for the LPT program if fewer than 85% of households had access to electricity, according to the 2000 Brazilian census. The decision to define this threshold was mainly technical and proposed by the Brazilian Ministry of Mines and Energy staff to reflect households' average electricity coverage in 2000.[6] Indeed, Table 2 shows that individuals benefiting from the

[6] Information acquired from an online interview conducted in September 2018 with a former Executive-Secretary for the Ministry of Mines and Energy.

LPT are more likely to be placed in municipalities below the 85% threshold. That is, the incidence of LPT beneficiaries tends to be higher on the left-hand side of the 85% cutoff, where the targeted municipalities are located. Furthermore, low-income individuals benefit the most from the program, corroborating our assessment that the program was successful in terms of target. This results from the fact that the choice of this threshold lent priority to communities with low levels of human development, a higher concentration of marginalized racial groups (e.g., indigenous populations), and/or the presence of traditional territories of descendants of enslaved peoples (Quilombos). Although communities with such characteristics were more likely to be located in municipalities below the 85% threshold, it is known that some targeted areas were located in municipalities above this threshold.

Municipal-level assessment

To account for this imperfect compliance, our baseline results rely on intention-to-treat (ITT) estimates that calculate the average effect of the treatment assignment. In our setting, this means calculating the difference between the average outcome of the targeted municipalities and the non-targeted ones, regardless of whether they actually participated in the program. This can be achieved by comparing municipalities below the 85% cutoff, therefore, potentially targeted by the program with those above this same threshold, i.e., municipalities less likely to be targeted by the LPT.

Individual-level assessment

The electoral consequences of policy ultimately translate into decisions made at the individual level. These include the decision to comply with compulsory voting laws and, more important for our concerns, the party choice. Accordingly, the use of aggregate data carries the risk of incurring an ecological fallacy - i.e., biases that may occur when an observed relationship between aggregated variables differs from the true causal association established at the individual level (King et al., 2004). In this paper, we address this concern using survey data from the Estudo Eleitoral Brasileiro (ESEB, 2010). ESEB's nationally representative sample comprises 2,000 individuals over 16 (distributed among all Brazilian states). The questionnaire contains information on whether respondents or someone in their household is an LPT beneficiary and identifies each municipality where the survey was conducted. This information allows us to merge survey data with information from the 2000 Brazilian census, which informs the percentage of households with electricity in 2000. We consider respondents living in municipalities below the 85% cutoff as potentially benefiting from the LPT and those above the 85% cutoff as less likely to be exposed to the program.

ESEB's data allows us to assess the internal validity of our findings using alternative identification strategies. The first one replicates our baseline ITT models using individual-level data. The second one relies on the quasi-experimental conditions of the LPT implementation to estimate its Local average treatment effect (LATE)[7] on our outcomes of interest. For this purpose, we employ the percentage of households with electricity in 2000 as our running variable, thus allowing for predicting the probability of being an LPT beneficiary in 2010. This as-if random treatment assignment is used as an instrument for treatment status as in a typical fuzzy-regression discontinuity design (FRD).

This is a credible instrument for several reasons. First, municipalities were randomly selected to integrate the ESEB's sample. Therefore, whether or not a respondent lives in a municipality around the 85% cutoff is defined by chance. Second, while the distance from the cutoff is the main predictor for the treatment allocation, it does not directly affect people's propensity to vote for the PT in presidential elections. That is the case because the choice (based on technical arguments) of the 85% threshold to determine targeted municipalities is not directly correlated with unobservable factors that could affect our outcomes of interest. Finally, as we show later in this paper, our instrument is

[7] We are unable to implement the same identification strategy employing municipal-level data because information detailing the uptake of the program within municipalities is not publicly available, so it is not possible to estimate the first-stage in standard two-stage models.

always statistically significant in the first stage of our FRD estimates. We interpret this finding as reassuring evidence of this empirical strategy's validity in our setting.

Data

Municipal-level data

We use data[8] from Brazil's Electoral Court (Tribunal Superior Eleitoral, TSE) to create the main electoral outcome (dependent) variable used in this study: the share of votes for the PT candidates in the presidential elections. Using the data from TSE, we calculated PT's vote shares from six consecutive presidential contests: the elections held just before (2002), the election held just after (2006), as well as those held several years after (2010-2022) the LPT started running in the Brazilian rural areas. To pin down potential mechanisms of our main electoral results, we use the 2010 Brazilian census data to assess the impact of LPT on school attendance in targeted municipalities. Census data reports the school attendance rate for different age cohorts. In this paper, we use as outcomes all age cohorts (0-3, 4-6, 6-10, 11-14, 15-17, and 18-24) reported in the 2010 Brazilian census.

Individual-level data

In the survey fielded in 2010, ESEB's respondents were asked how they voted in the first round of the 2006 and 2010 presidential elections[9]. We code these variables as dummies, and in both cases, 1 stands for individuals who voted for PT in the presidential election. In contrast, category 0 includes the remaining options but excludes respondents who declared, "I do not remember for who I voted. This procedure allows taking into account in our analysis only individuals who explicitly recalled their vote option, thus rendering a more restrictive (and perhaps conservative) categorization of this

[8]Online Appendix B provides the descriptive statistics for all variables used in this paper.

[9]Unfortunately, that was the only ESEB's round that asked whether respondents were beneficiaries of the LPT program. For this reason, when using individual-level data, we restricted our analysis to two years, 2006 and 2010, respectively.

outcome variable. Furthermore, ESEB's respondents were asked about their satisfaction with the quality of the provision of education at the primary/secondary and tertiary levels. We coded both variables as 1 if the respondent informed high levels of satisfaction and 0 otherwise. We use these outcomes to test further the impact of the LPT on the education supply in rural areas. The reasoning here is straightforward: If the LPT increased school attendance in rural areas, one should expect individuals living in targeted areas to report a higher level of satisfaction with public education provision.

Results

Municipal-level results

We begin with the discussion of our ITT estimates using municipal-level data. Table 1 summarizes the results of our Ordinary Least Squares (OLS) models with standard errors clustered at the state level. As expected, the coefficient that accounts for the 2002 presidential elections is not significant at the conventional levels of statistical significance (Panel A). As the LPT started running in 2004, one should not be surprised by this result. Furthermore, we find no effect of the LPT on vote shares for the PT in the 2006 presidential elections. It could be argued this result is mechanically induced by how the electrification program was carried out in rural areas. For example, suppose the implementation was inefficient in the first years and, as a consequence, the number of new households connected to the electrical grid was limited. In this case, there is no reason to expect voters to reward PT already in 2006. However, as shown in Figure 1, when presidential elections took place in 2006, the LPT had already delivered electricity to millions of households and public facilities in Brazil's rural areas.

In line with our theoretical expectations, the electoral impact of the LPT manifested for the first time in 2010, when we documented a 3.8pp increase in the share of votes for the PT. This positive and statistically significant impact holds consistent for all presidential elections held since then. In the 2014 and 2018 elections, the share of votes for the PT was 3.6pp and 3.5pp higher in targeted municipalities. Crucially, even in the presidential elections held in 2022, roughly two decades after the LPT started running, vote support for the PT was 1.8pp higher in municipalities targeted by the LPT. Corroborating our theoretical expectations, the LPT electoral effects appear in the medium to the long run and persist for several years.

Table 1: The impact of the LPT on the PT's vote share in the presidential elections (2002-2022) and the school attendance rate for several age-cohorts (2010)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Elections (Panel A)	2002	2006	2010	2014	2018	2022
	-1.108	.4162	3.802***	3.592***	3.553**	1.808**
	(1.633)	(1.257)	(.9030)	(1.313)	(1.646)	(.8326)
Obs.	5,560	5,560	5,560	5,560	5,560	5,560
R^2	0.155	0.647	0.610	0.699	0.782	0.775
Age-cohorts (Panel B)	0-3	4-6	6-10	11-14	15-17	18-24
	1.100	-.4961	-.5211	.2307	1.537***	1.085*
	(.958)	(.7679)	(.3441)	(.1874)	(.4321)	(.6118)
Obs.	5,560	5,560	5,560	5,560	5,560	5,560
R^2	0.256	0.364	0.228	0.187	0.123	0.143

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Compiled by authors with data from the Brazilian Institute of Geography and Statistics (IBGE, 2000 and 2010) and the Brazil's Electoral Court (Tribunal Superior Eleitoral, TSE). The unit of analysis is the municipality. We run intention to treat estimates with the following pretreatment (2000) controls: life expectancy, fertility rate, mortality rate, average years of schooling, income inequality, income per capita, percentage of rural population, and a dummy variable accounting for the region where the municipality is located. We cluster robust standard errors (in parentheses) at the state-level. In Panel A, the outcome variable is the share of votes for the PT candidate in each presidential elections held between 2002 and 2022. In Panel B, the outcome variable is the school attendance rate for all age cohorts (0-3, 4-6, 6-10, 11-14, 15-17, and 18-24) reported in the 2010 Brazilian census.

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Individual-level results

To address the concern of ecological fallacy, we replicate our ITT models using individual-level data. As explained before, we restricted our analysis to two presidential elections (2006 and 2010) due to the limitations imposed by data availability. In the face of this lack of data, we cannot replicate our placebo test using the 2002 elections as a reference. Also, we are unable to test for the impact of the LPT on individuals' propensity to vote for the PT in elections held after 2010.

As in models with aggregate-level data, Table 2 shows no evidence that the LPT has impacted voting behavior in the elections held just after (2006) the arrival of the electrification program. Furthermore, and perhaps crucially for testing the argument presented in this paper, we documented a positive and statistically significant impact on voters' propensity to vote for the PT in 2010, six years after the LPT started running. Furthermore, Table 2 reports our ITT estimates with a set of individual-level covariates[10] and disaggregated results by different income groups. Our findings are reassuring: for the 2006 presidential elections, we found null effects whether or not respondents classified themselves in the questionnaire as poor[11]. Also, the positive impact we find for the 2010 presidential elections is mostly driven by low-income respondents, i.e., voters that we know, based on the program's design, were more likely to benefit from the LPT.

Table 2: The impact of the LPT on the PT's vote support by income groups

	Benefiting from the LPT			Voted PT in 2006			Voted PT in 2010		
	Full sample	Poor	Not poor	Full sample	Poor	Not poor	Full sample	Poor	Not poor
	.2120***	.2201***	.1437	-.0284	-.0169	-.0719	.1213***	.1252***	.0899
	(.0314)	(.0271)	(.0818)	(.0336)	(.0359)	(.0748)	(.0329)	(.0351)	(.0699)
Obs.	1,964	1,621	384	1,570	1,320	287	1,811	1,491	358
R^2	0.2215	0.236	0.236	0.137	0.132	0.2554	0.201	0.211	0.226

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
The unit of analysis is the survey (ESEB) respondent. We run intention to treat estimates with the following controls: age, sex, schooling, marital status, race, religion, and a dummy variable that indicates whether the respondent benefits from the Bolsa Família. We cluster robust standard errors (in parentheses) at the municipal level. The first outcome variable (Benefiting from the LPT) is a dummy variable that indicates whether or not the respondent was an LPT beneficiary in 2010. The other two outcome variables are dummy variables that indicate whether respondents voted for the PT in the first round of the 2006 and 2010 Brazilian presidential elections, respectively. In our models, we treated as "Poor" those individuals below the median income according to their income self-declaration in the survey.

In a second and complementary step, we use the ESEB survey data to run fuzzy regression discontinuity (FRD) estimates. Table 3 reports our local linear[12] estimates using optimal bandwidth

[10]As the inclusion of post-treatment covariates might introduce bias in one's estimates (Montgomery et al., 2018), we run baseline models without controls. As shown in Online Appendix C, our results hold consistent and robust in the absence of individual-level covariates.

[11]In our models, we treated as "Poor" those individuals below the median income according to their income self-declaration in the survey.

[12]We use a local linear fit because high-order polynomials (i.e., cubic and quartic order fits) are likely to produce

selection (Calonico et al., 2020). Panel A informs our first-stage estimates, while panel B summarizes reduced-form estimates, i.e., the local average treatment effect (LATE).

Table 3: The impact of the LPT on the PT's vote support and the perception of the quality of education

	Model 1	Model 2	Model 3	Model 4
First-stage estimates (Panel A)				
	-.172**	-.151*	-.1952**	-.1476**
	(.076)	(.082)	(.098)	(.069)
Outcomes	Voted PT in 2006	Voted PT in 2010	Primary education	Tertiary education
LATE estimates (Panel B)	1.00	1.82*	3.56*	4.04*
	(1.06)	(.981)	(1.53)	(2.13)
BW est (h)	8.43	8.81	4.70	4.42
BW bias (b)	11.8	14.2	7.52	8.98
Order est. (p)	1	1	1	1
Kernel	Triangular	Triangular	Triangular	Triangular
Obs. left of cutoff	228	266	273	251
Eff. obs. left of cutoff	105	132	96	73
N. of clusters left of cutoff	22	22	15	19
Obs. right of cutoff	1348	1553	1644	1603
Eff. obs. right of cutoff	129	151	59	57
N. of clusters right of cutoff	21	64	7	7

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
The unit of analysis is the survey (ESEB) respondent. We run RD local linear estimates using Calonico et al. (2020) optimal bandwidth selection. We cluster robust standard errors (in parentheses) at the municipal level. Employing the percentage of households with electricity in 2000 as our running variable, we can thus predict the probability of being an LPT beneficiary in 2010. This as-if random treatment assignment is used as an instrument for treatment status in our first-stage estimates. The outcome variable in the first stage is a dummy variable that indicates whether the respondent was an LPT beneficiary in 2010 (LPT benef.). The outcome variables in reduced form estimates are dummy variables that indicate whether respondents voted for the PT in the first round of 2006 (Model 1) and 2010 (Model 2) Brazilian presidential elections and whether the respondent informed high levels of satisfaction with the quality of public provision of primary/secondary (Model 3) and tertiary education (Model 4).

Our first-stage estimates show that, as the share of households with electricity in a given municipality in 2000 increases, the lesser the probability of being an LPT beneficiary in 2010. In other words, municipalities with fewer than 85% of households with electricity in 2000 had a higher concentration of survey respondents benefiting from the LPT in 2010, as one should expect. Online Appendix E confirms this pattern graphically: the probability of being an LPT beneficiary increases discontinuously at the left-hand side of the threshold. This collection of findings provides further evidence that the LPT was well-targeted (and reached vulnerable populations living in rural areas),

noisy estimates with poor statistical properties and narrow confidence intervals (Gelman and Imbens, 2018). In Online Appendix D, we replicate our results using a quadratic polynomial fit. As shown, our main findings hold consistent in this alternative specification.

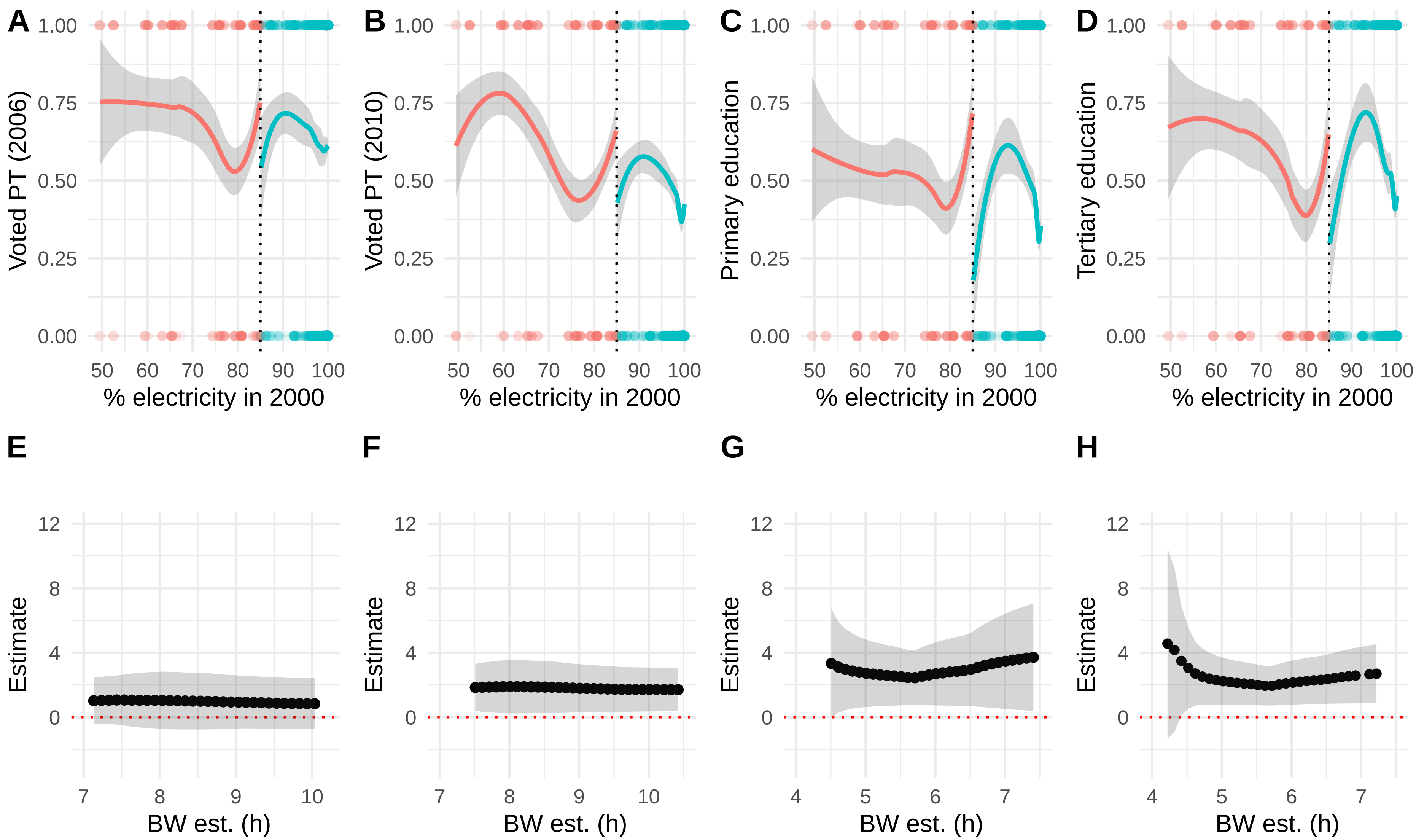
thus indicating that it has indeed enhanced access to electricity in targeted municipalities.

Reduced-form estimates in Table 3 show the effect of the LPT on vote support for the PT in the Brazilian presidential elections. Once more, due to the limitations imposed by the ESEB's questionnaire, we exploit the impact of the LPT on the results of elections held in 2006 and 2010. In line with our theoretical expectations, we find no effect of the LPT on respondents' propensity to vote for the PT in the 2006 presidential elections. Conversely, we documented a positive and statistically significant (CI of 95%) increment in the support for PT in 2010. On average, among voters living below the 85% cutoff (targeted municipalities), the vote share for the PT is around two percentage points higher than that of voters living in municipalities above the LPT threshold. Figure 2 (Panel B) reinforces our interpretation that the PT has boosted its electoral capital in rural areas as a consequence of the arrival of electricity. On average, the incidence of respondents who voted for PT in 2010 is substantively higher on the left-hand side of the cutoff.

Selecting the bandwidth around the cutoff in which we estimate the LATE is a crucial step in regression discontinuity designs, as the results and conclusions are typically sensitive to this choice. We rerun our estimates using several new bandwidth values slightly smaller and larger than the values selected by the Mean Squared Error (MSE)-optimal bandwidth, as recommended by Cattaneo et al. (2018). As we show in Figure 2 (Panels E and F), our main electoral findings are consistent and hold robust across several bandwidth choices.

In sum, aggregated and individual-level data estimates firmly support our claim that the party responsible for adopting the LPT has been consistently rewarded by voters in rural areas targeted by the electrification program. The available evidence corroborates our argument that infrastructure policies are more likely to generate electoral returns when the policy provision entails spillover effects through other policies.

Figure 2: Graphical representation of the impact of the LPT on several outcomes (Panels A-D) + BW est. (h) sensitiveness (Panels E-H) of FRD estimates reported in Table 3



Note: The unit of analysis is the survey (ESEB) respondent (N = 1,972). The outcome variables in panels A and B indicate whether the respondent voted for the PT in the first round of the 2006 and 2010 Brazilian presidential elections, respectively. The outcome variables in panels C and D indicate whether the respondent informed high levels of satisfaction with the quality of public provision of primary/secondary and tertiary education, respectively. Panels E-H report the results for each one of these outcomes using several new bandwidth values slightly smaller and larger than the values selected by the Mean Squared Error (MSE)-optimal bandwidth (Calonico et al., 2020).

Robustness

Our findings rely on the assumption that the LPT was implemented under quasi-experimental rules. In other words, it depends on the validity of our claim that the choice of the 85% threshold can be interpreted as exogenous. While this assumption cannot be directly tested, we assess its plausibility using several strategies.

First, we use data from the 2000 Brazilian census to test for pretreatment differences in municipalities around this threshold. As shown in Online Appendix F, we fail to reject the null hypothesis of continuity for the observable municipal-level covariates. Results from a set of socioeconomic variables show that, around the 85% threshold, municipalities were similar before the program started running in 2004. We find similar and consistent results using electoral outcomes with official data from Brazil's Electoral Court (TSE). On average, there is no indication of disparities regarding political preferences, voting behavior, or party dominance in municipalities around the 85% threshold.

Second, we test for selection around the 85% threshold. In our setting, self-selection is highly unlikely because the main rule employed to allocate the program was based on the Brazilian census held in 2000. Hence, local politicians (i.e., mayors and members of the local council) could not have anticipated under which rules the program would be allocated when the LPT started running in 2004. Still, we run a manipulation test using a polynomial density estimation to address this concern. The idea behind the manipulation test developed by Cattaneo et al. (2018) is that the number of targeted observations just below the cutoff should be approximately similar to the number of non-targeted units just above it. Online Appendix G provides a visual representation of the continuity test approach. The test exhibits the actual density estimate with a shaded 95% confidence interval of the running variable (i.e., the percentage of households with electricity in 2000). The density estimates for targeted and non-targeted municipalities at the cutoff are close, and the confidence intervals overlap. The value of the statistical test used is 1.453, and the associated p-value is 0.146. These values indicate that, under the continuity-based approach, we find no evidence of self-selection (manipulation) close to the 85% threshold.

Third, we test for the possibility of partisan-biased implementation of the LPT. Several studies have shown that municipalities in which the mayor belongs to the same party as the Brazilian president are more likely to receive discretionary infrastructure transfers from the federal government (Brollo and Nannicini, 2012; Litschig, 2012; Bueno, 2017). If this logic also applies to our setting, the assumption of exogeneity of the 85% threshold is potentially violated. To address this concern, we test whether municipalities governed by PT disproportionately benefited from the LPT. As reported in Online Appendix H, there is no indication that municipalities that elected PT mayors received a higher concentration of investments in electrification between 2005 and 2015, the last year for which the data is available.

Fourth, and specifically for the case of FRD estimates employing individual-level data, the robustness of our findings rests on the assumption that the distribution of individual characteristics around the 85% cutoff is balanced. For example, one could argue that our results could be explained by a higher incidence of *Petistas* (voters identified with the Worker's Party, PT) at the left-hand side of the cutoff or driven by higher levels of political participation in targeted areas. To address the concern of unbalance around the 85% threshold, we implemented a continuity-based formal analysis to compare respondents' characteristics just below and just above the cutoff. The idea behind this test is simple: Close to the cutoff, respondents should be similar in their characteristics. Following Cattaneo et al. (2018), we assume that if the treated units are similar in observable characteristics, there should be no systematic differences between units with similar values of our running variable. As suggested by reduced form estimates in Online Appendix I, there is only weak evidence of discontinuity at the cutoff for the respondents' sociodemographic and political attributes available in the ESEB. On average, respondents at both sides of the cutoff are similar in age, gender, race, schooling, religion, support for democracy, and party preference, among other individual-level characteristics. Hence, our results are unlikely to be driven by individual-level post-treatment covariates unbalance.

Mechanisms

As outlined in our theoretical framework, we expect electrification programs to generate spillovers with positive effects on the quality of life of populations living in targeted areas. If a given policy prompts positive externalities, one should expect sustained electoral rewards for the incumbent. Of course, such externalities can manifest in various human activities and economic sectors. For the sake of simplicity and empirical tractability, our analysis focuses on one of the main channels through which electrification programs can increase people's well-being in the medium to long run: education.

Brazil has one of the highest school withdrawal rates in the world (Swaffield and Thomas, 2019). Every year, around 13% of students leave school without completing the basic levels of education (Silva Filho and de Lima Araújo, 2017). Typically, the job market competes with schools and prevents low-income families from investing in education. For that reason, policies that can decrease the costs and risks associated with attending school are key to promoting development (Banerjee et al., 2011).

Policy initiatives aiming to offer evening classes can compensate for distortions created by inefficient decisions of time allocation because individuals from poor families can complete school while keeping their positions in the job market. In Brazil, public schools have long offered evening classes for young adults. Yet, due to the lack of electricity, populations in rural areas are typically prevented from benefiting from this public policy. To mitigate this lack of access to formal education, connecting public schools to the electrical grid was one of the priorities of the Brazilian federal government when designing the LPT in the early 2000s (Camargo et al., 2008; Slough et al., 2015). Notably, as one of its main goals, the creation of LPT sought to increase the supply of evening classes for low-skill workers in informal occupations living in rural areas (Morais and Costa, 2010; da Silveira Bezerra et al., 2017).

Municipal-level results

To assess the impact of LPT on the education supply in targeted municipalities, as previously reported in this paper, we run ITT models employing granular data on school attendance rates. Crucially, the 2010 Brazilian census data reports the school attendance rate for several age cohorts (0-3, 4-6, 6-10, 11-14, 15-17, and 18-24). As the law legally prohibits those below 16 years old from taking evening classes, one can disentangle the effect of LPT by checking whether the program impacted school attendance only for those directly affected by the increase in the supply of evening classes, i.e., the categories comprising young adults between 15 and 17 years old, and another one composed of adults older than 18 years.

As shown in Table 1 (Panel B), our estimates using the placebo age-cohort outcomes are never statistically significant at the conventional levels, meaning there is no evidence that LPT has enhanced the education supply for children and teenagers, as one should expect in our setting. By contrast, we find evidence that the arrival of electricity in rural areas of targeted municipalities prompted higher school attendance among those age cohorts affected by the LPT. For the cohort from 15 to 17, we find an increment of 1.5pp in targeted municipalities. This category also comprises teenagers aged 15, the ones typically attending regular classes. Therefore, from a measurement point of view, our estimates using this category are less accurate. Even in this scenario, we find a positive and robust effect of the LPT on school attendance. Perhaps less surprisingly, our estimate is positive and statistically significant for the age cohort fully affected by the arrival of electricity in rural areas. For those ranging from 18 to 25 years old, the school attendance rate was one percentage point higher in 2010, on average.

Individual-level results

As our municipal-level evidence reveals, the LPT indeed increased the supply of education for vulnerable populations in targeted municipalities. Yet, as for the electoral outcomes, there is also the risk that inference from aggregate-level data might not represent individuals' behavior and perceptions. In this case, a limitation to addressing the concern of ecological fallacy is that the ESEB survey does not provide clear-cut school attendance measures. As an alternative, we use two other questions in the same survey to assess how respondents in targeted municipalities perceive the quality of education provision.

While this strategy only indirectly assesses the impact of the LPT on the education supply, it allows for addressing whether respondents in targeted municipalities experienced an improvement in the quality of education as a consequence of the arrival of the electricity. A second advantage is that respondents were asked about their perceptions of the quality of education provision concerning primary/secondary and tertiary education levels. The reason one should expect a better assessment of provision for primary/secondary levels of education in targeted municipalities is simple: those are the steps of schooling more directly affected by the offer of evening classes. However, and often less discussed, the arrival of electricity in rural areas potentially affected the supply of tertiary education. While most elite universities are located in urban areas, the Brazilian federal government, in partnership with subnational authorities, offers tertiary education training in rural areas.

A remarkable example is the Universidade Aberta do Brasil (UAB) program, an initiative running since 2006 aiming to recruit and educate local populations to serve as teachers in schools in the countryside.[13] Importantly, most of UAB's classes and training activities are held online or, when in

[13]Currently, 61% of professionals teaching at primary and secondary levels in Brazil are former participants of the

person, at public schools, typically after the workday and/or over the weekends (Mendonça et al., 2019). For this reason, the arrival of electricity in rural areas had a direct impact on the opportunities to complete higher levels of schooling. Hence, one should observe ESEB's respondents in targeted municipalities expressing a better assessment of the quality of tertiary education.

As shown in Figure 2 (Panel C), the level of satisfaction with the state provision of primary/secondary education changes discontinuously and tends to be higher on the left-hand side of the cutoff, where the targeted municipalities are located. Figure 2 (Panel D) informs a similar pattern: On average, the incidence of respondents who depict a high level of satisfaction with the provision of tertiary education was substantively higher among those respondents living below the 85% threshold. Our point estimates corroborate our graphical findings. Respondents living in municipalities below the cutoff were more likely to depict a high level of satisfaction with primary/secondary education in 2010. On average, the share of respondents expressing that level of satisfaction was three percentage points higher among respondents below the 85% threshold (see Table 3). We find consistent and similar results when using satisfaction with the tertiary level of education as an outcome. As reported in Figure 2 (Panels G and H), these findings appear substantive and statistically significant across different bandwidth choices.

Estimates using municipal-level data suggest that the LPT enhanced school attendance in targeted municipalities. Additional evidence from survey data reveals that individuals living in targeted municipalities are more likely to express positive evaluations of the quality of education. We interpret this set of findings as evidence supporting our claim that LPT prompted development in rural areas of Brazil. While we focus on education in this paper, complementarities associated with electrification schemes spill over to multiple other dimensions, such as health, crime, and the environment. The consequences of such complementarities, in many cases, become apparent several years later, thus explaining the long-term nature of the observed electoral returns.

UAB's program (Pimenta et al., 2019).

Conclusion

This paper has shown that, under certain conditions, incumbents reap political rewards in the medium to long run. As established by prior contributions, these conditions involve the concentration of cost and benefits and effective implementation. In addition, we have argued and shown that policy spillovers play a central role in the process. When investments facilitate the expansion of policies to a broader constituency of potential supporters, incumbents claim credit effectively, and political returns accrue. Our evidence on the political consequences of a large-scale electrification scheme implemented in Brazil and the mechanisms driving them traces this process sequentially. Our strategy causally identifies the direction and size of the effects. We discuss several implications that suggest the potential benefits of additional research efforts.

The first one concerns the working of democracy and the importance of time horizons. Democracy institutionalizes voters' myopia. This is a critical problem for addressing climate change, building capacity, and any effort to improve the polities' institutional fundamentals. Our paper has shown how and when one such effort yields a return. The next step is to study comparatively the marginal returns of this strategy relative to other, more common, and more targeted mobilization strategies in developing countries and, more importantly, the interaction between these two ways of engaging voters.

To the extent that successful investments alter the playing field and limit vulnerability, they also undermine the effectiveness of commonly used clientelistic strategies. In the context of Brazil, it would appear that infrastructural developments and clientelism are indeed substitutes. As advanced by Calvo and Murillo (2019), electoral gains from clientelistic policies are conditional on the existence of territorial-based networks of brokers that allows for incumbents to target vulnerable constituencies. The PT did not have such networks in Brazil's rural areas in early 2000. Crucially, this political party was challenging well-established right-wing parties with consolidated networks for brokerage (Alves and Hunter, 2017). Under such circumstances, delivering benefits and services under programmatic rules was the optimal electoral strategy to sway the poor. Whether this is a generalizable conclusion remains an open question.

In addition, politicians face an interesting strategic trade-off when combining approaches in different contexts. If electoral returns accrue in the long run, incumbents must be able to survive the transition period. Paradoxically, highly competitive environments may undermine investments as the short-term transition costs of moving from clientelism to more programmatic strategies may prove fatal. Theorizing this dilemma across policy realms and contexts is a potentially fruitful area of scholarly effort.

Furthermore, much work is needed to understand the implications of major infrastructural investments for party organization and the coordination of electoral competition in multi-level systems. LPT had heterogeneous effects across parties within levels of government and across levels within party labels (local versus federal). This suggests strategic adaptation by parties to the new political markets. Understanding the logic of this adaptation within and across democracies is a natural next step in this research agenda.

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The electoral effects of large-scale infrastructure policies

Evidence from a rural electrification scheme in Brazil

Online appendix*

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*Replication files are available in the JOP Dataverse(https://dataverse.harvard.edu/dataverse/jop). The empirical analysis has been successfully replicated by the JOP replication analyst.

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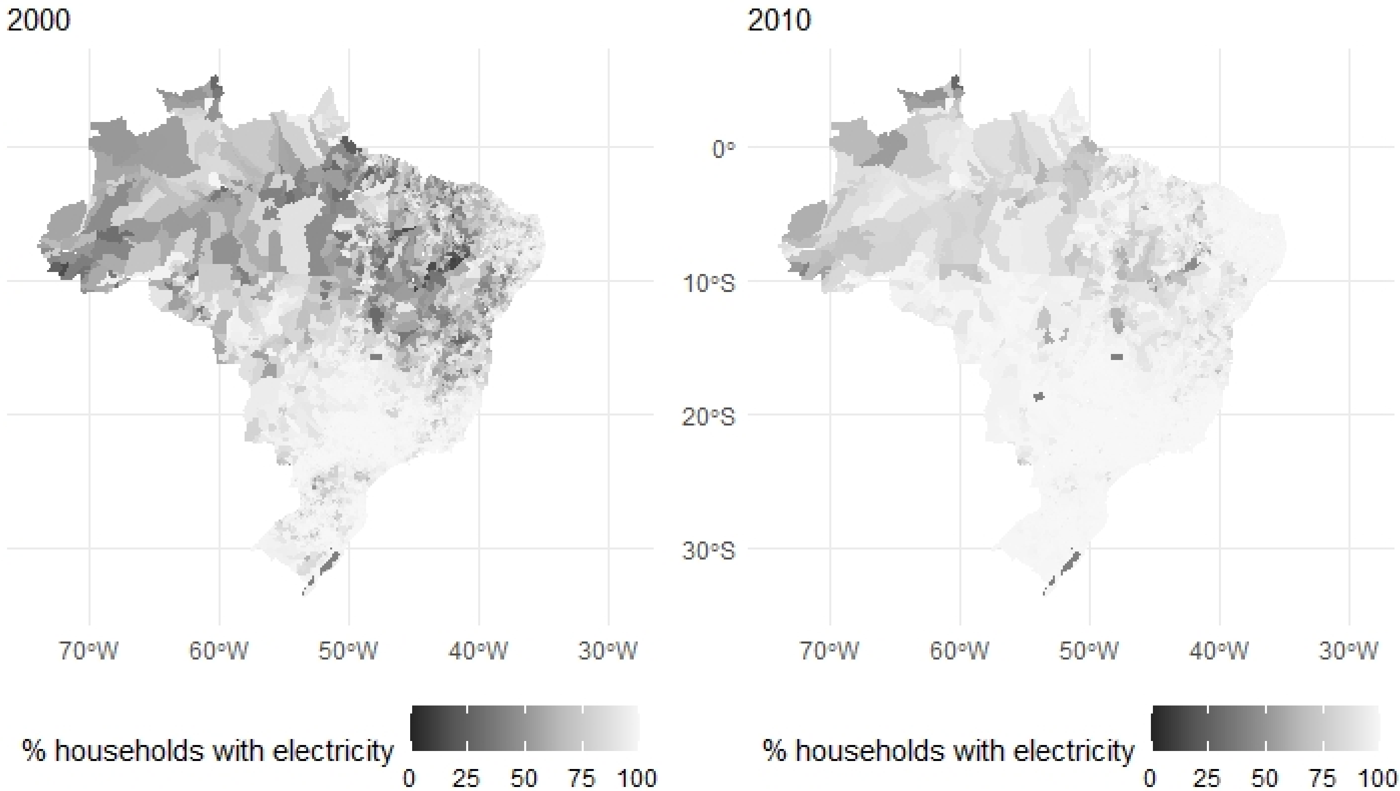
Contents

A Electrification coverage (before and after LPT) in the Brazilian territory

Figure 1: Percentage of households with electricity in the Brazilian municipalities (2000 and 2010)



Note: The authors compiled figures with data from the Brazilian Institute of Geography and Statistics (IBGE, 2000 and 2010). The unit of analysis is the municipality (N = 5,564). The dark spots indicate municipalities with low electricity coverage. The left-hand side panel shows electricity coverage in the Brazilian municipalities in 2000, whereas the right-hand one shows electricity coverage in the Brazilian municipalities in 2010.

B Descriptive statistics

B.1 Municipal-level

Table 1: Descriptive statistics - municipal-level data

Variable	Obs	Mean	Std.Dev.	Min	Max
Voter turnout (Local elections, 2000)	5,504	86.70	6.64	57.02	99.11
Number of voted parties (Local council elections, 2000)	5,504	8.35	4.31	1	30
Number of voted parties (Mayoral elections, 2000)	5,504	2.695	1.048	1	15
Number of voted parties in state parliament elections (2002)	5,558	24.15	3.787	8	30
Number of voted parties in federal parliament elections (2002)	5,558	23.34	4.043	10	30
Number of elected council members (PFL)	5,564	1.725	1.610	0	10
Number of elected council members (PMDB)	5,564	2.022	1.652	0	11
Number of elected council members (PPB)	5,564	1.248	1.504	0	12
Number of elected council members (PTB)	5,564	.8927	1.227	0	7
Is the elected mayor a member of the PT (1996)	5,564	.0210	.1434	0	1
Is the elected mayor a member of the PT (2000)	5,564	.0334	.1797	0	1
Is the elected mayor a member of the PT (2004)					
Is the elected mayor a member of the PT (2008)					
Is the elected mayor a member of the PT (2012)					
Number of elected council members (PT)	5,564	.4417	.9417	0	16
Fertility rate	5,564	2.870	0.736	1.560	7.790
Life expectancy	5,564	68.41	3.963	57.46	77.24
Child mortality rate	5,564	39.28	18.71	12.51	106.3
Human development index (HDI)	5,564	0.523	0.104	0.208	0.820
Illiteracy rate	5,564	23.56	13.51	1	63.01
Income inequality (measured by Gini index)	5,564	0.547	0.0687	0.300	0.870
Poverty rate	5,564	41.06	22.78	0.700	90.76
Unemployment rate	5,564	11.02	6.223	0	59.17
% of occupations in the formal sector	5,564	36.03	18.12	1.920	86.38
Economically active workforce	5,564	13725	91633	280	5.341e+06
Income per capita	5,564	347.2	188.1	74.95	1760
Level of urbanization	5,564	0.585	0.237	0	1
Population size	5,564	30149	183702	795	1.040e+07
% of households with electrification in 2000	5,564	86.60	17.03	10.30	100
Targeted municipalities	5,564	0.308	0.462	0	1
School attendance rate in 2010 (0-3 years old)	5,564	19.04	11.42	0	70.22
School attendance rate in 2010 (4-6 years old)	5,564	84.31	11.20	32.00	100
School attendance rate in 2010 (6-10 years old)	5,564	95.87	5.462	48.12	100
School attendance rate in 2010 (11-14 years old)	5,564	96.53	2.827	54.46	100
School attendance rate in 2010 (15-17 years old)	5,564	81.82	6.183	50.11	100
School attendance rate in 2010 (18-24 years old)	5,564	26.01	6.725	4.52	58.77
Share of votes for the PT in 2002 (presidential elections)	5,560	42.41	12.04	5.776	79.79
Share of votes for the PT in 2006 (presidential elections)	5,563	51.61	17.85	11.40	93.36
Share of votes for the PT in 2010 (presidential elections)	5,563	55.31	15.82	15.83	94.83
Share of votes for the PT in 2014 (presidential elections)	5,563	52.66	18.52	10.33	92.74
Share of votes for the PT in 2018 (presidential elections)	5,563	41.22	21.38	3.6331	93.23
Share of votes for the PT in 2022 (presidential elections)	5,563	54.38	18.21	10.34	92.14

Note: Compiled by authors with data from the Brazilian Institute of Geography and Statistics (IBGE, 2000 and 2010) Brazil's Electoral Court (Tribunal Superior Eleitoral, TSE). The unit of analysis is the municipality. The variation in the number of observations is explained by missing cases in the dataset.

B.2 Individual-level

Table 2: Descriptive statistics - Individual-level data

Variable	Obs	Mean	Std.Dev	Min	Max
Vote for PT (2006)	1,596	.7850	.4108	0	1
Vote for PT (2010)	1,845	.4878	.4999	0	1
Voter turnout	2,000	.9310	.2535	0	1
Satisfaction with democracy	2,000	.4795	.4997	0	1
Support for democracy	2,000	.8655	.3412	0	1
LPT beneficiary	2,000	.1161	.3203	0	1
PBF beneficiary	2,000	.5495	.4976	0	1
Age	1,999	4.330	1.446	1	6
Sex	2000	.483	.4998	0	1
Marital status (married vs. others)	2000	.456	.4981	0	1
Race (black versus others)	1,993	.1214	.3267	0	1
Religion (Christians vs. others)	2,000	.849	.3581	0	1
Schooling	2,000	5.038	2.275	1	10
Income (individual)	1,970	1.775	.9397	1	7
Income (household)	1,955	2.661	1.112	1	7
Party preference (PT)	2,000	.2445	.4298	0	1
Evaluation of economy	2,000	.5621	.4962	0	1
Evaluation of federal government	2,000	.4985	.5001	0	1
Evaluation of political parties	2,000	.1935	.3951	0	1
Evaluation of primary education	1,945	.3984	.4897	0	1
Evaluation of tertiary education	1,882	.4744	.4994	0	1

Note: The unit of analysis is the ESEB's survey respondent. Compiled by authors with data from the Brazilian electoral study (ESEB, 2010). The variation in the number of observations is explained by missing cases in the dataset.

C Baseline (without controls) ITT estimates using individual-level data

Table 3: The impact of the LPT on the PT's vote support by income groups

	Benefiting from the LPT			Voted PT in 2006			Voted PT in 2010		
	Full sample	Poor	Not poor	Full sample	Poor	Not poor	Full sample	Poor	Not poor
	.232***	.2457***	.1562*	.0172	.0411	-.0724	.1596***	.1756***	.0552
	(.034)	(.027)	(.082)	(.034)	(.040)	(.062)	(.035)	(.0396)	(.0752)
Obs.	1,972	1,628	384	1,576	1,325	287	1,819	1,498	358
R^2	0.186	0.197	0.184	0.044	0.037	0.143	0.058	0.062	0.091

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
The unit of analysis is the survey (ESEB) respondent. We run intention to treat estimates without controls. We cluster robust standard errors (in parentheses) at the municipal level. The first outcome variable (Benefiting from the LPT) is a dummy variable that indicates whether or not the respondent was an LPT beneficiary in 2010. The other two outcome variables are dummy variables that indicate whether respondents voted for the PT in the first round of the 2006 and 2010 Brazilian presidential elections, respectively. In our models, we treated as "Poor" those individuals below the median income according to their income self-declaration in the survey.

D FRD estimates with a quadratic polynomial fit

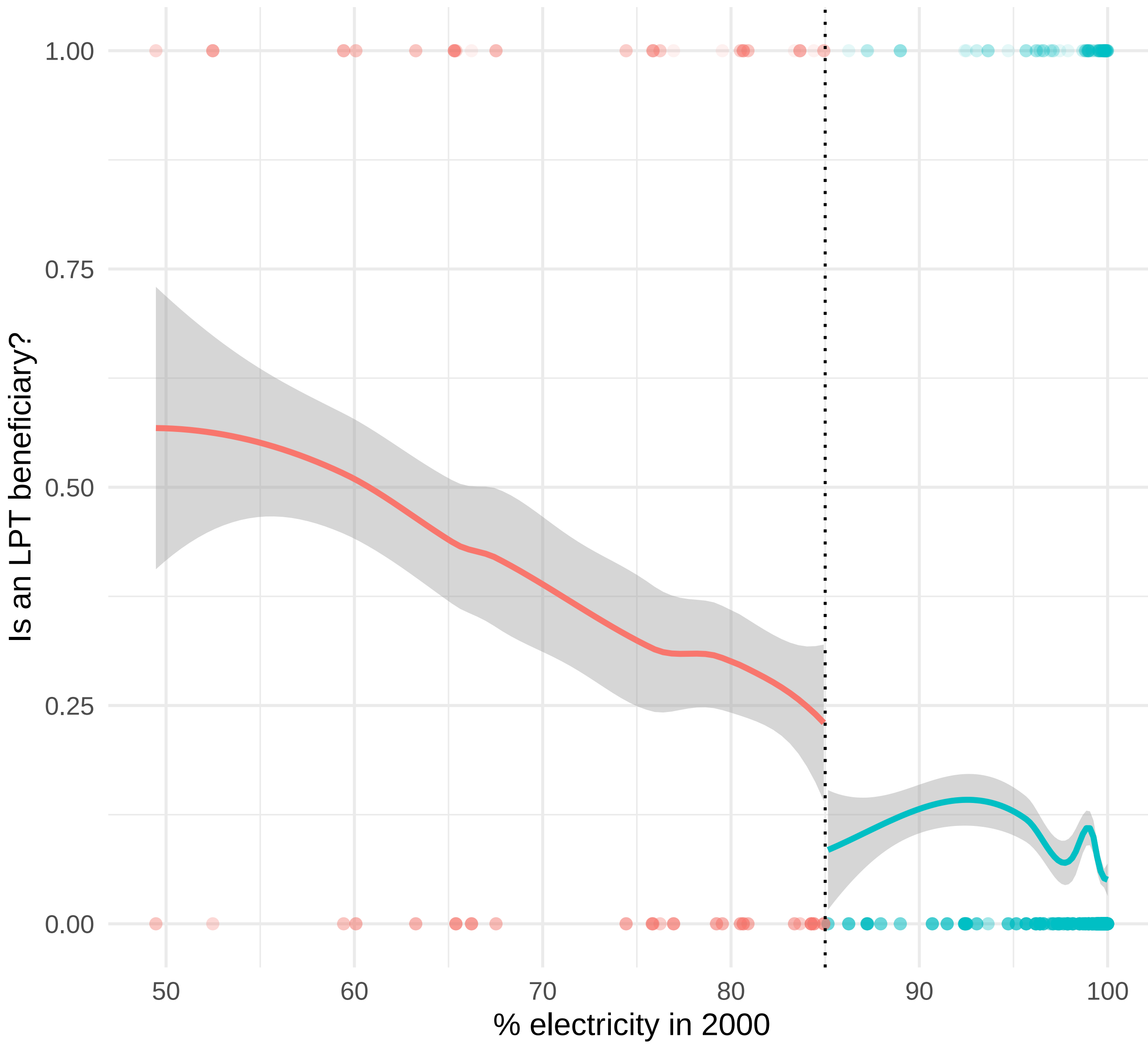
Table 4: The impact of the LPT on the PT's vote support and the perception of the quality of education

	Model 1	Model 2	Model 3	Model 4
First-stage estimates (Panel A)				
	-.224*	-.155	-.178	-.085
	(.125)	(.119)	(.162)	(.091)
Outcomes	Voted PT in 2006	Voted PT in 2010	Primary education	Tertiary education
LATE estimates (Panel B)	.483	1.93*	4.58	7.22
	(.942)	(1.20)	(4.88)	(8.17)
BW est (h)	8.43	8.81	4.70	4.42
BW bias (b)	11.8	14.2	7.52	8.98
Order est. (p)	2	2	2	2
Kernel	Triangular	Triangular	Triangular	Triangular
Obs. left of cutoff	228	266	273	251
Eff. obs. left of cutoff	105	132	96	73
N. of clusters left of cutoff	22	22	15	19
Obs. right of cutoff	1348	1553	1644	1603
Eff. obs. right of cutoff	129	151	59	57
N. of clusters right of cutoff	21	64	7	7

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
The unit of analysis is the survey (ESEB) respondent. We run RD local quadratic estimates using Calonico et al. (2020) optimal bandwidth selection. We cluster robust standard errors (in parentheses) at the municipal level. Employing the percentage of households with electricity in 2000 as our running variable, we can thus predict the probability of being an LPT beneficiary in 2010. This as-if random treatment assignment is used as an instrument for treatment status in our first-stage estimates. The outcome variable in the first stage is a dummy variable that indicates whether the respondent was an LPT beneficiary in 2010 (LPT benef.). The outcome variables in reduced form estimates are dummy variables that indicate whether respondents voted for the PT in the first round of 2006 (Model 1) and 2010 (Model 2) Brazilian presidential elections and whether the respondent informed high levels of satisfaction with the quality of public provision of primary/secondary (Model 3) and tertiary education (Model 4).

E Graphical representation of the first-stage in FRD models

Figure 2: RD plot of the first-stage in FRD models: the probability of being an LPT beneficiary given the value of the running variable – i.e., the percentage of households with electricity in 2000



Note: The unit of analysis is the survey (ESEB) respondent (N = 1,972).

F Testing for the balance of pretreatment municipal-level covariates (2000)

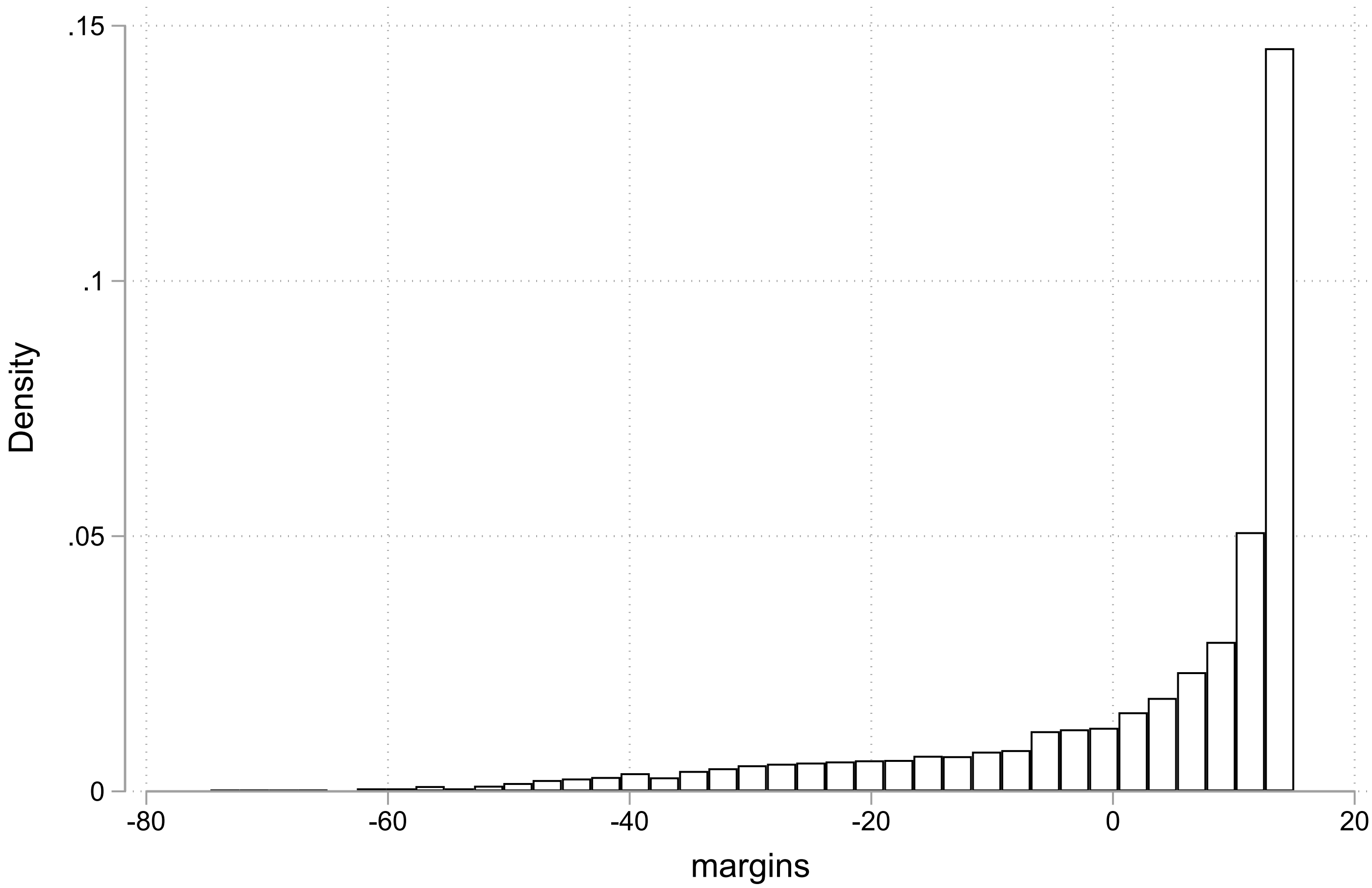
Table 5: Formal continuity-based analysis for pretreatment covariates (2000)

Variable	Coef. LATE	Std. Err.	Obs.	N. Clusters	BW est (h)
Voter turnout (Local elections, 2000)	-.270	.412	782	25	$80 \geq 85 \leq 90$
Is the elected mayor a member of the PT (1996)	-.005	.008	797	25	$80 \geq 85 \leq 90$
Is the elected mayor a member of the PT (2000)	.000	.005	797	25	$80 \geq 85 \leq 90$
Number of voted parties (Local council elections, 2000)	.070	.133	782	25	$80 \geq 85 \leq 90$
Number of voted parties (Mayoral elections, 2000)	-.052*	.026	782	25	$80 \geq 85 \leq 90$
Number of voted parties in state parliament elections (2002)	-.013	.156	795	25	$80 \geq 85 \leq 90$
Number of voted parties in federal parliament elections (2002)	.003	.166	795	25	$80 \geq 85 \leq 90$
Number of elected council members (PFL)	.049	.105	797	25	$80 \geq 85 \leq 90$
Number of elected council members (PMDB)	.047	.086	797	25	$80 \geq 85 \leq 90$
Number of elected council members (PPB)	.067	.100	797	25	$80 \geq 85 \leq 90$
Number of elected council members (PTB)	.039	.079	797	25	$80 \geq 85 \leq 90$
Number of elected council members (PT)	-.032	.034	797	25	$80 \geq 85 \leq 90$
Fertility rate	.002	.019	797	25	$80 \geq 85 \leq 90$
Life expectancy	-.209	.142	797	25	$80 \geq 85 \leq 90$
Child mortality rate	.903	.837	797	25	$80 \geq 85 \leq 90$
Human development index (HDI)	-.003	.003	797	25	$80 \geq 85 \leq 90$
Illiteracy rate	.389	.597	797	25	$80 \geq 85 \leq 90$
Income inequality (measured by Gini index)	.002	.002	797	25	$80 \geq 85 \leq 90$
Poverty rate	1.49*	.738	797	25	$80 \geq 85 \leq 90$
Unemployment rate	.072	.286	797	25	$80 \geq 85 \leq 90$
% of occupations in the formal sector	-.0205	.638	797	25	$80 \geq 85 \leq 90$
Economically active workforce	-122.1	191.5	797	25	$80 \geq 85 \leq 90$
Income per capita	-8.64*	4.97	797	25	$80 \geq 85 \leq 90$
Level of urbanization	-.003	.006	797	25	$80 \geq 85 \leq 90$
Population size	-294.7	442.6	797	25	$80 \geq 85 \leq 90$

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Compiled by authors using data from the Brazilian Institute of Geography and Statistics (IBGE, 2000), and Brazil's Electoral Court (Tribunal Superior Eleitoral, TSE). The unit of analysis is the municipality. We estimate the the Local Average Treatment Effect (LATE) using a linear model with clustered standard errors at the state level. Our LATE estimates rely on observations of our running variable (% of households with electrification in 2000) around the 85% threshold. To be conservative in our estimates, we choose BW est (h) ≥ 80 and BW est (h) ≤ 90.

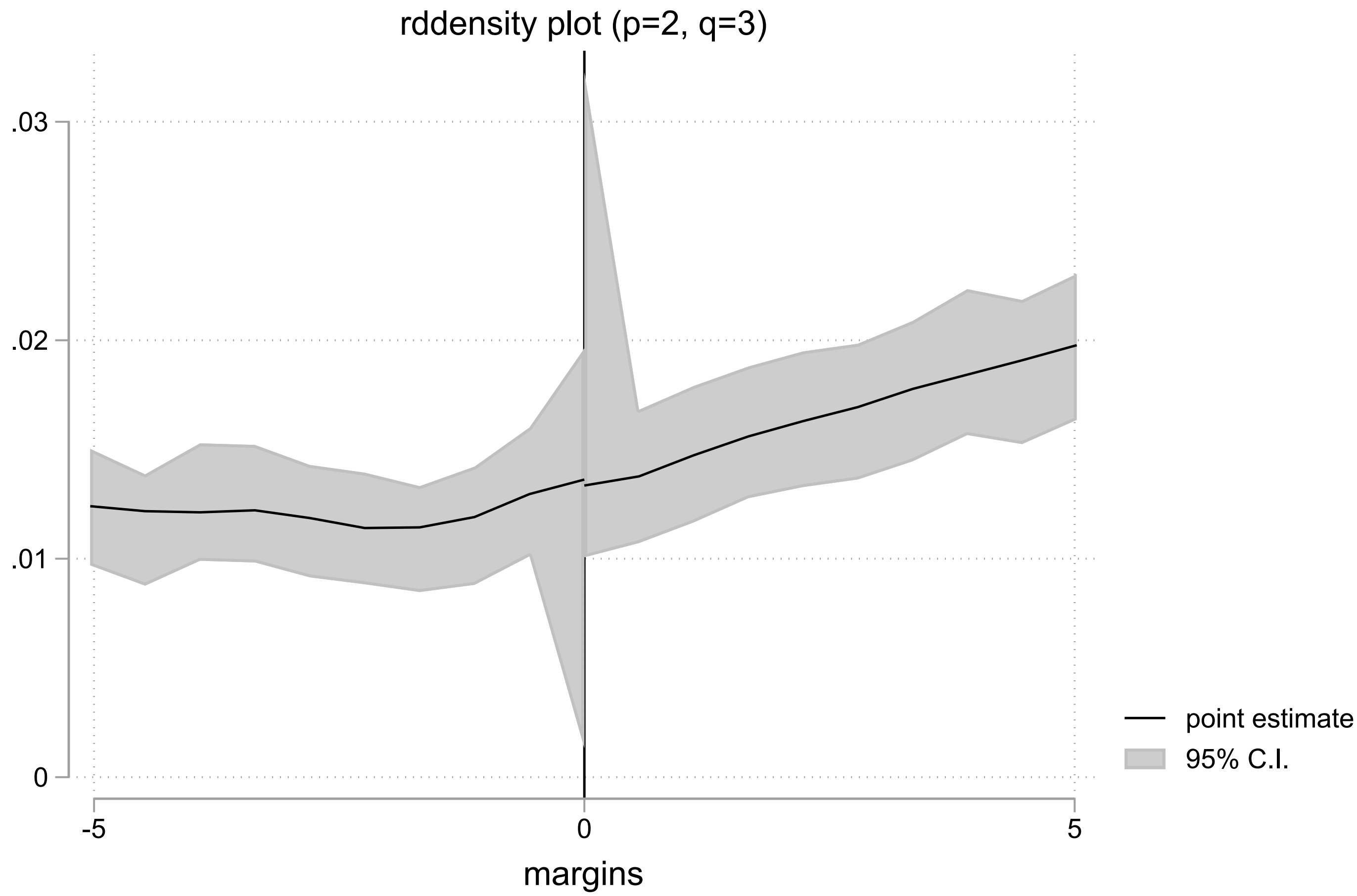
G Manipulation test based on density discontinuity

Figure 3: Histogram of the running variable



Note: Compiled by authors with data from the Brazilian Institute of Geography and Statistics (IBGE). The unit of analysis is the municipality (N = 5,564). The running variable (margins) is the percentage of households with electricity in 2000 according to the Brazilian census.

Figure 4: RD manipulation test plot



Note: Compiled by authors with data from the Brazilian Institute of Geography and Statistics (IBGE). We use the automatic manipulation test based on density discontinuity developed by Cattaneo et al. (2018). The running variable (margins) is the percentage of households with electricity in 2000 according to the Brazilian census.

Table 6: RD Manipulation test using local polynomial density estimation

Cutoff c = 0 (85)	Left of c	Right of c
Number of obs	1712	3852
Eff. Number of obs	119	137
Order est. (p)	2	2
Order bias (q)	3	3
BW est. (h)	1.680	1.674
Method	T	P>¦T¦
Robust	1.4530	0.1462
Number of obs	5564	
BW method	unrestricted	
Model	comb	
Kernel	triangular	
VCE method	jackknife	

Note: Compiled by authors with data from the Brazilian Institute of Geography and Statistics (IBGE, 2000). We use the automatic manipulation test based on density discontinuity developed by Cattaneo et al. (2018). The running variable (margins) is the percentage of households with electricity in 2000 according to the Brazilian census.

H Do municipalities governed by PT disproportionately benefited from LPT?

Table 7: Per capita number of households/facilities connected to the electrical grid through the LPT program in municipalities governed by PT versus municipalities governed by other political parties (2004-2015).

	DV: Per capita number of connections (2004)
Candidate from PT elected in the 2000 mayoral elections	.000
	(.000)
% of households with electricity (2000)	2.51
	(8.42)
% of rural population (2000)	.0015***
	(.000)
Human development index (2000)	-.0001
	(.001)
N. of obs	5564
N. clusters	27
R-squared	0.018
	DV: Per capita number of connections (2005-08)
Candidate from PT elected in the 2004 mayoral elections	-.001
	(.000)
% of households with electricity (2000)	-.000***
	(.000)
% of rural population (2000)	.0309***
	(.004)
Human development index (2000)	.0118
	(.015)
N. of obs	5564
N. clusters	27
R-squared	0.265
	DV: Per capita number of connections (2009-12)
Candidate from PT elected in the 2008 mayoral elections	-.001
	(.000)
% of households with electricity (2000)	-.001***
	(.000)
% of rural population (2000)	.0058*
	(.003)
Human development index (2000)	.001
	(.013)
N. of obs	5564
N. clusters	27
R-squared	0.391
	DV: Per capita number of connections (2012-15)
Candidate from PT elected in the 2012 mayoral elections	.000
	(.000)
% of households with electricity (2000)	-.000***
	(.000)
% of rural population (2000)	-.001***
	(.000)
Human development index (2000)	.001
	(.004)
N. of obs	5564
N. clusters	27
R-squared	0.168

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Compiled by authors with data from the Brazilian Institute of Geography and Statistics (IBGE, 2000), and the Ministry of Mines and Energy (MME, 2015). The unit of analysis is the municipality. The main independent variable assume the value of 1 if the candidate elected in the correspondent mayoral elections was affiliated to PT; and 0 otherwise. The other variables included in the OLS models are the following: the percentage of household with electricity in 2000, the percentage of households located in rural districts in 2000, and the human development index calculated for 2000.

I Testing for the balance of individual-level covariates (2010)

Table 8: Formal continuity-based analysis for individual-level covariates (2010)

	LATE	BW est (h)	Obs. left	Eff. obs left	Obs. right	Eff. obs right
Sociodemographic attributes						
Age	2.21 (2.25)	9.67	288	168	1683	156
Male	.002 (.579)	10.8	288	180	1684	204
Married	-1.520** (.405)	11.7	288	180	1684	284
Race (Black versus others)	-.857 (.601)	2.86	288	64	1677	44
Religion (Christians vs others)	.283 (.291)	9.95	288	168	1684	168
Schooling	-2.30 (2.99)	9.80	288	168	1684	168
Income (individual)	-1.471* (.831)	9.23	288	168	1655	156
Income (household)	-1.82 (1.42)	9.40	287	168	1644	154
Bolsa Família benef.	.948** (.456)	7.12	288	124	1684	88
Evaluation of economy	-1.05 (.830)	8.90	288	144	1684	156
Political attributes						
Support for democracy	-.344 (.450)	9.31	288	168	1684	156
Satisfaction with democracy	.049 (.701)	6.88	288	124	1684	88
Voter turnout	-.050 (.303)	8.01	288	124	1684	136
Party preference (Petista)	.7541 (.674)	10.5	288	188	1684	180
Evaluation of political parties	.742 (.861)	8.16	288	136	1684	148
Evaluation of federal government	-.526 (1.16)	4.13	288	76	1684	64

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Compiled by authors with data from the Brazilian electoral study (ESEB, 2010), and the Brazilian Institute of Geography and Statistics (IBGE, 2000). The unit of analysis is the survey (ESEB) respondent. We run RD local linear estimates using Calonico et al. (2020) optimal bandwidth selection. We cluster robust standard errors (in parentheses) at the municipal level, where the survey was conducted. Employing the percentage of households with electricity in 2000 as our running variable, we can thus predict the probability of being an LPT beneficiary in 2010. This as-if random treatment assignment is used as an instrument for treatment status. The outcome variable in first-stage estimates is a dummy variable that indicates whether the respondent is an LPT beneficiary in 2010 or not (LPT benef.).

J FRD estimates with district-level fixed effects

Table 9: The impact of the LPT on the PT's vote support and the perception of the quality of education

	Model 1	Model 2	Model 3	Model 4
First-stage estimates (Panel A)				
	-.179**	-.152***	-.206*	-.229***
	(.081)	(.082)	(.098)	(.044)
Outcomes	Voted PT in 2006	Voted PT in 2010	Primary education	Tertiary education
LATE estimates (Panel B)	1.07	1.66*	3.48*	2.26***
	(1.07)	(.930)	(1.96)	(.774)
BW est (h)	8.20	9.30	4.70	5.69
BW bias (b)	11.3	15.2	7.47	9.41
Order est. (p)	1	1	1	1
Kernel	Triangular	Triangular	Triangular	Triangular
Obs. left of cutoff	228	266	273	251
Eff. obs. left of cutoff	105	154	96	91
N. of clusters left of cutoff	22	22	15	22
Obs. right of cutoff	1348	1553	1644	1603
Eff. obs. right of cutoff	129	151	59	69
N. of clusters right of cutoff	12	288	7	7

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
The unit of analysis is the survey (ESEB) respondent. We run RD local linear estimates using Calonico et al. (2020) optimal bandwidth selection. We cluster robust standard errors (in parentheses) at the municipal level. Employing the percentage of households with electricity in 2000 as our running variable, we can thus predict the probability of being an LPT beneficiary in 2010. This as-if random treatment assignment is used as an instrument for treatment status in our first-stage estimates. The outcome variable in the first stage is a dummy variable that indicates whether the respondent was an LPT beneficiary in 2010 (LPT benef.). The outcome variables in reduced form estimates are dummy variables that indicate whether respondents voted for the PT in the first round of 2006 (Model 1) and 2010 (Model 2) Brazilian presidential elections and whether the respondent informed high levels of satisfaction with the quality of public provision of primary/secondary (Model 3) and tertiary education (Model 4).

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